Book of abstracts

Second International Conference

Philosophy of Science Between the Natural Sciences, the Social Sciences, and the Humanities

08 – 11 March 2016

http://www.wissphil.de/gwp2016

Duesseldorf Center for Logic and Philosophy of Science (DCLPS)
Heinrich Heine University Duesseldorf, Germany

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Edited by
Alexander Christian, Christian J. Feldbacher, Alexander Gebharter, David Hommen, Nina Retzlaff, Gerhard Schurz & Paul D. Thorn

Impressum
Heinrich Heine University Duesseldorf
Department of Philosophy (Institut fuer Philosophie)
Universitaetsstrasse 1, Geb. 24.52
40225 Duesseldorf, Germany

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Committees

Local Organizing Committee

(DCLPS: Duesseldorf Center for Logic and Philosophy of Science, Heinrich-Heine-University Duesseldorf, Germany)

Chair
Gerhard Schurz

Members
Alexander Christian
Christian J. Feldbacher
Alexander Gebharter
David Hommen
Nina Retzlaff
Paul D. Thorn

GWP Committee

President
Holger Lyre (University of Magdeburg)

Members
Uljana Feest (University of Hannover)
Ulrich Krohs (University of Münster)
Thomas Reydon (University of Hannover)

Preface

Preface by the GWP President

Dear Colleagues,

The second international conference of the GWP in Düsseldorf is a very special event. It demonstrates that it was possible to establish the GWP as a 'normally functioning' scientific society after its inauguration in 2011 and kick-off meeting in 2013 in Hanover. We started to think about having a philosophy of science society in the German-speaking community in 2009 among a group of about ten colleagues, and communicated our initiative into the broader community by contacting almost 100 colleagues via email and asking them for suggestions and feedback. My personal benchmark was that the GWP should at least be able to reach a 3-digit number of members, and I am happy to report that the member count of the GWP is already around 130. This number shows that the GWP has a broad acceptance in the community.

The GWP has several aims: to foster the interconnections between the members of the German philosophy of science community on all levels of the academic system, and to become a representative and authoritative voice of the field. The young and talented are of special interest. We initiated a program where young philosophers of science may apply for funding (mainly for conference travel and workshop organization). We already supported several projects over the last few years. Finally, the GWP seeks to maintain professional relations to all national and international societies in the field of philosophy of science. Concrete steps in this direction have already been taken in cooperation with the EPSA, the GAP and the Swiss Philosophical Society (SPS), in the two latter cases by means of joint conference symposia.

The most important instrument to fulfill the above aims is of course to have a regular international conference. We decided on a tri-annual pattern with GWP.2013 as our first and GWP.2016 as our second international

event. The preparation of GWP.2016 was shared between the executive board of the GWP and the local organizing committee in Düsseldorf. It should be mentioned that the local organizers around Gerhard Schurz, the chair of the local committee, took care of the bulk of the work and efforts. Many thanks, hence, to all members of the local organizing committee for a fruitful, efficient and always very friendly cooperation.

The general topic of the GWP.2016 is intentionally broad: "Philosophy of Science Between the Natural Sciences, the Social Sciences, and the Humanities." The GWP seeks to cover philosophy of science in all respects, but we deliberately tried to address the often-neglected philosophy of science of the social sciences and of the humanities in particular. I very much look forward to our Düsseldorf meeting and, on behalf of the GWP, want to express a warm welcome to all participants. Enjoy the conference!

Prof. Dr. Holger Lyre
University of Magdeburg
President of the GWP

Preface by the Local Organizing Committee Chair

The Duesseldorf Center for Logic and Philosophy of Science (DCLPS) has the honor to host GWP.2016, the second triennial conference of the German Society for Philosophy of Science and to constitute its LOC. The first task in a preface like this consists in giving thanks to all people and institutions without whom this event would not have been possible. Thus my first thank goes to the local organization team, which consists of Alexander Christian, Christian J. Feldbacher, Alexander Gebharter, David Hommen, Nina Retzlaff and Paul D. Thorn – I want to express my sincere thanks to the great efforts made by these people. My second thanks goes to the members of the governing board of the GWP: Holger Lyre, Uljana Feest, Ulrich Krohs and Thomas Reydon, who supported us in various valuable ways. Thirdly my thanks goes to our sponsors, the DFG, the HHU, and to publishing house Springer, as well as to the other publishing companies who contribute by means of book exhibition - not to forget the Journal of General Philosophy of Science, who will publish a selection of papers held at this conference.

The today's occasion gives me also the opportunity to briefly report on the history and activities of the DCLPS at the Heinrich Heine University (HHU) in Duesseldorf. The DCLPS would not be possible without the major German research funding organization, the DFG. With help of DFG grants I was able to enlarge my department beyond a critical threshold that eventually allowed us to regularly organize workshops and conferences on a variety of topics in logic and philosophy of science. With the support of the DFG and the HHU we were able to invite philosophers of science from all over the world into our workshops and weekly research colloquia. In 2005 we organized our first conference (on "compositionality, concepts and cognition"), which was followed by a series of workshops and conferences – on topics such as scientific realism, epistemological reliabilism, conditionals, modularity of mind, novel predictions, theory-ladenness and explanation. A long list of renowned philosophers of science gave talks at these events, including Nancy Cartwright, Steven French, Michael Friedman, Peter Gärdenfors, Clark Glymour, Alvin Goldman, Philip Kitcher, David Papineau, Stathis Psillos, John Worrall, and many others.

Stimulated by these great experiences the idea came up to establish the Duesseldorf Center for Logic and Philosophy of Science. This happened in the year 2011. With this center firmly in place we continued to flourish and expanded our activities. Besides continuing our workshop series we started to host fellows at the DCLPS. Our fellowship activities started in the year 2009 with a one semester visit of Hannes Leitgeb, being followed by fellowships of Kevin Kelly, Theo Kuipers, Jeff Pelletier, Jonah Schupbach, Clark Glymour and Christopher Hitchcock. DCLPS provides also the infrastructure for research fellows to visit our center and engage into our activities. Videos of, and other materials from, the talks and workshops are presented at the webpage of the DCLPS (http://dclps.phil.hhu.de). You are cordially invited to visit this page, on which you also find information about our research activities, publications and other Philosophy of Science centers with which we cooperate.

A first summit of our activities was the organization of EPSA15, the biannual conference of the European Philosophy of Science Association in September 2015. A second summit is the organization of GWP.2016. Last but not least I wish to thank all of you, the participants, for coming to Duesseldorf and contributing to the GWP.2016. So let us look forward to an exciting conference!

Gerhard Schurz
DCLPS, Dept. Philosophy, HHU Duesseldorf
Chair of LOC

Programme Overview

TUESDAY, Mar 08th

13:00 – 14:30	**Registration** (Foyer)
14:30 – 15:00	**Opening** (Room 3D)
15:00 – 16:15	**Plenary Lecture I**: Alexander Rosenberg (Room 3D)
16:15 – 16:45	**Refreshments** (served in foyer)
16:45 – 18:45	**Parallel Sessions I** (Rooms 3B, 3C, 3D, 22 and 24)
18:50	**Departure to Conference Dinner** (Meeting at main entrance, walking to station Christophstraße: 19:16 (U71), [for delayed persons: 19:26 (U83), 19:30 (U73)]. Exit at station Heinrich-Heine-Allee, there take exit to Bolkerstrasse.)
19:30 – 22:00	**Conference Dinner** (Brauerei *Zum Schlüssel*, Bolkerstraße 41 – 47)

WEDNESDAY, Mar 09th

09:15 – 10:30	**Plenary Lecture II**: Michela Massimi (Room 3D)
10:30 – 11:00	**Refreshments** (served in foyer)
11:00 – 13:00	**Parallel Sessions II** (Rooms 3B, 3C, 3D, 22 and 24)
13:00 – 15:00	**Lunch Break**
15:00 – 16:15	**Plenary Lecture III**: Rainer Hegselmann (Room 3D)
16:15 – 16:45	**Refreshments** (served in foyer)
16:45 – 18:45	**Parallel Sessions III** (Rooms 3B, 3C, 3D, 22 and 24)
19:00 – 20:00	**GWP Meeting**

THURSDAY, Mar 10th

09:15 – 10:30	**Plenary Lecture IV**: Paul Hoyningen-Huene (Room 3D)
10:30 – 11:00	**Refreshments** (served in foyer)
11:00 – 13:00	**Parallel Sessions IV** (Rooms 3B, 3C, 3D, 22 and 24)
13:00 – 15:00	**Lunch Break**
15:00 – 16:15	**Plenary Lecture V**: Gila Sher (Room 3D)
16:15 – 16:45	**Refreshments** (served in foyer)
16:45 – 18:45	**Parallel Sessions V** (Rooms 3B, 3C, 3D, 22 and 24)

FRIDAY, Mar 11th

09:15 – 11:15	**Parallel Sessions VI** (Rooms 3B, 3C, 3D, 22 and 24)
11:15 – 11:45	**Refreshments** (served in foyer)
11:45 – 13:00	**Plenary Lecture VI**: Stathis Psillos (Room 3D)

Programme

TUESDAY, Mar 08th

13:00 – 14:30 Foyer	**Registration**
14:30 – 15:00	Opening
Room 3D	**Opening Words** ANDREA VON HÜLSEN-ESCH (Vice-President for International Relations) HOLGER LYRE (President of GWP) GERHARD SCHURZ (Chair of the LOC)
15:00 – 16:15	Plenary Lecture I
Room 3D	**Plenary Lecture: *The Biological Character of Social Theory*** ALEXANDER ROSENBERG Chair: Paul Thorn
16:15 – 16:45	Refreshments (served in foyer)
16:45 – 18:45	Symposia & Contributed Papers I
Room 22 Symposium	**Constitution and Constitutional Discovery** Chair: Christian Feldbacher *A Theory of Constitutive Inference for the Regularity Account of Mechanistic Constitution* JENS HARBECKE *Bayesian Constitutional Discovery* MICHAEL BAUMGARTNER & LORENZO CASINI *Uncovering Constitutive Relevance Relations in Mechanisms* ALEXANDER GEBHARTER

Room 24 Symposium	**From Genetics to Culture – Lines, Gaps and Bridges** Chair: Maria Kronfeldner *Epigenetics and the Explanation of Development: The Mirage of Moving Beyond Reductionism* FRANCESCA MERLIN *Possible Limits of Reductive Explanations* CHRISTIAN SACHSE *Information and the Evolution of Social Preferences* CÉDRIC PATERNOTTE
Room 3B	**Philosophy of the Natural Sciences I** Chair: Mathias Frisch *Quantum Gravity: An Ideology of Unification?* KIAN SALIMKHANI *Physical Composition as Bonding* JULIAN HUSMANN & PAUL NÄGER *Antecedent-Strengthening and Ceteris Paribus Laws* CARSTEN HELD
Room 3C	**Philosophy of the Life Sciences I** Chair: Gottfried Vosgerau *Pitfalls and Perspectives in Comparative Psychology* TERESA BEHL *What are Organizational Principles in Contemporary Brain Mapping?* PHILIPP HAUEIS *The Stimulus Perception Connection* PAUL D. THORN (NEW SLOT: Confirmation, Friday, March 11, 09:15-09:55, room 3C)

Room 3D	**General Philosophy of Science I** Chair: Peter Brössel *Emergence for Better Best System Laws* MARKUS SCHRENK *Past Realists Thought the Same …* LUDWIG FAHRBACH *Some Do's and Don'ts of Defining Empirical Significance: A Carnapian Analysis* JONATHAN SUROVELL
18:50	**Departure to Conference Dinner** Meeting at main entrance, walking to station Christophstraße: 19:16 (U71), [for delayed persons: 19:26 (U83), 19:30 (U73)]. Exit at station Heinrich-Heine-Allee, there take exit to Bolkerstrasse.
19:30 – 22:00	**Conference Dinner** Brauerei *Zum Schlüssel*, Bolkerstraße 41 – 47

WEDNESDAY, Mar 09th

09:15 – 10:30	Plenary Lecture II
Room 3D	**Plenary Lecture: *A (Possibly) Even Better Best System Account of Lawhood*** MICHELA MASSIMI Chair: Uljana Feest
10:30 – 11:00	Refreshments (served in foyer)
11:00 – 13:00	Symposia & Contributed Papers II
Room 24 Symposium	**Absences, Deficiencies and Malfunctions in Biological and Medical Explanations** Chair: Thomas Reydon *Functions, Malfunctioning, and Negative Causation* LUDGER JANSEN *The Quantitative Problem for Theories of Function and Dysfunction* THOMAS SCHRAMME *Speciesism, Species Norm and the Lack of Species-Typical Traits in Moral Argumentation* PETER MCLAUGHLIN
Room 3B	**Philosophy of the Natural Sciences II** Chair: Michael Stoeltzner *The Role of the Concept of Causation in Physics* ENNO FISCHER *Causality in General Relativity. "Partial Determination" Revisited* ANDREA REICHENBERGER *Quantum Mechanics and Retro-Causation* MATHIAS FRISCH

Room 3C	**Philosophy of the Life Sciences II** Chair: Marie Kaiser *On Life's Dual Nature: Complex Systems Dynamics and Gene-Centeredness* ALEXIS DE TIÈGE *The Philosophical Concept of Agency between Systems Biology and Artificial Intelligence* ANNE S. MEINCKE *Teleosemantics and the Meaning of Adaptation* HAJO GREIF
Room 3D	**General Philosophy of Science II** Chair: Markus Schrenk *From Ontological Interaction, to Epistemic Integration and Integrative Pluralism* HARDY SCHILGEN *Scientific Pluralism and its Trade-Offs* RICO HAUSWALD *The Perspective of the Instruments: Mediating Intersubjectivity* BAS DE BOER
Room 22	**History of Philosophy of Science** Chair: Helmut Pulte *Kant's Views on Preformation and Epigenesis* INA GOY *Theoretical Construction in Physics: The Role of Leibniz for Weyl's 'Philosophie der Mathematik und Naturwissenschaft'* NORMAN SIEROKA (CANCELLED)

	The Vibe around 1930: Scientism and Political Philosophy of Science MARKUS SEIDEL
13:00 – 15:00	Lunch Break
15:00 – 16:15	Plenary Lecture III
Room 3D	**JGPS Lecture: *Thomas C. Schelling and James M. Sakoda – How to Become an Unknown Pioneer?*** RAINER HEGSELMANN Chair: Ulrich Krohs
16:15 – 16:45	Refreshments (served in foyer)
16:45 – 18:45	Symposia & Contributed Papers III
Room 24 Symposium	**The Relation between Philosophy of Science and Philosophy of Engineering after the Practice Turn** Chair: Rafaela Hillerbrand *What is a Philosophy of Science for the Engineering Sciences?* MIEKE BOON *Internalism and Externalism in the Philosophy of Engineering* PETER KROES *A Causal Perspective on Modeling in the Engineering Sciences* WOLFGANG PIETSCH
Room 3B	**Philosophy of the Natural Sciences III** Chair: Richard Dawid *The Fine-Tuning Argument for the Multiverse Under Attack* SIMON FRIEDERICH *Coordination, Measurement, and the Problem of Representation of Physical Quantities* FLAVIA PADOVANI

	Holism of Climate Models and their Construction with Empirical Data and Theoretical Knowledge RISKE SCHLÜTER
Room 3C	**Philosophy of the Life Sciences III** Chair: Jens Harbecke *Disease Entities, Negative Causes of Disease, and the Naturalness of Disease Classifications: Some Philosophical Problems of Scientific Medicine* PETER HUCKLENBROICH *The (Dys)functionality of Psychopathy: Perspective from the Philosophy of Science* MARKO JURJAKO *Mental Disorders as Higher-Order Theoretical Terms* GOTTFRIED VOSGERAU
Room 3D	**General Philosophy of Science III** Chair: Carsten Held *Agent-Based Modeling and Democratic Theory: Improving Normative Arguments through Simulation* SIMON SCHELLER *The Synchronized Aggregation of Beliefs and Probabilities* CHRISTIAN J. FELDBACHER *Exploratory Modes of Scientific Inquiry: From Experimentation to Modeling* AXEL GELFERT

Room 22	**Causality** Chair: David Hommen *Interventions or Ranks?* TOBIAS HENSCHEN *Causal Modelling and the Metaphysics of Causation* VERA HOFFMANN-KOLSS (CANCELLED) *Is There A Monist Theory of Causal and Non-Causal Explanations? The Counterfactual Theory of Scientific Explanation* ALEXANDER REUTLINGER
19:00 – 20:00 Room 3D	GWP Meeting

THURSDAY, Mar 10th

09:15 – 10:30	Plenary Lecture IV
Room 3D	**Plenary Lecture: *Are there Good Arguments Against Scientific Realism?*** PAUL HOYNINGEN-HUENE Chair: Thomas Reydon
10:30 – 11:00	Refreshments (served in foyer)
11:00 – 13:00	Symposia & Contributed Papers IV
Room 24 Symposium	**Methodological Challenges in Quantum Gravity** Chair: Wolfgang Pietsch --- *The Use of Black Hole Thermodynamics as Non-Empirical Confirmation* CHRISTIAN WÜTHRICH *On Predictions and Explanations in Multiverse Scenarios* KEIZO MATSUBARA *Can We Make Sense of the Final Theory Claim in String Theory?* RICHARD DAWID
Room 3B	**Values in Science I** Chair: Cornelis Menke --- *Agnotological Challenges: How to Capture the Production of Ignorance* MARTIN CARRIER *The Suppression of Medical Evidence* ALEXANDER CHRISTIAN *Die Ethik des Plagiierens / The Ethics of Plagiarizing* LEONHARD MENGES (CANCELLED)

Room 3C	**Philosophy of the Life Sciences IV** Chair: Anne S. Meincke *Evolutionary Explanations* SUSANNE HIEKEL *Types of Environments and Multi-Level Natural Selection* CIPRIAN JELER *On the Explanatory Character of the Serial Endosymbiotic Theory of the Origin of Eukaryotic Cells* JAVIER SUÁREZ & ROGER DEULOFEU
Room 3D	**General Philosophy of Science IV** Chair: Ludwig Fahrbach *Reflective Equilibrium – A Method for Philosophy of Science?* CLAUS BEISBART *How Theories Travel: The Case of 'The Theory of Games and Economic Behavior'* CATHERINE HERFELD & MALTE DOEHNE *Different Solutions to the Problem of Conflicting Reference Classes and their Application to Personalized Medicine* CHRISTIAN WALLMANN
Room 22	**Mechanisms** Chair: Alexander Gebharter *Mechanisms: A Curious Trinity?* LENA KÄSTNER (CANCELLED) *Empirically Assessing Mechanistic Constitution With Interventions* BEATE KRICKEL *Viewing Marr as a Mechanist* CARLOS ZEDNIK

13:00 – 15:00	Lunch Break
15:00 – 16:15	Plenary Lecture V
Room 3D	**Plenary Lecture: *Truth and Scientific Change*** GILA SHER Chair: Gerhard Schurz
16:15 – 16:45	Refreshments (served in foyer)
16:45 – 18:45	Symposia & Contributed Papers V
Room 24 Symposium	**Evidence of Mechanisms in Medicine** Chair: Beate Krickel *Evidence of Mechanisms in Medicine* MICHAEL WILDE *Rethinking the Epistemic Significance of Mechanisms* ALEXANDER MEBIUS (CANCELLED) *Defending the Epistemic Significance of Mechanisms* VELI-PEKKA PARKKINEN
Room 3B	**Values in Science II** Chair: Alexander Christian *Cognitive Interests and Scientific Objectivity* TORSTEN WILHOLT *Is the Argument from Inductive Risk Applicable to Pure Research?* CORNELIS MENKE *Values in Species Classification* STIJN CONIX
Room 3C	**Philosophy of the Life Sciences V** Chair: Mieke Boon *Explaining Human Behaviour, Changing Human Behaviour: How to be an Evolutionary Social Constructionist* JESSICA LAIMANN

	Natural and Social Kinds: Overlaps and Distinctions ZDENKA BRZOVIC & PREDRAG SUSTAR *The Biological Reality of Race Does Not Underwrite the Social Reality of Race: A Response to Spencer* KAMURAN OSMANOGLU
Room 3D	**General Philosophy of Science V** Chair: Claus Beisbart *Why the Psychology of Reasoning Needs Normativity: The Complex-First-Paradox* PETER BRÖSSEL & NINA POTH *Predictive Coding and the Rationale of the Conjunction Fallacy* BENJAMIN HORRIG & PETER BRÖSSEL *Normativity in the Philosophy of Science* MARIE I. KAISER
Room 22	**Philosophy of the Social Sciences and the Humanities I** Chair: Rafaela Hillerbrand *Reconsidering the "Experimental Turn" in the Humanities* EVA-MARIA JUNG *From Stability to Validity: How Standards Serve Epistemic Ends* LARA HUBER *Human Nature Between Science and Politics: Dehumanization, Essentialism and the Call for Elimination* MARIA KRONFELDNER

FRIDAY, Mar 11th

09:15 – 11:15	Symposia & Contributed Papers VI
Room 24 Symposium	**Symposium: Mathematics as a Tool** Chair: Nina Retzlaff *Empirical Bayes as a Tool* ANOUK BARBEROUSSE *Mathematics in the Era of Big Data is not the Tool of Science, but the Science of Tools* Juergen Jost *Boon and Bane: On the Role of Adjustable Parameters in Simulation Models* JOHANNES LENHARD
Room 3B	**Philosophy of Mathematics** Chair: Annika Schuster *Gödel on Intuitionistic Logic, and Davidsonian Radical Interpretation: The Case of the Logical Constants* FABRICE PATAUT *Hilbert's Axiomatic Method and Carnap's General Axiomatics* MICHAEL STOELTZNER *Recognition Procedures and Dag Prawitz's Theory of Grounds* ANTONIO PICCOLOMINI D'ARAGONA
Room 3C	**Confirmation** Chair: Torsten Wilholt *Of German Tanks and Scientific Theories: Estimating the Number of Unconveived Alternatives* BURKAY OZTURK (CANCELLED)

	Defending Selective Confirmation Strategy YUKINORI ONISHI *Qualitative Research and Evidential Support* Corrado Matta (CANCELLED)
Room 3D	**General Philosophy of Science VI** Chair: Hajo Greif *Toulmin's Logical Types* DAVID BOTTING *Goodman's Paradox and Hansson's Puzzle* WOLFGANG FREITAG *Why Coherence Cannot be Measured as Relative Overlap* JAKOB KOSCHOLKE
Room 22	**Philosophy of the Social Sciences and the Humanities II** Chair: Eva-Maria Jung *The Role of "Ought" in Value Theory: Philosophical and Sociological Perspectives* ELIZAVETA KOSTROVA *The "Invisible Hand" as a Natural Law* JUDITH WÜRGLER *Micro Economics Between the Natural Sciences and the Humanities* KARSTEN K. JENSEN
16:15 – 16:45	Refreshments (served in foyer)
11:15 – 11:45	Lunch Break
15:00 – 16:15	Plenary Lecture VI
Room 3D	**Plenary Lecture: *Induction and Natural Necessities*** STATHIS PSILLOS Chair: Holger Lyre

Abstracts

Plenary Lectures

Plenary Lecture I	**Plenary Lecture**
Chair: Paul Thorn	Room 3D, Tuesday 15:00 – 16:15

The Biological Character of Social Theory

ALEXANDER ROSENBERG
Duke University
alexrose@duke.edu

The social science need to take seriously their status as divisions of biology. As such they need to recognize the central role of Darwinian processes in all the phenomena they seek to explain. An argument for this claim is formulated in terms of a small number of relatively precise premises that focus on the nature of the kinds and taxonomies of all the social sciences. The analytical taxonomies of all the social sciences are shown to require a Darwinian approach to human affairs, though not a nativist or genetically driven theory by any means. Non-genetic Darwinian processes have the fundamental role on all human affairs. I expound a general account of how Darwinian processes operate in human affairs by selecting for strategies and sets of strategies individuals and groups employ. I conclude by showing how a great deal of social science can be organized in accordance with Tinbergen's approach to biological inquiry, an approach required by the fact that the social sciences are all divisions of biology, and in particular the studies of one particular biological species.

Plenary Lecture II **Plenary Lecture**
Chair: Uljana Feest Room 3D, Wednesday 09:15 – 10:30

A (Possibly) Even Better Best System Account of Lawhood

MICHELA MASSIMI
The University of Edinburgh
michela.massimi@ed.ac.uk

Two questions have catalyzed the debate surrounding laws of nature: do laws govern nature or not? And what is the fundamental ontology of nature compatible with laws and their (governing or not) role? David Lewis famously laid down an influential approach to these two questions. On his account, laws do not govern nature because nomic facts reduce to non-nomic facts about natural properties.

In what follows, I build on van Fraassen's objection against what he aptly called Lewis's eschatology of science and argue for the need of rethinking Lewis's Best System Account of Lawhood. I will be looking at some prime candidates for Lewisian natural properties and laws of nature and defend an elaborated variant of what Cohen and Callender have called a Relativised Mill-Ramsey-Lewis Best System. More precisely, I will be arguing for a Perspectival Mill-Ramsey-Lewis Best System that can vindicate Humean ontology without buying into any far-fetched eschatology of science.

Plenary Lecture III **JGPS Lecture**
Chair: Ulrich Krohs Room 3D, Wednesday 15:00 – 16:15

Thomas C. Schelling and James M. Sakoda – How to Become an Unknown Pioneer?

RAINER HEGSELMANN
University of Bayreuth
Rainer.Hegselmann@uni-bayreuth.de

Schelling's model has become a classical reference in many scientific contexts: explanation of residential segregation, unintended consequences, micro-macro relations, clustering, social phase transitions, invisible hand explanations, and emergence of spontaneous order. The model has also become a paradigmatic case for the study of mechanisms and the discussion of the status of models in general. Schelling's model is often considered as the earliest and pioneering example for an agent-based computer simulation.

Without any doubt, Schelling's model is a wonderful model: It is very simple, it generates surprising results, that *ex post* can easily be understood.

However, there is a model, developed by James Minoru Sakoda, that is much more general, much more flexible, and generates much more surprising results. In a certain sense it is fair to say that Sakoda's model contains Schelling's model as an instance. And even more, Sakoda's model was developed decades earlier than Schelling's – a first version already by the end of the 1940s. In the 1970s Sakoda's model was a well-known pioneering model in the small but growing community of computational social scientists.

But today Sakoda and his model is basically forgotten. How could that happen? The answer to that question is a thrilling story, but it is not a thriller. Something went wrong, but nobody did something wrong. The talk will solve the puzzling case.

Plenary Lecture IV **Plenary Lecture**
Chair: Thomas Reydon Room 3D, Thursday 09:15 – 10:30

Are there Good Arguments Against Scientific Realism?

PAUL HOYNINGEN-HUENE
University of Hannover
hoyningen@ww.uni-hannover.de

In the talk, I shall discuss and evaluate some arguments for scientific realism. In fact, I shall widen the range of positions to be discussed by including structural realism and other forms of selective realism. The starting point of my discussion will be the no-miracle argument. For many realists, it is still the most powerful argument for selective realism if put in its strongest form, namely, by cashing out scientific success in terms of use-novel predictions. The fundamental intuition of many realists is the following. Consider those aspects of the theory in question that are responsible for the successful use-novel predictions. What is more likely: that those aspects are (at least) approximately true or that they are radically false? It appears that only aspects that are at least approximately true are capable of producing correct use-novel predictions, otherwise the predictive success is unexplainable (a miracle). However, we can produce model situations in which successful use-novel predictions are by no means indicators for approximate truth, and in fact, even multiple consecutive successful use-novel predictions are not. Are these model situations sufficiently similar to real situations in science such they invalidate the realist's intuition?

My second main topic will be the discussion of the main strategy that many selective realists (of various kinds) used in recent times. Roughly speaking, this strategy suggests identifying those aspects of theories that are responsible for their successes (especially their correct use-novel predictions) and, at the same time, have survived scientific revolutions. Those aspects that are then the prime candidates for (approximate) truth. In this strategy, historical stability across revolutions – basically an empirical argument – is blended with the no-miracle argument, an inference to the best explanation. In recent years, a lively debate has developed about concrete historical cases, and some modifications and refinements have been demanded for the strategy to be a successful defense of selective realism.

However, the whole logic behind this strategy can be questioned. Given doubts about the strength of the no-miracle argument, how suggestive for realism is the continuity of some aspects of theories trough scientific revolutions? Could this continuity have an equally plausible explanation that does not endow those aspects with a realistic status?

Plenary Lecture V **Plenary Lecture**
Chair: Gerhard Schurz Room 3D, Thursday 15:00 – 16:15

Truth and Scientific Change

GILA SHER
University of California, San Diego
gsher@ucsd.edu

In this talk I seek to examine how scientific change affects our conception of truth and how a new approach to truth might help us solve problems arising from scientific change. Among the challenges scientific change poses for truth are incommensurability and pessimistic meta-induction. An influential solution to the second challenge is “approximate truth”, but this idea is fraught with difficulties. To avoid these difficulties I will propose a new, dynamic, conception of truth, based on recent work on “manifold” correspondence.

Plenary Lecture VI **Plenary Lecture**
Chair: Holger Lyre Room 3D, Friday 11:45 – 13:00

Induction and Natural Necessities

STATHIS PSILLOS
University of Athens
psillos@phs.uoa.gr

Those philosophers who believe that there are necessary connections in nature take it that an advantage of their belief is that the problem of induction is solved. In this talk I examine and refute the arguments necessitarians employ to show that if natural necessities are posited, then there is no problem of induction. As will be explained, there are two models of natural necessity. The first takes it that there is a relation of contingent necessitation between distinct properties F and G. The second model takes it that the necessitating relation between F and G is such that it is metaphysically impossible that F is instantiated without G being instantiated. Though the conceptions are importantly different, they both rely on the same strategy to 'solve' the problem of induction, viz., on the argument that positing relations of necessitation is the best explanation of observed past regularities and at the same time the ground for the future extendability of the regularity. I will first discuss David Armstrong's explanationist 'solution' to the problem of induction. Then I will go in detail into the claim that natural necessity is the best explanation of observed regularity. Finally, I will criticize Brian Ellis's dispositional essentialist 'solution' and Howard Sankey's attempt to blend dispositional essentialism and explanationism.

Symposia & Contributed Papers I

Constitution and Constitutional Discovery **Symposium**
Organizer: Michael Baumgartner & Lorenzo Casini
Chair: Christian Feldbacher Room 22, Tuesday 16:45 – 18:45

A Theory of Constitutive Inference for the Regularity Account of Mechanistic Constitution

JENS HARBECKE
University of Witten/Herdecke
jens.harbecke@uni-wh.de

Bayesian Constitutional Discovery

MICHAEL BAUMGARTNER
University of Geneva
michael.baumgartner@unige.ch

LORENZO CASINI
University of Geneva
lorenzodotcasini@gmail.com

Uncovering Constitutive Relevance Relations in Mechanisms

ALEXANDER GEBHARTER
University of Duesseldorf
alexander.gebharter@phil-fak.uni-duesseldorf.de

General Description

Theories of mechanistic explanation are gaining popularity among philosophers of science (Machamer et al., 2000; Bechtel and Abrahamsen, 2005; Glennan, 2002; Craver, 2007; Harbecke, 2010; Hedstrøm and Swedberg, 1998; Steel, 2008; Hedstrøm and Ylikoski, 2010). A mechanistic explanation aims to account for an explanandum phenomenon occurring on the macro level by reference to its constituting entities, which engage in causal activities on the micro level (e.g. Machamer et al. 2000; Bechtel and Abrahamsen 2005). The explanation is typically conveyed by pictures or diagrams, which may or may not be associated with quantitative information (Casini et al.,

2011; Clarke et al., 2014; Gebharter, 2014; Gebharter and Schurz, 2014; Casini, 2015; Gebharter, 2015a). As constitution is the core dependence relations exploited by mechanistic explanations, theories of mechanistic explanation require a theory of constitution as well as a method for discovering constitutional relations.

It is widely agreed that constitution is a non-causal form of dependence (but see Leuridan, 2012). Causation holds among mereologically independent entities and is unidirectional. By contrast, constitution is a part-whole relation and is bidirectional (Craver and Bechtel, 2007). It is also clear that, as there are many spatiotemporal parts of phenomena that do not constitute them, parthood is only necessary but not sufficient for constitution. Apart from this consensus, however, opinions on how to best analyze constitution diverge considerably. The best-known theory of mechanistic constitution is Craver's (2007) mutual manipulability (MM) theory. In short, MM states that the behavior of a spatiotemporal part of a phenomenon constitutes that phenomenon iff it is possible to (ideally) intervene on the part such that the phenomenon changes, and on the phenomenon such that the part changes. As such, MM entails a straightforward experimental protocol for constitutional discovery, based on an ideal top-down intervention and an ideal bottom-up intervention. However, MM has been criticized—for various reasons—by a number of authors, for instance, by Harbecke (2010), Couch (2011), Leuridan (2012), Glauer (2012, §3.3.2), Schindler (2013), Baumgartner and Gebharter (2015), Baumgartner and Casini (2015).

The conceptual debate on the adequacy of different theories of constitution has sparkled corresponding methodological debates on how to uncover constitutional relations. Harbecke (2015) develops a regularity theoretic methodology of constitutional discovery that is designed in parallel to Mill's famous method of difference of causal discovery. As an alternative to MM, Baumgartner and Casini (2015) propose an abductive methodology of constitutional discovery based on a special type of interventions. Gebharter (2015c) exploits available procedures for causal discovery, supplemented with information about spatiotemporal relations.

This symposium will bring together researchers working at the interface between the aforementioned conceptual and methodological issues, with the aim of advancing the philosophical understanding of constitution and the methods for constitutional discovery. Each talk will be concerned with

operationalizing a different intuition on constitution into a method for constitutional discovery by implementing experimental evidence of regularities (first talk), considerations of how to best explain how models behave under variable set expansions (second talk), and nonexperimental probabilistic evidence (third talk).

Abstracts

1. Jens Harbecke: *A Theory of Constitutive Inference for the Regularity Account of Mechanistic Constitution*

This paper focuses on the role of interventions for constitutional inference. In contrast to the second talk in this symposium, it argues that experimental evidence can conclusively warrant constitutional inference. In particular, the question of constitutive discovery is investigated within the framework of a regularity account of mechanistic constitution (cf. Harbecke 2010; Couch 2011). Its main tool is an adaptation of Mill's method of difference of causal discovery.

In a first step, a case of neuroscientific research is reviewed that has become a standard reference in the debate on mechanistic explanation, namely research on the neural basis of spatial memory acquisition in rats (cf. Churchland and Sejnowski 1992, ch. 5; Craver and Darden 2001, 115-119; Craver 2002, sec. 2; Bickle 2003, chs. 3-5; Craver 2007, 165-170). It is argued that the hypotheses about various constitution relations between the neural components and activities and the cognitive phenomenon of spatial memory are at the heart of the explanation.

In a second step, the regularity account of mechanistic constitution is introduced. It is shown how the theory analyses the logical and conceptual structure of the constitution hypotheses mentioned in the example of spatial memory research. As its main contribution, the paper develops and discusses at set of inference rules that allow to establish constitutive regularities in light of certain kinds of empirical evidence. The inference rules are based on difference tests and tests-of-four, both of which alternate a specific set of factors in a range of test conditions and record the occurrence or non-occurrence of an investigated phenomenon.

It is emphasized that these tests require satisfaction of a homogeneity condition which excludes the influence of confounders on the observed test results. Accepting the truth of this condition carries an ineliminable inductive risk, and an example of the failure to identify its falsity is provided.

Moreover, it is pointed out that the methodology presupposes that all logically possible combinations of the tested factors are instantiable. But since constituents of macro-phenomena are often themselves connected by causal relations, only a limited number of combinations is usually testable. A potential solution to this problem is discussed.

As its overall conclusion, the paper argues that the extended inference rules can be considered as forming the basis of a general methodology used in neuroscientific research.

2. Michael Baumgartner & Lorenzo Casini: *Bayesian Constitutional Discovery*

This paper proposes a method for constitutional discovery by combining standard Bayesian network (BN) methods for causal discovery (Spirtes et al., 2000; Pearl, 2000) with recent methodological developments in constitutional discovery. Differently from the first talk in this symposium, it endorses the results in (Baumgartner and Gebharter, 2015; Baumgartner and Casini, 2015) indicating that there cannot exist conclusive experimental evidence for constitution, and takes constitutional inference to be of inherently abductive nature.

BN methods for causal discovery presuppose that a set of analyzed variables contains no semantically, logically, or constitutionally related variables, so that all recovered dependencies are amenable to a causal interpretation. Such methods often rely on a faithfulness assumption, which guarantees that observed probabilistic dependencies are due to causal relations, and on the use of interventions for producing probabilistic evidence, from which causal relations are recovered. By contrast, there are no well-developed procedures for constitutional discovery in variable sets including not only causally but also constitutionally related variables.

Baumgartner and Casini (2015) have recently offered a no decoupling theory of constitution (NDC), which exploits the unbreakable common-cause coupling of phenomena and their constituents. NDC provides an analysis of constitutional relevance as well as a normative guideline for constitutional discovery. Constitutional discovery must proceed by progressive expansions of analyzed variable sets to check whether previously uncovered common-cause couplings can be broken. If and only if these attempts fail, constitutional inference is (abductively) justified. However, NDC has not yet

been methodologically operationalized, and its connections to traditional BN causal methods remain unexplored.

The present paper fills this gap by operationalizing NDC in the framework of recursive Bayesian networks (RBNs) (Casini et al., 2011). Based on the so-called recursive causal Markov condition (RCMC), RBNs allow the probabilistic representation of mechanisms. However, at present, they are not serviceable to constitutional discovery, as they lack a notion of an intervention that can produce evidence for constitution and, related, a notion of faithfulness that warrants a constitutional inference based on such interventions. We use NDC to turn RBNs into a probabilistic framework for constitutional discovery, in two steps.

First, we define the notion of a do*-intervention, which differs from Pearl (2000)'s notion of a do-intervention, in that it does justice to the unavoidable fat-handedness of manipulations of constitutionally related variables, and we introduce a new faithfulness assumption applicable to mechanisms. Second, we operationalize Bayesian constitutional inference based on the impossibility of violating RCMC under do*-interventions across variable set expansions. The procedure halts when repeated failures in finding violations warrant an abductive inference to constitution between phenomena and their constitutional sets.

3. Alexander Gebharter: *Uncovering Constitutive Relevance Relations in Mechanisms*

The goal of this paper is to investigate new ways in which one can account for constitutive relevance relations in mechanisms that do not share the problems (see, e.g., Leuridan, 2012; Baumgartner and Gebharter, 2015) that come with Craver's (2007) mutual manipulability approach to constitution. Contrary to the second talk in this symposium, I suggest a discovery method not relying on experiments and interventions, whose interaction with variables standing in supervenience relationships is still not very well understood (see, e.g., Baumgartner, 2013; Gebharter, 2015b; Woodward, 2015); contrary to the first talk in this symposium, the search procedure I suggest does not require strict regularities. In particular, I will explore in how far it is possible to uncover constitutive relevance relations on the basis of non-experimental data.

I start this paper by briefly introducing the characteristic marks of constitutive relevance relationships. I then present the central notions and axioms of the causal Bayes net formalism (Spirtes et al., 2000; Pearl, 2000) and argue that this formalism can also be used to model constitutive relevance. In particular, I argue that constitutive relevance relations share the same formal properties as direct causal relations represented by the arrows of a causal Bayes net. If this diagnosis is correct, then standard search procedures for causal relations should also be applicable to uncover constitutive relevance relations. I present a standard algorithm for causal discovery: the PC algorithm, which was developed by Peter Spirtes and Clark Glymour. I then illustrate by means of a simple example how this algorithm works when applied to variable sets containing variables standing in causal as well as in constitutional relationships. Whenever the algorithm outputs an edge between two variables, the question arises whether this edge stands for a causal or a constitutional relationship. I suggest to use information about time order to answer such questions. Finally, I show how time order information together with part-whole relationship knowledge can be used to test the empirical adequacy of my suggestion to formally treat constitution like causation. I also discuss possibilities to apply the suggested search procedure for constitutive relevance relations in different situations and with different goals in mind.

References

Baumgartner, M. (2013). Rendering Interventionism and Non-reductive Physicalism Compatible. *Dialectica*, 67(1):1–27.

Baumgartner, M. and Casini, L. (2015). An Abductive Theory of Constitution. Manuscript.

Baumgartner, M. and Gebharter, A. (2015). Constitutive Relevance, Mutual Manipulability, and Fat-handedness. *The British Journal for the Philosophy of Science*. doi: 10.1093/bjps/axv003.

Bechtel, W. and Abrahamsen, A. (2005). Explanation: a Mechanist Alternative. *Studies in the History and Philosophy of the Biological and Biomedical Sciences*, 36:421–441.

Bickle, J. (2003). *Philosophy and Neuroscienc*e: A Ruthlessly Reductive Account. Dordrecht: Kluwer.

Casini, L. (2015). How to Model Mechanistic Hierarchies. Paper presented at the *Philosophy of Science Association*, Chicago 6–9 November 2014.

Casini, L., Illari, P. M., Russo, F., and Williamson, J. (2011). Models for Prediction, Explanation and Control: Recursive Bayesian Networks. *THEORIA*, 70:5–33.

Churchland, P. S. and Sejnowski, T. J. (1992). *The Computational Brain*. Cambridge, MA: MIT press.

Clarke, B., Leuridan, B., and Williamson, J. (2014). Modeling Mechanisms with Causal Cycles. *Synthese*, 191:1651–1681.

Couch, M. B. (2011). Mechanisms and Constitutive Relevance. *Synthese*, 183:375–388.

Craver, C. (2002). Interlevel experiments and Multilevel Mechanisms in the Neuroscience of Memory. *Philosophy of Science*, 69(3):83–97.

Craver, C. and Bechtel, W. (2007). Top-down Causation Without Top-down Causes. *Biology and Philosophy*, 22:547–563.

Craver, C. and Darden, L. (2001). Discovering Mechanisms in Neurobiology. In Machamer, P., Grush, R., and McLaughlin, P., editors, *Theory and Method in the Neurosciences*, pages 112–137. Pittsburgh: University of Pittsburgh Press.

Craver, C. F. (2007). *Explaining the Brain*. Oxford: Oxford University Press.

Gebharter, A. (2014). A Formal Framework for Representing Mechanisms? *Philosophy of Science*, 81(1):138–153.

Gebharter, A. (2015a). Another Problem with RBN Models of Mechanisms. Manuscript. Gebharter, A. (2015b). Causal Exclusion and Causal Bayes Nets. *Philosophy and Phenomenological Research*. doi:10.1111/phpr.12095.

Gebharter, A. (2015c). Uncovering Constitutive Relevance Relations in Mechanisms. Manuscript.

Gebharter, A. and Schurz, G. (2014). A Modeling Approach for Mechanisms Featuring Causal Cycles. Paper presented at *PSA 2014*, Chicago 6–8 November 2014.

Glauer, R. D. (2012). *Emergent Mechanisms. Reductive Explanation for Limited Beings*. Mentis, Münster.

Glennan, S. (2002). Rethinking mechanistic explanation. *Philosophy of Science. Supplement: Proceedings of the 2000 Biennial Meeting of the Philosophy of Science Association. Part II: Symposia Papers (Sep., 2002)*, 69(3):S342–S353.

Harbecke, J. (2010). Mechanistic Constitution in Neurobiological Explanations. *International Studies in the Philosophy of Science*, 24:267–285.

Harbecke, J. (2015). The Regularity Theory of Mechanistic Constitution and a Methodology for Constitutive Inference. *Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences*. Forthcoming.

Hedstrøm, P. and Swedberg, R. (1998). Social Mechanisms: An Introductory Essay. In Hedstrøm, P. and Swedberg, R., editors, *Social Mechanisms: An Analytical Approach to Social Theory*, pages 1–31. Cambridge University Press.

Hedstrøm, P. and Ylikoski, P. (2010). Causal Mechanisms in the Social Sciences. *Annual Review of Sociology*, 36:49–67.

Leuridan, B. (2012). Three Problems for the Mutual Manipulability Account of Constitutive Relevance in Mechanisms. *British Journal for the Philosophy of Science*, 63:399– 427.

Machamer, P. K., Darden, L., and Craver, C. F. (2000). Thinking About Mechanisms. *Philosophy of Science*, 67(1):1–25.

Pearl, J. (2000). *Causality. Models, Reasoning, and Inference*. Cambridge University Press, Cambridge.

Schindler, S. (2013). Mechanistic Explanation: Asymmetry Lost. In Karakostas, V. and Dieks, D., editors, *EPSA11 Perspectives and Foundational Problems in Philosophy of Science, volume 2 of The European Philosophy of Science Association Proceedings*, pages 81–91. Springer International Publishing.

Spirtes, P., Glymour, C., and Scheines, R. (2000). *Causation, Prediction, and Search*. MIT Press, Cambridge, 2 edition.

Steel, D. (2008). *Across the Boundaries. Extrapolation in Biology and Social Science*. Oxford University Press.

Woodward, J. (2015). Interventionism and Causal Exclusion. *Philosophy and Phenomenological Research*, 91(2):303–347.

From Genetics to Culture – Lines, Gaps and Bridges **Symposium**
Organizer: Christian Sachse
Chair: Maria Kronfeldner Room 24, Tuesday 16:45 – 18:45

Epigenetics and the Explanation of Development: The Mirage of Moving Beyond Reductionism

FRANCESCA MERLIN
IHPST
francesca.merlin@gmail.com

Possible Limits of Reductive Explanations

CHRISTIAN SACHSE
University of Lausanne
christian.sachse@unil.ch

Information and the Evolution of Social Preferences

CÉDRIC PATERNOTTE
MCMP – LMU
cedric.paternotte@gmail.com

General Description

This symposium approaches the main theme of the conference – *Philosophy of science between natural sciences, the social sciences, and the humanities* – by questioning the possible line that can be drawn when considering genetics, epigenetics, development and cultural evolution.

Coming from different and, for the main aim of this symposium, complementary backgrounds, the first part of the symposium is about epigenetics and the explanation of development. After considering epigenetic research in more detail and highlighting that there is no such thing as a genetic program for development, it will be nonetheless argued that nothing fundamental has changed in the way biologists explain this process – that is, reductively. In a nutshell, the complexity of development and its phenotypic outcome are not explained by investigating the causal mechanisms involved;

rather it is by reference to the information coded in the DNA sequence in combination with epigenetic marks that development is explained. However, this first part of the symposium will end by evaluating to what extent the discussed empirical facts and future scientific progress could (and should) lead to a more comprehensive and *non*-reductionist view on organisms undergoing developmental phases.

This question and discussion will then picked up and further developed in the second part of the symposium where the guiding question is that of the *in principle* limits of reductive explanations. After outlining the general arguments in favor of the extreme reductionist position – that there are *no* limits – two main objections are identified in discussed by means of genetic examples. The first concerns the commonly known multiple realization objection enabling certain non-reductive explanations to compete convincingly with reductive explanations. As shall be explained in detail, such a move comes at high philosophical costs. The second concerns the question whether the rather new debate on complex systems and thus the complexity of genetic networks provide another and more convincing argument against the *in principle* reductionist hubris that is defended here in the first place.

Against the background of this balanced analysis – making the reductionist and antireductionist framework as strong as possible –, the third part of the symposium is then able to add another layer required for addressing the main theme of the conference: when considering humans and social utilities, it becomes clear that cultural evolution is partly independent from the genetic level and that our interests (and thus choices) do not always track the evolutionary advantage – even if this independence has evolutionary origins at the genetic level. One of the questions then is whether such facts can be, once again, rather seen as in the case of epigenetics and development where arguably nothing fundamental has changed (since the explanation of development is 'simply' based on a combination of genetic information and other relevant factors). Or whether – notably because of the difference in the notion of "function" – no such combination is available, which then leads to a (further) constraint of reductionist approaches.

Abstracts

1. Francesca Merlin: *Epigenetics and the Explanation of Development: The Mirage of Moving Beyond Reductionism*

Epigenetics is a relatively new research domain having its origin, both as a word and as a field, in Waddington's work on development. By "epigenetics", he meant the study of the set of causal processes through which gene activity causes the phenotype to emerge (1942): his aim was to shed light into such a developmental black box. During the last twenty years, epigenetics has often been presented as a revolutionary field, which challenges genetic determinism as a reductionist thesis about the explanation of development and its final result (i.e., the phenotype of an individual organism). Actual epigenetic research indeed shows that the relation between genotype and phenotype cannot be reduced to the information coded in the DNA sequence: there is no genetic program for development. Gene expression, cellular differentiation and, more generally, the developmental process tightly depend on a variety of biochemical marks on DNA and histones. In epistemological terms, there is no place for reductionist explanations in terms of the information of the DNA.

This represents a big challenge for the epistemological and methodological reductionism characterizing genetics, in particular molecular genetics, in the 20th century. Despite that, we argue that, until now, current research in epigenetics has shown to be as reductionist as genetics in its methodology and in its epistemological approach. Development is absent and the recurring references to Waddington betray this author's motivation to bridge the gap between genetics and embryology. Indeed, the developmental black box containing the whole complex of causal processes connecting the genotype to the phenotype (using Waddington's words, 1942: 10) is still closed: for the moment, biologists look for correlations between differences in epigenetic marks and differences in phenotypic features. In this way, epigenetic research has made possible to add an epigenetic layer to traditional genetic explanations, but still sticking to reductionist accounts of the way individual organisms develop a particular phenotype. In short, the kind of explanation used today to account for development has not changed: it reduces the complexity of the developmental process and its phenotypic result to the DNA sequence *plus* epigenetic marks (in particular, methylation marks). So, it is not surprising to see in the literature of the field several mentions of the "epigenetic

program" driving, with the genetic one, gene expression and cellular differentiation (see, for instance, Jaenisch & Bird 2003).

This analysis applies to research in molecular epigenetics, dealing with the molecular mechanisms and interactions taking place inside the cell as well as in other research domains, from developmental biology to evolution biology and behavioral ecology (for some example, see Bird 2007). Having shown that, one should be charitable because epigenetics is rather new as a research field. For instance, biologists do not yet know enough about the way chromatin marks are propagated from cell to cell during development and transmitted from across generations. This is particularly true for chromatin marks. Thus, the way explanations of development are built in this field – i.e., adding an epigenetic layer to reductionist genetic explanations could (and should) be considered as a first step towards a more comprehensive, non-reductionist, account of the construction of individual organisms through development.

2. Christian Sachse: *Possible Limits of Reductive Explanations*

The debate on epigenetics can be seen as a debate on what is the best (or better) causal explanation of some phenotypic result, some organismic trait. Based on the previous discussion, it became clear that genetic information does play a causal role, but it is not the only player and not necessarily the most important one. This is analogue to a shift in the general debate on scientific explanations from questions like "what is *the* cause of y?" to questions like "is the causal chain from x to y a robust one?" (cf. Woodward 2010). Against this background, the 2nd part of the symposium links to the 1st and its final critical discussion – and thus mainly is about the limits of reductive explanations.

For addressing this issue, I shall first of all recall the well-known premises and arguments in favour of the somewhat provocative claim that, *in principle*, there are *no* limits for reductive explanations (neither in the discussed case of epigenetics nor in biology general) and reductive explanations always are the best explanations. It then follows a discussion of a genetic example that satisfies recent refinements of what multiple realization should mean (cf. Polger & Shapiro *forthcoming*, chapter 3-4).

Against this background, I shall reconsider the largely admitted argument that under certain conditions non-reductive explanations may be at least equally good explanations compared to reductive ones. After all, as

general wisdom goes, non-reductive explanations are about what matters most since they abstract from non- or less relevant physical details. However, what is generally ignored is the high philosophical price to pay: non-reductive explanations necessarily require arbitrary choices in their construction of functionally defined types (which is only to a lesser degree the case in the construction of reductive explanations and finally completely absent at the level of fundamental physics). In a nutshell, the major point here is a strong criticism of anti-reductionist claims and arguments.

The question then is whether the superiority of reductive explanations has been vindicated once and for all in the following sense: *in principle*, reductive explanations are the overall winner and only *in practice*, because of certain constraints, non-reductive explanations may be sometimes or often preferred. Here, I shall discuss arguments coming from the claimed complexity of genetic networks and developmental processes (cf. Coffman 2011) and a major issue is a critical reconsideration of what "constraints" may mean in that case and whether "top-down constraint" is compatible with the premises outlined at the beginning. This aims at analysing whether the complexity of living system may lead to clear and understandable limit of reductive explanations.

Against this balanced (reductionist *and* anti-reductionist arguments) analysis, our discussion will continue in the analysis of the 3rd part of the symposium that, among others, clearly distinguished the notion of biological function based on fitness and that of individual utility, which is not necessarily correlated with fitness. One of the implications are that here discussed issues of multiple realization and complex systems get at least one more dimension when it comes to the link between the natural and the social sciences.

3. Cédric Paternotte: *Information and the Evolution of Social Preferences*

It has long been recognized that in species that have a cultural dimension, genes and culture have mutual influences and coevolve. Along with epigenetic factors, cultural factors causally contribute to phenotypes along with genetic ones, the causal primacy of which has consequently been contested. There is no consensus as to how much room epigenetic mechanisms leave to genetic reductionist approaches; but it is generally admitted that cultural evolution 'floats free' (Sober 2000) from the genetic level. This is for two reasons: first, cultural selective processes may be

different from genetic natural selection (although this is still debated); second, even if cultural and genetic selective processes are similar in nature, they are respectively based on fitness and utility, which may not be correlated.

Still, the strength of this second reason is unclear. It is clear that humans (at least) do not only care about their reproductive prospects (Sterelny 2012); still, some or many of our interests (e.g. food, or even money) may be positively correlated with fitness. In particular, because we clearly cannot compute and weigh against one another the various fitness consequences of our actions, our motivations may well act as proxies that steer us (or would have steered us in past environments) towards evolutionarily advantageous outcomes.

This contribution aims to resist this intuition by showing that simple social evolutionary situations can very easily lead individual utility to diverge from fitness. Importantly, social preferences can emerge from purely informational aspects: biases in assessing the probabilities of some evolutionary benefits may lead to the appearance of preferences that diverge from fitness, even in populations of individuals purely motivated by reproductive values. In other words, social utilities that are not proxies for fitness can easily be selected.

More precisely, I develop a model-based approach based on the seminal approach of Samuelson & Swinkels (2006). Their original (although neglected) insight is that our interests do not track the evolutionary advantage but our lack of information about them. Social utilities appear from the selective pressure of stable informational biases that cause agents to repeatedly miss out on opportunities for cooperation. Evolution can then act on the agents' utilities so that their expected subjective utility matches their expected objective payoffs again.

I present simple, quantitative models for three possible sources of biased information: reputation, lie detection and signaling, all of which can lead to evolved social utilities. Evolved social utilities are typically weak, which is consistent with the existing doubts concerning their existence (Binmore 2006, Guala 2012), and the absence of consensus concerning their forms even among those who accept their existence (Rabin 1993, Fehr&Schmidt 1999, Bicchieri 2006). An additional benefit of the proposed explanation is that it allows us to find constraints on the form of evolved social utility functions.

References

Bicchieri, C. (2006). The Grammar of Society The Nature and Dynamics of *Social Norms*, Cambridge: Cambridge University Press.

Binmore, K. (2006). 'Why do people cooperate?', *Politics, Philosophy and Economics 5*, pp. 81-96.

Bird, A. (2007). 'Perceptions of epigenetics', *Nature*, 447, pp. 396-398.

Coffman, J. A. (2011). 'On causality in nonlinear complex systems: The developmentalist perspective', in Hooker, C. (Ed.). *Philosophy of complex systems*, Amsterdam: Elsevier, pp. 287-309.

Fehr, E. and Schmidt, K.M. (1999). 'A Theory of Fairness, Competition and Cooperation', *The Quarterly Journal of Economics* 114(3), pp. 817-868.

Guala, F. (2012). 'Reciprocity: Weak or Strong? What Punishment Experiments Do (and Do Not) Demonstrate', *Behavioral and Brain Sciences* 35, pp. 1-59.

Jaenisch, R. & Bird, A. (2003). 'Epigenetic regulation of gene expression: how the genome integrates intrinsic and environmental signals', *Nature Genetics*, 33, pp. 245-254.

Polger, T. W. & Shapiro, L. A. (*forthcoming*): *The multiple realization book*, Oxford: Oxford University Press.

Rabin, M. (1993). 'Incorporating Fairness Into Game Theory and Economics', *American Economic Review 83(5*), pp. 1281-1302.

Samuelson L. & Swinkels J. (2006). 'Information, Evolution and Utility', *Theoretical Economics 1(1)*, pp. 47-92.

Sober, E. (2000). *Philosophy of Biology*, Boulder, CO: Westview (2nd ed.)

Sterelny, K. (2012). 'From Fitness to Utility', in S. Okasha & K. Binmore (Eds), *Evolution and Rationality: Decisions, Co-operation and Strategic Behaviour*, New York: Cambridge University Press, pp. 246–273.

Waddington, C.H. (1942). 'The Epigenotype', *Endeavour*, pp. 18-20, reprinted in *International Journal of Epidemiology* 2012, 41, pp. 10-13.

Woodward, J. (2010). 'Causation in biology: stability, specificity, and the choice of levels of explanation', *Biology & Philosophy* 25(3), pp. 287-318.

Philosophy of the Natural Sciences I **Contributed Papers**
Chair: Mathias Frisch Room 3B, Tuesday 16:45 – 18:45

Quantum Gravity: An Ideology of Unification?

KIAN SALIMKHANI
University of Bonn
ksalimkhani@uni-bonn.de

Modern physics is based on two pillars, the Standard Model of Elementary Particle Physics (SM) and the General Theory of Relativity (GR). While the first is a quantum field theory (QFT) that describes the electromagnetic, the weak and the strong interaction of subatomic matter particles as well as the Higgs mechanism, the latter is a classical field theory that addresses the fourth fundamental interaction, i.e. gravity. Although both theories are in perfect agreement with experiment, this dualism is often understood to be unpleasant. Hence, physicists attempt to find a unified theory of all fundamental forces, called Quantum Gravity (QG).

However, it is often argued that all approaches to QG rest on an external, i.e. aesthetical or metatheoretical, paradigm of unification (cf. Mattingly 2005 and Wuthrich 2005). Not only does it seem to be the case that "the real justification for quantizing gravity has yet to be articulated" (Mattingly, 2005), one could even conjecture "that the conceptual disunity of the two theories reflects a disunity in nature" (Wuthrich, 2005).

On the contrary, I will claim that unification in modern high energy physics is not an explicit aim in addition - or even opposition - to empirical adequacy, but its result. The problem of QG arises within QFT and it is a problem at high energies, not in principle. The two seemingly isolated theories are in fact deeply connected.

References

Mattingly, James (2005), Is Quantum Gravity Necessary?, in A. J. Kox and Jean Eisenstaedt (eds.), The Universe of General Relativity. Boston: Birkhäuser, 327-338.

Wuthrich, Christian (2005), To Quantize or Not to Quantize: Fact and Folklore in Quantum Gravity, Philosophy of Science, 72, pp. 777-788.

Physical Composition as Bonding

JULIAN HUSMANN
University of Münster
Julian.Husmann@web.de

PAUL NÄGER
University of Münster
paul.naeger@uni-muenster.de

Peter van Inwagen (1990, Material Beings) famously asked what it is that makes some xs compose something (special composition question). While the question has triggered an extensive debate in analytic metaphysics (e.g. Heil 2003, From an Ontological Point of View; Tallant 2013, Against Mereological Nihilism; Sider 2013, Against Parthood), very few empirically informed work has been advanced to approach an answer. In this paper we examine possible answers to the question on a scientific basis. We understand this work as a contribution to the metaphysics of science.

Starting from a scientist's picture of the world our idea is to investigate whether there are facts that are suitable to yield a moderate answer to the special composition question, i.e. an answer that avoids both nihilism (the thesis that there are no composite objects at all) and universalism (the thesis that any collection of xs compose something; scattered objects). We also accept van Inwagen's duplication principle which says that "if the xs compose something, and if the ys perfectly duplicate the xs (both in their intrinsic properties and in the spatiotemporal and causal relations they bear to one another), then the ys compose something." (1990, 138)

On this basis, we discard several prima facie interesting possibilities and propose an answer in terms of bonding: the xs compose something when they are in a bound state (plus some extra conditions). Bonding here is defined in physical/chemical terms as a specific relation between potential and kinetic energy. In this way we can explain why protons, atoms, molecules, solid bodies (such as rocks, glasses, cars), planets, solar systems, galaxies and interstellar gas clouds exist.

We discern between attractively bonded objects (such as atoms) and repulsively bonded objects (such as two pieces of wood nailed together). While the former type of bonding can occur on all levels, the latter is restricted to higher levels when objects with a repulsive geometry have emerged (due to attractive bonding).

In order to discern objects on different levels we introduce degrees of existence measured by the strength of the bonding. The three quarks that make up a proton, for instance, are far more strongly bound together than the electron and the proton that make up the hydrogen atom. And the latter are more strongly bound than a hydrogen atom to the other parts of a DNA molecule. Along these lines, we can precisely define a hierarchy of nested objects, whose degrees of existence decrease the weaker bound an object's parts are.

Finally, we shall discuss the consequences of our approach for the question of reduction: In which sense do the objects whose composition criteria we have expounded exist? We provide some suggestions how our approach might help to clarify both a reductionist as well as a non-reductionist stance.

Antecedent-Strengthening and Ceteris Paribus Laws

CARSTEN HELD
University of Erfurt
carsten.held@uni-erfurt.de

Antecedent-strengthening is a trivial theorem of classical logic but its equivalent in informal reasoning often fails. Intuitively, the effect is closely related to the problem of ceteris paribus laws and one recent explanation (due to Graham Priest in 2008) explicitly makes reference to a ceteris paribus qualifier. On a second look, however, the relation seems to dissolve. As a matter of fact both problems are most plausibly explained by referring to features of the natural-language conditional distinguishing it from the material one - but to different features. The failure of antecedent-strengthening is best explained by assuming that a naive reasoner evaluates the conditional constituting the conclusion via the Ramsey test and makes a tacit assumption such that the net result is an inconsistent set of premises - from which set classical logic infers anything, while the naive reasoner infers nothing. On the other hand, the ceteris paribus qualifier is best explained by assuming that an indicative conditional in some cases can express that a sufficient condition obtains - which makes it possible for 'if A and B, then B' to express an informative (possibly false) claim instead of a logical triviality. Thus, both

explanations manifestly invoke different features of the indicative conditional and it becomes hard to see the connection that seemed so obvious in the beginning. The paper aims to find an answer to the question whether, and eventually how, the two problems and solutions are related, after all. Steps toward seeing the relation are the following insights. Examples of failing antecedent-strengthening always involve conditionals expressing sufficient conditions, for the premise and conclusion conditionals plus the background knowledge invoked. The background knowledge itself expresses just one of the exceptions the ceteris paribus law seeks to exclude via the ceteris paribus operator. Finally, the concept of logical consequence is the well-known idea that a proposition P follows from a set of propositions M iff: if the elements of M are jointly true, then so is P. Clearly, classical logic here interprets the definiens conditional as material (such that from an inconsistent M anything follows), while the naive reasoner interprets it as expressing a sufficient condition (such that from an inconsistent M nothing follows.)

Philosophy of the Life Sciences I **Contributed Papers**
Chair: Gottfried Vosgerau Room 3C, Tuesday 16:45 – 18:45

Pitfalls and Perspectives in Comparative Psychology

TERESA BEHL
Ruhr University Bochum
teresa.behl@gmx.de

Comparative psychology has to face several methodological challenges like avoiding an anthropomorphic bias. Morgan's canon is one of the most famous epistemic rules to avoid anthropomorphism. The psychologist Conwy Lloyd Morgan argued in his textbook An Introduction to Comparative Psychology (1894) that "in no case may we interpret an action as the outcome of the exercise of a higher psychical faculty, if it can be interpreted as the outcome of the exercise of one which stands lower in the psychological scale" (Morgan 1894, p.53). However, it is still debated how the talk of higher and lower psychical faculties should be interpreted. Furthermore, it is under debate whether obeying Morgan's canon too strictly results in the opposite bias, neglecting too often higher order cognitive faculties, especially when a multitude of different low level explanations have to account for a set of experimentally observed behavior for which one single higher level explanation would suffice.

In my paper I will analyze systematically the comparative approach to insight as one example of a higher order cognitive faculty. I will individuate the methodological problems and challenges arising in the problem-solving debate around the definition and investigation of insight and how Morgan's canon influenced the debate. First, I analyze the definitions of insight in human creativity research. Second, I report how Wolfgang Köhler and others tried to adapt the definition to animal behavior and to what extent they risk to succumb anthropomorphism or the opposite. Third, I will introduce a new definition of insight avoiding several problems of the current definitions and approaches.

In human creativity research insight is seen as a special cognitive faculty that can elicit new and useful solutions to problems that usually cannot be solved through direct approaches as e.g. trial and error or analytical reasoning. Definitions of insight in humans rely heavily on introspective reports

about phenomenological experience like the experience of a sudden Aha-moment and that the solution seemed to appear from nowhere. Furthermore, the solution usually occurs suddenly and often after an impasse.

In his influential book The mentality of apes (1925) Wolfgang Köhler defines insight in animals as the sudden solution after an impasse taking the two indicators for insight in humans that are observable also in animal behavior. This definition is highly problematic for a comparative approach as it leads to a different subset of phenomena than the definition in humans. A sudden solution after an impasse can e.g. also occur through mental trial and error that would not count for insight in humans.

Eventually, both definitions of insight in human and animal cognition resemble a pre-scientific rule of thumb distinction. Even though legitimate and fruitful as the starting point of further investigation, both conditions are neither necessary nor sufficient for insight. I propose a new definition for insight based on phenomenological considerations and findings in EEG studies that does not have the same shortcuts as the currently used definitions.

What are Organizational Principles in Contemporary Brain Mapping?

PHILIPP HAUEIS
Berlin School of Mind and Brain
philipp.haueis@gmail.com

Philosophers of science commonly assume that neuroscientists primarily aim to uncover mechanisms of biological or cognitive phenomena (Craver 2007). In recent years, however, data-driven and graph-theoretical approaches to brain connectivity attempt to find organizational principles (Biswal 2010, Sporns 2011). While organizational principles have gained renewed attention in the philosophy of biology (Green 2015), an analysis of this notion in the neurosciences is missing so far.

In this paper, I attempt to give a first analysis of organizational principles in contemporary brain mapping. I begin by discussing their relation to mechanisms via the example of the pyloric rhythm in the lobster. The production of rhythmic oscillations by the pyloric network is taken to exemplify a general principle of how neuronal populations produce ongoing network activity in a wide variety of species with different cellular and synaptic properties

(Prinz et al. 2004). This suggests that an organizational principle subsumes various species-specific mechanism types by projecting them to all possible circumstances under which a type would have produced oscillatory activity. The projection provides the organizational principle with a maximal invariance against counterfactual changes of causal details in the particular mechanisms covered by it (see Lange 2000 for a similar definition of natural laws as counterfactually stable sets). By describing a projected set of mechanism types, an organizational principle would therefore describe the "rules about what organizational structures can achieve a particular type of biological function" (Green 2015, 646).

I then combine the above notion with the idea that brain networks at all scales exhibit a limited number of topologies, such as high clustering and short path length (Bassett and Bullmore 2006) or consisting of hierarchically clustered modules (Meunier et al. 2009). Such topologies provide constraints on which network configurations can exhibit functions such as ongoing oscillations, given other general functions living brains need to maintain (e.g., biosynthesis, optimal energy consumption and negative entropy). Because the topological and other general principles thus become mutually co-constraining, the invariance of any particular principle stems from its membership in a set of principles that are collectively stable under counterfactual change (Lange 2000). Because a collectively stable set of principles spells out which spatiotemporal organizations of brains are biologically possible or impossible, it fulfills the same role as "design explanations", by providing constraints on when an organism can be alive (Wouters 2007). Against the one-sided focus on mathematical analysis and simulation to find principles via abstraction from causal detail (Huneman 2010, Green 2015), I finally emphasize the importance of inter-species comparison to reveal similarities between differently organized nervous systems (Striedter et al. 2014). Without the mechanistic knowledge about causal details of how a principle is implemented, similarities that are invariant under counterfactual change can be hardly identified (Green et al. 2015). By calling attention to the hitherto neglected role of this research strategy in the search for principles, this paper contributes to a descriptively adequate picture of organizational principles in both the philosophy of neuroscience and the philosophy of biology in general.

The Stimulus Perception Connection
(NEW SLOT: Confirmation, Friday, March 11, 09:15-09:55, room 3C)

PAUL D. THORN
University of Duesseldorf
thorn@phil-fak.uni-duesseldorf.de

The present talk introduces a simple framework for modeling the relationship between environmental states, perceptual states, and action. The framework represents situations where an agent's perceptual state forms the basis for choosing an action, and what action the agent performs determines the agent's payoff, as a function of the environmental conditions in which the action is performed. My intent in introducing the framework is to determine what sorts of connections between environmental (stimulus) conditions and perceptual states are conducive to high payoffs (where high payoffs stand in as a proxy for effectiveness in navigating the environment). I begin by considering four *categorical* principles concerning the 'stimulus perception connection' which have an esteemed historical provenance. One of the principles states that if two perceptual state tokens are of different types, then they coincide with tokens of different environmental state types. Another of the principles states that if two perceptual state tokens are of the same type, then they coincide with tokens of the same environmental state type. The other two principles are the contrapositives of the preceding two. For any given agent, it is sensible to consider the degree to which the agent's perceptual states conform to the four categorical principles, as measured by the frequency with which applicable cases conform to the respective principles. The talk presents results of a simulation study, investigating the manner in which varied degrees of conformity to the four principles affect payoff. Some of the results are surprising, and conflict with long held views about the kind of stimulus perception connection that is important for knowledge of the world. For example, increasing degrees of conformity to first principle (above) has almost no positive impact on payoff, whereas knowing the degree of conformity to second principle is about as good a predictor of payoff as knowing the degrees of conformity to all four of the principles taken together.

References

Descartes, R. (1641). In S. Tweyman (Ed.), Meditations on first philosophy. London: Routledge. 1993.

Fodor, J. (1984). Observation reconsidered. Philosophy of Science, 51(1), 23–43.

Frigg, R., & Votsis, I. (2011). Everything you always wanted to know about structural realism but were afraid to ask. European Journal for Philosophy of Science, 1(2), 227–276.

Helmholtz, H. ([1878] 1971). Selected writings of Hermann von Helmholtz (R. Kahl, Ed.). Middletown, CT: Wesleyan University Press.

Hume, D. ([1739] 1975). In L. A. Selby-Bigge & P. H. Nidditch (Eds.), A treatise of human nature. Oxford: Clarendon Press.

Locke, J. ([1689] 1996). An essay concerning human understanding (K. P. Winkler, Ed.). Indianapolis, IN: Hackett Publishing.

Quine, W. (1998). From stimulus to science (2nd ed.). Cambridge: Harvard University Press.

Russell, B. (1927). The analysis of matter. London: George Allen & Unwin.

Weyl, H. ([1949] 2009). Philosophy of mathematics and natural science. Princeton: Princeton University Press.

General Philosophy of Science I **Contributed Papers**
Chair: Peter Brössel Room 3D, Tuesday 16:45 – 18:45

Emergence for Better Best System Laws

MARKUS SCHRENK
University of Dusseldorf
markus.schrenk@ccc.oxon.org

The Better Best System Account, short BBSA, developed inter alia by Cohen & Callender 2009, 2010; Author 2007, 2008; etc. is a variation on David Lewis's theory of laws of nature. The major difference to the latter is that the BBSA suggest that best system analyses can be executed for any fixed set of properties and their distribution in the world rather than only for the mosaic of perfectly natural properties. This move affords the possibility to launch system analyses separately for the set of biological properties yielding the biological laws, chemical properties yielding the chemical laws, and so on for other special sciences. The BBSA thus delivers laws separately for each of these sciences.

As such, the BBSA remains silent about possible interrelations between these then freestanding special sciences and their laws. I aim to explicate one possible relation between the different sets of laws which, I argue, could be called "emergence". If it does in fact hold (which would be up to empirical research) biological laws, for example, could be said to emerge from chemical ones, and the latter from the physical ones.

The classical emergentists (J.S. Mill, C. D. Broad, S. Alexander) put forward a multilayered picture of the world where the layers enjoy autonomy, yet, also dependence: "The higher quality emerges from the lower level […] and has its roots therein, but it […] constitutes its possessor a new order of existent with its special laws of behaviour." (Alexander 1920: 46)

BBSAs can deliver just that, (i) autonomy or novelty of levels and also (ii) their dependence: autonomy, (i), is given due to the fact that which generalisations classify as laws is decided autonomously within each separate system analysis.

However, (ii), regarded as mere true regularities, laws of one special science might supervene on the laws of more fundamental special sciences. Here's how: let $\forall x\ (Fx \supset Gx)$ be a true regularity of one special science S1

(with nomological status conferred to it by its own BBSA competition as in (i)). Now, entities with properties F and G typically have parts C1, …, Cn with their own properties Pi of a more fundamental special science S2 (plants have molecules, which have atoms…). The set of properties {F, G} might supervene on the set of all the Pi {Pi} in the following way: It is necessary that for all x1 and x2 with parts x1=C1+... +Cn and x2=C1'+... +Cn': if there is a total match for all Pi between <C1, ..., Cn> and <C1', ..., Cm'> then Fx1 ≡ Fx2 and Gx1 ≡ Gx2 and, because of the laws amongst the Pi, ∀x (Fx ⊃ Gx).

If this supervenience holds we have both the autonomy (from (i)) and dependence (from (ii)) the emergentists aimed for: the laws' nomicity is autonomously conferred to them via the respective system analysis, yet, their truth supervenes on the more fundamental laws. This emergence would also interrelate the otherwise separated BBSA laws.

Past Realists Thought the Same …

LUDWIG FAHRBACH
University of Duisburg/Essen
ludwig.fahrbach@googlemail.com

Scientific realism, the thesis that current successful theories are probably approximately true, is threatened by the pessimistic meta-induction (PMI) which states that the history of science is full of theories that were once empirically successful but later refuted. Many realists have responded to the PMI by arguing that there are important differences between past and present theories which block the PMI. This response comes in different versions: Current theories enjoy more success than past theories (Bird 2007), are more unifying than past theories (Doppelt 2007), result from better methods than past theories (Devitt 2011), and so on.

Against these pieces of reasoning anti-realists can offer a powerful counterargument, as follows. People in the past could have reasoned in exactly the same way as the realist today does: "Our theories are more successful (more unified, from better methods, …) than past theories. This difference blocks the PMI." But this reasoning would have been proven wrong by the theory refutations that subsequently ensued. Hence, we should expect that

the reasoning of realists today will likewise be proven wrong by future theory refutations (Wray 2013). I aim to analyze this counterargument, and show its limitations.

I start by noting that the reasoning of people in the past is not exactly the same as the reasoning of realists today. Rather there is a difference, namely precisely a difference in success. So, the reasoning of past people is only analogous to the reasoning of the realists today; it proceeds at lower degrees of success. This means that the counterargument of the anti-realist has to end with an inductive step from past to present, from the statement that the reasoning of people in the past failed (due to subsequent theory refutations) to the conclusion that the reasoning of current realists fails. But this inductive step derives its negative power entirely from the theory refutations of the past, because they are responsible for the failure of the reasoning in the past, or so I argue in my paper. This means that the inductive step, which is third-level in the sense that it refers to pieces of reasoning about theories, is equivalent to an induction on the second level (a meta-induction), namely the extrapolation of theory failure along degrees of success from past to present.

This extrapolation of theory failure (which is a version of the PMI) is easier to assess than the counterargument of the anti-realist. To assess it we have to determine the pattern of theory change and theory stability in the history of science up to the present. This pattern should not be gauged to time, but to degrees of success. Once we have determined the pattern, we can decide what to project into the future, theory change or theory stability or neither. For this it is plainly relevant how long – in terms of increase in success, not in terms of time – current theories have been stable. I close by offering some reasons to project theory stability into the future.

Some Do's and Don'ts of Defining Empirical Significance: A Carnapian Analysis

JONATHAN SUROVELL
Texas State University, San Marcos
jonathansurovell@gmail.com

A core tenet of logical empiricism is that there is a sharp distinction between claims that are in some way observationally confirmable (or "empirically significant") and claims that are not. Some logical empiricists attempted to clarify this distinction by constructing definitions of empirical significance that would tell us, for any given theory (or "language for science"), which of the component sentences were empirically significant and which weren't. The received view on this project is that as successive definitions succumbed to counter-examples, it became increasingly clear that no adequate definition of empirical significance would be found, and consequently, that the assumption that science can be sharply distinguished from metaphysics was just an empiricist dogma.

It should be noted, first, that the project of defining empirical significance has been more successful than is generally realized: there is currently no counter-example to the definition given by Creath in his 1976 paper. This definition makes minor revisions to an earlier definition of Carnap's. Furthermore, Justus has recently shown that fundamental differences between the Carnap-Creath definitions and Ayer's earlier definitions make the former less likely than the latter to have decisive technical flaws.

I hope to make two contributions to our understanding of the logical empiricist project of defining empirical significance. First, I argue that the Carnap-Creath definitions are inadequate. According to these definitions, whether or not a given sentence of theoretical science is empirically significant is entirely a matter of whether it allows us to infer novel observational predictions. From the point of view of Carnap's philosophy of language, which provides the initial impetus to find a definition of empirical significance, such a requirement is unduly restrictive. Carnap viewed languages for science as instruments for making more efficient inferences to and from observation sentences. The "empirically significant" sentences were supposed to be those whose inclusion in the language would contribute to this

aim. But given this aim, we should include in our language for science a sentence that simplifies the inference from theory to prediction, even if the sentence doesn't make any novel predictions. The Carnap-Creath definitions wrongly classify such a sentence as without empirical significance. I construct a simple language to illustrate this point.

My second goal is to briefly sketch an alternative approach to defining empirical significance. My proposal draws on Carnap's conception of explication. Carnap, at least in his later work, held that a relatively vague concept (an "explicandum") could be given precise definitions ("explicata") in various particular languages for science; he saw no need for an overarching definition subsuming the various definitions given within individual languages. Such was his later approach to the informal concept of analytic truth. (After his acceptance of the semantic concepts of meaning and reference, Carnap did not attempt to define 'analytic in L' for arbitrary language L.) I suggest that taking the same approach to defining empirical significance would make the project far less susceptible to counter-examples and would be, at least from Carnap's perspective, methodologically sound.

Symposia & Contributed Papers II

Absences, Deficiencies and Malfunctions in Biological and Medical Explanations **Symposium**
Organizer: David Hommen & Peter Hucklenbroich
Chair: Thomas Reydon Room 24, Wednesday 11:00 – 13:00

Functions, Malfunctioning, and Negative Causation

LUDGER JANSEN
University of Münster
ludger.jansen@uni-muenster.de

The Quantitative Problem for Theories of Function and Dysfunction

THOMAS SCHRAMME
University of Hamburg
thomas.schramme@uni-hamburg.de

Speciesism, Species Norm and the Lack of Species-Typical Traits in Moral Argumentation

PETER MCLAUGHLIN
Heidelberg University
Peter.McLaughlin@urz.uni-heidelberg.de

General Description
Absences, deficiencies and malfunctions play an indispensable role as causal factors not only in everyday life but also in many scientific explanations and models, especially in the biological and medical sciences. Thus, molecular biological and neurophysiological mechanisms typically involve not only positive factors, but also the absence of inhibiting or disturbing factors. Many diseases are prima facie caused by the lack of certain substances in the body (e.g., scurvy is explained by a shortage of vitamin C) or by the prevention of processes that are vital to life (e.g., HIV causes death by reducing the number of immunocompetent cells in the organism).

Often, it does not only make a causal difference whether a certain factor has occurred or not, but also to which extent it has occurred, i.e., whether it has exceeded or fallen below a certain threshold. It may be, for instance, that vitamin C has not been completely absent in the body, but that there has not been enough of it, so that the patient developed scurvy. A similar phenomenon located somewhere between the total presence and the total absence of causal factors is the phenomenon of dysfunctions. Dysfunctional processes in a complex system are not 'nothings,' strictly speaking – they are processes, after all. Yet, in some quantitative or qualitative respect, these processes fall short of their proper function.

A fundamental question related to the causality of absences, deficiencies and dysfunctions is whether the distinction between what happens or fails to happen and between what is enough or not corresponds to an objective matter of fact, or whether these are distinctions which cannot be accounted for without reference to human expectations, interests or normative principles that are otherwise taken to be alien to the purity of a metaphysical theory of causation. The upcoming workshop is intended to address these and adjacent questions:

- Are there, objectively speaking, such things as negative events, states or processes in biological and medical systems?
- Are negative factors bestowed with causal relevance only insofar as they are focused from a 'therapeutic' perspective which aims to fix or remove that which is absent, deficient or disturbing, or may they also be causally relevant apart from such a focus?
- What is the ontological status of biological and medical functions?
- What are the conceptual differences between dysfunction, functional deficit, malfunction, hypofunction, and hyperfunction? Are these differences of ontological and/or medical significance?
- What is the causal relationship between functions/dysfunctions and life/survival/death/early death? Is there a quantitative, measurable or calculable relationship? Is the "amount" of functional deficit a numerical, quantitative magnitude?

- In most cases, one kind of dysfunction – e.g., kidney or heart failure – may be caused by very different causes and may be symptom of very different diseases. Should all these occurrences really count as the same kind of dysfunction, or should they be conceptually differentiated in philosophical analyses?
- Might privations and dysfunctions have explanatory value without being real or without being causal?
- What is the status of scientific explanations that refer to fully or partly negative entities?

Abstracts

1. Ludger Jansen: *Functions, Malfunctioning, and Negative Causation*

Functional explanations are common currency both in biology and engineering: Causal features of a system part or processes in which a part participates can be explained by reference to its function; system processes can be explained by reference to the functions of the parts of the system. At least some of these explanatory patterns can not only be applied in cases of normal functioning, but also in the case of malfunctioning.

According to a straightforward (though not consensual) analysis, a bearer of the function to F is malfunctioning if and only if it has the function to F but not the disposition to do so.

This makes explanations of malfunctioning peculiarly problematic. First, they seem to be a case of negative causation, as they refer to absent dispositions. Second, this analysis seems to require that the function to F cannot be identical with the disposition to F. But then we seem to be trapped in a dilemma: Either the realm of functions is separated from the realm of dispositions; then functions seem to be causally inefficient. Or functions are identical with dispositions; but then malfunctioning seems to be conceptually impossible.

The paper reviews how the causal, etiological and intentional theories of functions can deal with these problems. In particular, it will discuss how the interdependent distinctions between historical vs. intrinsic features on the one hand and between types vs. tokens on the other hand can be exploited to develop a coherent account of the causal role of functions. While function types are not identical to disposition types, there are important interrelations between functions and dispositions. These will be found (1) in the historical dimension, (2) on the type level, and, (3) maybe

also on the instance level: Instances of functions may well be considered also to be instances of dispositions.

References

del Frate, L. (2012) Preliminaries to a formal ontology of failure of engineering artifacts, in: M. Donnelly, G. Guizzardi (eds.), Formal Ontologies in Information Systems (FOIS 2012), Amsterdam: IOS, 117-132.

Jansen, L./Röhl, J. (2014) Why functions are not special dispositions. An improved classification of realizables for top-level ontologies, in: Journal of Biomedical Semantics 5:27, doi:10.1186/2041-1480-5-27

Krohs, U. (2004) Eine Theorie biologischer Theorien: Status und Gehalt von Funktionsaussagen und informationstheoretischen Modellen. Berlin: Springer.

Krohs, U. (2010) Dys-, mal- et non-: l'autre face de la fonctionnalité. In: J. Gayon, A. de Ricqlès,

Mossio, M. (eds.): Les Fonctions: Des Organismes aux Artefacts. Paris: PUF, 337-351.

Spear, A. D./Smith, B. (2015), Defining 'Function', Third International Workshop on Definitions in Ontologies (IWOOD 2015), Lisbon, 27 July 2015, https://sites.google.com/site/defsinontos2015/accepted-papers

2. Thomas Schramme: *The Quantitative Problem for Theories of Function and Dysfunction*

Biological mechanisms have effects that we deem their function. For instance, the function of the heart is to pump blood. One of the problems of function theory is to draw a distinction between effects that are functional and other effects that do not serve a function. This can be called the qualitative problem of function theory. Obviously, there can be many different accounts as to why an effect is supposed to be a function, for instance based on its etiology, or its contribution to overarching goals of the organism, or for reasons of human interest. Accordingly, these different theories lead to different accounts regarding the normativity of explanations of functions. Some theories of function hold the qualitative problem to be solved by natural science, most importantly by biology and psychology; other theories consider mainly societal interests, for instance regarding medical treatability or enhancability of an organismic process.

A second important problem of function theory can be called the quantitative problem. It mostly affects medical science and practice. The problem consists of drawing the boundary between function and dysfunction. For instance, the function of the heart is to pump enough blood to sustain the organism. But how much is enough? There might be too little or too much blood being pumped, therefore two ways of dysfunction are possible. Whether a particular mechanism – a mechanism token, as it were – is dysfunctional, will probably require a different quantitative value in different individual organisms. After all, the amount of blood required to sustain other organismic functions will depend on individual values, such as age, weight, or stamina, as well as environmental factors, such as altitude or temperature. But be that as it may, surely we need generic accounts of proper quantitative function, especially in medicine, hence types of quantitative values. Perhaps these might be more fine-grained in being tailored to different age groups, gender etc. Nevertheless, there seems to be considerable scope for particular interests to influence the quantitative threshold of dysfunction. There are numerous examples from the history of medicine where these thresholds were set or influenced by non-scientific considerations, such as interest in the health of the population (e.g. Body Mass Index) or economic interests of doctors and the pharmaceutical industry (e.g. hypertension).

After setting up the problems for theories of function in the way described, in my paper I will focus specifically on the quantitative problem. I will argue that we can solve this problem in a scientific way, without the need to resort to societal interests or values. Statistics and biological theory are the means to solve the problem. Still, there is the interesting issue of individual levels of quantitative dysfunction. I will end the paper by proposing that the task of identifying tokens of dysfunction is to be performed by clinical diagnosis. After all, diagnosis is an application of typological classification to individual cases. So in medicine the quantitative problem calls for both theoretical and practical expertise.

3. Peter McLaughlin: ***Speciesism, Species Norm and the Lack of Species-Typical Traits in Moral Argumentation***

One of the most important concepts structuring current philosophical argument in applied bioethics is 'speciesism', introduced by Peter Singer as an analogue to 'racism' to characterize our privileging of members of our own

species in questions of ethics. Wherever claims to moral considerability are based on the possession of particular properties (such as the ability to feel pain, have preferences or experience self-consciousness), the question arises, whether an individual who does not in fact possess such species-typical properties to the species-typical extent can nonetheless still have the claims or rights that 'normal' individuals of the species have, based on those traits. The position denounced as 'speciesist' maintains that the natural properties used to justify or motivate ascriptions of moral considerability play the same role for all individual species members, independent of how far they deviate from the species norm for those traits. The question arises whether anyone can legitimately speak of dysfunction, disease or handicap without in some sense being a speciesist. Thus, this question points to the intersection of bioethical questions, questions of the normativity of classifications of dysfunction/malfunction or sickness in philosophy of medicine and political questions concerning the purportedly discriminatory nature of the vocabulary of the disadvantaged or handicapped.

This talk will analyze the role of the species norm, type-token distinctions and the function/malfunction distinction in the ascription of claims to moral considerability.

Philosophy of the Natural Sciences II **Contributed Papers**
Chair: Michael Stoeltzner Room 3B, Wednesday 11:00 – 13:00

The Role of the Concept of Causation in Physics

ENNO FISCHER
University of Cambridge
enno-fischer@gmx.de

In 1912, Bertrand Russell described the principle of causality as a "relic of a bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm". Moreover, according to Russell, "[t]he word 'cause' is so inextricably bound up with misleading associations as to make its complete extrusion from the philosophical vocabulary desirable."

Today, about 100 years later, a lively philosophical debate concerning the concept of causality points out that Russell's demand for its complete extrusion is not fulfilled. The relevance of this debate is amply justified since the concept of causation has a strong presence in many fields.

A special relationship to the concept of causation is often ascribed to physics or certain branches of physics. Russell's influential position is exemplary. According to Russell, causal laws in physics compete with laws that are formulated as mathematical functions. Since functions are much more precise than causal laws, they should be preferred. Thus, causal concepts do not play any role in advanced sciences like gravitational astronomy.

In my talk, I will address the question of which role the concept of causation plays in physics. Taking up Russell's criticism, I will present my own position which is subdivided into two theses. On the one hand, a principle of causality cannot play a fundamental role in the context of advanced theories that are remote from practical applications. On the other hand, a complete elimination of any causal concepts from all of physics would go too far. If in an experiment the actual result deviates from the predicted result, then the determination of causes of the deviation is the crucial step.

References

Russell, Bertrand: On the Notion of Cause. Proceedings of the Aristotelian Society, 13 1912/13, pp. 1-26.

Causality in General Relativity. "Partial Determination" Revisited

ANDREA REICHENBERGER
Ruhr University Bochum
andrea.reichenberger@rub.de

According to the common view general relativity describes gravity as a consequence of the curvature of space-time caused by the uneven distribution of mass/energy. However, one should exercise caution in embracing the conclusion that the distribution of mass/energy causes space-time and that this curvature causes the gravitational field. This is the lesson we can learn from Annette Garbe's book "The Partial Conventionally, Partial Empirically Determined Reality of Physical Space-Times" (German: "Die partiell konventional, partiell empirisch bestimmte Realität physikalischer Raum-Zeiten" (2001)). Garbes's thesis is: Gravitational potentials are coordinated to the pseudo Riemannian space-time metric. Due to this coordination physics and geometry are connected with each other by conventional procedures. The law which connects the metric of the Riemannian space-time with the sources of the gravitational field (including the boundary/initial conditions) is given by the field equations. These equations have been experimentally confirmed. That's what Garbe means when she speaks of "partial determination". Contrary to Garbe I take the view that the position of "partial determination" is compatible with the interpretation of causality as a relativized physical principle. Different space-time theories require different kinds of causal determination. The Friedmann equations provide a good example that causality holds true in general relativity, but only locally determined, if the global solution is time-orientable.

Quantum Mechanics and Retro-Causation

MATHIAS FRISCH
University of Maryland, University of Hannover
mfrisch@umd.edu

Huw Price, among others, has argued that certain features of quantum mechanical systems favor the introduction of retrocausal structures. For defenders of retro-causation in quantum mechanics, one crucial advantage of introducing retrocausal structures is that they allow for a Lorentz-invariant explanation of Bell-type correlations without action-at-a-distance, by positing causal connections that "zigzag up and down the lightcone" associated with entangled photon pairs. But Price has argued that there are is an even simpler argument, not involving appeals to Bell-type correlations, which suggests that we should interpret quantum mechanics retrocausally.

The argument appeals to a thought experiment involving a pair of polarizing beam splitters that are arranged such that the first beam splitter with its incoming and outgoing photons is the time-reverse of the second beam splitter. Price presupposes a broadly interventionist notion of causation to argue that the time-reversed beam-splitter involves forward causal relations. By time-symmetry it follows that the standard beam splitter involves backward causal relations. That is, Price argues for retro-causality in a standard quantum case from forward-causality in the time-reverse situation. He contrasts the quantum case with a classical beam splitter, where, he argues, there is no retro-causality. Retro-causality, Price argues, follows from the following three assumptions: Time-symmetry, discreteness, and realism, where the "heavy lifting", is done by the assumption of time-symmetry.

In this paper I examine Price's argument for retro-causality critically. I begin by asking under what condition we ought to interpret classical physical systems causally. I argue that there is a tight link between time-asymmetric causal structures and an initial independence assumption. This follows from a well-known result in the causal modeling literature. It follows from the assumption that the exogenous variables in a causal model are probabilistically independent, together with the assumption of deterministic laws of evolution, that the model satisfies the causal Markov condition, according

to which every variable is probabilistically independent of its non-descendants conditional on its parents. I show how this result can be applied to paradigmatic classical physical systems.

Then I apply this lesson to the quantum case and Price's thought experiment. I argue that the difference between the classical case and the quantum case is not, as Price suggests, the existence of discrete outcomes in quantum mechanical single-photon-experiments, but rather how, at various stages of the thought experiment, an independence assumption is (or is not) carried along in time-reversing the setup. For both classical and quantum systems, the direction of causation closely tracks any asymmetry in independence assumptions.

I conclude that the challenge to representing quantum systems in terms of asymmetric forward-causal causal structures is limited to the problem as to how to adequately account for non-locality. While this problem remains, there is no additional problem associated with the fact that quantum systems will have discrete measurement outcomes, that would favor symmetric causal structures involving both forward and backward causal relations.

Philosophy of the Life Sciences II **Contributed Papers**
Chair: Marie Kaiser Room 3C, Wednesday 11:00 – 13:00

On Life's Dual Nature: Complex Systems Dynamics and Gene-Centeredness

ALEXIS DE TIÈGE
Ghent University
alexis.detiege@ugent.be

Living cells and organisms are complex physical systems. Does their organization or complexity primarily rely on the crystalline structure of genetic nucleic acid sequences? Or is it, as critics of the 'gene-centred' perspective claim, predominantly a result of the complex and/or holistic network dynamics of genetic and various extra-genetic factors? The twentieth-century successes in several branches of genetics caused intensive focus on the causal role of genes in the biochemistry, development and evolution of living organisms, resulting in a relative abstraction or even neglect of life's complex systems dynamics. Today, however, partly due to the success of systems biology, a number of authors defend life's systems complexity while criticizing the gene-centred approach. Here, I offer a way out of the impasse of the gene-centred 'versus' complex systems perspective to arrive at a more balanced and complete understanding of life's multifaceted nature. Living cells and organisms are complex physical systems constituted by (i) intricate network organization which is, however, pervaded by functional gene products (i.e., functional RNAs and proteins) and, thus, by genetic sequence information derived from (ii) the crystalline structure of nucleic acid sequences. I show how the present state of knowledge in biology vindicates both the holistically complex and gene-centred nature of life on Earth, but decisively falsifies extreme genetic 'determinism' and 'reductionism' as well as extreme 'gene-de-centrism' or 'gene-relativism'. That is, (a moderate) gene-centrism is the only tenable midway between untenable genetic determinism/reductionism and equally untenable gene-relativism/de-centrism. Contrary to what is often claimed, the fact that genes are one among many extra-genetic causal factors contributing to the biochemistry and development of cells and organisms, only undermines or falsifies genetic determinism and reductionism but not necessarily gene-centrism. The importance of these issues for our understanding of the nature of life on Earth

is highlighted, and some implications for evolutionary theory, i.e., for the controversy between the Modern Synthesis and a so-called 'Extended Synthesis', are outlined.

The Philosophical Concept of Agency between Systems Biology and Artificial Intelligence

ANNE S. MEINCKE
University of Exeter
a.s.meincke@exeter.ac.uk

Within the philosophy of action, there have recently been made promising steps towards a biologically grounded concept of agency. A growing number of philosophers take it that agency must be understood as 'bio-agency', recognising the fact that humans are not the only agents in the world but that there are many other bio-agents such as dogs, dolphins and ants. This development is mainly driven by new insights from systems biology about the evolution and organisational structures of organisms. Organisms, according to the systems biological approach, are seen as dynamical systems that exhibit biological autonomy, varying from basic to more sophisticated forms and giving rise to basic as well as to more sophisticated forms of agency.

Understanding agency as bio-agency is attractive to philosophers as it seems to allow for naturalising agency in a non-reductive way (which would offer a solution to a whole bunch of intricate difficulties plaguing standard philosophy of action). Moreover, the systems biological approach to agency has also been an important source of inspiration for the new embodied robotics which has superseded classical AI. Rather than focussing on high-level abstract skills of human minds in the manner of classical AI, embodied robotics is mainly concerned with the more basic skills as possessed also by non-human animals, building artificial agents which are embedded or situated in an environment by means of sensorimotor loops.

There has, however, been criticism raised against this programme, namely by philosophers endorsing a systems biological view of agency. Thus Moreno and Etxeberria have argued that the latter approach, in so far as it takes metabolism to be crucial for autonomous agency, ultimately proves embodied robotics to be a deeply problematic project: as long as we are not

able to exactly artificially rebuild the material structure of organisms so as to allow for metabolic realisation of basic autonomy, the construction of artificial agents deserving this name either is doomed to failure or would have to rely on entirely different organisational principles from those of organisms.

In my talk, I shall discuss this claim and explore its implications for the philosophical concept of agency as well as for the relation between philosophy, systems biology and AI research.

Teleosemantics and the Meaning of Adaptation

HAJO GREIF
Technische Universität München
hajo.greif@tum.de

Being the current paradigm of naturalistic theories of mental and linguistic content, the teleosemantic programme heavily relies on Darwinian evolutionary theory.

Applied literally or by analogy, evolutionary patterns are supposed to explain how specific mechanisms within or outside an organism come to produce items with a certain semantic content, and what either's adaptive functions are. Such an explanation will recur to a history of effects of variant mechanisms over a sequence of generations, and require the presence and operation of mechanisms of heredity and natural selection, or analogues thereof.

The purpose of this paper is to match this evolutionary argument against a long-standing division between interpretations of Darwin's theory about the nature of evolution, and to make a suggestion as to which of these interpretations better supports the teleosemantic project. This issue appears to receive less attention from proponents of that project than it deserves, where authors like Millikan, Dretske or Neander seem to be too ready to adopt an "adaptationist" view when treating non-biological structures, such as learned behaviours and linguistic forms, in evolutionary terms.

In a nutshell, the matter of contention is this: Is natural selection the key driving force in shaping organic functions, operating with determinacy on effects of genetic variance? This adaptationist view is brought forward by

Dawkins, Axelrod/Hamilton, Maynard-Smith, Williams, and others. Or should one expect a dynamic interplay between organism and environment, where intra-population, developmental and environmental factors will equally act as enablers and constraints on an organism's traits, and where non-genetic mechanisms of heredity may operate? This view is brought forward by, inter alia, Gould/Lewontin, Odling-Smee, Griffiths, and Oyama.

My argument considers two key criticisms of the adaptationist programme that are relevant to teleosemantics: Firstly, the ascription of adaptive functions to a certain trait and their distinction from contingent effects will be arbitrary unless one is able to identify the concrete history of its establishment. Secondly, even if that history can be traced, it will remain difficult to identify it as a process of selection for that trait. An adaptationist reply will highlight G.C. Williams' strict criteria for a trait to be an adaptation, and accept the burden of proof imposed by them: being a variation, being genetically transmitted, and enhancing reproductive success under a given set of environmental conditions. Hence, at least the biological functions-by-analogy envisioned in teleosemantics will be incompatible with a sound adaptationist argument.

If, however, one is prepared to concede that, firstly, some degree of indeterminacy of functions in evolutionary phenomena is inevitable and that, secondly, the drivers of evolutionary processes may include factors beyond genetic variation and natural selection, arguments for biological functions-by-analogy will become permissible. The cost of indeterminacy actually works to the advantage of teleosemantic reasoning: As the argument from disjunctive content goes, it cannot provide us with a fail-safe method of determining the content of some mental or linguistic token – but it does not need to do so either. Do the small, elongated, moving shapes recorded by the frog have the function of denoting "fly", "nourishing object" or merely "small, elongated, moving shape"? Any such determination will be transient, less than perfect, and only occurs with respect to the concrete situations in which the users of such tokens act, given their concrete constitution and abilities. By thus waiving the determinacy of adaptationism, the teleosemantic programme will be biologically more realistic and counter the philosophical habit of seeking for foundational, immutable relations.

General Philosophy of Science II **Contributed Papers**
Chair: Markus Schrenk Room 3D, Wednesday 11:00 – 13:00

From Ontological Interaction, to Epistemic Integration and Integrative Pluralism

HARDY SCHILGEN
University of Cambridge
hschilgen@gmail.com

Philosophers have put forward a number of different theories of explanatory pluralism. What typically unites them is their opposition against reductionist explanatory attempts.

Despite this shared preference for explanations at multiple organizational levels, theories of pluralism differ with regard to the relations they take these explanatory levels to stand in to each other. While some take explanations at different organizational levels to stand in a competitive relationship (Lakatos, 1978; Kitcher, 1999, 2001), others take them to be compatible and cumulative (Jackson & Pettit, 1992; Weber & Van Bouwel, 2004).

It is important to emphasize that the above theories of pluralism get a lot right (i.e., correctly capture some relations between explanatory levels as they hold in practice). However, it's equally true that they fail to capture others: firstly, explanations formulated at different organizational levels do not always vie for the spot of the one-and-only best explanation; secondly, explanatorily relevant causal factors operating at different organizational levels cannot always be examined one after the other in an isolationist manner as is suggested by compatible-cumulative theories of pluralism. Mitchell (2009) is right when she emphasizes that often "relationships between factors at various levels are not independent of each other: the analyses of each must be integrated with results from study of the others to determine the roles they play in generating the behaviour of interest" (110).

This leads Mitchell to develop her own notion of 'integrative pluralism'. Integrative pluralism, she claims, tries to do justice to the sometimes-interactive nature of causal factors that jointly lead to a complex behaviour, phenomenon or trait. Her paradigm case is severe depression, which is jointly caused by the interaction of both genetic dispositions and environmental

factors (especially stressful life-events). Mitchell repeatedly hints at the applicability of her integrative pluralism to complex phenomena in other fields. This paper will actually attempt such applications.

First, I want to illustrate why economic inequality is a similar multilevel, interactive phenomenon that requires integrative explanatory efforts. This seem like a useful clarification to be made because even though recent work on inequality (Piketty, 2013; Atkinson, 2015) recognizes multiple causal factors influencing economic inequality, these factors – often operating at different organizational levels – are not being integrated but rather examined separately. Such an isolationist examination following a strict levels-of-analysis conception is flawed here.

Secondly, I want to compare the conception of explanatory integration as it underlies Mitchell's case of severe depression and the inequality case. I will show that inequality as a phenomenon displays a different kind of (ontological) interaction between causal forces and the institutional environment and thus calls for a different notion of (epistemic) integration and integrative pluralism respectively.

Thirdly, I want to talk about grounding epistemological suggestions on ontological features of the explanandum. Recent work on explanatory pluralism explicitly eschews such ontological considerations for pragmatic reasons. I think this is wrong. While one shouldn't wait for ultimate truths about ontology, there is no harm in using those bits of knowledge about ontology that are well-understood and empirically supported.

Scientific Pluralism and its Trade-Offs

RICO HAUSWALD
TU Dresden
ricohauswald@gmx.de

Philosophers of science have increasingly come to acknowledge pluralism as an attractive approach to explain how science, even though it proves to be imbued with biases, is able to maintain some form of objectivity and have considerable epistemic success. One reason, among others, to adopt pluralism is the idea that epistemic success is guaranteed not so much by the individual scientists being as impartial, detached, and cognitively virtuous as

possible, but by the socio-institutional organization of science. Every scientist is biased; but a research field as a whole can still be successful, provided that it is sufficiently diversified, i.e. if there are many scientists acting on different values, background assumptions etc., such that their different biases "cancel each other out" (see, e.g., Longino 1990, 2002; Chang 2012; Wylie 2015).

However, as compelling as this argument may be, pluralism is confronted with a number of challenges. In particular, diversity (or, equivalently, "plurality") does not seem to be desirable in all circumstances (e.g., Solomon 2008, Intemann/Melo-Martin 2014, Biddle/Leuschner 2015). Pluralism faces various trade-offs because a high level of diversity can often be realized only at the cost of other desirable epistemic and non-epistemic goals. But no study of the general structure of such trade-offs and their epistemological consequences has yet been done. The purpose of this paper is to provide this.

First, I provide conceptual clarifications of central notions like "pluralism" and "trade-off". I characterize pluralism as a view according to which achieving a plurality of entities of some type X ("X-plurality", for short) is valuable with respect to some goal(s) G1…Gn, where X may be theories, methods, subject matters, or other entities. For example, a plurality of alternative theories is usually valued by pluralists because it facilitates mutual criticism, which in turn is considered to be a means to achieve fundamental epistemic goals, like finding significant truths, understanding, etc.

However, pluralism faces trade-offs because there are a variety of legitimate epistemic and non-epistemic goals that need to be taken into account when considering how science should be organized, but not all of which call for the same extent of X-plurality. The general form of such trade-offs is as follows: Achieving goal G requires realizing a certain level L of X-plurality, while achieving another goal G' is incompatible with realizing X-plurality on level L.

In the second section, I examine concrete goals that call for rather high levels of plurality of some type(s) (including the enhancement of mutual criticism and the improvement of the situation of oppressed social minorities), as well as goals that require comparably lower levels (including the effective distribution of scarce resources, the assemblage of scientific communities of persons who are most competent to do the research in question, the

maintenance of the role of science as an epistemic authority in society, and the preservation of some minimal ethical standards in science).

Finally, I examine possible strategies to deal with these trade-offs. One option is to argue that some of the mentioned goals are not legitimate insofar as they need not really be taken seriously when considering the optimal organization of science. A strategy to deal with the remaining goals is to try to weigh and rank them to find an optimal compromise.

The Perspective of the Instruments: Mediating Intersubjectivity

BAS DE BOER
University of Twente
s.o.m.deboer@utwente.nl

Numerous studies within Science and Technology Studies (STS) and philosophy of technology have repeatedly stressed the current collective and instrumental nature of scientific practice. It was not until recently that attempts to systematically integrate these insights were made in philosophy of science, most importantly in Ronald Giere's Scientific Perspectivism (2006) and Davis Baird's Thing Knowledge (2004). While they diverge in their specific approach towards scientific instruments, Giere and Baird both attempt to understand the epistemic function of instruments in scientific practice. Giere argues that the essential role of scientific instruments becomes immediately clear when looking at scientific practice. In this view, all instruments allow to look at objects only from a specific perspective. The central point of Baird's philosophy is that instruments are solidified knowledge. The solidity makes it possible for different scientists to study the same phenomenon in a similar way, which makes scientific instruments essential in the development of scientific knowledge.

In both cases it is clear that scientific instruments have impact on the way scientific knowledge is gathered, yet it remains unclear how they do so. This would be unproblematic if we were to assume that scientific instruments a) are capable of offering only one perspective, and the related idea b) that all human members of the system share this perspective immediately. However, research on the impact and use of concrete technologies in

philosophy of technology suggests otherwise. The function of the instruments is not fixed, but rather is determined in the relation with the scientist. The question I aim to answer is how these individual human-technology relations can give rise to scientific knowledge.

As a starting point, the concept of technological mediation developed by Don Ihde and Peter-Paul Verbeek will be used to do right to the idea that a certain perspective is always the product of a relation between scientists and scientific instruments. The main idea behind the concept is that technologies are no neutral intermediaries, but have an active role in determining how the world is revealed to the scientist. Doing science is neither merely an activity of the scientist nor one of the instrument. Attaining scientific knowledge is understood as grounded in the relation between scientists and instruments. As I will try to make clear, it is in this relation that it is determined both what scientists are investigating and how they do so.

In this paper, I will firstly discuss Giere's and Baird's understanding of the relation between scientists and technologies in scientific practice. Secondly, I will discuss Giere's account of how networks of humans and non-humans are capable of generating knowledge. Thirdly, the concept of technological mediation will be used to criticize this understanding, and to stress that scientific instruments can offer multiple coherent perspectives in relation with scientists. Lastly, I will try to give an account of how these different individual relations are integrated into a larger scientific system, thereby clarifying how scientific instruments can give rise to intersubjectivity within such a system.

History of Philosophy of Science **Contributed Papers**
Chair: Helmut Pulte Room 22, Wednesday 11:00 – 13:00

Kant's Views on Preformation and Epigenesis

INA GOY
University of Tübingen
inagoy1@gmail.com

Among philosophers and historians of the life sciences it is controversial whether Kant's account of animal generation can be considered an ovist or animalculist account of preformation, or a mechanical or vitalist account of epigenesis. Whereas Zumbach (1984, 79–113) negated that Kant was a vitalist, Reill (2005, 246) and Huneman (2006, 651–4; 2007, 12) thought that Kant partook in the program of 'enlightenment vitalism'. Zammito (2003, 80; 2006; 2007, 51, 56–66) pointed out that Kant held ambivalent views with regard to preformationist and epigenetic accounts of animal generation in different periods of his thought. He repeatedly emphasized that Kant was never entirely comfortable with epigenesis. Grene/Depew (2004, 95) and Roth (2008, 284) tried to show that Kant combined preformation and epigenesis, in particular in §81 of the Critique of the Power of Judgment (CPJ). Steigerwald (2006, 716) argued that Kant favored an epigenetic explanation of the generation of the individual but preferred a preformationist explanation of the generation of the species. The debate has not reached a consensus yet.

In this paper, I will analyze whether and to what extent Kant's approach related to ovist and animalculist preformationist, or mechanical and vitalist epigenetic accounts of animal generation. I will, first, analyze the passages in Kant's writings in which he discussed preformation and epigenesis and criticized its various defenders. I will, then, analyze whether Kant adopted central preformationist claims, such as the assumption of creation and of the existence of preformed germs, or of a unisexual doctrine of heredity. After this, I will analyze whether Kant shared significant epigenetic claims, such as the assumption of nature's autonomy and of creative natural powers and laws, or of a bisexual doctrine of heredity.

I will argue that Kant's position represented a stronger version of preformation in the writings on races since Kant presupposed God's creation. It

represented a weaker version of preformation in the CPJ since the critical turn of Kant's philosophy did no longer allow him to adopt a dogmatic concept of God. Kant developed a theory of preformed germs and dispositions in his writings on races, but he neither shared ovist nor animalculist interpretations of the preformed germ. In the CPJ Kant no longer focused on preformed germs, but he still mentioned dispositions at the beginning of generation. Kant never accepted a unisexual doctrine of heredity.

Beside more or less weak accounts of preformation, Kant held a weak version of epigenesis since he accepted the existence of mechanical and vitalist powers and laws of nature as secondary causes of animal generation. In Kant's writings on races the creative aspect of natural powers and laws lay in a generative power; in the CPJ in a formative power and physical teleological laws of nature. In particular, epigenetic powers and laws accounted for the generation of the individual and its adaptation to the specific environment. With epigenetic accounts Kant also shared a bisexual doctrine of heredity. In general, Kant stayed closer to vitalist than to mechanical versions of epigenesis.

Theoretical Construction in Physics: The Role of Leibniz for Weyl's 'Philosophie der Mathematik und Naturwissenschaft'

(CANCELLED)

NORMAN SIEROKA
ETH Zurich
sieroka@phil.gess.ethz.ch

This paper aims at closing a gap in recent Weyl research by investigating the role played by Leibniz for the development and consolidation of Weyl's notion of theoretical (symbolic) construction. For Weyl, just as for Leibniz, mathematics was not simply an accompanying tool when doing physics – for him it meant the ability to engage in well-guided speculations about a general framework of reality and experience. The paper begins by discussing some particular Leibnizian inheritances in Weyl's 'Philosophie der Mathematik und Naturwissenschaft', such as the general appreciation of the principles of sufficient reason and of continuity. Then the paper focuses on two themes: first, Leibniz's primary quality phenomenalism, which according to Weyl marked the decisive step in realizing that no physical quality is given

Cancelled

by direct intuition, and second, Leibniz's notion of 'expression', which allows for a certain type of (surrogative) reasoning by structural analogy and gave rise to Weyl's optimism regarding the scope of theoretical construction. Finally, it is suggest that the discussion of these Leibnizian concepts is of ongoing relevance for current debates concerning the role and nature of transformations, invariances, and objectivity in the philosophy of science.

The Vibe Around 1930: Scientism and Political Philosophy of Science

MARKUS SEIDEL
University of Münster
maseidel@hotmail.com

Research by scholars on the history of the Vienna Circle has established a more comprehensive picture of the connection between its members' avowed scientism and their political philosophy of science (see e.g. Uebel 2005, Reisch 2005). At least some members of the Vienna Circle argue that precisely a scientific world conception devoid of metaphysical and theological content provides the means to an enlightened social policy. This idea is quite obvious in their manifesto The Scientific Conception of the World: The Vienna Circle from 1929.

Around the same time knowledge and science get into focus of reflection also from a genuinely sociological point of view: Karl Mannheim's classic Ideologie und Utopie is also published in 1929, followed six years later by Ludwik Fleck's monograph Entstehung und Entwicklung einer wissenschaftlichen Tatsache—scarecely noted by contemporaries, but vividly discussed since its rediscovery in the 1970s. The orthodox view on these works in history of philosophy and sociology of science is that their authors stand in strong opposition to the scientistic project of the Vienna Circle: Mannheim by invoking a strong opposition between methodology in the natural sciences and the humanities and Fleck by lamenting the "excessive respect, bordering on pious reverence, for scientific facts" (Fleck 1979, 47) in past sociology and philosophy of science.

In this talk, I will argue that the orthodox view on early sociology of knowledge and science—claiming that its proponents aim to undermine the scientistic project of the Vienna Circle—is incorrect, insofar as it does not

give due weight to the political ambitions of early sociology of science. In fact, a look at the writings of Neurath, Mannheim and Fleck shows that reflection on science around 1930 in the German speaking world was thoroughly political: It was thought to be itself the best means to enlighten the masses about the pitfalls of political ideologies. Contrary to appearances, the argument for such a project of enlightenment brought forth by the early sociologists of knowledge and science rests on a plea for a thoroughgoing scientism in philosophy and sociology, devoid of speculative and metaphysical elements. Therefore, I wish to establish the conclusion that—non-negligible disagreement notwithstanding—philosophy AND sociology of science were united in their view that a thoroughgoing scientistic methodology provides the means to a politically enlightened society: Scientism and political philosophy of science were the vibe around 1930.

References

Fleck, L. (1979). Genesis and Development of a Scientific Fact. Edited by Thaddeus Trenn and Robert Merton. Chicago and London: University of Chicago Press.

Reisch, G. (2005). How the cold war transformed philosophy of science: To the icy slopes of logic. New York: Cambridge University Press.

Uebel, T. (2005). Political philosophy of science in logical empiricism: The left Vienna Circle. Studies in History and Philosophy of Science, 36, 754–773.

Symposia & Contributed Papers III

The Relation between Philosophy of Science and Philosophy of Engineering after the Practice Turn **Symposium**
Organizer: Rafaela Hillerbrand
Chair: Rafaela Hillerbrand Room 24, Wednesday 16:45 – 18:45

What is a Philosophy of Science for the Engineering Sciences?

MIEKE BOON
University of Twente
m.boon@utwente.nl

Internalism and Externalism in the Philosophy of Engineering

PETER KROES
TU Delft
P.A.Kroes@tudelft.nl

A Causal Perspective on Modeling in the Engineering Sciences

WOLFGANG PIETSCH
TU München
pietsch@cvl-a.tum.de

General Description

Philosophy of science after the practice turn pays attention to various scientific fields (Soler et al. 2014). It seems only natural to include engineering into this study. Areas like biotechnologies or climate engineering seem to not even allow a clear separation between science and engineering. Technological progress, moreover, plays a decisive role in the generation of scientific knowledge. Contemporary large-scale experiments as those at CERN are examples at hand (cf. Nordmann et al. 2011, Baird 2004).

Most contemporary scholars reject the classical view on engineering as applied science. While a lot of work is devoted to carving out differences in

forms of knowledge in engineering and science, the relation between the corresponding philosophical disciplines and what they may learn from each other, have received much less attention. However, the scholarly reflections on engineering and science show similar developments in recent years. The empirical turn is visible in both fields. In the wake of this turn, philosophy of engineering emerged as a new field that aims to ground philosophical analyses of technology on design and modeling practices in the field, much different from more traditional approaches of philosophy of technology (cf. Mitcham 1994, Kroes & Meijers 2000). The turn to the practice of engineers also came with a greater emphasis on the epistemic role of models (cf. Boon & Knuuttila 2009, Zwart 2009), mirroring the focus on models in philosophy of science.

Besides all its virtues, the practice turn entails certain perils. It may risk unwarranted generalizations, based on too few or unrepresentative cases. It could reduce philosophical reasoning to mere recounting science or a history of scientific cases. For philosophy of engineering these problems are aggravated, as context-dependent features are more prevalent in engineering: Engineering models or design are often tailor-made for specific applications. The question stands to reason to what extent can philosophy after the practice turn make conceptual claims that go beyond the case studies.

The aim of this symposium is to explore this issue by touching on the following questions: How can philosophy of engineering make conceptual claims that go beyond mere case studies? Which traditional philosophical problems need to be reconsidered in the light of engineering practice? What kind of normativity guides scientific and engineering enterprises? What is the epistemic function of models? What concept of causality can account for engineering practices? Can we distinguish conceptual features from mere contextual ones? In addressing these issues, questions regarding the relation between philosophy of science and of engineering are central. The symposium thus aims to contribute to the overall conference theme by locating philosophy of science in relation to engineering. The first paper in this session addresses the question as to how engineering relates to science and what this can imply for its philosophical reflection. The second paper scrutinizes the distinction between external and internal factors in scientific and engineering practice – a distinction that is argued to actually be undermined by the practice turn. The third paper scrutinizes the nature of causality underlying modeling in engineering practice.

Abstracts

1. Mieke Boon: *What is a Philosophy of Science for the Engineering Sciences?*

The engineering sciences are often equated with technology, and therefore are not considered as science. Contrariwise, I defend, firstly, that the engineering science(s) must be considered as a special science(s), and secondly, that a philosophy of science for the engineering sciences must be understood as a philosophy of a special science(s) similar to philosophy of physics, chemistry, biology, medicine and so on.

Philosophies of special sciences have branched off from general philosophy of science because general, 'traditional' philosophical issues, concepts and assumptions that were commonly related to physics as a paradigm-example of science, (a) needed to be addressed in the context of those other sciences, (b) but often happened not to fit very easily, leading to their refinement or revision, and (c) also resulted in new kinds of issues, concepts and assumptions that appear to be relevant to general philosophy of science as well. Conversely, working through general issues of philosophy of science by using examples from a specific science may feed back into that scientific practice, as these exercises usually result into philosophical accounts that are more specific and more relevant than general philosophy of science can offer. Such exercises feed back into that scientific practice, for instance through clarifications and recommendations on its basic concepts, ontology, epistemology, methodology, as well as value-issues concerning the application of science in societal contexts. Philosophy of biology is a well-known example in this respect (Griffiths 2014).

In this paper, I will present an outline of traditional philosophical issues that, when related to the engineering sciences, need to be reconsidered. I will first explain in what sense engineering science is a science. The engineering sciences share characteristic features with what is usually called 'fundamental science,' or 'basic natural sciences.' They aim at scientific knowledge, they use similar research methodologies, and scientific results are published in scientific journals. At the same time, the engineering sciences differ from other natural sciences in more than only their subject-matter. The fact that these sciences often study technologically produced physical phenomena and the technological instruments themselves, is much

more prominent. Moreover, these scientific research practices aim at epistemic results that guide and enable productive, innovative and reliable epistemic uses in the development of technology. It will be argued that a philosophy of science for the engineering sciences puts into question several commonly held assumptions (and normative ideas!) in general philosophy of science, such as: 'what is the aim of science', 'what is the character of epistemic results', 'what is a scientific model', 'how is it possible that scientific knowledge can be applied,' 'what is the role of scientific instruments,' 'what are productive epistemic strategies,' and 'what ought to be the criteria for accepting a philosophical account.' Finally, it will be defended that the development of a philosophy of science for the engineering sciences brings to the surface features that can be taken to be relevant also more generally to other natural and experimental sciences and in that very sense should feed back into the general philosophy of science.

2. Peter Kroes: *Internalism and Externalism in the Philosophy of Engineering*

More or less in the wake of Kuhn's The structure of scientific revolutions there has been an intense debate in the philosophy of science about whether the development of the substantial content of science is primarily determined by factors internal or external to science. According to internalistic approaches this development is mainly determined by methodological/epistemological considerations, whereas externalistic approaches appeal to contextual factors, that is factors related to science as a social practice embedded in a broader societal context. A similar debate has played a role in the philosophy of technology with regard to the development of technology, with defenders of technological determinism mainly taking an internalistic and advocates of social construction of technology taking an externalistic approach to the development of technology.

What all these approaches have in common is the assumption that somehow it is possible to distinguish between science or technology and its context. It is this assumption that I will put into question from the point of view of the Practice Turn. I will address the issue to what extent the shaping of a technological artifact is due to factors that may be deemed internal to technology and to what extent to external (social/societal) factors. Prima facie external factors appear to play an important role in shaping a techno-

logical artifact, since the function it is intended to perform is very often derived from the wishes and needs of various social groups. In line with this I will first present a schematic, coarse-grained conceptual model of the role of internal and external factors in the shaping of a technical artifact (Kroes 1996). Then I will discuss a case study in which I zoom in on an actual engineering design practice; the case study concerns the development of a new type of sludge water reactor (Zwart and Kroes 2015). This fine-grained view of what goes on in engineering practice shows that it is difficult to classify factors that influence the outcome unambiguously as internal or external to engineering practice. The Practice Turn, therefore, appears to undercut the underlying assumption that it is possible to distinguish between internal and external factors shaping technology and so the dichotomy between technological determinism and social construction of technology appears to be spurious.

I will argue, however, that we have to be careful in drawing this conclusion, since underlying the internalism-externalism debate there is the issue of defining the notions of engineering and engineering practice. Following Radder (2009) I will distinguish between the conceptual-theoretical and the nominalistic-empirical approaches to defining engineering practice and argue that both are necessary. Any study of an engineering (science) practice, whatever its aim and whether performed before or after the Practice Turn, will have to conceptually frame ('define') its object of study and will therefore somehow have to distinguish between that object and its context, that is, between what is considered internal and external to an engineering practice.

3. Wolfgang Pietsch: *A Causal Perspective on Modeling in the Engineering Sciences*

Based on a distinction between phenomenological and abstract modeling, I argue that the engineering sciences are mainly engaged in the former. Key to my argument is a specific notion of causation broadly standing in the counterfactual tradition. The causal perspective also throws some light on the dual nature of technical artifacts and engineering models, i.e. structural and functional as well as relatedly internalistic and externalistic.

My point of departure is a distinction drawn by the new experimentalists between a phenomenological and an abstract level in scientific epistemology which for example constitutes a crucial premise of Hacking's claim that experimentation has a life of its own. Some aspects of this distinction are: On the phenomenological level, one deals with causal laws that are contextual and hold almost always only ceteris paribus, while the theoretical and generally non-causal laws of the abstract level hold universally. The phenomenological level mainly aims at prediction, while the abstract level establishes a unifying conceptual framework. Also, the nature of the considered phenomena differs. On the phenomenological level, the world is addressed in its full complexity, while paradigmatic phenomena are singled out on the abstract level.

On the basis of this epistemological framework, I argue that, very broadly, modeling in the engineering sciences belongs to the phenomenological level, while physical theorizing happens mostly at the abstract level. Indeed, I claim that this aspect constitutes one of the crucial differences between the engineering sciences and physics. The perspective fits well with recent accounts of engineering models, e.g. that they are "epistemic tools for creating and optimizing concrete devices or materials", as defended by Boon and Knuuttila (2009).

I further corroborate the outlined epistemological perspective by specifying an appropriate concept of causation that fits well with scientific practice in engineering. It employs a counterfactual definition but takes a different route than conventional accounts regarding the evaluation of truth-values of counterfactuals—this alternative route is inspired by Mill's methods, in particular the method of difference. The main advantage is that unlike e.g. Stalnaker's and Lewis's influential semantic approach, which relies on similarity between possible worlds, the suggested account refers only to observations in the actual world and thus is metaphysically less demanding and much closer to scientific practice. Furthermore, the proposed account renders causal statements background-dependent—as suggested by Anderson and Mackie—and thereby establishes a ceteris-paribus character that characterizes many phenomenological laws used in the engineering sciences.

Some consequences are discussed. In particular, I argue that the proposed notion of causation fits well with the dual nature of technological artifacts as discussed extensively by Kroes (2012). The structure of artifacts

should be understood in terms of a network of causal relations, while artifact functions can be interpreted, as first suggested by Hempel, in terms of certain effects of causal relations that are singled out by a context. I briefly indicate how on this basis, some internalistic and externalistic aspects of design and modeling practices can be distinguished.

References

Baird, Davis (2004). Thing Knowledge: *A Philosophy of Scientific Instruments*. University of California Press.

Boon, Mieke, and Tarja T. Knuuttila (2009). Models as Epistemic Tools in Engineering Sciences. In *Philosophy of Technology and Engineering Sciences*. Edited by Anthonie Meijers, Elsevier: 683–726.

Griffiths, Paul (2014). Philosophy of Biology, The Stanford Encyclopedia of Philosophy (Winter 2014 Edition), Edward N. Zalta (ed.), URL = <http://plato.stanford.edu/archives/win2014/entries/biology-philosophy/>.

Kroes, P. (1996). Technical and contextual constraints in design; an essay on determinants of technological change. The role of design in the shaping of technology. J. Perrin and D. Vinck. COST A4, vol. 5; European research collaboration on the social shaping of technology: 43-76.

Kroes, P. (2012). *Technical Artefacts: Creations of Mind and Matter*. Dordrecht: Springer.

Mitcham, Carl, ed. (2005). *The Encyclopedia of Science, Technology, and Ethics*. Detroit, MI, USA: Macmillan Reference.

Nordmann, Alfred, Hans Radder, and Gregor Schiemann, eds. (2011). *Science Transformed? Debating Claims of an Epochal Break*. Pittsburgh University Press.

Radder, H. (2009). Science, technology and the science-technology relationship. Handbook of philosophy of technology and engineering sciences. In *Philosophy of Technology and Engineering Sciences*. Edited by Anthonie Meijers, Elsevier.

Soler, L., Zwart, S., Lynch, M., Israel-Jost, V. (2014). *Science after the Practice Turn in the Philosophy, History, and Social Studies of Science*, Routledge.

Zwart, S. and P. Kroes (2015). Substantive and Procedural Contexts of Engineering Design. In *Engineering Identities, Epistemologies and Values*. Edited by S. H. Christensen et al., Springer International Publishing. 21: 381-400.

Zwart, Sjoerd (2009). Scale Modelling in Engineering. In *Philosophy of Technology and Engineering Sciences*. Edited by Anthonie Meijers, Elsevier: 759–798.

Philosophy of the Natural Sciences III **Contributed Papers**
Chair: Richard Dawid Room 3B, Wednesday 16:45 – 18:45

The Fine-Tuning Argument for the Multiverse Under Attack

SIMON FRIEDERICH
University of Groningen
email@simonfriederich.eu

According to the laws of physics as presently known, had the values of some constants of nature been slightly different, life could not possibly have existed. A common reaction to this finding is to propose that our universe is just one among vastly many in an encompassing multiverse where the values of the constants differ in the different subuniverses. Since we can only exist where the constants permit life, the apparent fine-tuning of the constants where we live is unsurprising if we accept the multiverse – or so its proponents argue. String theory in combination with inflationary cosmology is often presented as supporting the multiverse idea by independently suggesting the string landscape multiverse as a promising implementation of it. The present contributions assesses the strength of the fine-tuning argument for the multiverse in the light of these considerations by focusing on three objections against it.

The first objection (the most-discussed one in the philosophical literature) states that the fine-tuning argument for the multiverse commits what Hacking (1987) calls the “inverse gambler's fallacy” (White 2000); the second objection states that the fine-tuning argument can never fully achieve its declared aim in that it can never result in rational belief in an actual multiverse, for such belief would inevitably be based on the fallacy of illegitimate double-counting (Juhl 2009); the third objection states that, for the argument to work, there would have to be a physically well-motivated probability distribution over possible values of the constants (Juhl 2006, Mellor 2012), which we do not have. I propose novel responses to the first and the second objection, respectively. The third objection is especially interesting from the point of view of metaphysics because it implicitly raises the complicated question of how to distinguish between the constants of nature, i.e. real physical quantities, on the one hand, and derived computational artifacts on the other. I outline why multiverse proponents' frequent appeal to

the so-called naturalness criterion may be part of a successful strategy towards answering this third objection and conclude by assessing the status of the multiverse idea in view of all the three objections discussed and by presenting an outlook on the challenges to the multiverse that remain.

Coordination, Measurement, and the Problem of Representation of Physical Quantities

FLAVIA PADOVANI
Drexel University
fp72@drexel.edu

A condition for the objectivity of scientific knowledge rests on the ability to coherently represent the behaviour of measured objects as a good approximation of a theoretical ideal, which appears as some form of "natural prior" with respect to actual measurements. Measurement outcomes can be inferred from instrument indications only against the background of an idealised model, which strictly depends on the scientific theory in use. What one obtains is thus a construct, rather than a "brute fact". Furthermore, the parameters that appear in scientific theories and equations are not pre-existing quantities. As the history of science illustrates, simple items of our scientific knowledge that we take for granted actually arise as outstanding achievement of our scientific conceptualisation and technical progress. In fact, the individuation of certain quantities as parameters for the relevant laws and equations is often developed together with the instruments required in order to measure them.

In his Scientific Representation (2008), van Fraassen has emphasised how measuring should be considered as a form of representation. In fact, every measurement identifies its target in accordance with specific operational rules within an already-constituted theoretical space, in which conceptual interconnections can be represented. So, this space provides the range of possible features related to the measured items expressed within the language of the relevant theory. Without this space of pre-ordered possibilities no objects of representations can be given. In this sense, the act of measuring is "constitutive" of the measured quantities as it allows for the coordination of mathematical quantities to elements of reality, thereby

providing meaning to the abstract representations through which we seek to capture physical phenomena.

In recent years, there has been a revived interest in the notion of "coordination" especially in relation to the issue of scientific representation as van Fraassen has described it. In this connection, Reichenbach's original account of coordination has revealed to be particularly interesting. In his early work, however, the idea of "coordination" was employed not only to indicate a class of very general, theory-specific fundamental principles to be potentially revised (or relativized) in the passage to a new scientific theory—as is usually emphasised—but also to refer to a number of other "more basic" principles. These principles are related not much to the structural features of a theory, but rather to the conceptual presuppositions required in order to approach the world through measurement in the first instance, so they are primarily necessary to translate the unshaped material from perception into some quantities that can be used within the mathematical language of physics. Quite interestingly, in his early writings many of these coordinating principles are conceived as preconditions both of the individuation of physical magnitudes and of their measurement. In other words, they are not limited to the definition of quantity terms but they also involve the individuation of what these quantity terms are supposed to be coordinated to.

The aim of this paper is to reassess Reichenbach's approach to coordination in light of recent literature on measurement and scientific representation.

Holism of Climate Models and their Construction with Empirical Data and Theoretical Knowledge

RISKE SCHLÜTER
University of Münster
riske.schlueter@uni-muenster.de

There are different possibilities to reach the aims of explanation and prediction of climate phenomena. One central method is the development of climate models, which are used to run computer simulations. To fulfil the aims the models need to be testable and provide reliable results. However the

most comprehensive models are very complex and are constructed with the use of different kinds of methods. The methods of model development could be discerned in methods based on large scale empirical data and methods based on theoretical knowledge.

Empirical modelling is sometimes accused for being less reliable, especially when large scale empirical data is used to calibrate the model in order to run computer simulations. However theoretical knowledge alone is not sufficiently applicable for running simulations. This leads to the jointly application of different methods and complex arranged models. Due to the complex arrangement of the models it is not easily possible to attribute shortcomings of the comprehensive models to particular model parts. This forms a problem of holism, as stated by Lenhard and Winsberg in their article "Holism, entrenchment, and the future of climate model pluralism". Their thesis is that climate models are only testable as a whole because of specific problems in the set-up of the computer programs. They emphasise two problems. First the problem of clear division in the computer code, named "fuzzy modularity" and secondly adoptions of the code to particular problems at dependent on development stages, named "kludging". Additionally they claim that sobering results of model intercomparison studies are a corollary of holism.

I will to take a look on the scope of this hypothesis of holism. To achieve this I will analyse different methods of model development and how they potentially can restrict the described holism. The analysed type of study is focused on certain processes and to build better representation of it by theoretical work and specific empirical measurement. With these process studies it is possible to restrict the influence of the holism insofar as it is still possible to test the representation of singular processes. But this is only possible in their isolated behaviour. Even though singular parts can be tested by this method there remains the problem of estimation the relevance of known shortcomings in specific model parts. This problem remains because of the complex interactions between different climate processes.

This analysis enables us to explain, how different approaches of model evaluation and development could be used to improve model results and gain better understanding and better prediction of the earth's climate.

Philosophy of the Life Sciences III **Contributed Papers**
Chair: Jens Harbecke Room 3C, Wednesday 16:45 – 18:45

Disease Entities, Negative Causes of Disease, and the Naturalness of Disease Classifications

PETER HUCKLENBROICH
University of Muenster
hucklen@uni-muenster.de

This paper addresses some philosophical problems that are of fundamental importance to scientific medicine. Generally speaking, these problems are: What are disease entities, and how are they defined? Is it possible to identify causes of disease in a unique and unambiguous way? Are there „negative“ causes of disease, in the sense of absent or lacking conditions? Is it possible to classify diseases according to their causes in a systematic, unequivocal way? What is meant by the „naturalness“ of disease classifications?

One purpose of the paper is to show that these problems are intimately interconnected, and that all solutions to one of them must, simultaneously, solve the interconnected problems.

The paper will start from a critical analysis of Caroline Whitbeck's well-known and important 1977 paper on the disease entity model and will discuss her position alongside the medical and chemical examples she herself employs in this paper, and additionally by a detailed analysis of the case of scurvy as an example of a vitamin deficiency disease. The following theses of Whitbeck's paper are reassessed and, essentially, rejected:
The definition of disease entities depends on practical interests of physicians and/or patients.

The case of multifactorial causation shows that a unequivocal identification of causes of diseases is conceptually impossible.
The „naturalness“ of disease classifications depends on the criterion of „maximizing the number of correct inferences provided by the classification“.

By an in-depth analysis of Whitbeck's own examples, these claims are refuted. Instead, a different analysis and reconstruction of disease entities, causes of disease, and disease classifications is put forward.

References

Whitbeck, C. (1977), Causation in medicine: the disease entity model, Philosophy of Science 44, 619-637

Simon, J. R. (2010), Advertisement for the ontology for medicine, Theoretical Medicine and Bioethics 31, 333-346

The (Dys)functionality of Psychopathy: Perspective from the Philosophy of Science

MARKO JURJAKO
University of Rjeka
mjurjako@gmail.com

The debate on the appropriate social response to psychopathy crucially concentrates on whether psychopaths should be held morally responsible for their actions. The implicit argument has the following structure:

1. Proper function of the psychological capacity X is a necessary condition for the ascription of moral responsibility.

2. Empirical evidence shows that the capacity X is (dys)functional in psychopaths.

3. Therefore, psychopaths are morally (non-)responsible for their actions. (Morse, 2008; Sifferd & Hirstein, 2013)

There seems to be a consensus that at least incarcerated psychopaths are not morally responsible because they lack the necessary moral capacities (Malatesti & McMillan, 2010).

In this paper I argue that the consensus might be premature. Since the debate depends on the empirical evidence we need to be sensitive to problems of interfacing folk-psychological notions (presupposed in moral and legal theories) and neuropsychological data on which the evidence depends. Most crucially the gloss is on the notion of function. Currently there is no explicit view on how the ascription of dysfunction is supposed to be grounded.

Psychological and neuropsychological data has shown that there are behavioural and brain differences between psychopaths and non-psychopaths (Blair, 2008). However not every statistical difference amounts to a dysfunction (Boorse, 1977).

In the psychopathy-literature there seems to be two salient approaches to this issue. The first is top-down. We start with an a priori (folk-psychological) account of capacities that are necessary for ascription of responsibility and then the dysfunction is attributed if the evidence shows that a person does not execute the capacity (Vincent, 2008). Here functions are ascribed via folk-psychology. On the bottom-up approach we start with a reduction thesis according to which folk-psychological terms refer rigidly to specific brain mechanisms (Hirstein & Sifferd, 2010). Then we directly infer dysfunctions from neuropsychological data.

In this paper I examine the bottom-up approach, which promises a more objective route for determining responsibility. This route includes ascriptions of mental disorders. Here the supposition is that functions are ascribed via the selected-effects theory of functions. Therefore the debate on psychopaths' responsibility crucially relates to the question whether psychopathy is an evolutionary adaptation. This is problematic in two respects: first, whether psychopathy is an adaptation is far from being resolved (Glenn, Kurzban, & Raine, 2011); second, it is not clear why determining responsibility should depend on the resolution of this issue.

My proposal for advancing the debate is to explore the idea that psychopathy represents a developmental mismatch (Garson, 2015, chapter 8). The idea is that although psychopathic traits might have had some adaptive value, they still present a harmful mismatch between the present environment and that in which those traits were adaptive. However, judgments of mismatch, being depraved of the notion of dysfunction, will rely on our value judgments that pertain to the issue of harmfulness of the mismatch. This leads to the insight that the resolution of the responsibility issue will include some elements from the top-down approach.

References

Blair, J. R. (2008). The amygdala and ventromedial prefrontal cortex: functional contributions and dysfunction in psychopathy. *Philosophical Transactions of the Royal Society B*, 363, 2557-2565.

Boorse, C. (1977). Health as a Theoretical Concept. *Philosophy of Science*, 44, 542-573.

Garson, J. (2015). The Biological Mind: A *Philosophical Introduction*. London and New York: Routledge: Taylor and Francis Group.

Glenn, A. L., Kurzban, R., & Raine, A. (2011). Evolutionary Theory and Psychopathy. *Aggression and Violent Behavior*, 16, 371-380.

Hirstein, W., & Sifferd, K. (2010). The legal self: Executive processes and legal theory. *Consciousness and Cognition*, 20, 156-171.

Malatesti, L., & McMillan, J. (Eds.). (2010). *Responsibility and psychopathy: Interfacing law, psychiatry and philosophy*. Oxford: Oxford University Press.

Morse, S. J. (2008). Psychopathy and Criminal Responsibility. *Neuroethics*, 1, 205-212.

Sifferd, K., & Hirstein, W. (2013). On the Criminal Culpability of Successful and Unsuccessful Psychopaths. *Neuroethics*, 6, 129-140.

Vincent, N. A. (2008). Responsibility, Dysfunction and Capacity. *Neuroethics*, 1, 199-204.

Mental Disorders as Higher-Order Theoretical Terms

GOTTFRIED VOSGERAU
University of Düsseldorf
gottfried.vosgerau@uni-duesseldorf.de

This talk aims at bringing together the discussion about the nature of mental disorders and the debate about the causal efficacy of higher-level properties, including mental, cognitive and social properties. Considerable theoretical clarification has been achieved through the discussion of the "causal exclusion problem" (Kim, 2005). The argument is grounded in the largely accepted claim that the physical domain is causally complete: any physical event that has a cause (e.g. a behavioral symptom), has a physical cause that is both sufficient and complete (Papineau, 2002). Accepting this claim raises a dilemma for putative mental causes of such behavioral symptoms: either these causes systematically over-determinate the behavioral effects or they are purely epiphenomenal. A way out of the dilemma consists in denying an ontological distinction between the mental causes of symptoms and their physical underpinnings, endorsing a token-identity thesis (Esfeld, 2005; Soom, 2011).

The consequences of this dilemma stand in sharp contrast to the main contemporary accounts of mental disorders. According to the DSM 5 approach, mental disorders are sets of co-occurring symptoms and hence cannot cause symptoms at all. According to the disease analogy of mental disorders, disorders are supposed to cause their symptoms, which raises the causal exclusion problem if mental disorders are purely mental entities (psychodynamic accounts) or impaired cognitive processes (e.g. Andreasen 1997). Ruthlessly reductive accounts assuming mental disorders to be purely physical or neurological impairments (e.g. Kandel 1998) fall prey to the multiple realization argument (Fodor, 1974), thus ultimately resulting in an entirely new classification based on physical criteria exclusively, completely discarding our current classification and diagnostic criteria. Finally, symptom-network approaches (e.g. Borseboom and Cramer 2013) face the causal exclusion problem when it comes to the non-physical variables they take into account.

We offer a theoretically flexible account, according to which mental disorders should be individuated as dispositions to cause specific sets of symptoms. These dispositions are ontologically token-identical to complex states that include neurological impairments, genetic factors, and environmental factors such as social factors. We thereby secure the causal efficacy of mental disorders without conflicting with the completeness of physics, while allowing for multiple realizability. Psychiatric categories should then be conceived of as theoretical terms, individuated by their relations to specific collections of co-occurring symptoms. The individuating relations themselves are understood as high-level causal relations; while there might be very different causal chains on the physical level for two different patients, they can still fall under the same psychiatric category because the different detailed causal stories share common causal features on a higher level of description. These common features allow classifying the different detailed causal chains as belonging to one higher-order class, namely the psychiatric disorder. However, the common causal features are not detectable on a lower level of description (e.g. the physical), since the physical features are too divergent to form a class. Thus, we offer a conservative reductive account that allows identifying mental disorders to complex physical states, while it does not render the higher-level categories eliminable.

General Philosophy of Science III **Contributed Papers**
Chair: Carsten Held Room 3D, Wednesday 16:45 – 18:45

Agent-Based Modeling and Democratic Theory: Improving Normative Arguments through Simulation

SIMON SCHELLER
Otto-Friedrich University Bamberg
simon.scheller@uni-bamberg.de

In this paper, I claim that Agent-based Models (ABM) can improve descriptive models of social interaction. This has an impact on normative arguments since they often rely on factual claims about the target systems. This claim is illustrated using the example of deliberation models in democratic theory. I want to present an ABM in this context, showing how models in deliberative theory can be improved, and what impact this can have on the normative evaluation of democratic mechanisms.

One common justification of democracy makes the argument that democratic mechanisms are fair procedures that respect each individual's preferences equally. However, the ability to aggregate preferences has been strongly criticized by authors like Arrow (1963) and Riker (1982), who mainly argue that such aggregation mechanisms are fundamentally flawed and unable to fulfil even basic conditions of rationality and fairness.

As a result of this criticism, democratic theory has been said to have taken a 'deliberative turn' with many scholars have arguing that democracy's virtue lies in the epistemic quality of the outcomes that it produces (e.g. Estlund 1997). This turn often includes a focus on deliberative procedures instead of mere voting procedures.

Using different modeling techniques, scholars have come to alternative conclusions about the vices and virtues of democracy. For example, Austen-Smith (1990), based on an analytic game theoretic model, concludes that information transmission in debate is usually envisaged as 'cheap talk' and therefore can only have an impact under very rare circumstances. In contrast, Dryzek & List (2003), based on a more informal model, argue that deliberation can serve to overcome the social choice problems of democracy, e.g. via structuring preferences or altering the incentives for truthfulness.

One major problem of analytic models like Austen-Smith's is that they sacrifice descriptive appropriateness for the sake of analytic rigor. Important examples are the assumptions of perfect rationality or unlimited agent capacities. These assumptions are necessary for the derivation of equilibrium results, but are far from describing agents in a realistic manner.

Informal models like the ones by Dryzek & List may be better able to provide a higher level of descriptive detail. Yet they often fail to conclusively depict the conditions under which the implied conclusions actually come to bear. For example, while deliberation may enable issue-specific logrolling to overcome aggregation problems, there may be hidden assumptions that are necessary to produce that kind of result, which go along with other, unforeseen consequences.

To address the weaknesses of analytic and informal models, this paper recommends ABM as a way to minimize the inherent trade-off between analytical rigor and descriptive detail. ABMs are able to set up more realistic models, which remain tractable via computer simulation.

In democratic theory, normative arguments about the value of democratic procedures must be based on descriptive models of those procedures. A statement like 'democratic mechanisms produce epistemically superior outcomes' should be substantiated by models that show under what conditions this is plausible. In many cases, ABMs can provide those descriptive underpinnings.

The Synchronized Aggregation of Beliefs and Probabilities

CHRISTIAN J. FELDBACHER
University of Düsseldorf
christian.feldbacher@gmail.com

In this paper, we connect two debates concerning doxastic systems. First, there is the debate on how to adequately bridge quantitative and qualitative systems of belief. At the centre of this discussion is the so-called Lockean thesis (LT), according to which a proposition A is believed by an agent iff the agent's degree of belief in A exceeds a specific threshold r, i.e.: Bel(A) iff P(A)>r. It is well known that this thesis can come into conflict with other

constraints on rational belief, such as consistency (CO) and deductive closure (DC), unless great care is taken. Leitgeb's (2014) stability theory of belief provides an elegant means for maintaining (LT), (CO), and (DC). The theory is based on the notion of P-stability. A proposition, B, is P-stable-r (for a probability function P) iff for all C consistent with B: $P(B|C)>r$.

Beyond the debate concerning how to relate quantitative and qualitative systems of belief, there are debates concerning how to adequately aggregate qualitative belief sets, on the one hand, and probability functions, on the other. In the literature on opinion pooling and social choice, several constraints on such aggregations are discussed, centering on Arrow's (1950) impossibility result. Similar results apply to the aggregation of qualitative belief sets and probability functions. The former result is known as `discursive dilemma'.

Given the debate on the relationship between qualitative and quantitative belief, and the debate concerning how to aggregate belief systems of the two types, it is quite natural to ask whether qualitative and quantitative aggregation can be performed in a 'synchronized' way. In particular, is it possible to devise systems of qualitative and quantitative belief aggregation, such that when we aggregate corresponding qualitative and quantitative belief systems, that are related according Leitgeb's stability theory (thereby ensuring the satisfaction of (LT), (CO), and (DC)), the outputted belief systems are also related according Leitgeb's stability theory (so satisfying (LT), (CO), and (DC))?

We present a variety of results bearing on the preceding question. Under the assumption of reasonable aggregation principles, stability-r is not generally preserved when aggregating corresponding qualitative and quantitative belief systems. On the other hand, if a pair of inputted belief sets, B1 and B2, are stable-r for their corresponding probability functions P1 and P2, and $r > 2/3$, then the aggregate of B1 and B2 is stable-r/(2-r) for the aggregate of P1 and P2 (where the aggregate of B1 and B2 is conservative, in a sense to be explained, and the aggregate of P1 and P2 is formed by a weighted average of P1 and P2).

Exploratory Modes of Scientific Inquiry: From Experimentation to Modeling

AXEL GELFERT
National University of Singapore
phigah@nus.edu.sg

The importance of exploratory modes of scientific inquiry has recently begun to receive attention from historians and philosophers of science, especially those who focus on scientific experimentation. Thus, Friedrich Steinle has argued that 'exploratory experimentation' is a suitable strategy in situations where no well-formed theory or conceptual framework is available (or is regarded as reliable); in such a scenario, the elementary desire to obtain any sort of empirical regularity may prevail over more specific theoretical research questions. Similarly, Richard Burian has argued that an important goal of exploratory experiments is the stabilization of phenomena or concepts that, at a theoretical level, are at best partially understood. Finally, as Uljana Feest has pointed out, concepts themselves—such as the notion of operationalization in psychology—may themselves play an exploratory role in enabling the experimental study of empirical phenomena. The present paper analyzes and clarifies the notion of exploration in scientific research in two ways: first, by distinguishing between a 'convergent' and a 'divergent' sense of 'exploration', and second, by applying the idea of exploration to the case of scientific modeling. The shift from experimentation to modeling necessitates a number of significant changes in emphasis: for one, several strategies that have been identified as typical of exploratory experimentation, such as the simultaneous variation of a large number of different parameters, do not readily generalize to exploratory modeling. For the experimenter who intervenes in nature and explores the dynamics of the target system through causal means, variation of experimental parameters requires skill and, when successful, constitutes a great achievement. For models, this need not be the case, since variation of parameters, for example in the case of mathematical models, may come too cheaply: curve-fitting would not, in either the convergent or the divergent sense, count as an interesting case of 'exploration'. Instead, I shall identify four distinct functions that models serve in exploratory research: they may (1) function as a starting point for future inquiry, (2) feature in proof-of-principle demonstrations,

(3) generate potential explanations of observed (types of) phenomena, and (4) may lead us to assessments of the suitability of the target. These functions are neither mutually exclusive, nor should they be seen as exhausting the exploratory potential of models. They do, however, represent the spectrum of exploratory uses to which models may be put, which ranges from 'weak' uses—such as taking a model as a 'starting point', for want of a better alternative—to 'stronger' (in particular, explanatory) uses, which may result in greater understanding or lead to a reformulation of the initial research question. This general framework for thinking about the exploratory uses of models will be illustrated via examples of models from population biology, human geography, organic chemistry, and statistical physics.

Causality **Contributed Papers**
Chair: David Hommen Room 22, Wednesday 16:45 – 18:45

Interventions or Ranks?

TOBIAS HENSCHEN
University of Konstanz
tobias.henschen@uni-konstanz.de

In The Laws of Belief, Spohn (2012: 361) claims that the similarity of his definition (14.3) of direct causation to Woodward's (2003: 55, 59) definition (DC) of direct causation "is obvious". This claim is somewhat surprising because the dissimilarity of (14.3) and (DC) is at least as obvious as their similarity. The paper is to emphasize their dissimilarity and to dwell on the question of what might lead one to prefer the one to the other.

It will first recapitulate the three commonalities and six differences mentioned by Spohn (2012: 361-2):

(C1) (14.3) and (DC) refer to wiggling.

(C2) (14.3) and (DC) are frame-relative.

(C3) Conditioning is basic in (14.3) and (DC).

(D1) (14.3) is a definition of token-level, (DC) one of type-level causation.

(D2) (14.3) obtains asymmetry through temporality, (DC) through interventions.

(D3) (14.3) is non-circular, (DC) circular.

(D4) (14.3) is subjectivist, (DC) objectivist.

(D5) (14.3)-wiggling is epistemic, (DC)-wiggling actual or counterfactual.

(D6) Conditioning is different: (14.3) defines direct causation in terms of conditional ranks; in the case of (DC), the distribution resulting from interventions is defined in terms of conditional probabilities.

Because of its frame-relativity and the dependence of variable selection on "serious possibility" (Woodward 2003: 56), (DC) may also be read as subjectivist. But this reading conflicts with the type of realism that Woodward (2003: 121) wishes to endorse. The paper therefore interprets (DC) as objectivist and, consequently, (DC)-wiggling as non-epistemic.

It will secondly add the commonality that

(C4) (14.3) and (DC) perform excellently with respect to counterexamples (cf. Woodward 2003: 74-86; Spohn 2012: 362-9),

a commonality that puts them on top of competing definitions, and argue that while (D4) – (D6) are differences "in essence", (D1) – (D3) are differences "in style". That they are differences in style is supposed to mean that they cannot decisively influence preferences for (14.3) or (DC): (D1), for instance, cannot influence these preferences because direct token-level causation in the sense of (14.3) can be generalized to direct type-level causation in the sense of a subjective causal law (cf. Spohn 2012: 289-90). Of the three differences in essence, it's especially (D6) that Spohn (2012: chs. 3.3, 10) has a lot to say about. But the paper will focus attention to (D4) and (D5).

It will finally argue that what might lead one to prefer a subjectivist to an objectivist definition and epistemic to actual or counterfactual wiggling is one of two conceptions of causal evidence. According to both conceptions, causal evidence is necessarily inconclusive: the truth of claims of direct type- or token-level causation relies on the truth of the non-testable hypothesis that common causes are absent. But while according to the first conception, the inconclusiveness of causal evidence implies that such claims cannot be justified at all, according to the second conception, such claims can be justified if the inconclusiveness of the evidence is sufficiently low. While the first conception is philosophically forceful and supportive of (14.3), the second makes concessions to scientific practice and is more in line with (DC).

References
Spohn, W. (2012): The Laws of Belief. Oxford: OUP.
Woodward, J. (2003): Making Things Happen. Oxford: OUP.

Causal Modelling and the Metaphysics of Causation
(CANCELLED)

VERA HOFFMANN-KOLSS
University of Cologne
vera.hoffmann-kolss@uni-koeln.de

Causal modelling accounts and the interventionist account of causation have gained wide acceptance in contemporary philosophy of science. One putative advantage of these accounts is that they rely on fewer metaphysical assumptions than alternative theories, Particularly, Lewis's counterfactual account, which presupposes similarity relations between possible worlds. The aim of this paper is to argue that unless causal modelling accounts are supplemented by additional metaphysical assumptions, that is, similarity relations among possible worlds, they are too weak to distinguish genuine causation from pseudocausation.

The central idea underlying causal modelling accounts is that causes are difference-makers for their effects. A variable X (representing states or properties) is classified as causally relevant to a variable Y iff it is possible to carry out an intervention on X which changes the value or the probability distribution of Y (Hitchcock 2007; Woodward 2003).

This approach to causation turns out to be problematic when applied to examples of the following type (Jackson 1982; Kroedel 2015): polar bears have warm and thick coats which protect them from icy temperatures. Insulation capacity correlates with weight, that is, warm coats tend to be heavy. Now, consider the following three variables:

W: 1 if the polar bear's coat is warm; 0 otherwise
H: 1 if the polar bear's coat is heavy; 0 otherwise
P: 1 if the animal is protected from icy temperatures; 0 otherwise

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Intuitively, only W, but not H is causally relevant to P. However, according to a causal modelling account, both, W and H, are classified as causally relevant to P. If the value of W was changed from 1 to 0, the value of P would change, too. Moreover, assume that the value of H is changed from 1 to 0. Given the correlation between W and H, this will likely change the value of W from 1 to 0. But then the probability distribution of P will change, too, and H is misclassified as causally relevant to P.

I argue that the difficulties raised by these and analogous cases are currently underestimated. Causal modelling accounts do not provide the conceptual resources to distinguish genuine causation from pseudo-causation in all cases. Furthermore, I argue that this problem can be solved by re-introducing similarity relations among possible worlds. The account which I propose relies on the observation that classifying a variable such as H as causally relevant to P requires more divergence from the actual world than classifying W as causally relevant to P: in the case of H, the value of W has to be changed, too, whereas in the case of W, it does not matter whether or not the value of H is kept fix.

References

Hitchcock, C. (2007), 'Prevention, Preemption, and the Principle of Sufficient Reason', *Philosophical Review 116(4)*: 495-532.

Jackson, F. (1982), 'Epiphenomenal Qualia', *Philosophical Quarterly 32*: 127-136.

Kroedel, T. (2015), 'A Simple Argument for Downward Causation', *Synthese 192(3)*: 841-858.

Woodward, J. (2003), *Making Things Happen: A Theory of Causal Explanation*, Oxford University Press.

Cancelled

Is There A Monist Theory of Causal and Non-causal Explanations? The Counterfactual Theory of Scientific Explanation

ALEXANDER REUTLINGER
LMU Munich
Alexander.Reutlinger@lrz.uni-muenchen.de

In current philosophy of science, the most widely accepted account of scientific explanation is the causal account of explanation. I argue that a re-evaluation of the received causal account is needed for the following reason: the causal account cannot provide a general theory of all scientific explanations, since there are compelling examples of what appear to be non-causal explanations. The main goal of this talk is (1) to develop a more adequate account of scientific explanation – a counterfactual account – that provides a unified framework for both causal and non-causal explanations, and (2) to identify criteria for distinguishing between causal and non-causal explanations.

According to the causal account of explanation, the sciences explain phenomena iff they identify the causes of (or the causal mechanisms for) the phenomenon to be explained (see Cartwright 1983, 1989; Salmon 1984, 1998; Lewis 1986; Machamer, Darden and Craver 2000; Woodward 2003; Craver 2007; Strevens 2008). However, is it really the case that all scientific explanations causal explanations?

The answer to this question seems to be negative, because scientists give non-causal answers to why-questions. Since the early 2000s, a number of compelling examples of (seemingly) non-causal explanations have re-entered the arena of philosophy of science. Examples of non-causal explanations come in a surprising diversity: for instance, they are based on non-causal laws, purely mathematical facts, symmetry principles, renormalization group methods, inter-theoretic relations, and so forth (see, for instance, Batterman 2002; Bokulich 2008; Huneman 2010; Lange 2011, 2013a,b; Pincock 2012; Saatsi and Pexton 2013; Weatherall 2011).

The goal of this talk is to advance a constructive approach to non-causal explanations:

(1) I provide an account of what makes non-causal explanations explanatory.

(2) I propose criteria for distinguishing causal and non-causal explanations.

Regarding (1), I argue that non-causal explanations work by revealing non-causal counterfactual dependencies between explanandum and explanans. Such a counterfactual account of non-causal explanations is an extension of Woodward's (2003) causal version of the counterfactual account. Hence, the counterfactual account provides a unifying framework for causal and non-causal explanations – both are explanatory because they reveal counterfactual dependencies, or so I will argue (see Frisch 1998; Bolkulich 2008; Saatsi and Pexton 2013). Causal explanations are explanatory in revealing causal counterfactual dependencies (based on causal generalizations) between explanandum and explanans. Non-causal explanations are explanatory in revealing non-causal counterfactual dependencies (based on non-causal generalizations) between explanandum and explanans. I will argue for the adequacy of the counterfactual account by applying it to three paradigmatic kinds of non-causal explanations: (a) purely statistical explanations, (b) renormalizations group explanations, and (c) genuinely mathematical explanations.

Regarding (2), I propose to distinguish between causal and non-causal explanations on the basis of so-called Russellian criteria of causation – including criteria such as asymmetry, time-asymmetry, the distinctness and locality of causal relata, intervenability, and so on. I argue that an explanatory relation is causal iff all of the Russellian criteria apply to it; otherwise it is non-causal.

Symposia & Contributed Papers IV

Methodological Challenges in Quantum Gravity **Symposium**
Organizer: Christian Wüthrich, Keizo Matsubara, Richard Dawid
Chair: Wolfgang Pietsch Room 24, Thursday 11:00 – 13:00

The Use of Black Hole Thermodynamics as Non-Empirical Confirmation

CHRISTIAN WÜTHRICH
University of Geneva
Christian.Wuthrich@unige.ch

On Predictions and Explanations in Multiverse Scenarios

KEIZO MATSUBARA
University of Illinois at Chicago
keizom1@uic.edu

Can We Make Sense of the Final Theory Claim in String Theory?

RICHARD DAWID
LMU Munich, MCMP
richard.dawid@univie.ac.at

General Description

There are excellent reasons to believe that gravity must ultimately be described by a quantum theory, just like all other fundamental forces. Most prominent among the myriad approaches to formulating a quantum theory of gravity are string theory and loop quantum gravity, but none of them stand completed. Although fully formulating such a theory thus remains an unfulfilled challenge, there are strong indications that any viable quantum theory of gravity will radically modify our understanding of foundational aspects of physics, such as necessitating a deep reconceptualization of space and time. In fact, there are strong suggestions that the very methodology of empirical science may have to be reconsidered.

Often, the methodological shifts which emerge in the field are related to one of the core problems faced by quantum gravity: due to the very high energy scales involved, it is exceedingly difficult to probe the regimes in which quantum gravity becomes relevant to find direct empirical evidence. It therefore becomes an increasingly important element of methodology in quantum gravity to consider indirect evidence for physical assumptions. The three talks of this symposium will all address contexts of research in quantum gravity where entirely new perspectives on physics - and as a result, new methods of dealing with physics - emerge or may emerge not based on a direct reaction to observed empirical anomalies but based on conceptual reasoning that suggests that those new perspectives may become unavoidable.

Talk 1 discusses the case of the generalized second law of thermodynamics, which has been considered a pivotal pillar of our understanding of semi-classical and quantum theories of gravity and whose fate may be decided in part based on the formal analysis of the physical principles of quantum gravity. The interesting point in the given case is that already the belief in the viability of the generalized second law was not based on empirical data but rather on plausibility arguments within the context of theory building.

Talk 2 discusses the problem of multiverse scenarios, which cannot get direct empirical support beyond the regime of “our” universe. The legitimacy of multiverse constructions thus crucially relies on specifying the methodology of accounting for indirect evidence. Whether multiverse scenarios end up being generally acknowledged as legitimate physical claims will in part depend on whether a satisfactory methodology of this kind will be forthcoming.

Talk 3, finally, addresses the case of final theory claims, which are by their very nature inaccessible to direct empirical evidence but emerge from the character of string theory. Once again, whether or not final theory claims will eventually be understood as a genuine and legitimate element of physics will depend on the success of physical research in providing a method of establishing conditions for acknowledging the plausibility of a final theory claim based on indirect or non-empirical evidence.

Abstracts

1. Christian Wüthrich: *The Use of Black Hole Thermodynamics as Non-Empirical Confirmation*

Physicists working in quantum gravity vehemently disagree as to what we should take the fundamental principles of physics to be. It seems that hardly any principle of hitherto successful physics is immune to this disagreement: from the unitarity of quantum-mechanical evolution to the general covariance of general relativity, to whether there is spacetime at all at the fundamental level, the status, relevance, and even the very articulation of principles that constitute the pillars of our firmly established physics have been challenged in our quest to find the elusive theory of quantum gravity.

It is surprising, then, that physicists from all camps appear to agree on one thing: that black holes have entropies, and that this entropy is correctly given by Bekenstein's famous formula. As suggestive as Bekenstein's reasoning is, he bases the formula on analogies between black hole physics and thermodynamics, information theory, and on the validity of what he dubs the "generalized second law". The generalized second law asserts that the total entropy of a system, i.e., the sum of the common entropy and the black hole entropy, never decreases. In my talk, I analyze the grounds on which the entropy formula as well as black hole thermodynamics more generally is justified.

My analysis will focus on the following aspects of black hole thermodynamics. The proportionality of the entropy of a black hole to its surface area is justified by an analogy based on an information-theoretic understanding of entropy. The proportionality factor is fixed using semiclassical considerations concerning the size and informational content of particles falling into the black hole. Finally, the generalized second law is asserted.

All this raises questions first concerning the status and ultimate justification of information theory in quantum gravity. Since it relies on the notion of an epistemic agent, the use of information theory in fundamental physics is questionable. Second, though reasonable, the semiclassical assumptions entering the calculation of the proportionality factor may require revision in the light of a full quantum theory of gravity and cannot be used to trump facts about which principles are, or are not, incorporated into a fundamental theory.

Third, the validity and the implications of the generalized second law will be scrutinized. Here, two facts must be noted. First, the asserted validity of

the law depends precariously on thought experiments involving the physics in the strong gravitational field in the vicinity of the event horizon of a black hole. This raises worries as to the reliability of these speculations in a regime where arguably both quantum and relativistic effects will interact, in perhaps unpredictable ways. Second, as Aron Wall has recently shown, the generalized second law can be used to prove a quantum singularity theorem in the vein of the celebrated singularity theorems in general relativity. If borne out in the actual semiclassical regime of black holes, this would either mean that the classical singularities will not be washed out in a quantum treatment, or that the generalized second law does not hold.

2. Keizo Matsubara: *On Predictions and Explanations in Multiverse Scenarios*

Claims that our universe is part of a multiverse have become prevalent in physics. For instance, string theory seems to allow for a large number of possible ground states resulting in different effective laws of nature. This is called the "landscape" of string theory. Thus, most string theorists have abandoned the hope of deriving the parameters of the standard model as a more or less unique prediction.

To explain why our universe is apparently fine-tuned to allow for our existence, arguments using the controversial anthropic principle have been used. Many have argued that a multitude of the ground states are physically realized; we live in a multiverse.

For explanatory purposes, string theorists need a physical mechanism to populate the landscape so that many ground states are physically realized. The most popular is to describe the multiverse in terms of "bubbles" formed in eternal inflation. Another option could be to use an Everettian understanding of quantum mechanics.

However, if a suitable mechanism for populating the landscape can be established such that string theory entails the existence of a multiverse; then this would provide an explanation for the fine-tuning in our universe. This would be similar to the explanation for why we live on a planet hospitable to life. When a theory can give an explanation to something previously unexplained, this is typically taken as giving support to the theory.

But a problem here is that the ideas are pretty generic. They do not connect specifically to string theory. The same explanation could be used by

another alternative multiverse scenario if there exists solutions compatible with our existence.

This is true but it could be argued that only real alternatives, which are presented, embedded in sufficiently developed theories, should be taken into account. Thus an anthropic explanation for fine-tuning should be seen as providing some evidence - although far from conclusive - in favor of string theory. If other alternative explanations for the fine-tuning, anthropic or otherwise, would be provided by different sufficiently developed theories then the epistemic value of the explanation provided by string theory would of course diminish. Internal theoretical considerations and consistency with earlier established theories would be relevant for deciding what would count as a sufficiently well developed theory.

While the anthropic explanation could be taken to lend some epistemic support for string theory, it would definitely be even better if some testable predictions could be performed.

For a theory entailing a multiverse to produce testable predictions it is important that the allowed solutions are sufficiently constrained.

I will argue that in the correct way of making predictions in a multiverse, our role as conscious observers has no special importance. This is in contrast to how the anthropic principle is typically invoked for explanatory purposes. This is because one must base predictions on all hitherto observed factors in the universe, regardless of whether or not they are conducive for the development of consciousness.

3. Richard Dawid: *Can We Make Sense of the Final Theory Claim in String Theory?*

One remarkable aspect of string theory is its final theory claim. This claim may be characterized as a two-step argument. First, string theory aims at providing a universal and unified theory of all fundamental interactions. Therefore, assuming that string theory is a viable theory, known phenomenology provides no reason for going beyond string theory in order to achieve a higher degree of universality. Second, and more importantly, T-duality implies that the characteristic scale of string theory, called the string length, constitutes a minimal length scale: statements on length scales smaller than the string length can, due to T-duality, always be expressed as statements about length scales larger than the string length and are therefore fully redundant. This implies that, given that string theory is true, no novel physics

can be found at length scales smaller than the string length. String theory is a final theory.

String theory's final theory claim sets the theory apart from all previous physical theories. It must not be equated with earlier finality claims sometimes associated with late 19th century physics or the posit of elementary particles. While the latter claims merely expressed an external assessment of a specific theory's status, string theory's final theory claim as established by the minimal scale argument is generated by the theory itself. For that reason, it has been strongly emphasized by leading string theorists; Edward Witten in particular, and arguably represents a crucial element of string theorists' view of their theory.

Nevertheless, string theory's final theory claim has a paradoxical flavor to it. In effect, it seems to say: if string theory is true, it will never be superseded by an empirically better theory. But doesn't this follow by definition from the theory's truth? A theory at variance with the phenomenology by definition cannot be true. So what can be the philosophical significance of a theory-based final theory claim at all?

The present talk will give a two-layered answer to this question. First, it is conceded that string theory's final theory claim cannot exclude the existence of a more fundamental theory that does not have the duality structure of string theory and therefore allows for new phenomenology at smaller distance scales. This implies that the final theory claim on its own carries little epistemic weight. Second, however, it will be pointed that the final theory claim can be embedded in arguments of non-empirical theory confirmation. The latter arguments provide research context-based indications for a theory's viability at its own characteristic scale by addressing the question of the underdetermination of theory building. They do not, however, address the question of finality. String theory's final theory claim can be understood as an argument that reduces the finality question to the more limited question of the underdetermination of theory building at a given energy scale. Therefore, if non-empirical confirmation is strong in a given context, a final theory claim can have epistemic relevance.

Values in Science I **Contributed Papers**
Chair: Cornelis Menke Room 3B, Thursday 11:00 – 13:00

Agnotological Challenges: How to Capture the Production of Ignorance

MARTIN CARRIER
Bielefeld University
martin.carrier@uni-bielefeld.de

Following Robert Proctor, the notion of "agnotology" is supposed to designate the active creation and preservation of confusion and ignorance. Certain positions are advocated in order to promote non-epistemic, i.e., economic or political, aspirations with the result of creating mock controversies. I intend to show, first, that a symmetrical, but neglected branch of agnotology concerns the maintenance of unjustified agreement. It is not only deliberate opposition, but also a deceiving consensus that might betray the impact of non-epistemic factors. Second, I wish to defend a rather broad notion of non-epistemic aspirations that includes metaphysical interests and allows us to extend the notion of agnotological machinations to fundamental research as well. Recent experience has made it obvious that epistemically driven research projects are not immune to falsification and deceit. Restricting agnotological endeavors to economic and political motives seems inappropriately narrow in that it would hide structural similarities across different research activities. Third, I wish to discuss a recent proposal which considers the shift of inductive risks by violating established methodological standards as the chief criterion for identifying agnotological maneuvers. I present a counterexample in which these characteristics obtain as a result of bona fide reasoning. I do not consider this case as an instance of agnotology and conclude that the appeal to intentions and goals is indispensable for identifying agnotological endeavors. Conversely, relying exclusively on the impact of methodological decisions (such as shifting risks on non-standard grounds) does not suffice for this purpose. Drawing on motives does not create serious empirical difficulties since the relevant actors are typically eager to lay open what drives them. Fifth, I wish to present an alternative proposal that sees agnotological ploys characterized by the adjustment of standards of judgment in a way that facilitates the adoption of hypotheses favored by non-epistemic interests. Further, this change is veiled and the

hypothesis thereby confirmed is used as if no such adjustment had been made. That is, it is the difference between the design of a study and the use made of this study that distinguishes agnotological ploys. Sixth and finally, although the relevant non-epistemic motives are often frankly divulged by agnotological actors and are thus publicly accessible, it is often not possible to identify a neutral, objective stance from which a supposedly biased approach deviates. To be sure, discrepancies between design and use of a study and the fit between the alleged outcome and the non-epistemic aspirations involved can be revealed. But the best way of correcting a deliberately one-sided study is conducting a contrasting study that addresses the perspective neglected in the first one. That is, the most effective antidote to agnotology is not neutrality but plurality.

The Suppression of Medical Evidence

ALEXANDER CHRISTIAN
Heinrich-Heine University, DCLPS
christian@phil.hhu.de

One of the most serious concerns about financial conflicts of interest in medical research is that they can lead to the suppression of medical evidence that is at odds with commercial interests of financiers, i.e. pharmaceutical companies. Suppression of evidence in terms of „active process[es] to prevent data from being created, made available, or given suitable recognition“ (Martin, 1999, 334) runs contrary to principles of good scientific practice like honesty, openness or respect for the law (Shamoo & Resnik, 2015). It can result in ignorance, misrepresentation of research findings and a suspension of scientific self-correction. Since it is widely assumed that clinical trial registries (CTRs) provide an effective means to prevent data suppression (Dickersin & Rennie, 2003), it is important to find out whether and how CTRs can be outwitted by pharmaceutical companies. I intend to show that there are indeed multiple strategies for the suppression of medical evidence, but all these strategies can be countered by a few individual and institutional arrangements.

Section 1 of this paper illustrates the problem of data suppression with the ongoing controversy about the antiviral medication Tamiflu®. Section 2 then analyzes data suppression in medical research and explores possible conflict between data suppression and responsible conduct of research. Sections 3 provides a detailed overview on questionable research practices used for the suppression of medical evidence in clinical trials and scientific publishing. In particular, it answers the main question, whether and how clinical trial registries can be outwitted by pharmaceutical companies. Finally, in section 4, I describe several responses from the scientific community and discuss additional instruments against data suppression that foster epistemic integrity and professional responsibility.

References

Dickersin, K., & Rennie, D. (2003). Registering Clinical Trials. Jama, 290(4), 516–523.

Martin, B. (1999). Suppressing Research Data: Methods, Context, Accountability, and Responses. Accountability in Research, 6, 333–372.

Shamoo, A. E., & Resnik, D. B. (2015). Responsible Conduct of Research (THIRD EDITION). New York: Oxford University Press.

Die Ethik des Plagiierens / The Ethics of Plagiarizing
(CANCELLED)

LEONHARD MENGES
Humboldt University of Berlin
almenges@gmx.de

In der wissenschaftsphilosophischen Literatur wird unter Plagiieren üblicherweise die Übernahme fremden Gedankenguts unter Anmaßung der Urheberschaft in einer wissenschaftlichen Arbeit verstanden. Ziel des Vortrags ist es, zu prüfen, wie sich das wissenschaftliche Plagiatverbot, wie es etwa die DFG formuliert, rechtfertigen lässt. Insbesondere wird diskutiert, ob das Plagiatverbot nur gerechtfertigt werden kann, indem man nicht-wissenschaftliche Werte und Normen an die Wissenschaft heranträgt, oder ob es auch durch Werte und Normen begründet werden kann, die Teil der Wissenschaft selbst sind. Denn nur wenn eine Wissenschaftlerin oder ein Wissenschaftler gegen ein Verbot verstößt, das sich auf diese Weise rechtfertigen lässt, handelt es sich um ein eigentlich wissenschaftliches Fehlverhalten.

Im Vortrag werde ich dafür argumentieren, dass typische Versuche, das Plagiatverbot zu rechtfertigen, aus drei Gründen nicht überzeugen: Erstens wird es oft mit Verweis auf wissenschaftsexterne Werte wie Gerechtigkeit begründet (etwa: Plagiate bringen die eigentlichen Urheber um ihren verdienten Lohn). Auf diese Weise kann jedoch nicht begründet werden, dass Plagiieren ein wissenschaftliches Fehlverhalten ist.

Zweitens wird in der Literatur versucht, das Plagiatverbot damit zu rechtfertigen, dass Plagiatoren nicht die Aufgaben erfüllen können, die wissenschaftliche Autoren erfüllen sollen, nämlich ihre Thesen auf bestmögliche Weise zu begründen. Es scheint mir plausibel, dass auf diese Weise gezeigt werden kann, dass Plagiieren in einer empirischen Arbeit ein wissenschaftliches Fehlverhalten ist. Denn es scheint einleuchtend, dass Plagiatoren nicht solche Thesen auf bestmögliche Weise begründen können, die auf empirischen Daten beruhen, an deren Erhebung und Interpretation sie nicht beteiligt waren. Doch kann dieses Argument nicht auf nicht-empirische Wissenschaften übertragen werden. Philosophisch geschulte Plagiatoren zum Beispiel können prinzipiell ein philosophisches Argument genauso gut begründen wie diejenigen, die es zuerst entwickelt haben. Wenn das stimmt,

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kann auf die skizzierte Weise kein allgemeines Plagiatverbot begründet werden, sondern nur eins für empirische Arbeiten.

Drittens wird in der Literatur, soweit ich sehe, nicht hinreichend der Tatsache Rechnung getragen, dass Plagiate dabei helfen können, eins der primären Ziele der Wissenschaft zu erreichen, nämlich Wissen der Öffentlichkeit zugänglich zu machen (man stelle sich vor, das Plagiieren geht mit einer Übersetzung von einer Sprache, die wenige lesen können, in eine andere Sprache einher, die viele verstehen).

Im Vortrag werden die hier skizzierten Probleme für typische Versuche, das Plagiatverbot zu rechtfertigen, genauer vorgestellt und diskutiert. Abschließend wird ein positiver Alternativvorschlag vorgestellt: Plagiieren lässt sich als wissenschaftliches Fehlverhalten ausweisen, wenn man annimmt, dass Originalität ein wissenschaftlicher Wert ist. Die Pointe dieser Begründung des Plagiatverbots besteht darin, nicht nur das Generieren von zuverlässigem Wissen als entscheidenden wissenschaftlichen Wert anzunehmen, sondern das Generieren von originellem zuverlässigen Wissen. Jemand, der plagiiert, gerät mit diesem Wert offensichtlich in Konflikt. Und so ließe sich zeigen, dass Plagiieren ein wissenschaftliches Fehlverhalten ist. Der Vortrag endet mit der Diskussion der Frage, ob sich allgemein begründen lässt, dass Originalität ein wissenschaftlicher Wert ist.

Cancelled

Philosophy of the Life Sciences IV **Contributed Papers**
Chair: Anne S. Meincke Room 3C, Thursday 11:00 – 13:00

Evolutionary Explanations

SUSANNE HIEKEL
University of Duisburg-Essen
susanne.hiekel@uni-due.de

In the philosophy of biology, two opposing interpretations of Darwin's ‚one long argument' are defended. The first interpretation, advocated for example by Michael Ghiselin or Michael Ruse, understands the argument in terms of a Hempelian account of historical explanation. The second interpretation, advocated by Stephen J Gould, emphasizes the historical dimension of the argument and regards it as implying a narrative historical methodology. According to the Hempelian account, a scientific explanation is only given if the event which is to be explained can be subsumed under a law-like universal hypothesis. According to Ghiselin and Ruse, the argument of the 'Origin of species' is to be reconstructed in that way. Gould, by contrast, stresses that evolutionary events are "particulars of history, rather than necessary expressions of law" (Gould, 2002, p.1333).

With this conflict in the background, two different, more or less tacitly presupposed methodologies of historical explanations – the Hempelian account and Arthur C. Danto's narrative account of historical explanation – are presented in general and then transferred to an explanation of an evolutionary event: the endosymbiosis. According to the theory of endosymbiosis, recent eukaryotic cells evolved because of symbiosis events that led to the development of the organelles (mitochondria and plastids) of eukaryotic cells.

More specifically, I argue that the Hempelian account – apart from the fact that it faces general difficulties such as the problems of overdetermination, of full description and of prediction – falls short of capturing a specific aspect of natural history: the particularity of evolutionary events. By contrast, a narrative account which draws on Arthur C. Danto´s explanation model avoids the problems of the covering law model and does justice to

this aspect of natural history. Consequently, a historical explanation of evolutionary events is defended, which is in tune with Danto's historical explanation.

References

Arthur C. Danto: Narration and Knowledge. (New York: Columbia University Press, 2007)
Michael Ghiselin: The Triumph of the Darwinian Method (Berkeley: University of California Press, 1969)
Stephen Jay Gould. The Structure of Evolutionary Theory (Cambridge Mass., London, Belknap Press of Harvard University Press, 2002)
Carl G. Hempel: The Function of General Laws in History. In: Patrick Gardiner (Hrsg.): Theories of History (New York: The Free Press, 1959)
Michael Ruse: The Darwinian Revolution: Science Red in Tooth and Claw. (Chicago: University of Chicago Press, 1979)

Types of Environments and Multi-Level Natural Selection

CIPRIAN JELER
University of Iasi
ciprianjeler@yahoo.com

This paper can be summarized in a very simple question: in order for us to say that differences in fitness between two types of organisms are the result of natural selection, do we need to have those fitnesses compared within a common selective environment or within an identical selective environment? This is a question that has remained unaddressed both in Robert Brandon's 1990 book Adaptation and Environment and in more recent papers (by Brandon himself, Roberta Millstein and others) about the notion of environment, and I will argue that significant conclusions may derive from the answer we decide to give to this question.

Certainly, this question is not a very important one for the cases of natural selection that most readily come to mind: in most situations, a common environment can reasonably be assumed to be an identical environment, not necessarily for the individuals of that population, but rather for the types of individuals therein. However, things may not be so simple for cases in

which fitnesses are frequency-dependent or for cases involving selection at multiple levels. In fact, through an analysis of frequency-dependent selection, this paper will show that we already have a traditional – even though implicit – answer to this question in evolutionary theory. In other words, I will show that since frequency-dependent selection is usually viewed as being natural selection (and not, for example, as being the evolutionary change that takes place due to the fact that types are distributed over heterogeneous environments), this means that common selective environments are taken to be relevant for natural selection, and not identical selective environments.

Furthermore, this conclusion allows us to intervene in a debate that has recently been taking place in multi-level selection theory regarding the correct definition of group and individual selections in multi-level selection scenarios in which group fitnesses are defined as the average individual fitnesses of their members and in which the "target of interest" is the change in the mean of the distribution of a particular individual trait. As has been argued repeatedly in recent years, the two statistical partitions of the evolutionary change that are usually used in this type of cases – known as the Price approach and the contextual approach – employ different definitions of group and individual selection, and the point of contention is which of these definitions are more appropriate. However, it can be shown that the definition of individual selection implied by the Price approach is in accord with the idea of the necessity of a common selective environment for natural selection, whereas the definition of individual selection implied by the contextual approach is consonant with the necessity of an identical selective environment. Therefore if, as argued above, the common selective environment view is preferable – or, in any case, is more in line with the traditional evolutionary view –, then this constitutes a noteworthy argument in favor of the Price approach.

On the Explanatory Character of the Serial Endosymbiotic Theory of the Origin of Eukaryotic Cells

JAVIER SUÁREZ
University of Exeter
jsuar3b@gmail.com

ROGER DEULOFEU
University of Barcelona
roger.deulofeu@gmail.com

The focus of this paper will be the justification of the explanatory character of the serial endosymbiotic theory of the origin of eukaryotic cells (SET) on the basis of two different accounts of scientific explanation, namely: Woodward's causal manipulativist account (Woodward 1997, 2003) and Díez's neo-Hempelian proposal (Díez 2013). Our purpose is twofold: on the one hand, to show that SET, a theory proposed to explain the origin of eukaryotic cell by means of endosymbiosis between two previously extant prokaryotic cells (cf. Sagan 1967; Margulis 1970; new evidence may be found in Ku et al. 2015; Spang et al. 2015), provides a pattern for explaining a wide-range of different why-questions (biochemical, biological, historical, etc.). On the other hand, to argue in favour of Díez's neo-Hempelian account which, according to us, and in contrast with Woodward's proposal, is able to justify the explanatory character of all the exemplifications of SET that we intuitively take as explanatory.

The structure of the talk will be as follows: after briefly introducing SET as applied to the origin of the nucleated cell, we will suggest four different main kinds of why-questions which we take as paradigmatic and that SET is supposed to answer, namely: (1) why is the DNA of the mitochondria and the chloroplast distinct from the nuclear DNA? [biochemical], (2) why can mitochondria and chloroplasts be in vivo replicated? [biological], (3) why is there a gap in the fossil record between prokaryotes and eukaryotes? [historical], (4) why did the proto-eukaryotic cell evolve its nuclear protection? [mixed – historical, biochemical].

Afterwards, we will show how Woodward's causalist model of scientific explanation can justify the explanatory character of SET answers to (1) and (2), but fails to capture the explanatory power of SET answers to (3) and (4). We will then present Díez's neo-Hempelian proposal and argue that it is suitable to accommodate the explanatory character of all the answers that SET provides for questions (1)-(4).

Finally, we will conclude that Díez's neo-Hempelian account seems more adequate to highlight the explanatory character of SET than Woodward's account. The conclusion will lie on the fact that Woodward's manipulativist strategies used in order to seek the causes (and therefore the causal explanations) fail to accommodate the real explanatory character of the historical why-questions that may be captured by the appeal to laws and embedding relations that Díez's neo-Hempelian account suggests.

References

Díez J (2013) Scientific W-explanation as ampliative, specialised embedding: a neo-hempelian account. *Erkenntnis* **79**: 1413-43.

Ku C et al. (2015): Endosymbiotic origin and differential loss of eukaryotic genes. *Nature* **524**: 427-432.

Margulis L (1970) *Origin of eukaryotic cells: Evidence and research implications*. Yale University Press.

Sagan L (1967) On the origin of mitosing cells. *Journal of Theoretical Biology* **14**: 225-274.

Spang A et al. (2015) Complex archaea that bridge the gap between prokaryotes and eukaryotes. *Nature* **521**: 173-179.

Woodward J (2003) *Making Things Happen: A theory of causal explanation*. Oxford University press.

Woodward J (1997) Explanation, Invariance, and Intervention. *Philosophy of Science (Proceedings)* **64**: S26-S41.

General Philosophy of Science IV **Contributed Papers**
Chair: Ludwig Fahrbach Room 3D, Thursday 11:00 – 13:00

Reflective Equilibrium – A Method for Philosophy of Science?

CLAUS BEISBART
University of Bern
Claus.Beisbart@philo.unibe.ch

Philosophy of science has intensely debated the question of what methods of science there are. But what method is most appropriate for philosophy of science? What method can it use to advance our understanding of science?

One proposal that is occasionally mentioned in the literature is reflective equilibrium (RE). Whereas Ladyman (2002, p. 54) suggests that philosophers of science construct a reflective equilibrium, Schurz (2014, p. 22) prefers a different method, viz., rational reconstruction to the RE (cf. also Thagard 1982, Sec. 2). The aim of this talk is to discuss whether the RE is an appropriate method for philosophy of science. For this purpose, we have to clarify what the method is and how it may be applied to philosophy of science.

There is a lot of motivation, both historic and systematic, to consider the application of the RE within philosophy of science. For one thing, Goodman (1955, Sec. 3.2) has suggested to justify rules of induction, and thus of scientific inference, in terms of the RE. It is thus natural to apply the method to other aspects of scientific practice. For another thing, the central task of philosophy of science is to understand science, and it has been suggested by Elgin (1996) that we obtain understanding of a range of phenomena by constructing an RE. The RE may also provide a useful framework in which historic case studies can be brought to bear on general philosophical questions.

How then may we apply the RE to philosophy of science? In this talk, I will rely on a recent characterization of the RE that Brun (2014, Sec. 2) has given. If we apply this conception to philosophy of science, we start with considered judgements or initial commitments about scientific work. They may either consist in assessments of particular examples of scientific work or general ideas about science. I assume that the initial commitments are at least partly evaluative or normative in character, because e.g. some scientific results are judged to credible or reliable. As a next step, principles are identified that explain the initial commitments; the principles are not only

supposed to cohere with the commitments, but also to fit background theories and epistemic goals that guide the application of the RE. Subsequently, the commitments are re-examined in view of the principles and the background theories. The process continues as long as a fixed point is reached.

To make a case for the application of the RE in philosophy of science, I argue for a two-fold claim: 1. A lot of work in philosophy of science can be understood as applying the RE to some approximation. 2. Adopting the RE as a method in philosophy of science provides a helpful perspective on some methodological questions in philosophy of science. Admittedly, though, the RE cannot answer all questions of this kind because central aspects of the RE await further elaboration.

References

Brun, G. 2014. Reflective Equilibrium without Intuitions? Ethical Theory and Moral Practice 17: 237–252.

Elgin, C.Z. 1996. *Considered Judgment*. Princeton: Princeton University Press.

Goodman, N. 1955. *Fact, Fiction, and Forecast*. Cambridge, MA: Harvard University Press

Ladyman, J., 2002 *Understanding Philosophy of Science*, London: Routledge Schurz, G. 2014, *Philosophy of Science*. *A Unified Approach*, New York: Routledge.

Thagard, P. 1982, From the Descriptive to the Normative in Psychology and Logic, Philosophy of Science 49: 24–42

How Theories Travel: The Case of 'The Theory of Games and Economic Behavior'

CATHERINE HERFELD
Munich Center for Mathematical Philosophy
c.s.herfeld@gmail.com

MALTE DOEHNE
Munich Center for Mathematical Philosophy
m.doehne@gmx.de

How are scientific theories developed and how do they spread across scientific communities? We address those two questions by applying network analysis to a case of theory development and diffusion. We conceptualize a scientific theory as an 'innovation' that is invented by one or more 'innovators', which is or is not adopted by other actors in a network and argue that a theory has to be conceptually translated before it can be taken up, in and across (preexisting) scientific communities. Our case study is the theory of games developed by John von Neumann and Oskar Morgenstern in 1944. Departing from the observation that the adoption of game theory has increased disproportionally only from the 1970s on, we trace its initial spread across economics, philosophy, and the behavioral and social sciences at large. By developing a measure for diffusion of scientific theories and apply it to a data set of more than 4000 publications, we construct a co-citation network of what we identify as seminal works that have contributed to the dissemination of game theory. We show that game theory was collaboratively developed and further modified between the 1940s and the 1960s by a small group of outstanding scholars from distinct disciplines that we identify as translators, before it spread to the social and behavioral sciences at large. We identify these translators using an innovative brokerage algorithm. The topology of this networks sheds light on how scientific theories become developed, adopted, and further modified within and across scientific communities. We thereby make a case for the fruitfulness of network analysis in the philosophy of (social) science in general and for studying knowledge transfer in science in particular.

Different Solutions to the Problem of Conflicting Reference Classes and their Application to Personalized Medicine

CHRISTIAN WALLMANN
University of Kent
christian.wallmann@stud.sbg.ac.at

I argue that the problem of conflicting reference class is very common in personalized medicine.

I present different approaches to solve the problem of conflicting reference classes. Discussing advantages and disadvantages of either approach will lead to desiderata for reference class choice.

I determine the circumstances under which either of the non-equivalent approaches works best. The results obtained are applied to examples in personalized medicine.

A typical example for reference class reasoning is predicting the probability of five-year survival of patient X. The doctor often knows size of the index lesion (S), node status (N) and (G) grade of tumor of patient. She then calculates the NPI score by NPI = [0.2 x S] + N + G. Let's suppose the patient has an NPI score of 4:2. Statistical knowledge contains the fact that 7 out of 10 people with a score 3:4 $<$ NPI $\leq$ 5:4 survived for more than 5 years. Therefore the patient has a probability of survival of 70 percent. In the example, X is subsumed under an appropriate reference-class for which statistical information is available.

However, X belongs to many reference classes. In our example, the patient has a certain nationality, a certain religious belief, social status, or finally a certain genetic profile. The problem of conflicting reference classes obtains, if we know that an individual belongs to different reference classes and we have no information about the frequencies within their joint subclass.

Suppose nine out of ten Englishmen are injured by residence in Madeira and seven out of ten consumptive persons are benefited by such a residence. John Smith is consumptive Englishmen. What is the probability that John Smith will benefit from residence in Madeira? (slightly changed from Venn, J.: The Logic of Chance. 3rd edition, Macmillan, 1888., p.222-223).

Mechanisms **Contributed Papers**
Chair: Alexander Gebharter Room 22, Thursday 11:00 – 13:00

Mechanisms: A Curious Trinity?
(CANCELLED)

LENA KÄSTNER
Humboldt University of Berlin
mail@lenakaestner.de

In their "In Search of Mechanisms", Craver and Darden (2013) offer a comprehensive overview of how mechanisms in the life sciences are being discovered. They outline detailed examples to demonstrate different strategies that scientists bring to bear in the process. In this context, three different kinds of mechanisms are distinguished: mechanisms that produce, maintain, or underlie a phenomenon (cf. chapter 5). I do not doubt that this trinity of mechanisms mirrors how practicing scientists reason about mechanisms. However, this is puzzling. For one thing, it is unclear how these three kinds of mechanisms go together with the familiar Machamer, Darden and Craver (2000) picture of mechanisms as "entities and activities organized such that they are productive of regular changes from start or set-up to finish or termination conditions". For another, the relations between these three kinds of mechanisms remains utterly unclear. Besides, production, underlying, and maintenance all seem to be causal in character. Yet, if the relations they are supposed to pick out are synchronous relations between a phenomenon and its implementing mechanism, we would not usually think of them as causal.

In this paper I suggest that in order to bring together scientists' reasoning about producing, maintaining, and underlying mechanisms with philosophical accounts of mechanisms and mechanistic explanations, a purely descriptive project is not enough. To be sure, we can learn from it. But either we need to revise our philosophical accounts in light of what we learn, or we need some kind of an interpretative project resolving apparent contradictions between scientific practice and philosophical theory.

This paper engages in the latter. I will argue that what kind of mechanism explains a given phenomenon essentially depends on the kind of explanandum phenomenon we consider. To explain a *process*, we will typically search for the mechanism underlying it; to explain how an *end product* or result is

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generated, we will usually search for mechanism that produced it. While this distinction between is rather clear cut, it seems that maintaining mechanisms are harder to locate. I suggest that they might best be considered a subcategory of either producing or underlying mechanisms depending on what exactly it is that is being maintained.

Note that this view has a certain *perspectival* feature: it mirrors scientists' natural flipping back and forth between different explanatory perspectives. As such, it is not only in tune with different accounts of mechanisms and mechanistic explanations that have been offered since Machamer, Darden and Craver's seminal definition but also gives us a means to resolve apparent contradictions between them.

Empirically Assessing Mechanistic Constitution With Interventions

BEATE KRICKEL
Ruhr University Bochum
beate.krickel@rub.de

Proponents of mechanistic explanations suggest there to be a constitutive relevance relation between the phenomenon and the components of its implementing mechanism. According to the standard view, this constitutive relation consists in a part-whole relation and mutual manipulability between the mechanism's components and the phenomenon (Craver 2007) which can be assessed by means of interventions (Woodward 2003, 2011).

While this view captures certain aspects of scientific practice, it creates a conceptual problem: Woodward's interventions are designed for detecting causal relations while constitutive relevance is explicitly described as non-causal. So can we use interventions at all to assess constitutive relations? How can we experimentally distinguish between causal and constitutive relations?

Gebharter & Baumgartner (2015) and Romero (forthcoming) have suggested a solution to the conceptual problem. However, their treatment of the empirical challenge remains unsatisfying. In this paper, I suggest a different way to meet the empirical challenge.

The Empirical Challenge
I take the empirical challenge to be the following: Suppose you intervene into a variable P and detect a change in some other variable C; what can you infer from that? There are three straightforward interpretations:

(1) Causation: P is a cause of C

(2) Common Cause: The intervention is a common cause of both P and C

(3) Constitutive Relevance: P is (partly) constituted by C

How can we empirically distinguish between these three interpretations?

Excluding Causation
To allow interventionism to capture the difference between causation and constitution, Baumgartner & Gebharter (2015) introduce time into the interventionist definition of a cause: if we are dealing with causation, changes in the cause variable have to occur prior to changes in the effect variable. In contrast, Common Cause allows for changes at the same time, while Constitutive Relevance requires simultaneous changes.

Excluding Common Cause
1. Fat-handedness
Baumgartner & Gebharter (2015) and Romero (forthcoming) argue that if C is constitutively relevant for P, necessarily, an intervention into P is a (direct) common cause of both P and C. Whether changes in C and P are merely due to a common cause can be settled by testing whether interventions into P are always fat-handed. This account is problematic because the fat-handedness criterion essentially involves a universal claim that cannot be empirically tested.

2. Mutual Manipulability as a Causal Relation
I suggest a different answer to the empirical challenge: although constitutive relevance is non-causal, mutual manipulability occurs due to a causal relation between temporal parts of the phenomenon (each represented by variables in the causal model) and the mechanism's components. On this view,

mutual manipulability consists in there being a (fat-handed) intervention on one of the phenomenon-variables with regard to the component-variable and there being a (fat-handed) intervention on the component-variable with regard to a further phenomenon-variable. We can empirically disambiguate between (2) and (3) by performing additional fixing-interventions.

I will discuss different empirical examples and show that this approach provides an adequate analysis.

Viewing Marr as a Mechanist

CARLOS ZEDNIK
Otto-von-Guericke University Magdeburg
czednik@uos.de

Whereas philosophers have recently sought to apply the framework of mechanistic explanation to understand experimental and modeling practices in cognitive science, practicing scientists still predominantly appeal to David Marr's levels of analysis framework instead. Because it remains unclear exactly how these two frameworks relate, it is not known whether researchers who seek to provide three-level explanations of behavioral or cognitive phenomena are in fact in the business of describing the mechanisms responsible for those phenomena. In this talk, I provide an interpretation of Marr's framework in which each level of analysis aligns with a particular aspect of mechanistic explanation. In so doing, I show how the principles of mechanistic explanation can help resolve several long-standing disagreements about how best to individuate and characterize levels of analysis, and I pave the way for a mechanistic construal of explanation in cognitive science.

Symposia & Contributed Papers V

Evidence of Mechanisms in Medicine **Symposium**
Organizer: Michael Wilde
Chair: Beate Krickel Room 24, Thursday 16:45 – 18:45

Evidence of Mechanisms in Medicine

MICHAEL WILDE
University of Kent
m.e.wilde@kent.ac.uk

Rethinking the Epistemic Significance of Mechanisms
(CANCELLED)

ALEXANDER MEBIUS
University of Oxford
alenik@kth.se

Defending the Epistemic Significance of Mechanisms

VELI-PEKKA PARKKINEN
University of Kent
v.k.parkkinen@kent.ac.uk

General Description

Evidence-based medicine is a relatively recent approach to medicine with a particular theory about what counts as good evidence for a causal claim (Evidence Based Medicine Working Group, 1992). This theory of evidence is perhaps best articulated in the various evidence hierarchies of evidence-based medicine, in which comparative clinical studies are typically ranked as providing greater evidential support for a causal claim than mechanistic reasoning (Howick, 2011b: 4). For example, the latest hierarchies tend to rank systematic reviews of comparative clinical studies at the top levels of the hierarchy and mechanism-based reasoning at the bottom levels (Clarke et al., 2014: 339–342). This ranking was in part a response to a number of com-

parative clinical studies, studies which demonstrated that various treatments proposed on the basis of mechanistic reasoning often caused more harm than good (Howick, 2011b: 20). Therefore, it seems that evidence-based medicine has a theory of evidence which rightly downplays the role of mechanistic reasoning. Call this the received theory of evidence in evidence-based medicine.

Recently, however, this received theory of evidence in evidence-based medicine has been called into question (Russo and Williamson, 2007, 2011; Clarke et al., 2014). For example, Federica Russo and Jon Williamson have put forward an alternative epistemological account, maintaining that establishing a causal claim in the health sciences typically requires both probabilistic and mechanistic evidence (2007). This is in stark contrast to the received theory of evidence in evidence-based medicine, since on this alternative account there remains a prominent role for mechanistic reasoning in establishing a causal claim in the health sciences. Consequently, this alternative account of evidence has generated a good deal of critical attention (Weber, 2009; Broadbent, 2011; Gillies, 2011; Illari, 2011; Howick, 2011b,a).

This debate on the epistemic significance of mechanisms in medicine is ongoing. It is even the topic of a major new collaborative research project involving a large number of philosophers and medical practitioners. The aim of this project is to develop a philosophical account of evidence that treats probabilistic and mechanistic evidence in a more balanced way. The motivation for this symposium is to bring members of the project together with critics of the project in order to contribute to the ongoing debate on the epistemic significance of mechanisms in medicine. To this end, there will be three talks. The first talk will be given by a member of the project, arguing in favour of a role for evidence of mechanisms in medicine. The second talk will be given by a critic of the project, arguing against a role for evidence of mechanisms in medicine. A third talk will be a response to the criticism, given by another member of the research project. Then a panel discussion can take place with questions from the audience.

Abstracts

1. Michael Wilde: *Evidence of Mechanisms in Medicine*

The Russo-Williamson thesis maintains that establishing a causal claim in the health sciences typically requires both probabilistic and mechanistic evidence (Russo and Williamson, 2007). Jeremy Howick (2011a) has put forward a number of potential counterexamples to this thesis. In particular, there seem to be cases in the health sciences where tightly controlled comparative clinical studies alone suffice to establish a causal claim. Unfortunately, in its original formulation, the Russo-Williamson thesis is ambiguous in a number of crucial respects. This makes it difficult to determine whether the putative counterexamples to the thesis are successful. In this essay, I aim to make clear the commitments of the Russo-Williamson thesis, by focusing on the theoretical motivation behind the thesis. Then, I will argue that the proposed counterexamples are unconvincing, on the grounds that they assume that the relevant successful tightly controlled comparative clinical studies provide only probabilistic evidence. Once the commitments of the Russo-Williamson thesis have been made sufficiently clear, it becomes apparent that further argument in favour of this assumption is required before the proposed counterexamples can be considered convincing. However, this also makes clear alternative possible objections to the Russo-Williamson thesis, viz., objections to the mechanistic account of explanation endorsed by proponents of the thesis.

2. Alexander Mebius: *Rethinking the Epistemic Significance of Mechanisms*

There is ongoing philosophical debate about how consideration given to mechanisms may facilitate thinking about causal claims and evidence in general (Machamer et al., 2000). And many leading philosophers of science are giving increasing attention to questions about the epistemic status of mechanistic evidence in medicine in particular (Anderson, 2012; Bluhm, 2013; Caze, 2011; Howick, 2011a; Clarke et al., 2014). For instance, Craver and Darden (two influential philosophical exponents of mechanisms) maintain in their recent book that knowledge of causal mechanisms is imperative for adequately planning and conducting experiments in the life sciences: “To design an experiment that rigorously tests a claim. . . one often has to know or presuppose a great deal about what the parts of the mechanisms are likely to be and how they are likely to be (and not be) organized. Meaningful experimentation (with useful interventions and detections) can take place

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only against a wealth of background knowledge about the active organization of the system under study" (2013: 125). Similarly, philosophers of science have criticized the Evidence-Based Medicine (EBM) movement's stance on mechanisms as downplaying the value of evidence of mechanisms derived from basic and preclinical research (usually through in vivo and in vitro experimentation) (e.g., Clarke et al. (2014)). However, the major concerns recently highlighted by medical researchers regarding the validity of mechanistic findings have received little attention from philosophers. For example, recent empirical studies suggest that 75% to 90% of major mechanistic findings presented in high-impact biomedical journals are irreproducible, and thus presumably false (Begley and Ioannidis, 2015). Meanwhile, the mechanistic models that can be reproduced in laboratory settings hardly ever translate to successful treatments in the clinic (Djulbegovic et al., 2014). I argue that many of the basic issues identified related to mechanistic research, such as the poor transferability of results from mechanistic animal models to the human clinical situation, warrant a rethinking of the epistemic significance and normative status placed on the ability of mechanistic models to predict (or reproduce) clinical benefit in humans (cf. Pound and Bracken (2014)).

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3. Veli-Pekka Parkkinen: *Defending the Epistemic Significance of Mechanisms*

There is an ongoing debate considering the role of mechanistic evidence vis-à-vis statistical evidence in establishing clinically relevant causal knowledge. The gist of the debate is at times unclear, as the mechanism-talk tends to be quite loose. In particular, animal models qua mechanistic evidence have been criticized by pointing out the poor replication and translation record of preclinical animal testing of treatments (Begley and Ioannidis, 2015; Djulbegovic et al., 2014), without specifying what makes such research 'mechanistic' or why these arguments generalize to other types of evidence lumped in the mechanism category. In this talk, rather than offering yet another ontological definition of mechanism, I propose that we identify mechanistic evidence by its role in explanatory inferences. As an illustration I will consider the use of animal models. From a well-conducted randomized trial that tests for treatment efficacy, one can infer that the experimental intervention explains why the groups differ in their average outcome post-treatment. In order to make further inferences about the treatment's efficacy

outside the study population, it is useful to ask a follow-up question: how does the treatment produce its effect in an individual? Answering the how-question that underlies the why-question requires elaborating the causal context and the process that leads from the treatment to the outcome: a 'process tracing', or mechanistic explanation (Steel, 2008). Mechanistic knowledge, in this sense, provides the context in which we evaluate what we can infer from statistical evidence that documents dependencies between variables. What makes a piece of evidence 'mechanistic' should not be seen as depending on the type of experimental system used (e.g., animal study vs human trial), but on its role in supporting the right type of explanatory inferences. Mechanistic evidence is evidence that supports inferences about the entities and activities that participate in the process that mediates a causal dependence. Such evidence can come from various sources, but typically mechanism-discovery involves attempts to intervene at intermediate points in the hypothesized causal process, to discover the components that realize it. In particular, animal testing is not always mechanistic evidence in this sense. A typical phase 1 preclinical animal study is a comparative statistical trial that—if well conducted—answers why we see a difference between the intervention and control groups in the animal population, but in itself provides no answers to the underlying how-question. The problems of poor replication and translation to humans of such experiments are real, but it would be a mistake to conclude that mechanistic understanding is therefore of little use. In practice much of the mechanistic evidence does indeed come from animal experiments, as practical and ethical constraints prohibit interventions for probing physiological processes directly in humans. There are many problems with this use of animal models as well. At best the evidence from animal models supports a how-possibly explanation that requires further validation by reproducing the results in many species or by direct comparison to humans when possible. These problems, however, are not the same as the problems in using animals in preclinical trials, as the inferences one envisions to make from the evidence are different: in preclinical efficacy testing we want to transfer a causal effect of a treatment from the animal model to humans, while in mechanism-discovery one aims to establish claims about the existence of a type of process or properties of entities. In the remainder of the talk I elaborate and discuss these problems and the strategies that scientists use to overcome and alleviate them.

References

Anderson, H. (2012). Mechanisms: What are they evidence for in evidence-based medicine? *Journal of Evaluation in Clinical Practice 18(5)*, 992–999.

Begley, C. G. and J. P. Ioannidis (2015). Reproducibility in science: Improving the standard for basic and preclinical research. *Circulation Research 116*, 116–126.

Bluhm, R. (2013). Physiological mechanisms and epidemiological research. *Journal of Evaluation in Clinical Practice 19(3)*, 422–426.

Broadbent, A. (2011). Inferring causation in epidemiology: Mechanisms, black boxes, and contrasts. In P. M. Illari, F. Russo, and J. Williamson (Eds.), *Causality in the Sciences*, pp. 45–69. Oxford University Press.

Caze, A. L. (2011). The role of basic science in evidence-based medicine. Biology and Philosophy 26(1), 81–98.

Clarke, B., D. Gillies, P. Illari, F. Russo, and J. Williamson (2014). Mechanisms and the evidence hierarchy. *Topoi 33*, 339–360.

Craver, C. F. and L. Darden (2013). In *Search of Mechanisms: Discoveries Across the Life Sciences*. University of Chicago Press.

Djulbegovic, B., I. Hozo, and J. P. A. Ioannidis (2014). Improving the drug development process: More not less randomized trials. *Journal of the American Medical Association 311(4)*, 355–356.

Evidence Based Medicine Working Group (1992). Evidence-based medicine: A new approach to teaching the practice of medicine. *Journal of the American Medical Association 268*, 2420–2425.

Gillies, D. (2011). The Russo-Williamson thesis and the question of whether smoking causes heart disease. In P. M. Illari, F. Russo, and J. Williamson (Eds.)*,Causality in the Sciences*, pp. 110–124. Oxford University Press.

Howick, J. (2011a). Exposing the vanities—and a qualified defense—of mechanistic reasoning in health care decision making. *Philosophy of Science 78*, 926–940.

Howick, J. (2011b). The Philosophy of Evidence-Based Medicine. BMJ Books.

Illari, P. (2011). Mechanistic evidence: Disambiguating the Russo-Williamson thesis. *International Studies in the Philosophy of Science 25*, 139–157.

Machamer, P., L. Darden, and C. F. Craver (2000). Thinking about mechanisms. *Philosophy of Science 67(1)*, 1–25.

Pound, P. and M. B. Bracken (2014). Is animal research sufficiently evidence based to be a cornerstone of biomedical research? *British Medical Journal 348*.

Russo, F. and J. Williamson (2007). Interpreting causality in the health sciences. *International Studies in the Philosophy of Science 21*, 157–170.

Russo, F. and J. Williamson (2011). Epistemic causality and evidence-based medicine. *History and Philosophy of the Life Sciences 33*, 563–582.

Steel, D. (2008). *Across the Boundaries: Extrapolation in Biology and Social Science*. Oxford University Press.

Weber, E. (2009). How probabilistic causation can account for the use of mechanistic evidence. *International Studies in the Philosophy of Science 23*, 277–295.

Values in Science II **Contributed Papers**
Chair: Alexander Christian Room 3B, Thursday 16:45 – 18:45

Cognitive Interests and Scientific Objectivity

TORSTEN WILHOLT
University of Hannover
torsten.wilholt@philos.uni-hannover.de

The concept of scientific objectivity is in crisis. Attempts to capture it in philosophical analysis bring to light a variety of concepts of objectivity, without revealing a common "brand essence". In particular, the ideal of value free science has been undermined by forceful criticism.

At the same time, the abandonment of objectivity as an epistemological aim and quality criterion is philosophically unsatisfactory. It would mean that cases of biased, distorting research could no longer be criticized as epistemologically problematic, but only as problematic with regard to non-epistemic criteria.

Wilholt (2009, 2013) has proposed to regard biases and distortions as deviations from standards that exist within research communities by convention. The binding character of such standards arises from the fact that they enable epistemic trust between individual cognitive actors and hence facilitate the emergence of real epistemic communities. An account that places trustworthiness at the core of the notion of scientific objectivity has the advantage of explaining why the quality criteria it posits are desirable from an epistemic perspective (because trust is a prerequisite for any collective epistemic enterprise) while allowing legitimate roles for values (as trust is a value-infused phenomenon).

In this talk, I want to explore the possibility of developing this analysis into a more comprehensive account of scientific objectivity. A decisive challenge of this approach are cases in which a research community loses its independence as a whole, so that the community standards themselves seem no longer objective (in a pre-theoretical, intuitive sense). Such cases seem to presuppose that scientific communities are in some sense obliged to stipulate the "right" kinds of standards. But an assessment of the appropriateness of methodological standards can only make sense in relation to the values and interests at stake. I will argue that the relevant values and

interests cannot be just any kinds that the researchers happen to hold. I will propose that standards of a community must be measured against a "cognitive interest" that is characteristic for the kind of inquiry that they are publicly perceived to be engaged in. A cognitive interest in the relevant sense is characterized by a set of objectives, most importantly a certain target reliability of positive results, a certain target reliability of negative results, and a certain target propensity to produce definitive results at all. I will discuss an analysis of scientific objectivity as the match between methodology and purported cognitive interest. A good match in this respect underlies the trustworthiness of science as an epistemic enterprise. This would mean that the ubiquity and diversity of cognitive interests is not undermining the very possibility of scientific objectivity as critical theory once suspected. Rightly understood, objectivity is a relation of appropriateness between a collective effort of inquiry and its stated cognitive interest.

References

Wilholt, T. (2009), Bias and Values in Scientific Research, Studies in History and Philosophy of Science 40, 92-101.

Wilholt, T. (2013), Epistemic Trust in Science, British Journal for the Philosophy of Science 64 (2), 233-253.

Is the Argument from Inductive Risk Applicable to Pure Research?

CORNELIS MENKE
University of Bielefeld
cmenke@uni-bielefeld.de

In a seminal paper of 1953, Richard Rudner argued that "The Scientist Qua Scientist Makes Value Judgements": Scientists have to make decisions as to accept or reject a hypothesis; but since hypotheses are only gradually confirmed by available evidence, this amounts to a decision what degree of confirmation – what amount of evidence – is enough to warrant acceptance. To make this decision, in turn, scientists have to take into consideration the possible consequences of a wrong decision, i.e., the seriousness of a mistake.

Within the recent debate on science and values, this “argument from inductive risk” has played a pivotal role (Douglas 2000; 2009; Wilholt 2009). The trust of the argument is to show that extra-scientific values play a legitimate and unavoidable role in science, and it has convinced many philosophers that they do – particularly within all fields of science in which the decision to accept a theory has clear or likely consequences. However, whether and how the argument could be applied to pure or basic research (a “special case” according to Douglas) is unsettled.

Usually, those who arm the question rely on Cognitive Decision Theory. The aim of the paper is to show that Charles Peirce's considerations of an “Economy of Research” provide an alternative and fruitful way to apply the argument to pure research.

First, I shall sketch the historical background of Rudner's argument: it was situated in a controversy about the role of (frequentist) statistical reasoning within science. While Ronald Fisher argued that statistics could only be applied to pure science when given an “epistemic” reading, Jerzy Neyman (1957) and Abraham Wald hold that a decision-theoretic approach is paramount; but their account ran into difficulties and never gained much acceptance.

Secondly, I shall present the main elements of Peirce's “Economy of Research”. Peirce developed his ideas on economic elements in methodology between 1976 and 1903, starting with an attempt to improve experiments using a reverse pendulum and finally stating “that Economy would override every other consideration even if there were any other serious considerations” (1903). Peirce argued that taking into account the costs of research and the prospective (epistemic) value allows for a resolution of several methodological questions, among others – or so I shall argue – the question how to decide when it is rational to end an experiment or line of research, i.e., when there is “enough” evidence – thereby providing exactly the kind of considerations necessary to apply the argument from inductive risk to basic science.

In the last part, I shall try to show that Peirce's account could fruitfully inform our understanding of the practice of setting evidential standards in pure science. I shall consider two case-studies: First, the origin and rationale of the ubiquitous if arbitrary standard of .05 as a level of significance. Using Fisher's work at Rothamstad Experimental Station (a case of “data mining”)

as an example, Peirce's account sheds light on the choice of .05: in this context, the threshold isn't arbitrary, but rational given the practical aim of Fisher' research, namely to decide which of the experiments performed at Rothamsted recommend themselves for further investigation. – Secondly, I shall consider the development of the (shifting) thresholds for the detection of exo-planets in astrophysics as an example of a decision when to stop experiments. This decision, too, is based on “economical” considerations, taking into account pragmatic but rational elements: the cost of further research, on the one hand, as well as the epistemic value of the results, on the other.

Values in Species Classification

STIJN CONIX
University of Cambridge
stijn.conix@gmail.com

In this paper, I discuss the role of values in biological taxonomy. My argument starts from the claim that species classification is not always fully determined by the facts. That is, I assume that it is sometimes the case that there are several ways to classify a group of organisms into species and that even knowledge of all the biological facts is insufficient to choose between

these different possible classifications. Although this starting point needs further argument, it is reasonable to assume this position for the sake of argument because (a) this is a relatively common position among philosophers of biology, and (b) problems with classifying hybrid species, prokaryotes and recently diverged lineages provide good prima facie arguments for this claim. Because species classification is underdetermined by the facts, something more is needed if we are to decide which of these alternative classifications should be generally accepted. I argue that this role is played by values.

The role of values in science has recently been one of the favourite topics of philosophers of science. While a few decades ago science was generally seen as value-free, it is now commonly accepted that values do and should play an important role in many if not all scientific disciplines. Examples in the literature on values in science mostly come from scientific work that has direct policy consequences for humans, like climate science and the medical sciences. Biological classification is rarely mentioned in this literature.

In this paper, I connect the literature on values in science to the literature on species concepts. Assuming that species classification is sometimes necessarily value-laden, I investigate how one might distinguish between legitimate and illegitimate values in taxonomy. More specifically, I argue that the distinction between epistemic and non-epistemic values, which is one popular way to distinguish the legitimate from illegitimate role of values in science, cannot be used for this in the case of taxonomy. I present two arguments for this. First, I argue that there are cases in which species classification is underdetermined by both the facts and epistemic values, so that non-epistemic values are still needed to bridge the gap. This is a comparatively weak argument, because it only shows that non-epistemic values have to play a role in case epistemic values fall short. The second argument makes a stronger claim. It consists of an example from taxonomy in which, I argue, non-epistemic values legitimately trump epistemic values. This example shows that non-epistemic values not only play a constructive role in shaping legitimate taxonomy, but that these values can sometimes justifiably override epistemic values. I conclude by suggesting that a strict dichotomy between the epistemic and non-epistemic is unhelpful if we want to understand the role of values in species classification.

Philosophy of the Life Sciences V **Contributed Papers**
Chair: Mieke Boon Room 3C, Thursday 16:45 – 18:45

Explaining Human Behaviour, Changing Human Behaviour: How to be an Evolutionary Social Constructionist

JESSICA LAIMANN
University of Bristol
jessica.laimann@web.de

Our practice of explaining human behaviour resembles a ravel of threads from natural scientific approaches on the one side, and social scientific approaches on the other. Though it is often unclear what, if anything, holds these two types of explanatory threads together, they tend to entangle into tense knots. This tension is nowhere more obvious than in the debate between evolutionary psychological and social constructionist explanations of gendered human behaviour, which has long been characterised by bitterly hardened battle lines. Yet, in recent years, calls for integration of these two approaches have become more and more frequent, hence suggesting that the relationship could become a more collaborative one.

Against this new-found optimism, I argue that there are strong reason to curb one's enthusiasm about integrating evolutionary psychology (even broadly conceived) and social constructionist approaches, at least in the way it has been envisioned by current proponents of integration. I demonstrate that the proponents' ideas of integration suffer from precisely the same problems and misunderstandings that motivated the original conflict between the two approaches. In particular, I argue, they combine an understanding of social constructionism that is too narrow with an understanding of evolutionary psychology that is too generous to a solution of the conflict that is not only wrong but also not very interesting. Discussing the example of rape explanations, this paper aims to do the opposite: to combine a nuanced understanding of social constructionism with a realistic account of evolutionary psychology into an analysis of the relationship between the two that is both correct and offers some surprising insights.

For that purpose, I first analyse the relationship between evolutionary psychological and social constructionist approaches as it has been understood so far, and argue that it provides no basis for fruitful integration. I then

replace the too narrow understanding of social constructionism that has informed previous accounts of integration with one that distinguishes between two different yet related forms of social constructionism, causal and conceptual. I argue that, while the idea of conceptual construction may be a valuable resource to natural scientific research on human behaviour, the concept of causal construction is confused and requires further clarification.

Natural and Social Kinds: Overlaps and Distinctions

ZDENKA BRZOVIC
University of Rijeka
zdenka@uniri.hr

PREDRAG SUSTAR
University of Rijeka
psustar@uniri.hr

The traditional view on social kinds is that they are different from natural kinds because their existence depends on human attitudes, interests and aims. However, on the increasingly influential approach to natural kinds, labeled the epistemology-oriented approach (Reydon, 2009), natural kinds and social kinds are more similar than once thought. This is because it takes natural kinds to depend on fulfilment of scientists' epistemic aims. We endorse this approach but argue that there is still a distinction to be drawn (that comes in degrees) between the theoretical or natural kinds and practical or social kinds, depending on the further goals achieved by a classification that can be primarily theoretical or practical. Ideally, practical aims achieved by a classification should dovetail with our theoretical aims; nevertheless there are cases where they can come apart. In order to illustrate the point we focus on the case study of psychopathy.

In the case of psychopathy classification there is a possibility that the underlying causal factors are disjoint and that there are actually two different subgroups that have been labeled primary and secondary psychopathy. There is some evidence that the two groups differ with regards to the etiology of the condition and the underlying mechanisms that produce it. However the behavior of both groups is homogenous enough that it allows us to make stable generalizations and predictions about them as a single group.

This is a nice illustration of how the theoretical and practical goals can come apart. Our theoretical goal is to explain the occurrence of this condi-

tion which implies the importance of underlying causes. However, our practical aim stems from the clinical and legal practice where we need to provide an answer regarding the correct treatment of people with such a condition. In this case, the hypothetical difference in underlying causal factors (or merely our ignorance of them) is not very relevant if their behavior is homogenous enough to allow us to predict their future behavior and form policies with regards to their treatments.

Within the epistemology-oriented approach, the causal approaches to kinds seem better suited for categories that are of theoretical interest, while the more encompassing approaches (Magnus, 2012; Ereshesky and Reydon, 2015; Slater, 2015) allow for both theoretical and practical aims to play a role. The proposed distinction vindicates the causal approaches for the classifications with mostly theoretical goals against criticism that they are too restrictive, while acknowledging that there is a wider set of scientifically interesting classifications. A continuum between social and natural kinds can be fruitfully reinterpreted in terms of a trade-off between the relative relevance of practical to epistemic aims (when they come apart). So that when the classification has a clearly greater practical relevance (e.g. psychopathy) as opposed to the theoretical we naturally categorize it as a social kind. In this respect we propose to keep the term natural kind for cases where there is a balanced overlap between practical and more theoretical aims.

References

Ereshefsky, M. & Reydon, T., 2015. Scientific kinds. *Philosophical Studies*, 172(4), pp. 969-986.

Magnus, P., 2012. *Scientific Enquiry and Natural Kinds: From Planets to Mallards*. New York: Palgrave Macmillan.

Reydon, T., 2009. How to fix Kind Membership: A Problem for the HPC Theory and a Solution. *Philosophy of science, 76(5)*, pp. 1-19.

Slater, M.H., 2015. Natural Kindness, *British Journal for the Philosophy of Science 66 (2)*:375-411.

The Biological Reality of Race Does Not Underwrite the Social Reality of Race: A Response to Spencer

KAMURAN OSMANOGLU
University of Kansas
kamuran.osmanoglu@gmail.com

In this paper, we criticize Quayshawn Spencer's (2014) 'radical' solution to the race problem in his 'A Radical Solution to the Race Problem'. Spencer defends the biological reality of 'race'. He argues that 'race', as used in the current US racial discourse, picks out a biologically real entity. He lays out his argument in two steps: first, he argues that race, in the US racial discourse, is a proper name for a set of human population groups, second, by relying on recent data from human population genetics, he contends that the set of human population groups matches the Blumenbachian partition, i.e. the US meaning of 'race' is the set of populations at the K = 5 level of human population structure. Therefore, Spencer argues that 'race', in its US meaning, picks out a biologically real entity.

We raise two criticisms against Spencer's account. First, we argue that limiting the racial discourse to the current US Census is not the right way to talk about 'race'. We find limiting the racial discourse to the US Census problematic. Why do we need to care only about what the US racial discourse tells us about human population groups? We think that Spencer, by limiting the racial discourse to the US race, does not do justice to the culturally diverse social reality of racial discourse. We argue that 'race' is a fluid concept and it takes different shapes in different cultural and historical contexts. Second, we argue that there are other biologically interesting ways to classify human populations into different groups (such as classifying human populations according to their hemoglobin production, lactose resistance, or classifying human beings into different groups by examining if they have Denisovan gene or not etc.) as opposed to K=5 clustering that Spencer defends. Therefore, Spencer needs to answer the following two questions if he wants to argue for the biological reality of race in the US racial discourse: First, how is it even possible to biologically support an inherently social category like 'race'? Second, what makes the Blumenbachian partition better than hemoglobin production (or any other biologically interesting classification) for social clustering of human populations? Unless this is done, his account cannot be considered successful.

General Philosophy of Science V **Contributed Papers**
Chair: Claus Beisbart Room 3D, Thursday 16:45 – 18:45

Why the Psychology of Reasoning Needs Normativity: The Complex-First-Paradox

PETER BRÖSSEL
Ruhr-University Bochum
peter.broessel@rub.de

NINA POTH
Ruhr-University Bochum
nina.poth@rub.de

Recently, it has been doubted whether normative considerations concerning rational reasoning should play an important role in the methodology of psychology and cognitive science (Elqayam and Evans 2011). Roughly the idea is, in psychology and cognitive science we want to describe the various neural and psychological processes underlying the cognitive capacities of agents. Bayesian theories of rationality, however, are not descriptive; they prescribe or evaluate how agents should reason. And since matters of fact are irrelevant for normative matters this also holds the other way around.

In this talk we demonstrate via example that this is not correct. In particular, we present a novel philosophical solution to the complex-first paradox (Werning 2010). The complex-first paradox consists in the fact that children acquire complex concepts (concrete nouns like dog) earlier than simple concepts (abstract attributes like green), even though in the light of our best neuroscientific theories of word learning one would expect learning the former is harder than learning the latter and, thus, takes more time. In particular, we know that concrete nouns "are semantically more complex and their neural realizations more widely distributed in cortex than those expressed by the other word classes in question" (Werning 2010, 1097). We also know that the more widely distributed neural realizations are, the more costly it is for the organism to implement these neural realizations and the more time it takes to implement them. Together these claims present a puzzle for cognitive scientists and psychologist: why do children learn complex concepts earlier than simple concepts?

Instead of focusing on descriptive neuroscientific theories about the development of neural realizations of mental representations, we suggest to rely on theories of rational learning to resolve the complex-first paradox. A very general observation has been already pointed out by Schurz (2011):

when one wants to explain reliable and successful learning (e.g. of concepts), one has to refer to learning that is instrumentally rational (for learning concepts). But more importantly, based on Bayesian theory of word learning one can even explain why children learn complex concepts first; learning complex concepts is an easy induction task, learning simple concepts on the other hand, is a hard induction task. (The specific theory we want to suggest is a combination of Xu & Tenenbaum's (2007) theory of word learning and Gärdenfors' (2000) theory of conceptual spaces.) Thus, even though it might be more costly for an organism to implement neural realizations of complex concepts children are nevertheless faster in learning them, because it is easier to infer their meaning from just a few instances than it is to learn the meaning of simple concepts.

We conclude that contra (Elqayam and Evans 2011) normative considerations can play a pivotal role in the methodology of psychology and cognitive science. What is more, they are even explanatory relevant in some cases; the complex-first paradox is a case in point.

Predictive Coding and the Rationale of the Conjunction Fallacy

BENJAMIN HORRIG
Ruhr-University Bochum
benjamin.horrig@rub.de

PETER BRÖSSEL
Ruhr-University Bochum
peter.broessel@rub.de

The conjunction fallacy is often considered to be the most paradigmatic example for human irrationality: the fallacy consists in judging a conjunction to be more probable than one of the individual conjuncts. This is taken to be irrational because rational agents are assumed to believe propositions that serve the goal of believing true propositions, and believing conjunctions is less probable to serve that goal than believing the individual conjuncts.

Ever since Tversky's and Kahneman's (1983) exposition of the conjunction fallacy, accounts have been proposed to save human rationality, at least partially. Nevertheless for all those theories one problem remained: in how far are agents rational that prefer to believe propositions that are demonstrably less probable to serve the goal of believing true propositions.

In my talk I want to suggest a completely new approach that takes it starting point in recent theories of cognitive science and the philosophy of

mind. According to the predictive coding framework, the brain (and the mind) is a prediction machine that constantly tries to predict its sensory inputs. Thus, agents do not have the epistemic goal of believing the truth; they have the goal of accepting beliefs that let them accurately predict (future and past) evidence. In accordance with this, I suggest a theory of belief that satisfies both desiderata: it is a good predictor of the conjunction fallacy and one can demonstrate that in these cases believing the conjunction is more probable to serve the goal of predicting evidence than believing only one of the conjuncts. Formally the suggested theory is a Bayesian variation of the inference schema inference to the best explanation.

Finally, I will argue that the presented theory is an equally good predictor of the conjunction fallacy as the incremental confirmation account of Crupi, Russo and Tentori (2012).

References

Crupi, V., Russo, S., & Tentori, K. (2012). On the determinants of the conjunction fallacy: Probability versus inductive confirmation. *Journal of Experimental Psychology*, 142, 235-255.

Tversky, A. & Kahneman, D. (1983). Extensional vs. intuitive reasoning: The conjunction fallacy in probability judgment. *Psychological Review*, 90, 293–315.

Normativity in the Philosophy of Science

MARIE I. KAISER
University of Cologne
kaiser.m@uni-koeln.de

In the past decades many areas of philosophy of science have undergone what is now referred to as a practice turn. More and more philosophers of science agree that philosophical theories about science must account for how science actually is done and must be informed, for instance, by the investigative practices and reasoning strategies that scientists employ. The turn to scientific practice, at the same time, is a turn away from traditional theories about science that construct normative ideals of how science should work or ideally works. These normative ideals were typically formulated ex cathedra and are thus criticized for being peripheral to the empirical reality of science.

Although the practice turn is associated with a shift from normative to descriptive perspectives on science, normativity continues to play a role in practice-oriented philosophy of science. When developing accounts philosophers critically reconstruct relevant empirical information about science and this methodology deeply involves certain kinds of normative claims. Furthermore, some philosophers subscribe to the practice turn but sustain their normative aspirations; they use the results of their descriptive analyses to offer also normative advices about how science should be done and about how certain concepts should be understood. Finally, the increased attention to scientific practice is often accompanied by the insight that science is an inherently collective activity and that philosophers should take into account also the social (and epistemic) norms inherent in the pursuit of scientific knowledge.

But how can it be that the practice turn involves moving away from normativity while, at the same time, still leaving room for normativity and moving towards normativity? It seems as if normativity in the philosophy of science cannot be treated as a single matter and as something that a philosophical theory either has or has not. Instead, multiple kinds of normativity, that is, different respects in which a philosophical account about science can be normative, must be recognized. My goal in this paper is to disentangle these different kinds of normativity and to clarify what it can mean for an account in the philosophy of science to be normative.

I distinguish three major kinds of normativity. Methodological normativity can be found even in philosophical accounts that aim at describing actual scientific practice. This kind of normativity is due to normative methodological assumptions that are made in selecting, interpreting, and evaluating the relevant empirical information on the basis of which a philosophical theory is developed. Object-normativity emerges from the fact that sometimes the object of philosophical theorizing X, itself, is normative, namely, if a philosophical theory refers to epistemic or social norms and their roles in scientific practice. Meta-normativity arises from the kind of statements that a philosophical theory about a given object of study contains. Some philosophers make normative claims about science as it should be, rather than factual claims about science as it actually is. By distinguishing these three kinds of normativity and relating them to each other a more comprehensive and clear view of normativity in the philosophy of science emerges.

Philosophy of the Social Sciences and the Humanities I **Contributed Papers**
Chair: Rafaela Hillerbrand Room 22, Thursday 16:45 – 18:45

Reconsidering the "Experimental Turn" in the Humanities

EVA-MARIA JUNG
WWU Münster
eva-maria.jung@uni-muenster.de

Most of the humanities are often been regarded as non-empirical disciplines since they do not involve experimental methods. However, this view has been challenged by recent developments that might be regarded as expressing an »experimental turn« within the humanities. The appeal to certain kinds of experimentation is widespread in various disciplines, leading to new labels such as »experimental history«, »experimental philology« or »experimental philosophy«. All these movements are connected to a more or less radical new understanding of the methods and the aims of the respective disciplines. But what exactly does it mean when scholars from the humanities regard their investigations as »experimental«?

This is the central question I will address in my talk. In a first step, I will analyze how the notion of experiment is used within these new approaches by referring to some prominent examples from history, literary criticism and philosophy. As I will argue, there is no unique meaning of »experiment« or »experimental«. Rather, at least four different meanings should be distinguished: (1) In a first sense, the reference to experimentation expresses a certain kind of »import« of experimental results from the natural or the social science into debates of the humanities. In this case, imported experiments are often regarded as »crucial experiments« that can be brought up against or in support of certain theories in the humanities. (2) In a second sense, scholars from the humanities regard themselves as experimental insofar as they participate in conducting experiments which are based on methods from the natural or the social sciences. Thus, the experimental character is expressed by an interdisciplinary collaboration with scientists, especially when philosophical, cultural or literary theories are used as inspirations for the development of experimental designs. (3) In a third sense, the experimental character of the humanities is understood in a very broad

way as an unconventional, innovative way of exploring. (4) In a fourth sense, experimentation includes certain imaginative methods in arguments such as thought experiments or mental simulations. Those methods are sometimes regarded as serving similar functions as laboratory experiments in the natural or social sciences.

In a second step, I will address the question as to which consequences from the different meanings of »experiment« can be drawn. I will argue that many ideas expressed in the different movements are rather new nor radical. Moreover, the experimental turn comprises very heterogenous approaches that are motivated by mutually conflicting motivations. Whereas the approaches that rely on the first and the second meaning of »experiment« mostly express naturalistic tendencies, those which rely on the third or fourth meaning do not. Rather, they aim at an methodological independence of the natural and social science by establishing an inherent experimental method of the humanities.

From Stability to Validity: How Standards Serve Epistemic Ends

LARA HUBER
University of Wuppertal
lara_huber@gmx.de

Case studies in the history of science and technology have shown that standards contribute significantly to the evolution of scientific practices. They arise predominantly, but not exclusively, on the basis of interactions with instruments of measurements and other technical devices (cf. Schlaudt & Huber 2015). Standards formalize scientific practices, define strategies of validation (e.g. experimental control), strengthen epistemic criteria (e.g. precision), and allow for stabilisation and homogenisation.

In engineering and the technical sciences, homogeneity or uniformity as an ideal corresponds to the paradigm of interchangeability. Here, standardization permits large production runs of component parts that can be readily fitted to other parts without adjustment. Homogeneity describes the specific quality of a standardized part or product, that is, its uniform design or its stable performing function.

The paper discusses different practices in the biosciences that address stability and homogeneity as epistemic ends of standardization: Drawing upon technoscientific ideals, synthetic biologists create, share, and use standardized biological 'parts,' most quintessentially and reductively, in the form of so-called "BioBricks." (cf. <http://parts.igem.org>) In biomedicine, homogeneity relates to the ideal of "pure culture" and corresponds to practices of "purification" on the basis of laboratory practices. Whereas chemists isolated and purified chemical compounds by repeated dissolution, distillation and crystallizations, medical bacteriologists such as Robert Koch adopted this ideal with regard to bacterial organisms. Early practices of genetic engineering extended the bacteriological ideal of pure culture to encompass the whole organism, as concerns the purity of lines and strains. Since the beginning of the 20th century, practices of purification inform the creation and maintenance of homogeneous animal populations (e.g., transgenic mice). Taken together, standardized regimes are mandatory preparatory procedures in a variety of bioscientific designs, reaching from DNA and plasmid purification, to inbreeding and genetic engineering of homogeneous strains of experimental organisms.

Scholars in the history and sociology of science have elaborated how "standardized packages" structure epistemic labour in the laboratory sciences, bringing together theoretical models, technical infrastructures and methodological tools (cf. Fujimura 1992). Others have discussed in how far stability, homogeneity, and purity are restricted to laboratory practices only (cf. Hacking 1983, Knorr Cetina 1999). Against this background, the paper analyses how standards shape our understanding of scientific knowledge, that is, how they define what count as stable entities and valid data in the laboratory biosciences.

References

Fujimura, J. H. (1992). Crafting science: standardized packages, boundary objects, and "translation". In: A. Pickering (Ed). Science as practice and culture. Chicago and London: The University of Chicago Press, 168-211.

Hacking, I. (1983). Representing and intervening. Introductory topics in the philosophy of natural science. New York: Cambridge University Press.

Knorr Cetina, K. Epistemic cultures. How the sciences make knowledge. Cambridge/MA, London: Harvard University Press.

Schlaudt, O. & L. Huber (2015) (Eds.). Standardization in measurement. Philosophical, historical and sociological issues. London: Pickering & Chatto.

Human Nature Between Science and Politics: Dehumanization, Essentialism and the Call for Elimination

MARIA KRONFELDNER
Central European University
kronfeldnerm@ceu.edu

Human nature is a concept that transgresses the boundary between science and politics, between fact and value. It is as much a political concept as it is a scientific one. This talk will, first, introduce an important political function of the concept – dehumanization. Human nature is a concept that has been used for denying (a) membership in humankind or (b) full humanness to certain people in order to include or exclude people from various forms of politically relevant aspects of human life, such as rights, power, etc. After a systematic reconstruction of the phenomenon, I will, second, discuss in which sense dehumanization depends on essentialist thinking, and then, third, ask: what follows for science given that the concept of human nature facilitates dehumanization? Given that some humans have been and still are dehumanized, should scientists still speak in the name of human nature, i.e., use human nature talk? In other words, should sciences get rid of the concept of human nature, as suggested by critics of the concept of human nature?

The first major claim will be: there is no way to make sure that the concept of human nature is used in a politically completely neutral manner. Because of its inherent dehumanizing potential, it will always have a political function and will thus be at risk of being misused in science. To establish this claim, I will show that the concept of human nature facilitated and will continue to facilitate dehumanization: independent of the content transported with a concept of human nature and independent of whether some essentialist baggage comes with the concept of human nature. With respect to the elimination question, I will show that it is unlikely that there will ever be a consensus reached since both the eliminative stance and the revisionary stance are value-laden epistemic attitudes. The second major claim of this paper thus is: there is no ultimate scientific fundament for deciding between elimination and revision of a contested category such as 'human nature.'

Symposia & Contributed Papers VI

Mathematics as a Tool **Symposium**
Organizer: Johannes Lenhard
Chair: Nina Retzlaff Room 24, Friday 09:15 – 11:15

Empirical Bayes as a Tool

ANOUK BARBEROUSSE
University of Paris IV
barberou@canoe.ens.fr

Mathematics in the Era of Big Data is not the Tool of Science, but the Science of Tools

JUERGEN JOST
Max Planck Institute Mathematics in the Sciences
jost@mis.mpg.de

Boon and Bane: On the Role of Adjustable Parameters in Simulation Models

JOHANNES LENHARD
Bielefeld University
johannes.lenhard@uni-bielefeld.de

General Description
The role mathematics plays in the sciences has received different and conflicting assessments. A main distinction is the one between a strong and a weak view. Put very roughly, the former holds that mathematically formulated laws of nature refer to or reveal the rational structure of the world. The weak view denies that these fundamental laws are of an essentially mathematical character, and rather suggests that mathematics is merely a tool for systematizing observational knowledge about these laws and makes additional use of these laws. The distinction between these two viewpoints recently has become a more urgent problem for philosophy of science, since the use of mathematical methods, thrives on computational methods and

computational modeling. It has spread out in many sciences to an astonishing degree.

However, the philosophical reflection on this use is less widespread. There are iconic examples, like the famous essay by the theoretical physicist Eugene Wigner about the "unreasonable effectiveness of mathematics". Wigner was deeply impressed by the fact that there is a concise mathematical formulation of quantum theory that captures a highly complicated dynamics at the atomic level. This fact is all the more surprising as it is hard to imagine conceptually what is going on at the atomic level, while working with the mathematical description seems to provide access to essential aspects of this strange dynamics.

This conceptually baffling observation is accompanied by a second surprise, not mentioned by Wigner: Regularly, the mathematical formulations of fundamental regularities are intractable in practical situations. In quantum theory, for instance, it is already demanding to approximate what happens when only a small number of atoms interact. Problems in chemistry or materials science are of a different order. Clearly, mathematics as a tool requires usability. This statement may be obvious, but opens up new questions for philosophical thought:

- How does science manage to use mathematics as a tool?
- Are there essential properties of mathematics as a tool that differ from the "strong" structure viewpoint?
- How does the computer influence the way mathematics is used in the sciences?

The symposium will address these questions from three interdependent directions.

First, it will relate mathematical tool use to the challenges of big data. It appears like relevant mathematical theory is neutral about the subject matter that is investigated, but is more a theory about tools. Second, new Bayesian approaches in statistics are investigated. They have been extremely successful in the practices of many sciences, but call into question the traditional philosophical motivation for Bayesianism. Third, the symposium

will scrutinize the role of adjustable parameters. While they have been conceived as pragmatic but insignificant amendments of theory, they seem to assume a crucial role in computational and simulation modeling.

The symposium will bring together speakers from philosophy of science, mathematics, and engineering.

Abstracts

1. Anouk Barberousse: *Empirical Bayes as a Tool*

My aim in this talk is to discuss the hypothesis of mathematics as a tool by focussing on the example of Empirical Bayes (EB). EB is a recent trend in statistics whose importance is growing in domains like phylogenetic, image analysis, and climate science. It is a set of statistical methods that rely on machine computation. I shall investigate the hypothesis that statistics is a tool by focusing on this new trend.

The talk will address the following questions. First, how to view statistics within mathematics. Second, how to understand the notion of a tool within a philosophically-oriented investigation of science. And third, assess whether EB is a tool.

1. Within the mathematical realm, statistics looks as if it were a tool par excellence. It does not seem to have a proper object and is used in a bunch of applications without being defined by its own domain. It is often associated with probability on the one hand and with theories of inference on the other, thereby fluctuating, as it seems, between mathematics per se and logic, or even applied logic. As a result, it looks as if it could adapt to a variety of tasks, as a good tool that would not be too specialized.

2. The notion of a tool is so large that many elements of scientific activity could be called tools, from measurement operations, data sets, theories, to models, templates, statistical procedures, computers, simulations, etc. However, there is a way to make the notion more precise; it is by distinguishing between two components of its meaning. The first component is that a particular tool is usually defined by a purpose it has to fulfill, like driving nails or screwing bolts. Some purposes may themselves be very large, like "computation", allowing computers to be called "tools" in this sense. However, purpose-directedness seems to be an important aspect of being a

tool. The second component of the notion of a tool is that nothing would be called a tool without being useful or being actually used.

3. EB both deals with statistics and computer models. Thus, its tool-aspect is slightly different from the tool-aspect of mathematics in general. Moreover, EB is difficult to characterize because of its heterogenous and rapidly evolving nature. Is it a scientific method by itself, or just a statistical method? Should we apply to it the even more general expression of "a new way to perform statistics", or is it a new technique for computing statistical results? Still another option is to link it with the philosophical foundations of older Bayesianism and describe it as a general, perhaps philosophical, theory of scientific reasoning.

I am going to argue that in EB, the tool is not the mathematical part, but rather a complex set both including a mathematical part and a stochastic model designed for computer processing.

2. Juergen Jost: *Mathematics in the Era of Big Data is not the Tool of Science, but the Science of Tools*

There is the traditional model of science where in a particular scientific domain a formal theoretical framework is developed that explains the phenomena observed in that domain and furnishes quantitative predictions that can be tested in experiments or through observation of new data. Mathematics has a clear role in that model, as a tool to analyze the formalism and to provide the methods for arriving at those quantitative predictions. This view, however, sometimes clashes with the self-image of mathematics as a formal science of abstract structures.

In my contribution, I shall propose two more radical objections to this view of the role of mathematics as a tool. First, such neat scientific theories as described above are the exception rather than the rule (but is not clear why this is so), and therefore, we are often faced with data whose meaning is obscure. Second, instead of providing precisely crafted tools for every specific data set, mathematics should and could lift itself to a higher level of abstraction where data analysis as such and its structural and conceptual priors become the object of study. This leads to new and challenging mathematical questions. Mathematics will then no longer be a mere tool, but become the science of formal tools.

3. Johannes Lenhard: ***Boon and Bane: On the Role of Adjustable Parameters in Simulation Models***

Our claim is that adjustable parameters play a crucial role in the process of building and applying simulation models. Two interconnected aspects make up our claim: First, two varieties of experiments, or if you prefer another terminology: simulation and classical experiment, cooperate. Second, this cooperation makes use of a feedback loop and works via adjusting parameters. Equations of state in thermodynamics will serve as examples throughout the paper. A critical discussion of how adjustable parameters function will show that they are boon and bane of simulation. They help to enlarge the scope of simulation much beyond what can be determined by theoretical knowledge, but at the same time undercut the epistemic value of simulation models.

In our contribution, we will start with a brief introduction into mathematical simulation models, their implementation on computers and their application. Also, equations of state in thermodynamics are introduced, as we will use examples from that area throughout the paper. We chose this field, because it is a theoretically well-founded field of science and engineering. It provides us with material that illustrates our claims while these claims are valid a fortiori in cases in which accepted theory is available to a lesser degree and hence parameter adjustment presumably plays a larger role.

Philosophy of Mathematics **Contributed Papers**
Chair: Annika Schuster Room 3B, Friday 09:15 – 11:15

Gödel on Intuitionistic Logic, and Davidsonian Radical Interpretation: The Case of the Logical Constants

FABRICE PATAUT
Centre National de la Recherche Scientifique (CNRS)
Fabrice.Pataut@univ-paris1.fr

May the theory of radical interpretation help fix the meaning of the logical constants and promote ways of doing it which should be accepted as exclusively correct? I examine the case of negation, disjunction and the existential quantifier, both in the context of a Davidsonian situation of radical translation and in the context of Gödel's objections to the intuitive constructive definitions of "¬", "v" and "(∃x)".

I conclude that (i) Davidson's interpretative strategy fails to provide a reason to prefer the classical reading to the intuitionistic one, and that (ii) despite Gödel's objection that intuitionistic logic is a renaming of classical logic, there remains a further disagreement over the meaning of the constants. Consider negation. When one applies the principle that ascription of meaning to the ascribee's constant should be identical to that made by the ascriber to his own, the non-equivalence of the classical reductio or absurdity rule to the intuitionistic one may in some weak sense be judged irrelevant to the debate over the meaning of the constant. But when one applies a stronger principle according to which any ascription of meaning to that constant should be justified irrespective of who is responsible for the ascription, the non-equivalence of "~" (classical) and "¬" (intuitionistic) is conspicuous. Mere definitions of the constants may not provide a justification for any particular deductive practice, classical or otherwise, and the crucial question remains whether a stable semantics may be obtained that would provide a neutral ground from where the validity of logical laws and the invalidity of merely putative logical laws may be objectively adjudicated.

Of course, under Glivenko's translation of the classical constants into the intuitionistic ones, the classical calculus turns out to be a subsystem of the Heyting propositional calculus. I examine Gödel's critical reflexions regarding the extent to which classical existence proofs may be transformed into

constructive ones, and argue that there is still ground for a disagreement over the grasp of classical proofs. Mere definitions of the classical constants, either formal or by way of agreement, take for granted that the truth of sentences containing occurrences of them is independent from our ability to provide justifications for the sentences. This remains a sticking point despite the merits of Glivenko's and Gödel's approach.

References

Davidson (Donald), [1973] 2001, "Radical Interpretation," Inquiries into Truth and Interpretation, Clarendon Press, Oxford, pp. 125-140.

Glivenko (Valerii Ivanovich), 1929, "Sur quelques points de la logique de M. Brouwer," Académie royale de Belgique, Bulletin de la classe des sciences, vol. 5, pp. 183-188.

Gödel (Kurt), [1941] 1995, "In what sense is intuitionistic logic constructive?," Collected Works, Volume III: Unpublished Essays and Lectures, S. Feferman, Ed.-in-Chief, Oxford UP, Oxford, pp. 189-200.

- [1958] [1972] 1990, "Über eine bisher noch nicht benütze Erweiterung des finiten Standpunktes / On an extension of finitary mathematics which has not yet been used," Collected Works, Volume II: Publications 1938-1974, S. Feferman, Ed.-in-Chief, [1958] revised and expanded in English as [1972] by W. Hodges and B. Watson, Oxford UP, Oxford, pp. 271-280.

Hilbert's Axiomatic Method and Carnap's General Axiomatics

MICHAEL STOELTZNER
University of South Carolina
stoeltzn@mailbox.sc.edu

I compare the axiomatic method of David Hilbert and his school with Rudolf Carnap's general axiomatics that was developed in the late 1920s and that influenced his understanding of logic of science throughout the 1930s, when his logical pluralism developed through various stages. While recent scholarship has analyzed Carnap's general axiomatics primarily as an instance of early model theory, my paper starts from Carnap's primary motivation, to wit, the axiomatization of science and its paradigmatic example Hilbert's "Foundations of Geometry".

The distinct perspectives of the axiomatic method and general axiomatics become visible most clearly in how Richard Baldus, along the lines of Hilbert, and Carnap and Friedrich Bachmann, in 1936, analyzed Hilbert's specific axiomatization of geometry. While Baldus discussed a large number of different reorganizations, Carnap and Bachmann exclusively focused on the logical status and the proper formulation of the completeness axiom that stated that the system could not be extended by further axioms without running into inconsistencies. The main problem was that – after Gödel – there existed different and logically inequivalent options to formalize "completeness". This created severe difficulties for Carnap's general axiomatics and its pivotal role within the epistemology of science.

Whereas Hilbert's axiomatic method started from a local analysis of individual axiom systems in which the foundations of mathematics as a whole entered only when establishing – in a second step – the system's consistency, Carnap and his Vienna Circle colleague Hans Hahn instead advocated a global analysis of axiom systems in general, in the form of an attenuated logicism or with reference to a formal language chosen on pragmatic grounds. A primary goal was to evade, or formalize ex post, mathematicians' 'material' talk about axiom systems. For such talk about the motivation and structural quality of different axiom systems was error-prone and susceptible to metaphysics, at least to the extent that it went beyond pragmatic considerations of simplicity or fertility. Most problematic from the Viennese perspective was Hilbert's repeated talk about 'deepening the foundations' of an axiom system, which they tried to counter by emphasizing that all axioms stood essentially on a par irrespective of whether they were close to or remote from concepts actually used by scientists. If used properly however, or so I will argue, Hilbert's 'deepenings' could be reformulated without raising 'metaphysical' concerns.

Recognition Procedures and Dag Prawitz's Theory of Grounds

ANTONIO PICCOLOMINI D'ARAGONA
Aix-Marseille University, "La Sapienza" University of Rome
piccolominidaragona@libero.it

According to a widespread proposal, the meaning of the logical constants should be explained through an epistemic notion accounting for evidence. The main development of such an idea, the BHK-semantics, dates back to the intuitionistic tradition. The burden is here put on proofs, generally coded by constructions of a typed and extended λ-calculus. Some authors have however questioned the BHK-clause for implication, namely the idea that a proof of A → B is an effective operation that always yields a proof of B whenever applied to a proof of A. An effective operation could in fact be highly complex, so that being in possession of it could amount to know "how" to obtain a proof from a proof, but not also to know "that" it behaves in the way required. In the first part of my talk, I will discuss the position of Dag Prawitz on this topic; in the second, I will investigate some aspects of his approach within his recent theory of grounds.

Prawitz's proofs are endowed with functions denoting the recognition that evidence has been obtained. In the implicational case, the recognition must presuppose an understanding that an effective operation transforms proofs into proofs. For closed λ-calculi, this understanding is available and mechanical. Because of Gödel's theorems, anyway, no closed λ-calculus generates all the effective operations one needs. The linguistic context has then to be open, so that mechanical understandings are ruled out. Therefore, there remain only two options: either the understanding indicates case-by-case strategies, or it is a uniform – though non-mechanical – procedure. This leads to different ways to frame the question of whether every effective operation can be understood as transforming proofs into proofs. The case-by-case picture is reasonable, while arguments can be raised against the idea of a uniform procedure.

The theory of grounds involves a decidability problem that closely recalls these issues. Ground-terms are typed on formulae of a background language, and the question is now whether it is decidable that an effective op-

eration yields a ground of a specific type when applied to grounds in its domain. I will try to show that, from a ground-theoretic point of view, understanding procedures play the role of higher order operations. This is attained in three steps.

1. Prawitz's language of grounds G(L) for a first-order background language L is introduced.

2. G(L) is expanded via quantification over ground-variables – which allows to formalize meta-formulae of the kind “the effective operation f always yields a ground of type B when applied to grounds of type A1 … An”.

3. A language of grounds G(G(L)) for the background language G(L) is proposed, with terms typed on equations between ground-terms – which allows to take higher order operations as constructions for the meta-formulae above.

Within this framework, it will also be possible to see that the epistemic problems posed by effective operations again lurk out in connection with higher order operations, and that the understanding procedures always involve higher order operations to solve equations between ground-terms.

Confirmation **Contributed Papers**
Chair: Torsten Wilholt Room 3C, Friday 09:15 – 11:15

Of German Tanks and Scientific Theories: Estimating the Number of Unconveived Alternatives
(CANCELLED)

BURKAY OZTURK
Texas State University
burkay.ozturk@gmail.com

During the Second World War, the Allies faced a question colloquially known as the "German Tank Problem": how many tanks will the Axis ever produce? The answer resulted from an elegant probabilistic argument which was used by allied mathematicians to make accurate upper-bound estimates for the total Axis tank production. This paper shows that if two empirical postulates are true of the history of science, a parallel argument can be used to generate lower-bound estimates for the number of alternative scientific theories that remain undiscovered in a field of science. The lower bound in question increases proportionally with the number of theories that have already been discovered. So, the problem of underconsideration is a serious problem and—perhaps counter intuitively—it will get worse as science advances, not better.

The first postulate the argument needs is the postulate of finitude (POF) which is the proposition that in any fields of science there are only finitely many empirically viable theories.[i] The second one is the postulate of mediocrity (POM) which is the proposition that our position in a given field of natural science is Copernican, the number of the most recently conceived theory is an unprivileged number that might be anywhere in the chronological order of all theories that were and will be conceived.

As I show in the paper, when POF holds for a field of science and POM holds for our position in the history of that science, we can generate lower-bound estimates for the number of unconceived alternatives remaining in that field. That is, when the number of alternatives—N—is finite, and the position of the most recently discovered theory—nth—is unprivileged in the history of that field, the probability of N being at least r times n is given by the following probability function:

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$$p(N \geq (n \times r)) = 1 - \frac{(r-1)}{r} \tag{1}$$

Suppose that POF and POM currently hold for a field of science such as gravitational physics. Since n is unprivileged, we might be anywhere between near the beginning of the history of gravitational physics and all the way up to the very end.

As a specific instance of (1), we have:

$$p(N \geq (n \times 2)) = 1 - \frac{(2-1)}{2} = {}^{1}/_{2} \tag{2}$$

In other words, the probability that there remain in at least as many unconceived alternative theories of gravitational physics as conceived theories is a coin toss.

This fact has important implications about not only the debate about the problem of underconsideration, but also the scientific realism debate at large. For instance, the most popular strain of scientific realism is what I would call "retentionist realism," according to which the parts of scientific theories (such as postulated theoretical entities, mathematical and logical structures, etc.) that have been consistently retained through scientific revolutions in a field of science are real. However, the probabilistic argument above can also be run for unconceived entities, structures and such. Since the number of unconceived entities swell as we conceive more entities, the more entities we conceive, the less confident we should grow that those that we have conceived are real.

In this regard, the argument at hand deserves critical attention.

Notes

[i] POF could be rejected. For instance, we might appeal to Humean underdetermination to argue that for any finite set of data, there are infinitely many mutually incompatible theories consistent with that data. However, since the denial of POF would entail that the problem of underconsideration is a serious -and perhaps intractable- problem, the proponents of the problem might want to grant it to their opponents for the sake of the argument. As the argument in the paper shows, even when we grant POF, the problem of underconsideration still remains a serious problem.

Cancelled

References

Davies, G. (2006, July 20) 'How a Statistical Formula Won the War', *The Guardian*, Retrieved from: http://www.theguardian.com.

Gott, R. J. (1993) 'Implications of the Copernican Principle for Our Future Prospects', *Nature*, 363: 315.

Lipton, P. (1993) 'Is the Best Good Enough?', Proceedings of the Aristotelian Society XCIII, 89-104; reprinted in D. Papineau (ed.), *Philosophy of Science, Oxford Readings in Philosophy*, Oxford: Oxford University Press, 1996.

Magnus, P. (2010) 'Inductions, Red Herrings, and the Best Explanation for the Mixed Record of Science', *British Journal for the Philosophy of Science,* 61: 803–819.

Stanford, K. (2006) *Exceeding Our Grasp: Science, History, and the Problem of Unconceived Alternatives*, Oxford: Oxford University Press.

Defending Selective Confirmation Strategy

YUKINORI ONISHI
The Graduate University for Advanced Studies
yukinori.onishi@gmail.com

It is a common strategy among recent scientific realists to restrict one's realistic commitment only to those components of a successful theory that essentially contributed to the success it enjoyed. That way, they claim, realists can explain the success, while making realism compatible with the historical fact that theoretical entities posited in past successful theories were often discarded at the time of theory change. In his Exceeding Our Grasp (2006), Kyle Stanford called this strategy of recent realists `selective confirmation strategy,' and raised two challenges against it (especially against the version proposed by Philip Kitcher and the one by Stathis Psillos; but I think the target can be more general).

The first challenge, which I shall call `the argument from the common sources,' criticizes ad hoc nature of the manner in which selective confirmation realists identify the confirmed or true parts of a successful theory, and claims the necessity of a prospectively applicable criterion with which scientists can distinguish the confirmed part from idle ones "in advance of any

future developments" (Stanford 2006). The second challenge against the strategy, which I shall call `the no refuge argument,' concerns the reliability of scientists' contemporary judgments regarding confirmed/unconfirmed status of the parts of a successful theory. Stanford cites many examples of poorly made those judgments by past scientists, such as Antoine Lavoisier, August Weismann and James Clerk Maxwell, and inductively argues that we are not able to reliably differentiate confirmed parts of a successful theory from idle ones. With these arguments, Stanford concludes, ``without some prospectively applicable and historically reliable criterion for distinguishing idle and/or genuinely confirmed parts of our theories from others, the strategy of selective confirmation offers no refuge for the scientific realist'' (2006, 168-169).

In agreement with Psillos' and Juha Saatsi's objection at a review symposium on (Stanford 2006), I think the common sources argument is based on Stanford's misunderstanding on the exact nature and purpose of selective confirmation strategy and is groundless. The focus of my presentation will be on the no refuge argument, which hasn't attracted so much attention despite its importance. Since it takes the form of induction, I shall criticize both its base cases and the inductive step. The essence of my objection lies in the insight about the distinction between individual-level and community-level properties, an insight that philosophers have learned through the dialog with sociologists of scientific knowledge in the past few decades. Once this distinction is made clear, I claim, it becomes questionable whether the historical evidence Stanford cites to support the no refuge argument is really appropriate to deliver any substantial conclusion about the reliability of the current scientific community's judgment.

Qualitative Research and Evidential Support
(CANCELLED)

CORRADO MATTA
Stockholm University
corrado.matta@edu.su.se

This paper discusses the concept of evidential support in the case of qualitative methods in social research.

My main thesis is that, although some (Lincoln (2002) and Freeman et al. (2007)) have claimed that qualitative evidential bases do not support hypotheses in the same way as their quantitative counterparts, there is no crucial epistemological limitation for evidential support in qualitative methodology.

First of all, I apply Glymour's bootstrap approach to confirmation (1980) to the skeptical claims discussed by Lincoln (2002) and Freeman et al. (2007). Here, I argue that these claims do not point at any special problem of confirmation for qualitative research, but only at simple restatements of general problems of confirmation common to all scientific practices.

Secondly, I argue that there is indeed a possible problem of confirmation that has not been discussed in the literature but that might entail that qualitative social research is epistemologically separated from the rest of scientific practices.

In order for researchers to use evidential bases such as interview data to support hypotheses, social researchers must use theories that bridge linguistic utterances to hypotheses. However, in order to connect qualitative data to hypotheses, these theories must either be semantically complete axiomatic theories of natural language, or they must coincide with the researchers' personal linguistic competences. Whereas the former alternative is not available and might never be, the latter makes evidential support a matter of arbitrary judgment of the researcher. I call this the problem of arbitrariness.

In order to look for a possible solution to this problem, I suggest examining the practice of qualitative research. I therefore consider a concrete case of from educational science. I analyze the relationship between the raw observational data and the hypotheses discussed by the researchers in the

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case, and complete my analysis with information retrieved interviewing the researchers.

From the analysis of my concrete case I identify three procedures that are used by researchers to cope with the arbitrariness problem. These are:

1) The assumption of a functionalist rather than referentialist approach to language,

2) The restriction of all possible functions to a set of few salient functions,

3) The use of functional differential structures (this means that all salient functions are related to one another in a way that a function can be present rather than one or more other salient functions and cannot only be present simpliciter. If this structure can be said to exist, then the observation of a function is relevant only if the function is present rather than some other function).

I use these three procedures to sketch a conceptualization of evidential support that: a) is specific for qualitative methods, b) can solve the arbitrariness problem and c) remains faithful to the basic principles of bootstrap confirmation.

I conclude that qualitative researchers use evidential bases to confirm claims in the same way as researchers in quantitative social science and natural science do, and that therefore skepticism against the concept of evidence in qualitative research is not justified.

Cancelled

General Philosophy of Science VI **Contributed Papers**
Chair: Hajo Greif Room 3D, Friday 09:15 –11:15

Toulmin's Logical Types

DAVID BOTTING
IFILNOVA
davidbotting33@yahoo.co.uk

In this paper I will show that Toulmin's distinction between logical types is in some ways a reformulation and expansion of the distinction made in logical empiricism between the directly and indirectly verifiable. I will show that Toulmin makes a straw man of the empiricists' own solution to the problem and hence does not, in his main example using prediction, prove there to be an inadequacy in the analytic ideal.

Goodman's Paradox and Hansson's Puzzle

WOLFGANG FREITAG
Freiburg University
wolfgang.freitag@philosophie.uni-freiburg.de

The paper explores Goodman's paradox with the aim of demonstrating that it is at heart identical to Sven Ove Hansson's puzzle (Hansson 1992, 1999): In view of the information that non-p, we cannot accept both that q and that p ↔ q. As a consequence, we need to choose between the two competing options and justify our choice. Goodman's paradox is but a specific instance of this type of problem: It arises from the fact that the green- and grue-hypotheses (where grue is defined, irrelevant subtleties apart, as green ↔ examined-before-t) cannot both be maintained in face of the information that there are future, as-yet-unexamined emeralds. The only difference is that, whereas Hansson's original puzzle is presented within the context of AGM belief revision theory and hence of deductive reasoning, Goodman's paradox is framed in a context of inductive inference: in the scenario Goodman describes, the green- and grue-hypotheses are equally supported by our inductive evidence. I will show, however, that this fact, rather than being a part of the problem, is an integral part of its solution.

While my main aim is to clarify the logical structure of the paradox, I will also propose a solution based on an epistemic asymmetry between the conflicting propositions. Drawing on recent results (author 2015a; 2015b) and taking the solution to Hansson's puzzle as a model (see author 2015c), I will argue that the green-hypothesis is preferable to the competing grue-hypothesis because the latter, but not the former, deductively depends on a proposition known to be false.

The argument takes three steps. Section 1 presents Hansson's original puzzle and its solution in terms of an epistemic asymmetry: If a belief deductively depends on another, the former must be abandoned if the latter is dropped. Section 2 discusses Hansson's puzzle and possible solutions in the context of inductive inference. Section 3 shows that Goodman's paradox is a specific instance of the puzzle discussed in Section 2. The paper concludes with a comparison of the proposed solution to prominent extant solution strategies presented by Carnap (1947/1971), Quine (1969), Lewis (1983), and Jackson (1975)/Schramm (2014).

References

Author 2015a. Author's work.
— 2015b. Author's work (under review).
— 2015c. Author's work (under review).
Carnap, Rudolf 1947: "On the Application of Inductive Logic", *Philosophy and Phenomenological Research* 8: 133–147.
— 1971: "A Basic System of Inductive Logic", in Carnap, R. & Jeffrey, R. C. (eds.), *Studies in Inductive Logic and Probability* (Vol. I). Berkeley and Los Angeles: University of California Press, 33–165.
Goodman, Nelson 1946: "A Query on Confirmation", *Journal of Philosophy 43*: 383–385.
—1983 [1965]: *Fact, Fiction and Forecast,* 4th edition. Cambridge MA: Harvard University Press.
Hansson, Sven Ove 1992: "A Dyadic Representation of Belief", in: Gärdenfors, Peter (ed.), *Belief Revision*, Cambridge: Cambridge Univesity Press, 89–121.
— 1999: A *Textbook of Belief Dynamics: Theory Change and Database Updating*, Dordrecht: Kluwer Academic Publishers.
Jackson, Frank 1975: "Grue", *Journal of Philosophy* 72: 113–131.

Lewis, David 1983: "New Work for a Theory of Universals", *Australasian Journal of Philosophy* 61: 343–377.
Quine, W. V. O. 1969: "Natural Kinds", in *Ontological Relativity and Other Essays*, New York: Columbia University Press: 114–138.
Schramm, A. 2014: "Evidence, Hypothesis, and Grue", *Erkenntnis* 79: 571–591.

Why Coherence Cannot be Measured as Relative Overlap

JAKOB KOSCHOLKE
University of Oldenburg
jakob.koscholke@uni-oldenburg.de

Coherence is the property of propositions hanging or fitting together. Intuitively, adding a proposition to some given set of propositions should be compatible with either increasing or decreasing the set's degree of coherence (or none of both). In this paper we show that probabilistic coherence measures based on relative set-theoretic overlap are in conflict with this intuitive verdict. More precisely, we prove that (i) according to the naive overlap measure of coherence by Glass (2002) and Olsson (2002) it is impossible to increase a set's degree of coherence by adding propositions and that (ii) according to the refined overlap measure of coherence by Meijs (2006) no set's degree of coherence can exceed the degree of coherence of its maximally coherent subset (which can only be a two-element set). We also show that this result carries over to all other subset-sensitive refinements of the naive overlap measure based on variations of the employed weighting procedure. As these two results stand in sharp contrast to elementary coherence intuitions, we conclude that coherence cannot be measured in terms of relative set-theoretic overlap.

Philosophy of the Social Sciences and the Humanities II **Contributed Papers**
Chair: Eva-Maria Jung Room 22, Friday 09:15 – 11:15

The Role of "Ought" in Value Theory: Philosophical and Sociological Perspectives

ELIZAVETA KOSTROVA
St-Tikhon Orthodox University
elizakos@mail.ru

The concept of value appears in research of many disciplines, especially economics, psychology, sociology. Their approaches to the definition of what value is are known to be dramatically different. A significant variation can be observed even within particular sciences. Moreover, there are also a philosophical and a common sense understandings of value. The report elaborates on the problem of sociological understanding of the value and its relation to the philosophical one.

To date, empirical study of values has advanced significantly (it is enough to name Shalom H. Schwartz, Ronald F. Inglehart, large-scale European and global surveys). A large amount of data has been accumulated, and methods of measuring value preferences of the population have become quite sophisticated. Meanwhile, one cannot yet speak about full clarity as to what the data show and how they relate to others (in particular, philosophical) approaches to values.

From the standpoint of empirical science, the most coherent and appropriate way is obviously to assess the subjective side of values. Accordingly, case studies give an idea of how public perceptions are integrated into the consciousness and activity of individuals. The key element of value thus turns out to be desirability: the object of the value is desirable, it guides individual's choice and provides motivation to behave in a certain way. The value is treated as an ideal of human life and society.

This approach captures a very important factor in the nature of values. However, it excludes from consideration the ought-dimension, which is present in the philosophical understanding of the value. The fact that an individual, for example, considers it desirable and important to follow the rules

does not imply that it ought to be desirable unless we suppose that all people are highly intolerant and cannot bear any difference in others. The requirement of scientific verifiability notably demanded such a limitation of the concept, so that it could be grasped empirically. But this raises the question of what is value and what exactly the results of the polls show, and whether these are the same values we are talking about.

This has some common points with the problem of inconsistency of declared values: not all respondents declaring certain values are really guided by them in their daily activities. If values really meant the principle of selectivity and the desired ideal, such motivational inconsistency could constitute a problem.

The "Invisible Hand" as a Natural Law

JUDITH WÜRGLER
University of Neuchâtel
judith.würgler@unine.ch

The present talk presupposes two theses for which independent support could be provided, but which would lead us too far for the present purpose: first, the Homo Economicus assumption, according to which people only pursue their rational individual interests, does not purport to be descriptive of real, existing human behavior. Secondly, this assumption rather aims at grounding the possibility of a prosperous society in a "realistic" picture of human psychology that does not expect "too much" of society's members. Thus, the question is: how to achieve collective prosperity without assuming the citizens to be morally motivated?

Some economists – and philosophers – have supported the thesis according to which the best way to bring individual interests to increase the prosperity of a society is to allow these interests to express themselves freely in action – this is the "laissez-faire" injunction. According to this view, the individual concern for one's own material situation has unintended consequences that are beneficial to society. In other words, there is an "invisible hand" that naturally makes the interests of each individual conform with the interest of society as a whole. And since egoism naturally brings prosperity, we should never interfere with such a beneficial natural law. But what kind

of natural law is this? How can we make sense of the "invisible hand" as a natural law?

Léon Walras, one of the founders of neo-classical economics, in his "Elements d'économie politique pure", suggests three different ways of seeing price formation, which is fundamental to the general law of the invisible hand, as a natural phenomenon. He says that price formation – or the determination of "exchange value" – can be considered to be natural (1) in its "origin", (2) in its "way of being" and finally (3) in its "manifestation". The aim of the talk is to show that all of these three interpretations do not work, and therefore that there is no way to conceive of the "invisible hand" as leading naturally to a materially prosperous society. In order to support this thesis, I will discuss the arguments in favour of each interpretation and show why these arguments do not work.

I will finish the talk by arguing for the even bolder claim that we cannot expect the market to bring prosperity without ascribing to its actors some moral motivation or disposition. We need to reject not only the idea that the pursuit of individual interests naturally leads to collective prosperity, but we also need to reject the idea that the market could in a non-natural way, using state regulation to enforce free-market conditions, make individual interests conform with the interest of society as a whole, without relying on a human concern for morality. And such a capacity for moral motivation runs counter to the fundamental Homo Economicus hypothesis of universal egoism.

The presentation is based on texts from (among others): ARROW Kenneth, 1992. BOWLES Samuel, 2008. BRENNAN Geoffrey, 1993. FYOT Jean-Louis, 1952. HAMEL Christopher, 2015. WALRAS Léon, 1926. SUGDEN Robert, 1989.

Micro Economics Between the Natural Sciences and the Humanities

KARSTEN K. JENSEN
University of Copenhagen
kkj@ifro.ku.dk

Many people consider micro economics the core of economics. But it is hard to characterize as a science. It is clearly different from other social sciences, such as sociology and anthropology, by aiming at uncovering basic mechanisms in any economy. But its justification of these mechanisms is unclear and contested.

One strain of justification is rationalistic. It can be found especially in the Austrian school; it claims that the theory of rationality is necessary in order to understand human action. However, classical rationalism runs into well known problems about justifying the basic axioms – i.e. socalled Trilemma of justification. One could then imagine a more modern hermeneutic stance on interpretation, but the criteria for a successful interpretation do not appear very clear.

The aim of understanding human action by reference to the underlying motivation is a clear humanistic trait. But economic's psychology is very thin. Hence, economics cannot be said to share the humanistic concern for detail and context. The aim at general mechanisms points more in the direction of natural sciences. But again, there are differences. Micro economics is really a very simplified mathematical model. And even though mathematical models are also used in the natural sciences, the relation between model and reality seems much more unclear in the case of economics. The empirical justification does not appear evident, but nevertheless the basic assumptions are almost immune to falsification. One could claim that micro economics is a research program in Lakatos' sense, but again it is not clear what it would take for an alternative theory to better.

This paper will try to characterize micro economics as a rather unique kind of science by synthesizing the above lines of reasoning. The claim is that each of the characteristics is unconvincing on their own, but together they may support each other. Micro economics does not produce natural laws, but rather present us with a basic framework to understand and analyze economies. This framework, however, is ultimately justified by its ability to

increase understanding ("explanatory power"). The challenge is to state criteria for increased understanding. I propose to do this though considering what an alternative theory could look like. The tentative conclusion is that once you give up on the generality of micro economics, you get a far less interesting theory.

However, this conclusion does not seem to hold in all areas. Giving up the assumption of complete knowledge leads to interesting the interesting theory of information economics. But here I claim that this is in fact to introduce greater generality (as decision theory is more general than the case of certainty).

Practical Information

Registration and information

You will find the conference registration and information desk directly at the conference venue. The registration and information desk will be in your service:

Tuesday:	13:00 – 18:45
Wednesday:	09:15 – 18:45
Thursday:	09:15 – 18:45
Friday:	09:15 – 13:00

Registration and information desk phone: +49 (0)211 81 11605

Conference venue

The conference venue is located in building 23.01 and 23.02 (directly connected) at the University of Duesseldorf. The address of the conference venue is Universitaetsstrasse 1, 40225 Duesseldorf.

To reach the conference venue from the main station, take U79 (below main station; roughly 15min) or tram 704 (in front of main station; roughly 20min), both direction: “Uni-Ost/Botanischer Garten”, and exit at the final stop.

Conference rooms

The parallel sessions and symposia will be held in rooms 3B, 3C, 3D, U1.22, and U1.24. The plenary lectures as well as the GWP meeting will take place in room 3D.

If you need technical assistance or encounter technical problems, please contact the conference assistants at the registration and information desk.

Venue Accessibility

All rooms are handicapped accessible. There are disabled toilets available and floors are connected via elevators. Also the canteen is handicapped accessible. For support just contact our crew at the registration and information desk.

Child care

Child care service will be offered for children of GWP participants. For child care service, please check the corresponding box when registering for the conference or send an e-mail to the local organizers. The local organizing committee will contact you and provide more details.

Internet

Eduroam is available at the whole university campus: https://www.eduroam.org/. In case you have no eduroam access, you can also use the university WLAN (HHUD-W) free of charge. Username and password are provided in the conference binder. If there is urgent need, conference participants may also use a computer that is located at the registration and information desk.

Printing

You have the opportunity to print at the registration and information desk. Please note that we can only print a few pages (e.g., flight tickets, but no handouts).

Luggage room

You can leave your luggage at the registration and information desk during the above mentioned service times.

ATM

The nearest ATM is located at the main library (see “Bibliothek” at the map). A second ATM is located at the main canteen (“Mensa”).

Coffee and refreshments

Coffee and tea will be served during the refreshment breaks. All refreshments are served in the foyer. There are also several cafeteria as well as a canteen at the university campus where you can pick up some drinks and sandwiches:

- Cafeteria at the ground floor of the conference venue (building 23.01)
- Ex Libris at the main library (right to the main entrance; see “Bibliothek” on the map)
- Main canteen at building 21.11 in the north of the campus (see “Mensa” on the map)
- Café Uno left to the main canteen
- Café Vita right to the main canteen

Dinner restaurants

Close to the campus are only a few small restaurants for dinner. The closest one is “Scottie’s” next to the tram stop Christophstraße (see map), where Burger’s and also local food is served for a reasonable price. There is also a Subway around the corner of the main canteen. There are many nice restaurants in the city center. For traditional/local food you may consider:

- Brewery “Füchschen”: Ratinger Straße 28
- Brewery “Zum Schlüssel”: Bolkerstraße 41 – 47
- Brewery “Schlösser Quartier Bohème”: Ratinger Straße 25
- Brewery “Uerige”: Berger Straße 1

Conference dinner

The conference dinner will take place at brewery “Zum Schlüssel” <http://www.zumschluessel.de/> in Duesseldorf's city center on Tuesday, 08 March, 2016. It includes three courses and one drink for 28 EUR (registration in advance necessary; the dinner fee for students is only 13 EUR). To get there take underground U71 (direction: Heinrichstraße), U73 (direction: Gerresheim) or U83 (direction: Gerresheim) from Christophstraße to Heinrich-Heine-Allee. There take the exit "Bolkerstraße" and walk on to nr. 41 – 47.
There will be guides leading you to Christophstraße. They will wait for you at the main entrance, starting at 18.50.

Tourist information

At the old town is a tourist information point located on Marktstraße/corner Rheinstraße. See also
<http:/ http://www.duesseldorf-tourismus.de/en/brochures/>.

Police and medical assistance

If you need to call the police or need an ambulance, the emergency number is 112.

On arrival

Transportation from/to airport: Please be aware that there are two airports associated with Duesseldorf: Duesseldorf Airport (DUS) and Airport Duesseldorf Weeze (NRN). While Duesseldorf Airport (DUS) is very close to the city, Airport Duesseldorf Weeze (NRN) is actually about 90 kilometers away from Duesseldorf. Transportation from Duesseldorf Airport (DUS) to the main station costs about 25 EUR with taxi and about 2,60 EUR with train. For the latter buy a zone A single fare ticket – valid up to 90 minutes after stamping – and take the train S11.

Public transportation in Düsseldorf

If you need to use buses, trams, or the metro, you might want to buy a single ticket (about 2,60 EUR) or a day ticket (6,60 EUR) all for the zone A (the university is within this zone). There is also a 7-day ticket for about 21,20 EUR. The tickets can be purchased in the buses, trams (coins only), and the metro (coins only) as well as at ticket machines at the main station or the old town. You can find information about routes, timetables, and prices at the website of the Rheinbahn: http://www.rheinbahn.de/. To reach the conference venue from the main station, take U79 (below main station; roughly 15min) or tram 704 (in front of main station; roughly 20min), both direction: “Uni-Ost/Botanischer Garten”, and exit at the final stop.

Taxi

You can phone up and book a taxi from a taxi office; call (24h): +49 (0)211 33333 or book at: <http://www.taxi-duesseldorf.com/>. A taxi from the university to the old town costs about 25 EUR.

Accommodation

Below you find a selection of hotels in Duesseldorf. A number of rooms have been reserved in Motel One Main Station for the conference participants to be booked at a fixed rate. Please be advised to make your reservations in good time because the offer is only valid until the date indicated below. Be sure to use the reservation code specified below when making the reservation.

(The following information is given without guarantee of being complete, correct, and up-to-date.)

Hotels close to the main station

Motel One Main Station
Address: Immermannstrasse 54, 40210 Duesseldorf
Connection to conference venue: enter at stop “Duesseldorf Hbf” U79 or tram 704 (both direction: “Uni-Ost/Botanischer Garten”); exit at final stop
E-Mail: duesseldorf-hauptbahnhof@motel-one.com
Reservation code: “GWP2016” (Please use this form (PDF) for reservation.)
Offer valid until: 25 January, 2016
Single room: 69,00 EUR (without breakfast)
Double room: 79,00 EUR (without breakfast)

Hotel Ibis Main Station
Address: Konrad-Adenauer-Platz 14, 40210 Duesseldorf
Connection to conference venue: enter at stop “Duesseldorf Hbf” U79 or tram 704 (both direction: “Uni-Ost/Botanischer Garten”); exit at final stop
E-Mail: h0793@accor.com

Hotels close to the conference venue

HK-Hotel Duesseldorf City
Address: Varnhagenstrasse 37, 40225 Duesseldorf
Connection to conference venue: enter at stop “Moorenstrasse” bus 835 (direction: “D-In der Steele”) or bus 836 (direction: “D-Universitaet Sued”); exit at stop “Universitaet Mitte”
E-Mail: info@hk-hotels-duesseldorf.de

Hotel Astra
Address: Ubierstrasse 36, 40223 Duesseldorf
Connection to conference venue: enter at stop “Merowingerplatz” bus 835 (direction: “D-In der Steele”) or bus 836 (direction: “D-Universitaet Sued”); exit at stop “Universitaet Mitte”
E-Mail: info@hotelastra.de

Residenz Hotel Eurostar
Address: Merowingerstrasse 84, 40225 Duesseldorf

Practical Information

Connection to conference venue: enter at stop “Merowingerstrasse” bus 835 (direction: “D-In der Steele”) or bus 836 (direction: “D-Universitaet Sued”); exit at stop “Universitaet Mitte”
E-Mail: kontakt@residenz-eurostar.de

Hotel Haus Mooren
Address: Witzelstrasse 79, 40225 Duesseldorf
Connection to conference venue: enter at stop “Uni-Kliniken” tram 704 (direction: “Uni-Ost/Botanischer Garten”); exit at final stop
E-Mail: info@hotel-haus-mooren.de

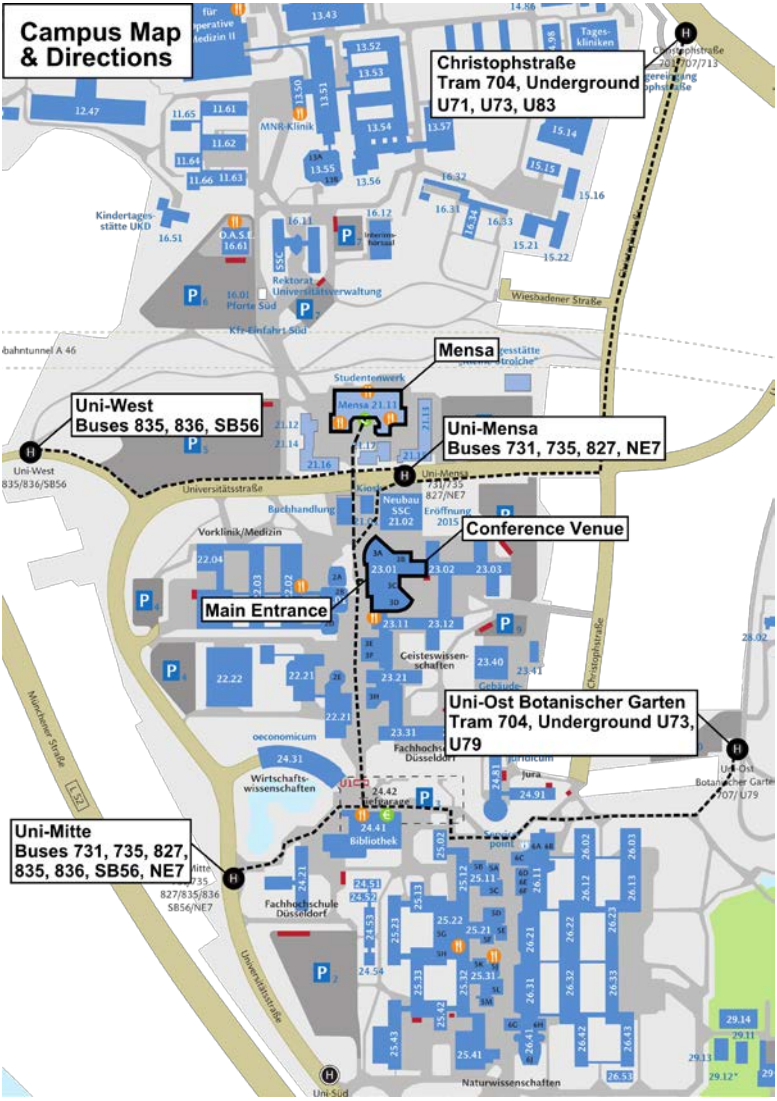
Hotel Flora
Address: Auf'm Hennekamp 37, 40225 Duesseldorf
Connection to conference venue: enter at stop “Auf’m Hennekamp” tram 704 (direction: “Uni-Ost/Botanischer Garten”); exit at final stop
E-Mail: office@hotel-flora-duesseldorf.de

Social Program

Tuesday 8, 19:30 –, “Zum Schlüssel”: Bolkerstraße 41–47 (old town): Conference Dinner

The conference dinner will take place at the brewery “Zum Schlüssel” in Duesseldorf's city center. It includes three courses and one drink for 28 EUR (registration in advance necessary; the dinner fee for students is only 13 EUR). To get there take underground U71 (direction: Heinrichstraße), U73 (direction: Gerresheim) or U83 (direction: Gerresheim) from Christophstraße to Heinrich-Heine-Allee. There take the exit "Bolkerstraße" and walk on to nr. 41 – 47.
There will be guides leading you to Christophstraße. They will wait for you at the main entrance, starting at 18.50.

Maps


Campus Map & Directions
Christophstraße
Tram 704, Underground
U71, U73, U83
Mensa
Uni-West
Buses 835, 836, SB56
Uni-Mensa
Buses 731, 735, 827, NE7
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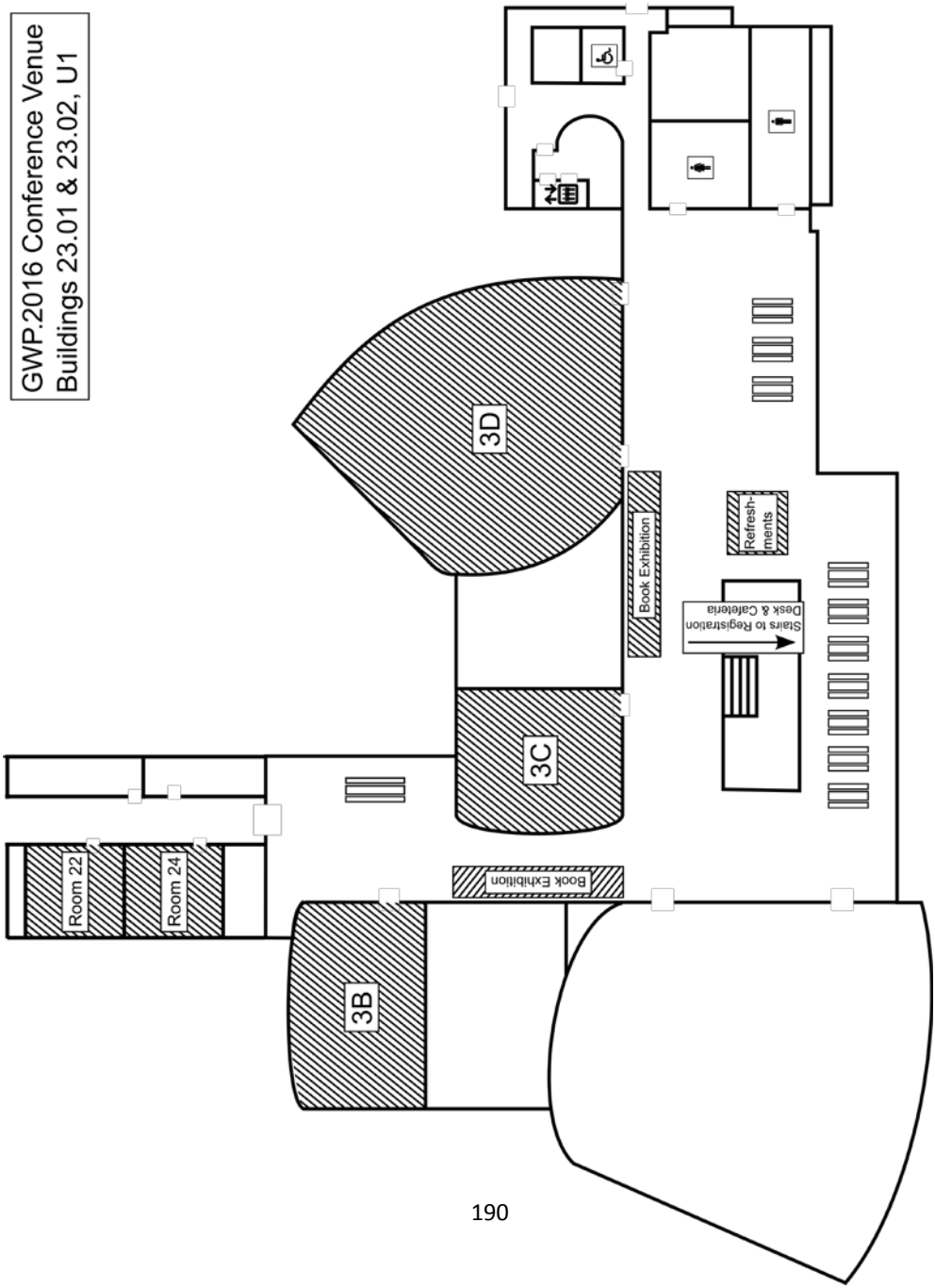
Geisteswissenschaften
Wirtschaftswissenschaften
Bibliothek
Fachhochschule Düsseldorf
Naturwissenschaften
Uni-Süd


GWP.2016 Conference Venue
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