Book of Abstracts

The Third International Conference of the German Society for Philosophy of Science (GWP)

25 – 27 February 2019
http://www.wissphil.de/gwp2019

Philosophisches Seminar
Universität zu Köln, Germany

http://www.philosophie.uni-koeln.de

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Philosophisches Seminar
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Universität zu Köln, Germany

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(Heinrich Heine University Duesseldorf)

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Preface

Preface by the GWP President

Dear Colleagues,

GWP has become a well-established scientific society which celebrates its third triennial conference, GWP.2019, in Cologne under the local organization team of Andreas Hüttemann. Founded under its first president Holger Lyre in 2011, the first of the triannual GWP conferences took place in 2013 in Hannover and the second one in 2016 in Düssseldorf. At the general assembly of that conference a new steering committee was elected in which I serve as president, Uljana Feest as vice-president, Christian Feldbacher-Escamilla as managing director, Alexander Gebharter as treasurer and Thomas Reydon as co-opted member.

Since 2016 the GWP was engaged in a number of activities, internally and externally. To begin with the internal activities, we have set up a new website with electronic user accounts that can be administered by our members. We have also installed a new electronic newsletter system that is meanwhile well received and helps to increase the international visibility of our society. Both achievements were implemented and are supervised by our managing director Christian Feldbacher-Escamilla, whom we owe our special thanks. The number of GWP members has increased from around 130 (in 2016) to about 185 and the number of newsletter recipients exceeds 200. In the first year of the new steering committee we had to perform some longsome legal adjustment operations in cooperation with the responsible district court in Hannover; this work was conducted by Alexander Gebharter whom we owe our thanks in this respect. We also changed the constitution by allowing the possibility of electronic general assemblies, for example in form of electronic polls. These changes were based themselves on an electronic survey in 2017 the result of which were published in our internal notices and documented a high agreement of the GWP members with these changes.

The GWP continued and extended its cooperation with related scientific organizations: at GWP.2016 we supported a joint SPS-GWP-Symposium (the French society for philosophy of science, by Christian Sachse et al.), at DGPhil.2017 a joint DGPhil-GWP-colloquium (by Andreas Hüttemann), at GAP.9 a joint GAP-GWP-colloquium (by Christian Feldbacher-Escamilla and myself), and at this conference there will be a joint GAP-GWP-colloquium (by Holger Lyre et al.). The GWP is a society-member (a member 'qua society') of two umbrella societies: of the European Society for Philosophy of Science (EPSA) since 2013, and one year ago we applied for society-membership in the DLMPST, the Division of Logic, Methodology and Philosophy of Science and Technology (which is part of the IUHPST). At present we are accepted as a candidate member[1] and the formal decision will take place in August 2019 at the DLMPST conference in Prague.

Since 2016 the GWP has been involved in three publications, the report about the 2nd International Conference of the GWP that appeared in the JGPS 48(2), 2016 (by A. Christian, A. Gebharter and C. Feldbacher-Escamilla), a special issue of the JGPS containing selected papers of GWP.2016 that appeared as JGPS 48/3, 2017 (ed. by C. Feldbacher-Escamilla, A. Gebharter and myself), and a special volume of the EPSA-series "European Studies in

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1 See https://dlmps.org/pages/members.php#candidatemembers

the Philosophy of Science" containing a further selection of papers of GWP.2016, published with Springer International Publishing, Cham 2018. All GWP members have free electronic access to these publications. Moreover, the cooperation of the GWP with the JGPS is continuing and flourishing, underscored by the co-optation of Thomas Reydon, one of the JGPS editors, as steering committee member. All GWP members have free electronic access to the JGPS via the member area of our website and can obtain the print version for a reduced price of 50 EUR per year.

The GWP is continuously funding young academics by reimbursing travel expenses for conference visits and supporting the organization of GWP-related workshops, to the extent that our budget allows. In 2016 we spent about 550 EUR for funding conference visits and about 1.300 EUR for workshops, in 2017 about 700 EUR conference visits and 500 EUR for workshops; and in 2018 700 EUR for conference visits and 500 EUR for workshops. Moreover, GWP is promoting women in philosophy of science; to increase the visibility of women in Philosophy of Science the GWP website contains a site entitled "Women in Philosophy of Science" including a list of all female members of the GWP, which are currently 44 in number. The list is accessible to all members of the GWP.

Last but not least, in 2018 the discipline of Philosophy of Science – in German Wissenschaftstheorie und -philosophie – was included in the list of small scientific disciplines ("Kleine Fächer") at the Arbeitsstelle Kleine Fächer supported by the German Federal Ministry of Education and Research. At present time this institution registers 151 small scientific disciplines, for example General Linguistics (Allgemeine Sprachwissenschaft), Biophysics (Biophysik) or History of Science (Wissenschaftsgeschichte). A portrait report about our discipline can be found at the homepage of this organization (see https://www.kleinefaecher.de/beiträge).

Let me come back to the occasion of this little report about GWP's activities in the last three years, namely our third international conference, GWP.2019. Compared to the last conference, the number of submissions has further increased to 140 paper submissions and 9 symposia submissions. Since we neither wanted to significantly increase the rejection rate of our conference nor its duration, we decided to increase the number of parallel sessions from 5 to 6. As always, we have six invited talks, including a JGPS lecture (funded by Springer); as a novum, we now also have a second funded lecture, namely the De Gruyter lecture. More details on data of the present conference are found in the preface of the chair of the local organization committee, Andreas Hüttemann. I conclude this preface by expressing my warm thanks to Andreas Hüttemann and his team for their really great work and excellent cooperation, and to all of you for coming to Cologne. Let us look forward to an exciting GWP.2019 conference!

Gerhard Schurz

President of the GWP

Head of DCLPS (Düsseldorf Center of Philosophy of Science), HHU Düsseldorf

Preface by the Local Organizing Committee Chair

Dear participants of the third GWP-conference,

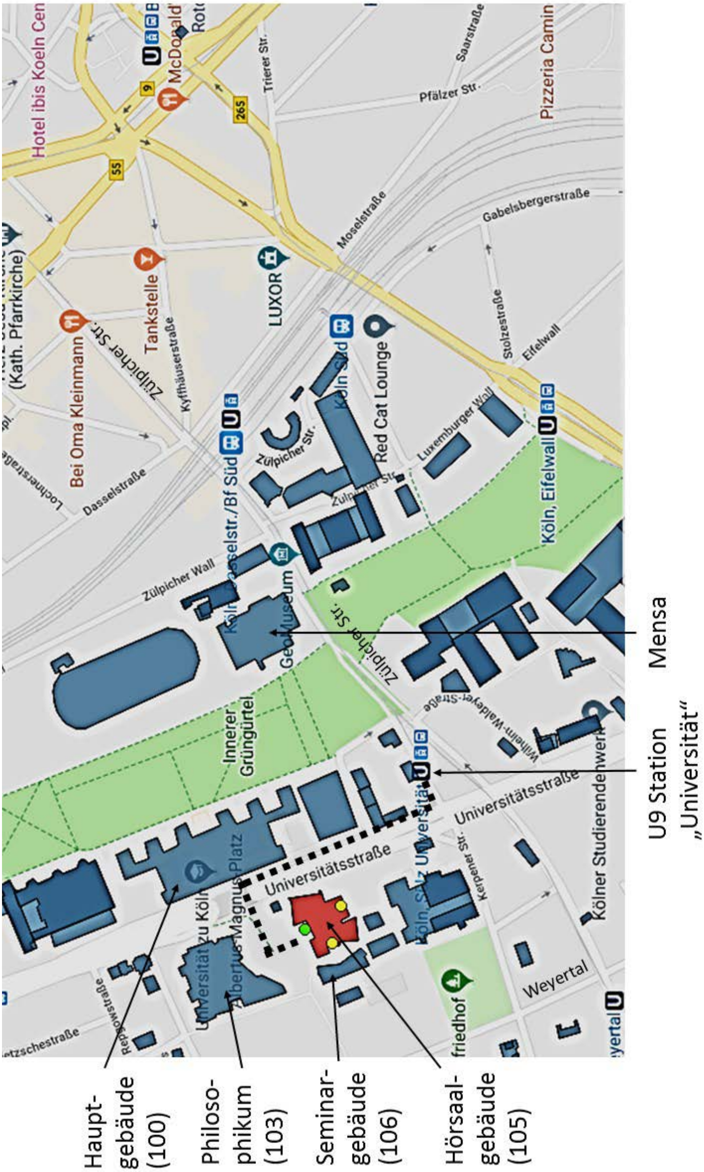
On behalf of the local organising committee I would like to welcome you in Cologne to the third GWP-conference. We are very happy to have what promises to be an exciting programme that covers a wide range of topics in the philosophy of science.

Let me take the opportunity to thank the GWP organizers Gerhard Schurz, Uljana Feest, Christian Feldbacher-Escamilla, Alexander Gebharter and Thomas Reydon as well as the members of the local organizing committee, in particular Ursula Heister, Michael Hicks, Jan Köster, Liane Lofink, Elisabeth Muchka and Martin Voggenauer.

I hope you enjoy the conference and encounter many inspiring papers and discussions.

Andreas Hüttemann
Local Organizer, Cologne

Maps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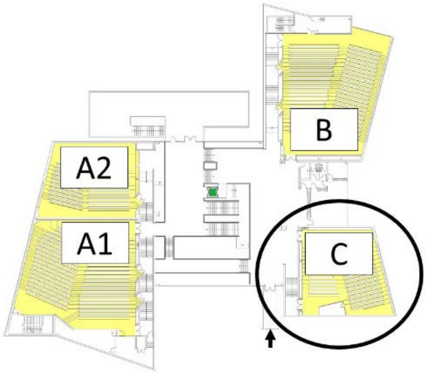
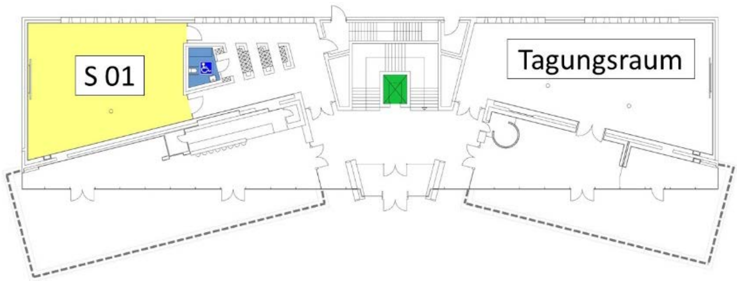
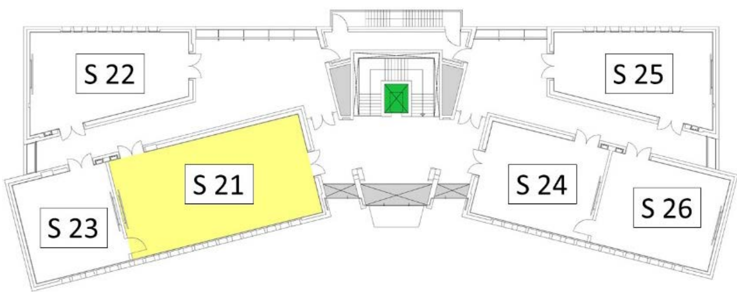


Hörsaalgebäude (105), 1st floor



Seminargebäude (106), ground floor



Seminargebäude (106), 2nd floor

Programme Overview

MONDAY, Feb 25th

08.00 – 09.00	Registration
09.00 – 09.15	Opening (Lecture Hall C, 105)
09.15 – 10.45	Plenary Lecture I: Kärin Nickelsen (Lecture Hall C; 105)
10.45 – 11.00	Refreshments
11.00 – 13.00	Parallel Sessions I (Rooms 22, 23, 24, 25 and 26; 106)
13.00 – 14.30	Lunch Break
14.30 – 17.30	Parallel Sessions II (Rooms 22, 23, 24, 25 26 and Tagungsraum; 105)
17.10 – 17.30	Refreshments
17.30 – 19.00	Plenary Lecture II: C. Kenneth Waters (Lecture Hall C; 105)
19.15 -	Reception (Foyer of Lecture Hall C; 105)

TUESDAY, Feb 26th

09.00 – 10.30	Plenary Lecture 3: Erik J. Olsson (Lecture Hall C; 105)
10.30 – 11.00	Refreshments
11.00 – 13.00	Parallel Sessions 3 (Rooms 22, 23, 24, 25 and 26; 106)
13.00 – 14.30	Lunch Break
14.30 – 17.10	Parallel Sessions 4 (Rooms 22, 23, 24, 25 26 and Tagungsraum; 106)
17.10 – 17.30	Refreshments
17.30 – 19.00	Plenary Lecture 4: Katherine Hawley (Lecture Hall C; 105)
19.15 -	General Assembly (Lecture Hall C; 105)

WEDNESDAY, Feb 27th

09.00 – 10.30	Plenary Lecture 5: Martin Carrier (Lecture Hall C; 105)
10.30 – 11.00	Refreshments
11.00 – 13.00	Parallel Sessions 5 (Rooms 22, 23, 24, 25 and 26; 106)
13.00 – 14.30	Lunch Break
14.30 – 17.10	Parallel Sessions 6 (Rooms 22, 23, 24, 25 26 and Tagungsraum; 106)
17.10 – 17.30	Refreshments
17.30 – 19.00	Plenary Lecture 6: Michael Strevens (Lecture Hall C; 105)
19.00 – 19.15	Closing (Lecture Hall C; 105)

Monday, February 25						
08.00 – 09.00	Registration					
	Opening, Lecture Hall C (Hörsaalgebäude #105)					
09.00 – 09.15	Gerhard Schurz (GWP President) Andreas Hüttemann (Chair of the LOC)					
	Plenary Lecture, Lecture Hall C (Hörsaalgebäude #105) (Chair: Andreas Hüttemann)					
09.15 – 10.45	**Kärin Nickelsen: Interactions and Interdependencies: Philosophy of Science and History of Science as Friends with Benefits (or more)**					
10.45 – 11.00	**Refreshments**					
	Seminargebäude #106					
	Room 22	**Room 23**	**Room 24**	**Room 25**	**Room 26**	**Tagungsraum**
	Symposium: Referencing in the Quantum Domain (Chair: Cord Friebe)	Symposium: Good Concepts (Chair: Joe Dewhurst)	Section: Evolution and Identity (Chair: Thomas Reydon)	Section: Non-Causal Explanation (Chair: Enno Fischer)	Section: Causation and Kinds (Chair: Beate Krickel)	
11.00 – 11.40	Fred A. Muller and Gijs Leegwater: The Case Against Factor-ism	David Hommen: Family Resem-blances and Essentialism	Anne Sophie Meincke: One or Two? A process per-spective on pregnant indi-viduals	Vera Hoffmann-Kolss: Interven-tionism and Non-Causal Dependence Relations: New work for a theo-ry of superveni-ence	Florian Fischer and Alexander Gebharter: Dispositions and Causal Bayes Nets	

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
11.40 – 12.20	Tina Wachter: Can Conventionalism Save the Identity of Indiscernibles?	Paul Thorn: Class Selection in Inheritance Inference	Rose Trappes: What Fish is This? Process ontology and biological identity	Daniel Kostic: Non-Causal Explanatory Asymmetries	Yukinori Onishi and Davide Serpico: Is Everything Fine if Natural Kinds are Nodes in Causal Networks?	
12.20 – 13.00	Adam Caulton: Effective Reference to Quantum Particles	Henk Zeevat and Corina Strößner: Natural Concepts in a Brain-Based Feature System	Maria Kronfeldner: Digging the Cannels: On how to separate nature and culture	Hugh Desmond: Shades of Grey: Granularity, pragmatics, and non-causal explanation	Philipp Haueis: Towards a Generalized Patchwork Approach of Scientific Concepts	
13.00 – 14.30	**Lunch Break**					
	Seminargebäude #106					
	Section: Explanation and Evolution (Chair:Simon Lohse)	Section: Quantum Mechanics (Chair: Kian Salimkhani)	Section: Realism (Chair: Olivier Sartenaer)	Section: Psychology (Hajo Greif)	Section: Causation (Chair: Christian J. Feldbacher-Escamilla)	Section: Biomedicine (Chair: Anne Sophie Meincke)

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
14.30 – 15.10	Antonio Danese: Exaptation: From Darwin's "botany" to evolutionary psychology	Andrea Oldofredi: Particle Identification through Time of Flight Measurements: Testing Bell's hypothesis on position observations in quantum physics	Mark Fischer: Pluralism and Relativism from the Perspective of Significance in Practice	Lena Kästner: Network Explanations in Psychiatry: Interventions and Causal Relations?	Enno Fischer: Causes, Interventions, and Responsibility	François Pellet: Disease as Essence Destruction: The Case of (Lung) Cancer
15.10 – 15.50	Walter Veit: How Evolutionary Game Theory Explains	Frida Trotter: Observables in Quantum Mechanics. An impasse for Bogen and Woodward's account of science?	Samuel Kahn: Revitalizing Realism	Anke Bueter: Epistemic Injustice and Psychiatric Classification	Dennis Graemer, Frenzis Scheffels and Alexander Gebharter: How to Establish Backward Causation on Empirically Grounds: An interventionist approach	Ludger Jansen: Dispositions in Biomedical Ontologies

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
15.50 – 16.30	Thomas Reydon: How Far do Evolutionary Explanations Reach? On the application of evolutionary explanations to explain non-biological phenomena	Stephan Fischer: Schrödinger's Glass – Opposing dispositions co-instantiated	Aimen Remida: What Kind of Realism – if any – is Whitehead's Organic Realism?	Julia Pfeiff: Relations between psychotherapeutic practice and models of mental disorders	Beate Krickel: Activity Causation	Sabine Baier: Discovery Narratives: Managing Epistemic Distances In Drug Discovery
16.30 – 17.10	Alexander Krauss: How our mind enables and constrains the scientific theories we formulate	Marij van Strien: David Bohm and Paul Feyerabend: Dissenting Positions in Quantum Physics and Philosophy	Ludwig Fahrbach: The No-miracles Argument is Not an Inference to the Best Explanation	Bojana Grujicic: Against mechanistic imperialism in the domain of psychology	Mariusz Maziarz: What is the Meaning of Causal Economic Claims?	Anja Pichl: Stem Cell Concepts: Broadening the scope of philosophy of science debate
17.10 – 17.30	**Refreshments**					
	Plenary Lecture, Lecture Hall C (Hörsaalgebäude #105) (Chair: Christian J. Feldbacher-Escamilla)					
17.30 – 19.00	**C. Kenneth Waters: Scientific Metaphysics of Hierarchy**					
19.15 -	**Reception, Foyer of Lecture Hall C (Hörsaalgebäude #105)**					

Tuesday, February 26						
Plenary Lecture, Lecture Hall C (Hörsaalgebäude #105) (Chair: Gerhard Schurz)						
09.00 – 10.30	**Erik J. Olsson: Explicationist Epistemology and Epistemic Pluralism**					
10.30 - 11.00	**Refreshments**					
	Seminargebäude #106					
	Room 22	**Room 23**	**Room 24**	**Room 25**	**Room 26**	**Tagungsraum**
	Symposium: Models in High Energy Physics (Chair: Cord Friebe)	Symposium: Individuality and Individuation in the Life Sciences (Chair: Idit Chikurel)	Symposium (GAP-GWP): Deep Learning and the Philosophy of Artificial Intelligence (Chair: Holger Lyre)	Geneology, Biology, and Race (Chair: Mario Santos-Sousa)	Scientific Progress (Chair: Mark Fischer)	Scientific Inference (Chair: Daria Jadreškić)
11.00 – 11.40	Cristin Chall: Model-groups as Scientific Research Programs	Hannah O'Riain: Towards a Process Ontology of Pregnancy: links to the individuality debate	Cameron Buckner: Empiricism without Magic – Transformational abstraction in Deep Convolutional Neural Networks	Michael Koerner: Genealogy as a Scientific System of Order	Catherine Herfeld: Crossing Domains: The Role of the translator in the spread of scientific innovations	Jorge Luis García Rodríguez: A Naturalized Globally Convergent Solution to Goodman's Paradox

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
11.40 – 12.20	Martin King: Explanation and the Rise of Model Independence	Ozan Altan Altinok: Hologenome Versus Holobiont: A Way to Extend Individuality in Vertebrates	Hajo Greif: On not Opening the Black Box. Transparency, Opacity, and the Pragmatics of Artificial Intelligence	Kamuran Osmanoglu: Against Phylogenetic Conceptions of Race	Chrysostomos Mantzavinos: Institutions and Scientific Progress	Tobias Henschen: How strong is the argument from inductive risk?
12.20 – 13.00	Florian Boge: Semi-Hierarchies and Networks: How Simulation Models at ATLAS Interrelate	Nina Kranke: Individuation Practices in Studies of Host-Parasite Systems	Carlos Zednik: The Black Box Problem and the Norms of Explainable AI	Anna Klassen: Methodological Signatures in Early Ethology and the Recent Problem of Qualitative Terminology	Geoffrey Blumenthal: Using Systematicity for Analysing how a Special Science Progresses	Karim Bschir: Unblackswaning Scientific Prediction
13.00 – 14.30	**Lunch Break**					

	Seminargebäude #106					
	Room 22	**Room 23**	**Room 24**	**Room 25**	**Room 26**	**Tagungsraum**
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15.10 – 15.50	Niels Martens and Dennis Lehmkuhl: Dark Matter = Modified Gravity? Scrutinising the spacetime-matter distinction through the modified gravity/ dark matter lens	Petter Sandstad: A Re-evaluation of E. J. Lowe's Account of Laws of Nature		Alexander Gebharter: A Causal Bayes Net Analysis of Glennan's Mechanistic Account of Higher-level Causation	Christoph Merdes: Moral Modeling	Meinard Kuhlmann: On the Exploratory Function of Agent-Based Modelling

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
15.50 – 16.30	Rico Gutschmidt: Reduction and Neighboring Theories. A new classification of the inter-theoretic relations in physics	Olivier Sartenaer: Humeanism, Best System Laws, and Emergence	Maria Forsberg: Patchy endorsements and explanatory depth	Christian J. Feldbacher-Escamilla: Simplicity in Abductive Inference	Charles Lowe: The Consequences of Consequentialism for Values and Science	Rui Maia: What is a model-narrative?
16.30 – 17.10	David Hyder: Kant and Einstein on the Causal Order of Time	Patryk Dziurosz-Serafinowicz: Justifying Lewis's Kinematics of Chance	Viorel Pâslaru: Descriptions for Explanation and Prediction of Conserved and Variable Mechanisms	Alexander Reutlinger: Objectivity as Independence	Silvia Ivani, Matteo Colombo and Leandra Bucher: Uncertainty in Science: A Study on the Role of Non-Cognitive Values in the Assessment of Inductive Risk	Paul Hoyningen-Huene: A constructive critique of Sugden's view economic models
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	Plenary Lecture, Lecture Hall C (Hörsaalgebäude #105) (Chair: Uljana Feest)					
17.30 – 19.00	**Katherine Hawley: Who Speaks for Science? (de Gruyter Lecture)**					
19.15 -	**General Assembly, Lecture Hall C (Hörsaalgebäude #105)**					

Wednesday, February 27						
Plenary Lecture, Lecture Hall C (Hörsaalgebäude #105) (Chair: Andreas Hüttemann)						
09.00 – 10.30	**Martin Carrier: How does Good Science-Based Advice to Politics Look Like? (Springer Lecture)**					
10.30 – 11.00	**Refreshments**					
Seminargebäude #106						
	Room 22	**Room 23**	**Room 24**	**Room 25**	**Room 26**	**Tagungsraum**
	Symposium: Modality in Physics (Chair: Meinard Kuhlmann)	Biological Modeling (Chair: Anna Klassen)	History (Chair: Alexander Christian)	Emergence and Interdisciplinary Science (Chair: Petter Sandstad)	Cognitive Science II (Chair: Cameron Buckner)	
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11.40 – 12.20	Andreas Bartels: Metaphysical and Physical Possibilities: How they relate and why we	Martin Zach: Idealization and Understanding with Diagrammatic Biological Models	Idit Chikurel: Maimon on Scientific Genius	Simon Lohse: Social Emergence and Unpredictability	Ori Hacohen: Representations in Cognitive Science: An argument against naturali-	

	need them				zation	

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
12.20 – 13.00	Kian Salimkhani: How Physical Practice Employs the 'Physical Possible'	Marcel Weber: From Theory Reduction and Reductive Explanation to Inter-level Scientific Practices: The Spemann-Mangold organizer and molecular developmental biology	Alan Park: Rhetorics of Empiricism and Disciplinary Purity: Alchemy and "protochemistry" in enlightenment Germany		Joe Dewhurst: Pluralistic Ontologies and Perspectival Mechanisms in Cognitive Neuroscience	
13.00 – 14.30	**Lunch Break**					
	Seminargebäude #106					
	<u>Symposium:</u> The Role of Empirical Methods in Philosophy of Science (Chair: Rico Gutschmidt)	Evidence in Medicine (Chair: Ludger Jansen)	Valuable Inferences (Chair: Chrysostomos Mantzavinos)	Maths (Chair: Corina Strößner)	Science and Public Policy (Chair: Ozan Altan Altinok)	

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
14.30 – 15.10	Miles MacLeod: Meeting in the Middle: Adapting Qualitative Methods to Philosophical Questions	Barbara Osimani: Varieties of Error and Varieties of Evidence in Scientific Inference	Nancy Abigail Nuñez Hernández & Francisco Hernández Quiroz: Computational Complexity as Evidence for the Epistemic Value of Deduction	Nicholas Danne: Mathematical Realism from Color Objectivism	Benedikt Knüsel: Understanding Climate Change with Process-Based and Data-Driven Models	
15.10 – 15.50	Nora Hangel: Benefits and Limitations of Including Scientists' Accounts in Philosophical Analysis	Saana Jukola: On Evidentiary Standards for Dietary Advice	David Botting: The Value Problem of A priori Knowledge	Deniz Sarikaya: Axiomatization as an Act of Mathematics Studies: Or the marvelick tradition and formalized mathematical theories.	Meghan Page: When Glaciers Prophesy: Building a case for predictive historical science	
15.50 – 16.30	Dunja Šešelja: Using Agent-based Models to Explain Scientific Inquiry: current limitations and future prospects	Alexander Christian: Disambiguating Scientific Disagreement	Daria Jadreškić: Time-sensitivity in Science	Peter P. Kirschenmann and Henk de Regt: On the Reasonable Effectiveness of Math. In Science	David Hopf: The Relevance and Weight of Scientific Evidence in Policy Decisions	

	Room 22	Room 23	Room 24	Room 25	Room 26	Tagungsraum
16.30 – 17.10				Mario Santos-Sousa: Grounding Numerals	Simon Friederich: Is Deploying Nuclear Power Unethical?	
17.10 – 17.30	**Refreshments**					
	Plenary Lecture, Lecture Hall C (Hörsaalgebäude #105) (Chair: Alexander Gebharter)					
17.30 – 19.00	**Michael Strevens: Necessity in Scientific Explanation**					
19.00 – 19.15	**Closing, Lecture Hall C (Hörsaalgebäude #105)**					

Abstracts

Plenary Lectures

Plenary Lecture I	**Plenary Lecture**
Chair: Andreas Hüttemann	Lecture Hall C, MONDAY 09.15 – 10.45

Interactions and Interdependencies: Philosophy of Science and History of Science as Friends with Benefits (or more)

Kärin Nickelsen
LMU Munich

This paper discusses the relationship between the philosophy and the history of science from a historian's point of view. I will give an overview of how the two fields and their interrelations have changed over time; and then carve out some of the more salient differences that render collaboration so difficult. These include analyticity vs. historicity, the normative vs. the descriptive; and divergent views of the meaning of context, scope and generality.
Despite these differences, I argue – in line with other HPS enthusiasts – that it is in the interest of both disciplines that the conversation is continued and, if possible, intensified: historians ought to sharpen their argument by using philosophical tools, while philosophers need to be constantly reminded of the complexity of science and the contingency of its development. One of the issues that would especially benefit from a more intimate relationship of the two subjects is the elaboration of a social epistemology that moves beyond trust and testimony. I will flesh these claims out at examples from the history and philosophy of biological sciences.

Plenary Lecture II **Plenary Lecture**
Chair: Christian J. Feldbacher-Escamilla Lecture Hall C, MONDAY 17.30 – 19.00

Scientific Metaphysics of Hierarchy

C. Kenneth Waters
University of Calgary

Scientists and philosophers generally assume that the world is structured in levels, organized in hierarchical fashion. This idea is exemplified in the biological sciences where systems are understood to be organized into levels, for example into the levels of macromolecules, cells, multicellular organisms, populations, and communities. But the biological world is structured by a multiplicity of hierarchies in addition to the one mentioned here. For example, the hierarchy of taxonomic levels and the hierarchy of trophic levels. I will begin my talk by introducing a pragmatic conception of scientific metaphysics, which will motivate the idea that biological sciences should be at the center of metaphysicians' attention. I will analyse the idea of hierarchical levels in the context of biological sciences. I will use this analysis to argue against the common assumption that there is a grand, hierarchical organization of nature. The world has lots of hierarchical structures, but no overall hierarchical structure.

Plenary Lecture III **Plenary Lecture**
Chair: Gerhard Schurz Lecture Hall C, TUESDAY 09.00 – 10.30

Explicationist Epistemology and Epistemic Pluralism

Erik J. Olsson
Lund University

I discuss Carnap's method of explication with special emphasis on its application to epistemology. I observe that explication has the advantage over conceptual analysis of not being vulnerable to the so-called paradox of analysis. Moreover, explicationist epistemology is intrinsically immune to the Gettier problem. I proceed to identify three senses in which the former is inherently pluralistic. For example, it allows for a plurality of legitimate and potentially interesting epistemological projects. Finally, I argue that while there are salient affinities with Alston's theory of epistemic desiderata, beyond a far-reaching commitment to pluralism, there are also important differences. Above all, Carnap's methodological outlook is reconstructive in ways in which Alston's is not.

Plenary Lecture IV | **Plenary Lecture**
Chair: Uljana Feest | Lecture Hall C, TUESDAY 17.30 – 19.00

Who Speaks for Science?

Katherine Hawley
University of St. Andrews

Science news is often reported in the media using formulations like 'Scientists say….', 'Scientists have discovered…', or 'Scientists find that…' In this talk I will use various philosophical tools to explore the implications of this journalistic habit. First, what does it mean that the superficial subject matter is scientists, rather than galaxies, genes, or glaciers? How does the reporter thereby borrow authority whilst maintaining some distance? Second, what is the function of bare plurals like 'scientists' in such reports and headlines? I will draw on recent work on bare plurals and generics by feminist philosophers and others interested in how we speak about social groups, to discuss how terms like 'scientists' may carry presuppositions about consensus and collective knowledge.

Plenary Lecture V **Plenary Lecture**
Chair: Andreas Hüttemann Lecture Hall C, WEDNESDAY
09.00 – 10.30

How does Good Science-Based Advice to Politics Look Like?

Martin Carrier
University of Bielefeld

Scientific policy advice is often criticized as being based on one-sided studies that are driven by economic interests and political missions. The prima-facie conclusion is that influences originating in the social arena may spoil the epistemic basis of such policy advice. By contrast, I argue that the point is not to expel non-epistemic values but rather to keep them separate from facts. The fact-value distinction should be used as a critical tool. The risk of bias can be avoided by drawing up alternative policy scenarios which invoke different socio-economic preferences. I support this claim by arguing that no justified distinction can be drawn between those preferences that support the knowledge-seeking character of science (such as feminist values) and other preferences that act counter to the epistemic endeavor of science (such as commercial values). This entails that a plurality of non-epistemic values should be invoked in scientific policy advice. Accordingly, scientists should take the courage to conceive alternative courses of action and to broaden the range for social choice.

Plenary Lecture VI
Chair: Alexander Gebharter

Plenary Lecture
Lecture Hall C, WEDNESDAY
17.30 – 19.00

Necessity in Scientific Explanation

Michael Strevens
New York University

I will examine various uses of necessity, in an objective or metaphysical sense, in scientific explanation. Many -- even appeals to logical and mathematical necessity -- can be subsumed under a sufficiently broad-minded causal approach to explanation. At least one, however, connected to the explanation of laws of nature, cannot: it constitutes an important species of distinctly metaphysical explanation in everyday scientific explanatory practice.

Symposia & Contributed Papers

Hologenome versus Holobiont: A Way to Extend Individuality in Vertebrates

Tuesday, February 26, 11.40 – 12.20
Room 23, Seminargebäude 106

Ozan Altan Altinok
WWU Münster

Currently, notions of biological individuality are drifting away from essentialism and towards pluralism. This is true for individuation of species, organisms, and many other biological entities (Kovaka, 2015). Along with this move to pluralism, notions of individuality have taken a pragmatic turn, with different definitions of individuality serving in different contexts, according to various research interests (Dupré, 1993).

In this discussion I will focus on the holobiont and hologenome concepts of evolutionary individuality, and I will argue that especially in vertebrates, hologenome approaches are more useful for evolutionary biology than the approaches centered around holobiont in the case of evolutionary individuals, as well as being more useful to biologically informed ethics.

Facing up the Problem of Scientific Idealization

Tuesday, February 26, 14.30 – 15.10
Room 24, Seminargebäude 106

Mustafa Efe Ates
M.S.K.U.

Mehmet Elgin and Elliott Sober argue that "the idealizations in a causal model are harmless if correcting them wouldn't make much difference in the predicted value of the effect variable" (Elgin & Sober 2002, p. 448). To support this view, they use a case study of optimality models in evolutionary biology and eventually claim that models can explain despite containing idealizations (like infinite populations). Similarly, Michael Strevens (2009) asserts that models can explain by causal factors that make difference to the explanandum. Given this view, the idealized parts of models (like the molecular volumes) are explanatory irrelevant and do not play a role in making difference to the occurrence of a phenomenon to be explained.

In general, we are able to estimate (or partially know) to what extent the correction of an idealization will effect on model's explanation and prediction. For example, in evolutionary optimality models, we estimate that finite population would have an influence on allele frequencies of organisms. For this reason, we falsely assume infinite populations in which non-selective forces (like genetic drift) are relatively weak. So, if we assume that populations are infinite, the sample size would be expanded and that would allow us to avoid the effect of drift. Take for example, the idealized model that explains Boyle's Law. We know beforehand that molecules' volume and intermolecular forces between each of them have causal influences on the behavior of gases or the distribution of energy. However, we estimate that these influences would be extremely small. It is because the smallness of molecules and weakness of forces give a clue about this, way before de-idealization.

The holders of both views emphasize the idea that, whether correcting or not, this type of idealizations do not make much difference in model's predictive outcome, or they do not make difference to the explanandum. In this manner, both views demonstrate -as expected- that particular false assumptions (the idealizations that were already stated to be causally less relevant) would not make much difference to the model's explanation and prediction. In doing so, they purport to establish how this type of idealizations are harmless or how they are explanatory irrelevant. These views are true of some typical models in science, that truly by emphasizing the worthlessness of correcting some idealizations. However, they fail to account for some particular idealizations which are contained in our successful scientific models. As I shall introduce by the help of a case study, the epistemic relevance of this type of idealizations is unknown to the modeler. In other words, the information that the causal influence of the idealized factor is far from being estimated by the modeler. The example I will be using is a mathematical model of insolation, which is proposed by Milutin Milanković. More specifically, my main focus will be to show how Milanković succeeded to explain and predict glacial/inter-glacial periods by correcting the idealized albedo effect (the reflective power of snow) which he had no idea whether it is explanatory relevant or not. To put it differently, he not only showed that the idealized factor (albedo) is harmless, but also showed why it is harmless for the model's explanatory and predictive outcome as well.

Discovery Narratives: Managing epistemic distances in drug discovery

Monday, February 25, 15.50 – 16.30
Tagungsraum, Seminargebäude 106

Sabine Baier
LSE London

Among the application-oriented sciences, hardly another endeavor is as disconnected in time from its possible outcomes and findings as target-based drug discovery. Developing a new drug takes up 10 to 12 years on average and navigating the vast chemical space throughout this tedious process in order to come up with a potentially successful new drug seems to be almost infeasible.

What makes it so difficult among a variety of reasons is also the fact that the desirable and undesirable medical effects of a new chemical compound in humans usually become apparent much later within the process of drug discovery. This epistemic distance, as I call it in my paper, complicates and impedes everyday decision making for the chemists: What looks good in the test tube simply does not necessarily work later on for laboratory animals or for humans and yet, decisions have to be made based on these later effects very early on. Particularly, if we are focusing on the stage of early molecular development in the beginning of the drug discovery process – where chemists in their laboratories sample a broad selection of different compounds in order to tackle the biochemical target in question – it becomes clear that barely any decision-making tools, theories and techniques are available to bridge the epistemic distance between the need for everyday decision making in the laboratory and future outcomes of clinical tests. As a result, the questions need to be raised of how this epistemic distance can be managed nonetheless and how the chemists are able to justify their actions even though they are lacking any sort of hard proof?

Based on the findings of my field studies within the research and development laboratories of Hoffmann-La Roche AG and Novartis AG in Basel, Switzerland, I therefore argue in my paper that by deploying carefully crafted discovery narratives, the laboratory heads are decreasing the epistemic distance and therefore manage to both maintain and even to increase their decision-making capacities. In my talk, I will not only describe what kind of discovery narratives are developed by the chemists but also how these discovery narratives function as valid heuristic tools within the process of drug discovery.

Metaphysical and Physical Possibilities: How they relate and why we need them

Wednesday, February 27, 11.40 – 12.20
Room 22, Seminargebäude 106

Andreas Bartels
University of Bonn

Physics is not only about matters of fact. Physical theories and models aim at describing the actual world, but they are also means of uncovering physical possibilities – in contrast to merely logical or mathematical possibilities. The pursuit of physical possibilities is not something of secondary importance compared to the search for the correct description of the actual physical universe. To the contrary, it is a precondition for the latter to make any progress that a range of physical possibilities has been determined at first place out of which representations of the actual world may be selected by empirical means.

A physically possible model is distinct from a merely mathematically possible one by fulfilling some essential background principles of contemporary physics that can be seen as preconditions for any successful physical research. Examples for such principles are energy conditions (see Curiel 2014) and causality conditions (see Curiel 2015) in General Relativity. The weak energy condition, for instance, is the requirement that “for all physically reasonable classical matter [. . .] energy density is nonnegative” (cf. Wald 1984, p. 218), whereas the strong energy condition holds that gravity is attractive (Wald, 1984, p. 220). Another condition that is universally applied in contemporary physics is the requirement of local Lorentz invariance. Those principles do not arise from some particular theory, and they cannot be seen as ‘laws of nature’. Instead, they comprise general physical knowledge that is used in order to apply theories to physical reality. They are generalizations that rest on inductive inferences from empirical evidence. In a way, they transfer empirical knowledge of the actual world in order to narrow the range of mathematical possibilities to the physically ‘nearby’ worlds. Physical possibilities appear not only as global, but also as local structures. Such local structures count as physically possible if they can be smoothly embedded into a physically possible (global) model. Beside the application of theories, there is an- other methodological domain where physical possibilities come to the fore. This domain is theory-extension, as it arises for instance in accounts of quantum gravity basing gravity on more fundamental quantum entities. In such cases, some formerly held methodologically ‘necessary’ structures (in that case the Riemannian structure of space-time) are shifted into the realm of mere physical possibilities which can ‘emerge’ only if some additional contingent conditions apply. Are physical possibilities just epistemic modalities, or can (or even must) a metaphysically modal status be attached to them? This physics cannot tell. What can be asserted on the basis of physics methodology, is that considering physical possibilities is some indispensable element of the practice of applying physical theories to reality and of theory-extension, and that inductive inferences from empirical evidence is an essential means of determining the range of such possibilities.

Using Systematicity for Analysing how a Special Science Progresses

Tuesday, February 26, 12.20 – 13.00
Room 26, Seminargebäude 106

Geoffrey Blumenthal
University of Bristol

This paper is a case study in the application and analysis of the set of criteria that has been collected under the term ‘systematicity’ by Paul Hoyningen-Huene (2013). While he proposes that the criteria enable the discussion of the difference between scientific knowledge and other forms of knowledge, especially everyday knowledge, this paper argues that the criteria are even more useful in identifying overall distinctions between relatively good and poor work within a particular special science.

Semi-Hierarchies and Networks: How Simulation Models at ATLAS Interrelate

Tuesday, February 26, 12.20 – 13.00
Room 22, Seminargebäude 106

Florian Boge
RWTH Aachen University

The ATLAS experiment at CERNs large hadron collider (LHC) is one of the largest collaborative efforts ever attempted in science. In this large scale experiment, a lot of effort is needed to extract the delicate data indicating the existence and properties of elementary particles. Part of this effort is carried by simulating investigated processes and detector responses and subjecting them to the usual reconstruction methods, in order to design and calibrate experimental procedures and improve on experimental errors. From a philosophy of science-point of view, the following questions arise:

What prior knowledge are these simulations based on? Which contributions stem from theory, which from previous experiment? Which ones are mostly practical in origin? In this paper, we will approach these issues in two steps. First, we suggest a classification of the simulation models used by ATLAS along two dimensions: (a) their generation and (b) their functioning. It will be shown that the dependencies on theory, experiment, and practical considerations will radically differ depending on which part of an overall experiment one simulates. Based on this first step, we will then map out the complex relations into which individual simulation models enter at ATLAS. More precisely, we will here proceed in two further steps: first, we dispute the applicability of traditional accounts of simulation models as ordered into hierarchies (e.g. Winsberg 1999, in turn inspired by Suppes 1962, Mayo, 1996, and Harris 1999) and suggest that they are rather ordered into more egalitarian structures that we call semi-hierarchies, meaning that there are distinct components to the modeling process that can figure on the same level. We will then show that there are multiple junctions to each semi-hierarchy at which they are influenced by other semi-hierarchies from the overall simulation infrastructure, making the
total structure a network of models.

Finally, this proposal will be compared to a recent, similar one by Karaca (2018), who equally suggests that there is a network of models in the context of simulation in high energy physics. It will be shown that the network we identify is embedded into Karacas, making the former an internal network of models, the latter an external one.

The Value Problem of A priori Knowledge

Wednesday, February 27, 15.10 – 15.50
Room 24, Seminargebäude 106

David Botting

This is a paper about the value problem of knowledge: to explain why it is that knowledge is taken to have a value that mere true belief does not. What extra value does being justified or being produced by a reliable process give to the true belief? It is a condition of adequacy on any theory of knowledge, whether externalist or internalist, that it be able to provide an answer to this question, and failure of a theory to answer it is tantamount to a refutation of that theory; for example, reliabilist theories are held to be inadequate because the only answer they can give to this question – that is to say, the only value they can give to the property of a belief's being reliably produced – is the instrumental one that such reliable belief-forming processes are more likely to have produced a truth than a falsehood, but such does not seem to be a value for a belief whose truth has already been granted. Any theory that gives to the knowledge-making property an instrumental value whose final value is truth-conduciveness will be vulnerable to this "swamping problem."

I am going to deny the intuitions driving the value problem. Following a thought experiment invented by Anne Meyland, I will compare the cases of a Competent Omniscient and a Lucky Omniscient: both have exactly the same beliefs but the Lucky Omniscient has them by luck whereas the Competent Omniscient knows. Meylan's argument is: the reliabilist cannot ground the extra value of reliability on truths that the reliable process leads to but which the Lucky Omniscient does not have, because there are no such beliefs. The reliabilist could say that this is just a particular occasion where knowledge does not have a greater value than true belief, but this conflicts with Meylan's intuition that it does have a greater value.

However, I am going to look at our intuitions when comparing these cases in detail and claim that when the correct contrast classes are set out, the Competent Omniscient is not better than the Lucky Omniscient, and will deduce from this that knowledge does not have the value that the driving intuition supposes. My strategy is this: break knowledge down into its different kinds and ask whether a Competent Omniscient who had all the knowledge possible of that kind is better than a Lucky Omniscient who has all the true beliefs possible of that kind. If there is no difference in value between the two, then for that kind of knowledge, knowledge is not more valuable than true belief, even in the ordinary case. I find no difference in value in the cases of perceptual knowledge and inferred knowledge, and so I deny that the value problem is a condition of adequacy on theories of perceptual or inferred knowledge. The only kind of knowledge in which I find a difference is in a priori knowledge, which does have a value that true belief does not, and that value is infallibility.

Revisionary Reductionism and the Classification of Mental Disorders

Wednesday, February 27, 11.00 – 11.40
Room 26, Seminargebäude 106

Inti Brazil, Marko Jurjako (University of Rijeka), Luca Malatesti

Conceptualisations of mental disorders assign different roles to biological genetic or neural factors in the categorisation of these conditions. Syndrome based accounts, that inform many diagnoses in classificatory systems such as the DSM (APA 2013) or the ICD (WHO 1992), categorise mental disorders in terms of symptomatic behaviours and mental states and personality traits. In these accounts, thus, the identity of a certain mental disorder does not depend on its neural or other biological aetiology or correlates. Proposals for biological and neurocognitive (for short biocognitive) based classification of mental disorders aim, instead, at grounding the categorization of mental disorders on genetic, neurological, or neurocomputational mechanisms. The Research Domain Criteria (RDoC) is a notable example of this proposal (see e.g. Insel and Cuthbert 2015; Lilienfeld 2014).
The network approach to mental disorders is a recent proposal that offers a more nuanced view on the role that biological factors

should have in the conceptualisation of mental disorders (Borsboom 2017). The core assumption of this account is that mental disorders should be conceptualised as networks of causally interacting symptoms. Denny Borsboom, and colleagues (Borsboom, Cramer, and Kalis 2018), argue that this approach is incompatible with a reductionist characterisation of mental disorders as "brain disorders" and, more than that, it shows why this type of reductionism is untenable. Although they are keen to assign some explanatory role to biological factors within their account, they think that causal connections between behaviourally individuated symptoms, inferred mental states, and personality traits are fundamental for the classification of mental disorders.

In this paper, without considering whether the network approach is correct, we investigate, from a philosophical perspective, the role that biological factors should have in it. Our main line of reasoning is that Borsboom et al. do not recognise that difficulties in the integration of biological and neurological information in the classification of mental disorders, as they are currently conceptualized in DSM 5 or ICD 10, is also due to the heterogeneity of those categories of mental disorders and associated symptoms. It seems that they exclude without reason a significant role that biological factors should have within their proposal.

We think that such a role could be spelled out by means of a plausible interpretation of the current biocognitive-based attempts at classification of mental disorders. Borsboom et al. appear to interpret some eminent instances of these attempts (e.g., Insel and Cuthbert 2015) as endorsement of the type of explanatory reductionism that they criticise. However, we think that there are interpretative grounds and, more importantly, theoretical reasons for thinking that these attempts might be underpinned by what we call revisionary reductionism. Revisionary reductionism is the view that current syndrome-based classifications of disorders, as those codified in the diagnoses in DSMs and ICDs, and those involved in the network approach could be revised or partly or completely replaced by individuating, amongst individuals that satisfy them, cognitive, genetic, neurobiological and even behavioural differences that might enable better treatment, prediction and explanation.

The Causal-Mechanical Explanation without Decomposition: The case of orphan genes

Wednesday, February 27, 11.00 – 11.40
Room 23, Seminargebäude 106

Zdenka Brzović (University of Rijeka)
Predrag Šustar (University of Rijeka)

In this paper, we focus on the structure of explanation in molecular biology, more specifically on the mainstream account of scientific explanation in the philosophy of molecular biology, the causal-mechanical (CM). This account is prone to different types of error that blur the distinction between acceptable and bad scientific explanations. Franklin-Hall (2016) gives a detailed assessment of the "standards" or "explanatory constraints", which should prevent CM from committing errors and single out acceptable mechanistic explanations (see Franklin-Hall (2016), 47-48). The explanatory constraints for a warranted CM account are (i) the causal constraint, i.e., the part of the account explicating the causal interactions among component-parts in a mechanism producing the corresponding biological phenomenon; (ii) the carving constraint, i.e., acceptable explanatory models should partition mechanisms "at their joints" or explanatorily relevant component-parts. Finally, (iii) the levels constraint which states that the explanandum phenomenon is explained by referring to an explanans at the appropriate level.
Proponents of the CM account usually hold that acceptable explanatory models in the corresponding scientific area fix the explanans at 'one level below' (n-1), where 'n' stands for a certain phenomenon being explained. An acceptable scientific explanation of the behavior of some biological object ought to proceed through a decomposition, respectively, into lower-level behaviors and corresponding component-parts. Now, the levels constraint raises two additional issues for CM in the molecular life sciences: (1) the right-level issue, that is, the demonstration of the appropriateness of 'one level be-

low' in accounting for the explanandum phenomenon, and (2) the role ascription issue, that is, the question whether detecting an item's role in a system is a perspectival matter.
We argue that the mechanistic approach to biomolecular explananda is fully operative without the 'one level below' explanatory strategy. We illustrate this by referring to the current genomic explanatory model of de novo genes synthesis (see Tautz et al. (2013)). The explanatory model in question accounts for the origin of so-called 'orphan genes', which, in case they become functional and evolutionary fixed in the genome, can ground a gene family. The mechanistic conditions in this model are fulfilled without complying with the 'one level below' condition or its further mechanistic cognates. With regard to the role ascription issue, we argue, that the item's role, is not a matter of interest or perspective, but is fixed by evolutionary constraints, i.e., its programmed character is maintained minimally by purifying selection. This, on our account, can be extrapolated more generally from the way in which functions are ultimately ascribed in molecular biology (see Šustar (2007)).

Un-blackswaning Scientific Prediction

Tuesday, February 26**,** 12.20 – 13.00
Tagungsraum, Seminargebäude 106

Karim Bschir
University St. Gallen

There is little doubt that predicting is an important part of scientific practice. Scientists frequently engage in debates about the value of successful predictions or about the limits of their predictive capacities. In economics and social science, debates about the very possibility of predicting the future of socio-economic systems are as old as the disciplines themselves, and today these debates are as pressing as the they ever were. In recent years, several bestselling books on issues related to prediction have attracted large audiences (e.g.

Taleb 2007, Silver 2012). We also observe an increased demand for science-based predictions by policy makers. Within the "evidence-based decision-making" framework, that is gaining increasing attention in areas like economic policy, heath care, education, environmental policy and many others, science-based predictions and questions regarding their reliability are of crucial importance.
Despite the huge practical importance of science-based predictions, in the philosophy of science prediction is rarely treated as a topic in its own right (cf. Douglas 2009). If prediction happens to become the subject of philosophical analyses, it often pops up in debates about explanation, confirmation, or realism. As a rough indicator for this, we may consider the fact that the number of publications on prediction is negligible compared to the number of publications on explanation. On philpapers.org, the category "Explanation in General Philosophy of Science" currently contains 2281 publications, whereas "Predictions in Science" contains no more than 56 items. In recent years, philosophy of science conferences had astonishingly few slots for prediction.
In this talk I will do three things:
First, I provide a tentative explanation for the lack of attention on prediction in the current philosophy of science. In particular, I will claim that the issue of temporal prediction does not receive the attention that it deserves. The reason for this, as I will show, is that many philosophers still look at temporal prediction as a special case of conformational prediction. What makes predictions epistemologically interesting is that they serve as potential confirmations of the theories from which they were derived. Accordingly, predictive success is often seen as a means and not a scientific end in itself.
Second, I provide reasons for why the philosophy of science should pay more attention to practices of temporal prediction in current science and why it is not correct to reduce predictions to their epistemic role in confirmation and theory testing. In many relevant cases, the goal of a prediction is not so much to confirm a hypothesis, but rather practical and immediately related to its social or econom-

ic value.
Third, I will provide counterarguments against some urban myths about temporal prediction that have been spread in the popular literature. In particular, I will engage and defeat Nassim Taleb's generic claims about the limits of predictability and the unpredictability of so-called Black Swan events, a task that has so far not been considered worthwhile by professional philosophers, but that is—as I believe—long overdue.

Uncertainty in Science: A Study on the Role of Non-Cognitive Values in the Assessment of Inductive Risk

Tuesday, February 26, 16.30 – 17.10
Room 26, Seminargebäude 106

Leandra Bucher (Tilburg University)
Matteo Colombo (Tilburg University)
Silvia Ivani (Tilburg University)

Scientific research involves uncertainty. Scientists have to take decisions about methodologies and hypotheses and each one of these decisions involves uncertainty. Lack of sufficient evidence and disagreements about methodologies are sources of uncertainty that can introduce error in scientific reasoning. One kind of error is associated with the notion of inductive risk, i.e., the chance of taking wrong decisions, such as accepting a hypothesis that is in fact false. Philosophers argue that inductive risk challenges the ideal of value-free science, i.e., the idea that non-cognitive values (e.g. moral and economic values) do not influence research, and it shows their actual beneficial role in science (Hempel 1965; Douglas 2000). Specifically, considering non-cognitive values is beneficial when taking wrong decisions may involve non-cognitive consequences, such as harming women's health.

Our study aims at investigating the relation between non-cognitive values and inductive risk. We present the results of an experimental study clarifying the psychological impact of political values and personal features like one's race and sex on the acceptance (or rejection) of scientific hypotheses in the face of inductive risk. Our hypotheses was that political and personal identity features reliably predict people's sensitivity to scientific errors. Specifically, people are less likely to accept hypotheses that they perceive as clashing with their political ideology and identity. In our study, participants were asked to read and evaluate three vignettes, where scientists disagree about the adequacy of a specific test and take decisions about hypotheses involving sexual or racial differences. In each vignette, the consequences of a mistaken decision could harm a group of people (either women, men, Black or White people). One of the vignettes concerned the exclusion of women from clinical trials. In this vignette, scientists decided to introduce a new drug tested on a group including only men into the market. Participants were asked to express how certain they were that the decision taken was a good one. Our hypothesis was that conservative men were more likely than women to see that decision as a good decision. At the end of the survey, information about political ideology, race, and sex was collected.
Our results provide us with a more nuanced understanding of the bearing of non-cognitive values on the psychology of inductive risk. Though philosophers of science have drawn on several historical case-studies to clarify the notion of inductive risk, little attention has been paid to how people actually reason about inductive risks. In this paper, we set out to begin filling this gap in the philosophical literature by investigating the relationship between reasoning, inductive risk, and non- cognitive values.

Empiricism without Magic – Transformational abstraction in Deep Convolutional Neural Networks

Tuesday, February 26, 11.00 – 11.40
Room 24, Seminargebäude 106

Cameron Buckner
University of Houston

In Artificial Intelligence, recent research has demonstrated the remarkable potential of Deep Convolutional Neural Networks (DCNNs), which seem to exceed state-of-the-art performance in new domains weekly, especially on the sorts of very difficult perceptual discrimination tasks that skeptics thought would remain beyond the reach of artificial intelligence. However, it has proven difficult to explain why DCNNs perform so well. In philosophy of mind, empiricists have long suggested that complex cognition is based on information derived from sensory experience, often appealing to a faculty of abstraction. Rationalists have frequently complained, however, that empiricists never adequately explained how this faculty of abstraction actually works.
In this talk, I tie these two questions together, to the mutual benefit of both philosophy and AI. I argue that the architectural features that distinguish DCNNs from earlier neural networks allow them to implement a form of hierarchical processing that I call "transformational abstraction". Transformational abstraction iteratively converts sensory-based representations of category exemplars into new formats that are increasingly tolerant to "nuisance variation" in input. Reflecting upon the way that DCNNs leverage a combination of linear and non-linear processing to efficiently perform this feat allows us to understand how the brain is capable of bi-directional travel between exemplars and abstractions, addressing longstanding problems in empiricist philosophy of mind. I argue that, rather than simply implementing 1980s connectionism with more brute-

force computation, transformational abstraction counts as a qualitatively distinct form of processing ripe with philosophical and psychological significance, because it is significantly better suited to depict the generic mechanism responsible for this important kind of psychological processing in the brain.

Epistemic Injustice and Psychiatric Classification

Monday, February 25, 15.10 – 15.50
Room 25, Seminargebäude 106

Anke Bueter
Leibniz University Hannover

Psychiatric classification is a highly controversial epistemic practice, as could be witnessed again in recent years with the latest revisions of both the Diagnostic and Statistical Manual of Mental Disorders (DSM-5, APA 2013) and the International Classification of Diseases (ICD-11, WHO 2018). While many critiques point out problems on the content level of these taxonomies, such as a lack of validity of individual diagnoses or diagnostic criteria, a growing amount of literature now targets the actual processes of revising psychiatric classifications. In particular, the DSM-revision process has been criticized as lacking diversity in terms of different theoretical and disciplinary perspectives as well as ethnical and cultural backgrounds. Another emergent controversial topic has been whether to increase the participation of laypersons, in particular patients and patient-advocates, in the revision process.

My paper provides a new argument in favour of such an increased integration of patients into taxonomic decision-making in psychiatry by drawing on resources from social epistemology. It argues that the exclusion of patients from these processes constitutes a special kind of epistemic injustice: Pre-emptive testimonial injustice, which precludes the opportunity for testimony due to a presumed irrelevance or lack of expertise on the side of patients and advocates.

This presumption is misguided here for two reasons: (1) the role of values in psychiatric classification and (2) the epistemic potential of first-person knowledge in this case.
(1) Psychiatric classification currently involves value-judgments at several points, due to the insecure state of our knowledge of psychopathologies and the need for decision-making under uncertainty resulting from the DSM's/ICD's application in clinical practice. For example, this can concern decisions on the disorder-status of conditions or behaviors and the weighing of associated risks. As taxonomic decisions always trade between risks of over- versus underdiagnosis, the perspective of patients is a relevant input regarding whether it would be better to err on the side of being too rigid or too inclusive in the criteria for particular mental disorders.
(2) In this situation characterized by significant uncertainty and error risks, patient perspectives can moreover function as a corrective means against implicitly value-laden, inaccurate, or incomplete diagnostic criteria sets. This argument falls in line with critiques that the DSM's/ICD's diagnostic criteria fail to sufficiently represent the clinical reality and phenomenology of mental disorders, which leads to a lack of clinical utility and has negative impacts on the treatment of patients. Including first-person accounts of the phenomenology of mental illnesses is therefore not only a matter of social justice, but can provide a helpful epistemic means here.
To sum up, patients' perspectives are relevant and contribute valuable viewpoints to the revision of psychiatric classifications, and their exclusion constitutes a case of pre-emptive epistemic injustice. This injustice not only harms patients in their capacity as knowers, but also leads to preventable epistemic losses in the practices of psychiatric classification, diagnosis, or treatment.

Effective reference to quantum particles

Monday, February 25, 12.20 – 13.00
Room 22, Seminargebäude 106

Adam Caulton
Oxford University

How does the formalism of quantum mechanics make reference to particles? A naïve answer, frequently assumed in discussions of “indistinguishable” elementary particles (i.e., permutation-invariant many-particle quantum mechanics), appeals to the tensor product structure of the many-particle Hilbert space and its associated algebra of quantities. This answer, further developed, goes something like this: the order in which some single-particle Hilbert space, single-particle state or single-particle quantity appears in the tensor product is a proxy for a name of some associated particle, where naming is understood in Millian terms.

However, this answer stands in need of justification. If permutation invariance is not imposed, so that the “full” tensor product algebra is available to the joint system, a justification may be provided; but it fails whenever permutation invariance is imposed. In this case, the restriction on the joint algebra makes the identification of constituent systems a non-trivial task. I aim to demonstrate how an alternative means of individuating constituent systems may be found, by appeal to state-dependent single- particle properties. On this method of individuation, so-called “indistinguishable” particles are (in some states, at least) perfectly distinguishable—without appealing to weak discernment through relations. In fact, it may be seen as a quantum analogue of Russellian naming via definite description.

This method of individuation has several implications and prospects for further development, which I hope to detail. The most significant implication is for entanglement: it gives us reason to abandon (in the permutation-invariant setting) the identification of entanglement with non-separability of the joint state, in exactly the way suggested elsewhere by Ghirardi and Marinatto. There are special implications for fermions, where it seems that there are several, apparently rival ways of decomposing a joint system into its constituent parts. The further development is in the direction of quantum field theory, to which the method of individuation described above may be straightforwardly extended. In particular, I will pursue the question whether particle-talk continues to make sense outside of the (dynamically trivial) Fock representationsModel-groups as Scientific Research Programs

Model-groups as Scientific Research Programs

Tuesday, February 26, 11.00 – 11.40
Room 22, Seminargebäude 106

Cristin Chall
University of Bonn

The Standard Model (SM) is one of our most well tested and highly confirmed theories. However, physicists, perceiving flaws in the SM, have been building models describing physics that goes beyond it (BSM). Many of these models describe alternatives to the Higgs mechanism, the SM explanation for electroweak symmetry breaking (EWSB). So far, no BSM model has been empirically successful; meanwhile, the Higgs particle discovered in 2012 has exhibited exactly the properties predicted by the SM. Despite this, many BSM models have remained popular, even years after this SM-like Higgs boson has been found. This is surprising, since it appears to fly in the face of conventional understandings of scientific practice to have competing models interacting in a complex dynamics even though none of them have achieved empirical success and all of them are faced with a predictively superior alternative. The question becomes: How do we rationally explain physicists continued work on models that, though not entirely excluded, are increasingly experimentally disfavoured?

I will argue that the best framework for explaining these complex model dynamics is the notion of scientific research programmes, as described by Lakatos (1978). To apply this frame- work, however, I need to modify it t collections of models which share the same core theoretical commitments, since Lakatos dismisses models to the periphery of research programmes. These collections of models, which I call model-groups, behave as full-fledged research programmes, supplementing the series of theories that originally defined research programmes. By allowing the individual models to be replaced in the face of unfavourable empirical results, the hard core of a model-group is preserved. The practical benefit of applying this framework is that it ex- plains the model dynamics: physicists continue to formulate and test new models based on the central tenets of a model-group, which provide stability and avenues for making progress, and rationally continue giving credence to BSM models lacking the empirical support enjoyed by the SM account of EWSB.
To demonstrate the model dynamics detailed by the Lakatosian framework, I will use the Composite Higgs model-group as an example. Composite Higgs models provide several benefits over the SM account, since many have a dark matter candidate, or accommodate naturalness. However, the measured properties of the Higgs boson give every indication that it is not a com- posite particle. I trace the changing strategies used in this model-group in order to demonstrate the explanatory power of Lakatosian research programmes applied in this new arena. Thus, I show that Lakatos, suitably modified, provides the best avenue for philosophers to describe the model dynamics in particle physics, a previously underrepresented element of the philosophical literature on modelling.

Maimon on Scientific Genius

Wednesday, February 27, 11.40 – 12.20
Room 24, Seminargebäude 106

Idit Chikurel
University of Potsdam

What constitutes a scientific genius? How does a genius arrive at new inventions and discoveries? How can we invent something new methodically, without assisting any "spirit of inspiration"? What differs a scientific genius from an artistic one? These questions were at the heart of discussions led by philosophers and scientists in the 17th and 18th centuries and are as relevant as ever. In my lecture, I present the various answers to these questions proposed by philosophers such as Leibniz, Gerard and Kant, using Salomon Maimon's enlightening work on the subject as the central point of discussion.

While philosophers in the 17th century concentrated on the importance of the methodical inventor, in the 18th century "the light of order" gave way to the rise of the genius. So prominent was this rise, that the second half of the 18th century is named Geniezeit. Many philosophers may have emphasized the importance of genius in the arts, but Maimon's interest was set on the scientific genius: its characteristics, its advantages and disadvantages, and what can one do in case he was not fortunate enough to be born a genius – in that case, one can aspire to improve his work as a methodical inventor. Consequently, Maimon proposed methods of invention that can guide methodical inventors to arrive at new knowledge more easily.

My discussion of what constitutes a scientific genius or a methodical inventor is intertwined with the questions of the role of talent, originality, imitation, chance and order. I present Maimon's criteria to what makes a philosopher "a true philosopher" rather than merely a "philosophical calculator". I conclude with a few examples of Maimon's methods of invention demonstrated on Euclidean geometry, using examples from Elements and Data.

Disambiguating Scientific Disagreement, Honest Mistakes, Lack of Care, and Misconduct in Medical Statistics

Wednesday, February 27, 15.50 – 16.30
Room 23, Seminargebäude 106

Alexander Christian
Heinrich Heine University Düsseldorf & DCLPS

A growing body of literature indicates persistent statistical errors in medical journals contributing to the replicability and reproducibility crisis (Ioannidis 2005, Ioannidis 2012, Strasak et al 2007, Worthy 2015). These common errors include the failure to report sample sizes, omission of a priori sample size calculation/ effect-size estimation, use of wrong statistical tests, failure to specify all tests used in a comprehensible way, inadequate graphical or numerical representation of basic data, and drawing conclusions not at all supported by study data. These common mistakes in medical statistics are remarkable, since they all seem to be avoidable and appear to reveal a distressing lack of care among some members of the medical community. Hitherto the debate about these findings has focussed on the scope of the problem, corrupting influences and good statistical practice in medical research. A neglected problem in this context concerns the normative practice of evaluating professional peers' conduct: due to scientific disagreement on statistical methods as well as a lack of conceptual clarity with regard to the exact meaning of *carefulness*, *honest mistakes*, *negligence*, and *questionable research practices*, medical scientists and research ethicists are often reluctant to evaluate conduct of professional peers in these normative terms (e.g. Fanelli 2013). This turns out to be a major problem, since self-correction in medical science depends on the identification and correction of statistical errors in medical studies and the correct evaluation of individual scientific peers' conduct.

In addressing this problem, I first provide an overview on common statistical errors in medical journals related to study design, data analysis, documentation, presentation, and interpretation. I then propose a model for the evaluation of particular cases. It rests on the idea that we should in a step-by-step procedure rule out — in this order — scientific disagreement, an honest mistake, a negligent mistake, questionable research practices and scientific misconduct. I will discuss a series of criteria for disambiguating these concepts, which are derived from reports about erroneous application of statistical methods in cardiology, oncology and anaesthesiology. It will be shown that we have reliable indicators available, often enabling us to substantiate nuanced evaluations about statistical errors in particular cases within the whole conceptual spectrum — ranging from non-culpable honest mistakes to culpable egregious violations of good statistical practice.

Exaptation: From Darwin's "botany" to evolutionary psychology

Monday, February 25, 14.30 – 15.10
Room 22, Seminargebäude 106

Antonio Danese
University of Padova

According to the theories that constitute evolutionary psychology, natural selection designed several independent computational modules, each of which would preside over a specific cognitive domain developed in an Environment of Evolutionary Adaptedness (EEA). On the assumption that different neural circuits have specialized during Pleistocene to solve corresponding adaptive problems, reverse engineering has been introduced to explain the evolution of the human mind and inherited social behaviors from our ancestors. Moreover, the obstinate research for an adaptive meaning for any behavioral trait has led to conceive the evolution in terms of problem solving.

As a result, evolutionary psychology has focused on natural selection by underestimating the other fundamental aspect of Darwin's explanation: common descent with modifications, or the Tree of Life. Tree Thinking is the development of this Darwinian heritage carried out by modern evolutionary biology and consists in the study of genealogical kinships among species and of comparative knowledge about common descent.

I will try to show that the adaptive hypotheses of evolutionary psychology without this explanatory approach cannot be confirmed or falsified. Moreover, I will argue that evolution does not contrive new traits ex novo but reuses the already available material: reusing or recycling formerly existing structures, the result of selective processes or not, is the main theoretical meaning of the modern concept of exaptation, inherited from Darwin as well.

To show how evolutionary psychology could benefit from this explanatory model I will focus on the original meaning of the modern concept of exaptation as Darwin conceived it in the Orchid Book (first edition 1862; second edition 1877). During the drafting of the manuscripts he deeply studied the ability of orchids to develop contrivances through the co-optation of the same organ to new functions and he submitted the new model of this explanation to Fritz and Hermann Müller and Friedrich Hildebrand. After having tested darwinian hypotheses into their researches, they decided to adopt them and from that moment exaptation was developed and handed down from plant sciences to all evolutionary sciences.

The core of my argument is that adopting within evolutionary psychology the heritage of Darwinian exaptation and common descent would show that not all traits set in a biological population are necessarily adaptations; the current function of a structure does not always coincide with its historical origin; reverse engineering does not allow us to elaborate falsifiable adaptive hypotheses; and, therefore, the fundamental aspect in the description of a trait is not found in its adaptive utility, in fact, some traits may prove useless and still maintain a relevant importance in providing evidence of kinship between species.

Mathematical Realism from Color Objectivism

Wednesday, February 27, 14.30 – 15.10
Room 25Nicholas, Seminargebäude 106

Nicholas Danne
University of South Carolina

Color objectivists such as David R. Hilbert define color as a dispositional property of surfaces that obtains independently of perceivers and of illuminating media. I criticize Hilbert's definition as conceptually incoherent. The disposition that Hilbert identifies with color is surface spectral reflectance (SSR), or the unitless ratio of the average powers, per wavelength, of reflected and incident light at an object's surface. What Hilbert ignores in his definition is the empirically confirmed, classical-physical (non-quantum) inverse relationship of spectral bandwidth to temporal duration for any given light pulse. This inverse relationship I call 'harmonic dispersion', and I argue that harmonic dispersion renders the SSR disposition non-surfacial, because it is undefined for pulses of short duration. Only light pulses longer than 1 picosecond fail to radically disperse, and so I argue that SSR can only be a surface property if it is the disposition to reflect not pulses, but the infinite-duration superimposed components of pulses (Fourier harmonics). Only such superimposed infinitudes could reflect dispersion-free, 'per-wavelength', for any given pulse duration, on a surface whose reflectance is medium-independent. To claim that Fourier harmonics or their instantiations reflect, however, is to claim that they exist.

On the Reasonable Effectiveness of Mathematics in Science

Wednesday, February 27, 15.50 – 16.30
Room 25, Seminargebäude 106

Henk De Regt (Vrije Universiteit Amsterdam)
Peter P. Kirschenmann (Vrije Universiteit Amsterdam)

In 1959, Nobel Prize winner Eugene Wigner delivered a famous lecture, entitled "The Unreasonable Effectiveness of Mathematics in the Natural Sciences", propounding the claim “that the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and that there is no rational explanation for it.” More recently, in 2014, Robbert Dijkgraaf, director of the Institute for Advanced Study, Princeton, gave a public lecture, entitled “The Unreasonable Effectiveness of Quantum Physics in Modern Mathematics”. Obviously, he argued for a some reverse effect.
Both physicists mentioned and discussed several examples in support of their claims. Presenting their examples and some of my own, I argue that this effectiveness can never be totally unreasonable. I suggest that there must be reasons for any particular successful influence or contribution from one field to the other. And, insofar as there are reasons, the cases concerned should be intelligible. Yet, noting the reasons in particular cases will not distract from our possible existential or cosmic wondering about the whence, wherefore, and whither of nature and mathematics in general.

Shades of Grey: Granularity, pragmatics, and non-causal explanation

Monday, February 25, 12.20 – 13.00
Room 25, Seminargebäude 106

Hugh Desmond
KU Leuven

Implicit contextual factors mean that the boundary between causal and noncausal explanation is not as neat as one might hope: as the phenomenon to be explained is given descriptions with varying degrees of granularity, the nature of the favored explanation alternates between causal and non-causal.
While it is not surprising that different descriptions of the same phenomenon should favor different explanations, it is puzzling why redescribing the phenomenon should make any difference for the causal nature of the favored explanation. I argue that this is a problem for the ontic framework of causal and non-causal explanation, and instead propose a pragmatic modal account of causal and non-causal explanation. This account has the added advantage of dissolving several important disagreements concerning the status of non-causal explanation.

Pluralistic Ontologies and Perspectival Mechanisms in Cognitive Neuroscience

Wednesday, February 27, 12.20 – 13.00
Room 26, Seminargebäude 106

Joe Dewhurst
Munich Center for Mathematical Philosophy, LMU

This paper will argue that a moderately perspectival form of mechanistic explanation can help make sense of debates about ‘cognitive ontologies’, i.e. taxonomical systems in cognitive science. This argument will support a recent move towards pluralistic cognitive ontologies, which allow for multiple, context sensitive ways of carving up a given domain.

Section 1 will introduce the cognitive ontology debate and describe the recent move towards pluralistic ontologies. The term 'cognitive ontology' was coined by Price & Friston (2005), who use it to refer to whatever taxonomy of states and processes best captures the functional organization of the brain. Subsequent contributions to this debate can be roughly classed as either 'absolutist' or 'pluralist' (cf. Burnston 2016). My focus in this paper is on developing a mechanistic approach that provides a bridge between these two kinds of strategies, by explaining how a single underlying structure can serve as the basis for functional attributions that are pluralistic and context-sensitive.

Section 2 will outline this strategy, which involves identifying an underlying mechanistic structure whilst accepting that any attribution of functions to this structure must be made from within an explanatory perspective (cf. Craver 2013). In any case where we think a pluralistic ontology might be appropriate, we should aim to identify an underlying mechanistic structure whose properties everyone can agree on. We can then begin to ask how the different parts of this structure might contribute to the production of different phenomena, and how different descriptions of those parts and their interactions might contribute to the explanation of those phenomena. These phenomena thereby define the explanatory contexts where different descriptions, and hence, ontologies, might be appropriate, but at the same time the agreed underlying structure provides a constraint on the number of possible ontologies that can be described in any given case.

Section 3 will return to the original cognitive ontology debate, focusing on one of the cases discussed by Price & Friston (2005), that of the left posterior lateral fusiform (LPLF) region. Activity in this region is implicated in several different kinds of task, leaving it unclear what function we should describe it as performing. Price & Friston's solution to this problem is to describe it as performing a

higher-level function, sensorimotor integration, which can account for each kind of task. I will argue that while Price & Friston are on the right lines in trying to give a general, context-free description of the area, it is wrong to think that this description by itself can tell us everything about the area's function. Rather we should treat this context-free description as a sketch of an underlying structure, which by itself does not tell us much, but which when buttressed with contextual attributions of functions can provide a foundation for full mechanistic explanations of various phenomena.
Finally, in section 4 I will explain in more detail what I take an 'underlying mechanistic structure' to be, how we can go about identifying one, and the role that it can play in mechanistic explanation. The relationship between our investigation of underlying structures and the development of mechanistic explanations will turn out to be one of iterative bootstrapping, where our growing knowledge of neural structures can be used to refine our attributions of mechanistic functions, and our attributions of mechanistic functions can be used to further develop our knowledge of neural structures.

Justifying Lewis's Kinematics of Chance

Tuesday, February 26, 16.30 – 17.10
Room 23, Seminargebäude 106

Patryk Dziurosz-Serafinowicz
University of Gdańsk

In his ``A Subjectivist's Guide to Objective Chance'', David Lewis argued that a particular kinematical model for chances (physical probabilities) follows from his Principal Principle. According to this model, any later chance function is equal to an earlier chance function conditional on the complete intervening history of non-chancy facts. This paper, first, investigates the conditions that any kinematical model for chance needs to satisfy to count as Lewis's kinematics of chance. Second, it presents Lewis's justification for his kinematics of chance, and explains why it is bound to be problematic. Third, it gives an alternative justification for Lewis's kinematics of chance that does not appeal to the Principal Principle. Instead, this justification appeals to a well-supported requirement for chance, according to which any prior chance function must be a convex combination of the possible posterior chance functions. It is shown that, under a plausible assumption, Lewis's kinematics of chance is equivalent to this requirement. Finally, by focusing on this requirement, it is explained why so-called self-undermining chances fail to obey Lewis's kinematics of chance.

The No-miracles Argument is Not an Inference to the Best Explanation

Monday, February 25, 16.30 – 17.10
Room 24, Seminargebäude 106

Ludwig Fahrbach
University of Bern

As is well known, scientific realism about our best scientific theories is usually defended with the Nomiracles argument (NMA). In its simplest form the NMA states that it would be a miracle, if our best scientific theories, i.e., theories enjoying tremendous empirical success such as modern atomism, the theory of evolution, and plate tectonics, were false. The NMA is usually explicated as an inference to the best explanation (IBE): “Given a body of data find potential explanations for the data, compare them with regard to explanatory quality, and infer the approximate truth of the best explanation.” I present another explication of the NMA, namely an improved version of hypothetico-deductivism, which I call HD+: “If *T* is a reasonably simple theory, the data is excellent, and *T* together with suitable auxiliaries entails, or probabilistically favors, the data, then *T* is approximately true.” Data counts as *excellent*, if it exhibits good making features such as diversity, accuracy, and so on to a high degree. For example, the theory of evolution is supported by many independent lines of data. The main aim of my talk is to show that HD+, but not IBE, is a good explication of the NMA. The two principles differ in an important respect: IBE tells us to construct rival explanations of the data, and compare them with regard to explanatory quality, whereas HD+ allows us to infer the truth of the given hypothesis *T* without considering any rival theories of *T*. I claim that HD+ is right: If the data is excellent, we need not look at any rival theories to infer the truth of the given theory. This claim plainly contradicts received wisdom about the confirmation of theories, so I have to provide arguments in its support.

The first argument alludes to scientific practice. When a theory is supported by excellent data, scientists usually don't discuss rivals theories. A telling example is Perrin's argument for the atomic hypothesis in his book *Atoms* (1916). Perrin marshals the relevant data, determines its relationship with atomism, and notes its good-making features, most importantly its diversity and accuracy. He famously states that the data comes from 13 entirely different phenomena such as Brownian motion, radioactive decay, and the blueness of the sky. Perrin does not engage in anything resembling IBE: He does not construct any rival explanations of the 13 different phenomena, compare them with respect to explanatory quality, and judge atomism to be the best explanation. The whole book is solely concerned with working out the 13 different applications of atomism. Perrin obviously thinks that this suffices to show that atomism is true. He briefly mentions one rival theory in the introduction, but otherwise does not mention or discuss any rival theories. Thus, his reasoning accords nicely with HD+, but not with IBE. I go on to discuss another example from scientific practice, namely the theory of evolution.

My second argument for the above claim aims to show that under certain plausible assumptions excellent data automatically refutes all reasonably simple rival theories, and we can know this without having to formulate and consider the rival theories explicitly. I show how the second argument can be embedded and justified in a Bayesian framework.

Simplicity in Abductive Inference

Tuesday, February 26, 15.50 – 16.30
Room 25, Seminargebäude 106

Christian J. Feldbacher-Escamilla
Heinrich Heine University Düsseldorf

Abductive inference is often understood as an inference to the best explanation, where an explanation is better than another one if it makes the evidence more plausible and is simpler. It is quite clear what the epistemic value of making evidence plausible consists in. However, regarding simplicity, it is debatable whether it bears epistemic value or not. According to the approach on simplicity of Forster and Sober (1994), one can spell out the truth-aptness of simplicity via constraints put forward in the curve fitting literature which are directed against overfitting erroneous data. Therein simplicity is measured via the number of parameters of a model. However, it remains open how the notion of simplicity spelled out in these terms relates to the notion of simplicity as is often used in abductive inferences, namely as the number of axioms or laws of an explanation. In this talk we show how the latter notion is related with the former by help of structural equations

Causes, Interventions, and Responsibility

Monday, February 25, 14.30 – 15.10
Room 26, Seminargebäude 106

Enno Fischer
Leibniz University Hannover

According to interventionist theories of causality, we are interested in causal claims because they enable us to interacting effectively with the world. Interventionists have also tried to explain the function of more specific causal claims that concern actual causation. They argue that while causal claims generally tell us where we could intervene in order to change the effect, claims of actual causation tell us where we should intervene (Hitchcock and Knobe, 2009). Interventionist accounts of the function of actual causation have been criticised widely, in particular, by philosophers and psychologists who see a close relation between causal judgement and the ascription of responsibility and blame. These opponents argue that an exclusive focus on interventions is neither inherently plausible nor does it fit the data (e.g. Alicke et al. (2011)).

In this talk I will present a novel taxonomy of causal claims that is based upon distinguishing three senses in which contributors to the debate have been using the term "actual causation". First, actual causation (AC1) refers to claims about sequences of token-events as opposed to claims that relate types of events. Second, actual causes (AC2) are often contrasted with merely potential causes. Merely potential causes are factors that can bring about a certain effect but in contrast to actual causes they do not bring it about, for example, because they are pre-empted. Third, actual causation (AC3) can be understood as referring to factors that are most salient in bringing about an effect and, therefore, are to be distinguished from background conditions. I will argue that AC1 describes a proper subset of AC2. Token causal claims like "c caused e" entail that c not only could but also did bring about e. By contrast, type level claims like "C1 tends to pre-empt C2 in causing an effect E" do not entail that

in any particular token situation c1 caused e. I will also show that AC1 and AC2 are independent of AC3.
Based on the taxonomy I will provide a more fine-grained analysis of the function of actual causation that sheds new light on the debate between interventionists and proponents of responsibility accounts. So far proponents of responsibility accounts have mainly targeted iterventionists claims about AC3. Yet interventionists seem to incorporate AC1 and AC2 as well. The clearest difficulties for interventionists arise from token causal claims that concern the past. Unless these claims are generalizable they do not inform us about future interventions. The most interesting and least covered case are type claims of AC2. While Hitchcock 2017 argues that such claims help us design goal-directed strategies in contexts with complex causal structure, I will object that some such claims are more plausibly related to responsibility.

Dispositions and Causal Bayes Nets

Monday, February 25, 11.00 – 11.40
Room 26, Seminargebäude 106

Florian Fischer (University of Siegen)
Alexander Gebharter (University Groningen)

In this talk we develop an analysis of dispositions on the basis of causal Bayes nets (CBNs). Causal modeling techniques such as CBNs have already been applied to various philosophical problems (see, e.g., Beckers, ms; Gebharter, 2017a; Hitchcock, 2016; Meek & Glymour, 1994; Schaffer, 2016). Using the CBN formalism as a framework for analyzing philosophical concepts and issues intimately connected to causation seems promising for several reasons. One advantage of CBNs is that they make causation empirically tangible. The CBN framework provides powerful tools for formulating and testing causal hypotheses, for making predictions, and for the discovery of causal structure (see, e.g., Spirtes, Glymour, & Scheines, 2000). In addition, it can be shown that the theory of CBNs satisfies standards successful empirical theories satisfy as well: It provides the best explanation of certain empirical phenomena and can, as a whole theory, be tested on empirical grounds (Schurz & Gebharter, 2016).

In the following we use CBNs to analyze dispositions as causal input-output structures. Such an analysis of dispositions comes with several advantages: It allows one to apply powerful causal discovery methods to find and specify dispositions. It is also flexible enough to account for the fact that dispositions might change their behavior in different circumstances. In other words, one and the same disposition may give rise to different counterfactual conditionals if its causal environment is changed. The CBN framework can be used to study such behavior of dispositions in different causal environments on empirical grounds. Because of this flexibility, our analysis can also provide novel solutions to philosophical problems posed by masks, mimickers, and finks which, one way or another, plague all other accounts of dispositions currently on the market. According to Cross (2012), the "recent literature on dispositions can be characterized helpfully, if imperfectly, as a continuing reaction to this family of counterexamples" (Cross, 2012, p. 116). Another advantage of our analysis is that it allows for a uniform representation of probabilistic and non-probabilistic dispositions. Other analyses of dispositions often either have trouble switching from non-probabilistic dispositions to probabilistic dispositions, or exclude probabilistic dispositions altogether.

The talk is structured as follows: In part 1 we introduce dispositions and the problems arising for classical dispositional theories due to masks, mimickers, and finks. Then, in part 2, we present the basics of the CBN framework and our proposal for an analysis of dispositions within this particular framework. We highlight several advantages of our analysis. In part 3 we finally show how our analysis of dispositions can avoid the problems with masks, mimickers, and finks classical accounts have to face. We illustrate how these problems can be solved by means of three prominent exemplary scenarios which shall stand proxy for all kinds of masking, mimicking, and finking cases.

Pluralism and Relativism from the Perspective of Significance in Practice

Monday, February 25, 14.30 – 15.10
Room 24, Seminargebäude 106

Mark Fischer
University of Heidelberg

My paper examines a recent concept of scientific pluralism introduced by Hasok Chang (2012). I focus on Chang's (2015) response to the relativist critique based on Sociology of Scientific Knowledge (SSK) by Martin Kusch (2015). Furthermore, I discuss the separation of pluralism from relativism in general. My argument is that both positions have major deficits on a social-practical level. Therefore, I su

gest improvements to Chang's pluralism, which might also be of interest to relativists.

Kusch's main argument (2015) against Chang (2012) is that the chemical revolution happened because there was no acceptable reason for the scientific group to consider phlogiston theory. He elaborates that there has never been a coherent phlogiston theory itself. In contrast, there have been good social reasons to accept oxygen theory. Chang does not agree. According to him, we should not seek "literal truth" (Chang 2012, p.219) in form of correspondence theory, but other possible ways "to maximize our learning from reality" (Chang 2012, p. 220). What distinguishes Chang from relativism is his way of defending pluralism as an important ideal of scientific research necessary to understand additional aspects of reality. His "active scientific realism" disagrees with the concept of theory unification. Chang argues for the coexistence of competing but somehow practical successful theories instead. Relativism based

on SSK, in contrast, supports “good social reasons” as an acceptable argument for unification (Kusch 2015, p.76, 78).
From my point of view, Chang is correct if he argues against unification based on social normativity. (See for a parallel discussion on moral relativism by David Velleman (2015) also.) A relativist theory, which does not accept pluralism, would not differ from scientific realism, as the kind of critical rationalism, on a practical level. The debate between realism and anti-realism would be empty if realism as well as relativism shared the same concept of theory unification. In contrast, Chang’s version of pluralism obviously offers practical impact. Unfortunately, its position about social standards of research is unsatisfactory.
Therefore, I suggest a more confident form of relativism, which includes pluralism. I am convinced that there is good reason to argue that nature as well as possibilities to interpret it are quite complex. However, from my perspective social-normative considerations play a major part in epistemology. As a result, pluralism must emphasize e.g. socially inducted aims of research as a reason for theory pluralism. I agree, that constrains of what we call nature influence the possible variability of theories. However, it does not mean that pluralism cannot constitute a convincing answer to the complexity of different social communities and their way of epistemology.

Schrödinger’s Glass – Opposing dispositions co-instantiated

Monday, February 25, 15.50 – 16.30
Room 23, Seminargebäude 106

Stephan Fischer

Can opposing dispositions be co-instantiated by one and the same object at the same time? In this paper, I will suggest a physical system with two much discussed dispositions: fragile and unbreakable. On contrary to most examples discussed in the debate on opposing dispositions, I will make use of probabilistic dispositions. Some philosophers seem to think that probabilistic dispositions are of no particular interest with respect to their different manifestations. “There seems to be nothing disputable about the thought that [an object] is disposed to have a chance of $^2/_3$ to [x] and, at the same time, is also disposed to have the chance of $^1/_3$ to [y]”.

A possibility to be probabilistically disposed in such a way “will be denied by nobody”. [i] Even the occurrence of contrasting manifestations and thus statistically antagonistic dispositions do not really play an important role within the debate on *opposing dispositions*.[ii] The latter seem to constitute a questionable problem only if sure-fire dispositions are involved. In the following I will consider a glass the molecular structure M of which includes a single radioactive atom ψ. The atom’s probability of decay shall be exactly 50%. Hence at any specific moment in time t the probability for the atom to be intact equals ½. At the same time the probability for the atom to be decomposed at time t equals ½ as well, in which case I will call it ψ^d. Atom ψ now shall have a strong influence on the molecular structure and subsequently on our glass.

- ψ is not decomposed and hence M = Mψ: a blow of a hammer does not break the glass
- ψ is decomposed and hence M = $M\psi^d$: a blow of a hammer does break the glass into pieces

The question will be, which dispositions are instantiated by that object, and it will turn out to be anything but safe how to properly understand any ‘statistical’ answer. The remaining task then will be to clarify whether to ascribe *none* or *both* dispositions to our object and only the latter answer will turn out to be suitable. Even though there is no problem of intrinsic versus extrinsic properties[iii] the reformed conditional analysis (RCA) isn’t suitable.[iv] In case of Schrödinger’s glass there are neither finks nor maskers and there is no antidote.[v] Both dispositions are grounded by an exclusively intrinsic basis, there is no wizard,[vi] no demon,[vii] and no key-lock pair.[viii] Hence we have to accept the consequence of two opposing dispositions being co-instantiated at the same time by a single physical object.

Patchy Endorsements and Explanatory Depth

Tuesday, February 26, 15.50 – 16.30
Room 24, Seminargebäude 106

Maria Forsberg
Stockholm University

Implicit attitudes are mental states that sometimes to cause subjects to respond in ways that seem to suggest that they believe that p despite the fact that the subjects sincerely assert that not-p, assent to sentences that means that not-p and ascribe the belief that not-p to themselves (Bayne and Hattiangadi 2013, Levy 2015). According to standard accounts, the states have associative content (Fazio 2007, Gawronski and Bodenhausen 2011, Gendler 2008a, 2008b). Recently, however, theorists have argued convincingly that no such account can explain the phenomena that they are supposed to explain, and suggested instead that the states have propositional content (Mandelbaum 2013, Levy 2015).

What kinds of states are implicit attitudes, more specifically? Neil Levy has recently suggested that the states are patchy endorsements (Levy 2015). These states are supposed to be different from beliefs and imaginings, and from all other mental states that we are already familiar with. They are also supposed to explain a wide range of puzzling phenomena, including these: that people have a stronger desire to own a sweater if they believe that their favourite celebrity has made contact with the sweater than if they believe that the celebrity has not made contact with it, that people experience more pleasure when they believe that they are in contact with a sweater if they believe that the celebrity has made contact with it than if they believe that the celebrity has not made contact with it, and that people have a stronger desire to own a sweater if they believe that their favourite celebrity has made contact with it than if they believe that the celebrity has made contact with the object, but that it was sterilized thoroughly afterwards (Newman, G, Diesendruck, G, Bloom, P. 2011).

In this talk, I argue that we should not appeal to patchy endorsements to explain the phenomena that implicit attitudes are supposed to explain. I start off by suggesting that we should only appeal to such states in our explanations of the phenomena if we have a case for thinking that the states can provide deeper causal explanations of them than states of other kinds (cf. Bayne and Hattiangadi 2013). I then draw on the tools provided by interventionism (Woodward and Hitchcock 2003; Woodward 2003) and state a sufficient condition for being a deeper causal explanation than the patchy endorsement explanation: that the explanation can be used to answer more what-if-things-would-have-been-different-questions. (Hitchcock and Woodward 2003). Finally, I describe another explanation in terms of a mental state that I call unconscious imagination, show that this explanation satisfies the condition, and conclude that we should not explain the phenomena in terms of patchy endorsements.

Is Deploying Nuclear Power Unethical?

Wednesday, February 27, 16.30 – 17.10
Room 26, Seminargebäude 106

Simon Friederich
University of Groningen

In view of the impending threat of climate change and the health risks associated with air pollution, there is now a broad consensus that humanity should transition away from fossil fuels in the next few decades. In Western countries, there is a similarly broad consensus that, among the two types of alternatives to fossil fuels – namely, renewable energy flows such as wind, water, and sunlight on the one hand and nuclear fission on the other – renewables are vastly preferable from an ethical point of view because of nuclear fission's incalulable accident risks and its dangerous waste legacy. In Germany in particular, there is nowadays a broad societal consensus that deploying nuclear power is in general unethical. Based on this consensus, the German government now actively tries to promote a nuclear phase-out and/or reduction of the nuclear fleet in neighbouring countries such as Belgium and France.

But is deploying nuclear power really unethical? In view of the fact that the greenhouse gas emissions from nuclear power are of the same order of magnitude as those from renewables, the following four considerations make the assessment more complicated than one might initially think:

First, nuclear plants can replace fossil fuel plants completely, whereas the most scalable renewables -- wind turbines and solar panels – will for a long time need fossil fuel plants as partners to bridge spans of time when neither the sun shines nor the wind blows. Correspondingly, CO2 emissions from electricity are currently more than eight times lower in France, where the share of nuclear energy is about 75%, than in Germany, where the share of wind and solar energy is much higher than in France.

Second, countries that put a premium on wealth and economic growth will abandon further expansion of wind and solar energy if and when it leads to constantly increasing economic costs. And it seems indeed likely that electricity becomes more and more expensive as the share of wind and solar energy grows even if wind turbines and solar panels get cheaper and cheaper. The problem, beside the rising cost of grid stabilization and expansion, are again the periods without any wind and sun. Because of them, fossil fuel plants must be kept online, running inefficiently, which causes a steep rise in costs.

Third, nuclear power, as the historical record shows, can be expanded much faster than wind and solar energy, even if those are considered in combination. Notably, as a study co-authored by the famous climate scientist James Hansen recently showed, the speed of expansion per person in (Western) Germany was larger for nucle-

ar power in its peak decade (1975-1985) than for wind and solar power combined in their peak decade (2004-2014). Many times higher speeds of expansion were even realized by France (1979-1989) and Sweden (1976-1986) in their peak decades for nuclear power – and at that time without the impending threat of climate change as a motivation.
Fourth, one can argue that nuclear power, its anti-green image notwithstanding, is every bit as environmentally-friendly as other energy sources, including renewables: for examples, per unit of energy gained, it uses much less resources and areas and produces much less waste. Unsurprisingly, in a systematic comparisons of different energy sources with respect to environmental impacts, Australian climate scientists Barry Brook and Corey Bradshaw and concluded that expansion of nuclear power has the biggest potential to preserve global biodiversity.
Do these considerations suffice for making the case that deploying nuclear power is ethically permissible? I will conclude my presentation by sketching a comparative assessment of the risks associated with the use of nuclear power and, based on it, suggest an opinionated response to the title question of this talk.

A Naturalized Globally Convergent Solution to Goodman's Paradox

Tuesday, February 26, 11.00 – 11.40
Tagungsraum, Seminargebäude 106

Jorge Luis García Rodríguez
Tsinghua University

Goodman´s new riddle of induction reveals important aspects of the relation between the structure of scientific language and that of scientific hypotheses. In particular, it shows that the instance model of confirmation is not language invariant and cannot be adequately formulated by syntactical means alone.
Attempts to solve this problem had traditionally focused on laying

down criteria favoring preferred predicates right from the outset. However, a different approach would be to accept the syntactical symmetries of competing partitions and studying their respective consequences in a broader sense. This would require a deepening of our understanding concerning the interactions between meaning change and hypotheses formation in the context of confirmation.

Thus, in this essay, I analyze how variations in the property (or condition) partition of language, into which confirmation is cast, impinge upon the degrees of inductive support lend to any given hypothesis. Such analysis shall show the necessity to supplement inductive inferences by non-inductive interpretative schemes. Subsequently, I will explain how any two different interpreted partitions corresponding to the same evidence can be related by means of a unique testable bridge-hypothesis as a consequence of the predicate inter-translatability condition expressed in Goodman's formulation of the problem. Stated in this way, the new riddle of induction is amenable to a naturalized solution. That is, by assessing the validity of the bridge-hypotheses through a coherent nomic chain covering of all the relevant experience.

This essay will show that if certain conditions for partition refinement are satisfied, then only the partitions corresponding to local domains of application of adequate hypotheses stabilize into a nomic chain which reflects the admissible bridge-hypotheses.

Such interpretatively supplemented inductive schema is globally convergent; that is, converges to a solution without any a priori knowledge of preferred partitions.

Another consequence of my approach is a duality thesis in confirmation theory. To wit, any alteration in the relations of inductive support produced as a consequence of alterations in some type of partitions of the inductive basis can be neutralized by restating the inductive basis in terms of a corresponding dual type of partition. Hence, I argue that any adequate solution to the Goodman paradox must also be a solution to the Raven paradox. It is shown that the solution sketched in this essay meets this condition.

Finally, I shall prove how the interpretative inductive schema sketched here avoids Norton's (2018) "no-go" results. Since inductive support will not only depend on the deductive relations within the algebra of propositions but also on the semantic relations among partitions; thus, the ensuing confirmatory symmetries will render the inductive schema asymptotically stable.

How to Establish Backward Causation on Empirically Grounds: An interventionist approach

Monday, February 25, 15.10 – 15.50
Room 26, Seminargebäude 106

Alexander Gebharter (University Groningen)
Dennis Graemer (Heinrich Heine University Düsseldorf)
Frenzis Scheffels (Heinrich Heine University Düsseldorf)

Our common sense understanding of causation tells us that causes precede their effects in time. But is this always the case or can an event be caused by another event that occurs later in time? The questions of how to characterize backward causation and, more importantly, on which basis one could infer the existence of backward causal relationships, is a central topic within the philosophy of causation. There are several possible ways to support ordinary causal hypotheses on empirical grounds, the most promising of which is probably the method of experimentation. If one can bring about events of type E by bringing about events of type C, then this is typically seen as strong evidence for the hypothesis that C-events are causally relevant for E-events. However, if one is interested in establishing a backward causal hypothesis, a number of worries and possible objections immediately come to mind. Even if one were able to find that manipulating C-events regularly co-occur with E-events lying in their past, it might, for example, intuitively be more plausible to raise concerns over the experimental setup than to accept the backward causal hypothesis.

In this paper we approach the problem of how to establish backward causal hypotheses from a new perspective. We propose that by subscribing to an interventionist understanding of causation a la Woodward (2003) one can avoid the problems mentioned which typically come with testing for backward causation—we argue that interventionism provides a basis for establishing such causal backward hypotheses on empirical grounds. In particular, we argue that because the interventionist theory of causation which inter-defines causation and intervention, the problematic task of finding evidence for backward causation can be reduced to the task of confirming several ordinary and unproblematic causal hypotheses.

When Less is (thought to be) More: Toy models, minimal models, and exploratory models

Tuesday, February 26, 14.30 – 15.10
Tagungsraum, Seminargebäude 106

Axel Gelfert
Techn. University Berlin

Scientific models, according to one important line of philosophical analysis, aim at representing real-world target systems, even if they only ever do so imperfectly. If a model is based on an underlying theory, its derivation will typically require abstraction and idealization; that is, we need to omit certain aspects of the full theoretical description of the target system and idealize the relations between the component parts that remain. In the absence of a theory, background knowledge and various heuristics are employed in order to construct viable models that put us in touch with a target phenomenon. What most of these accounts have in common is the assumption that we can unproblematically decompose models into their 'accurate' and 'inaccurate' parts (or, following Mary Hesse's early work on scientific models, into their 'positive' and 'negative' analogies with the relevant target systems). Yet much of the recent philosophical work on scientific models calls

this overall picture of how models map onto (aspects of) their target systems into question.
Indeed, the very 'decompositional strategy' outlined above may legitimately be called into question, on the grounds of the 'complex interaction of various modelling assumptions and idealizations' (Rice 2018). Furthermore, many contemporary models across the natural and social sciences -- from physics to biology to economics -- stand in an even more tenuous relationship with real-world target systems, and deliberately so. In the physics of correlated systems and phase transitions, *minimal models* are being employed that have been variously described as 'thoroughgoing caricatures of real systems' or even as 'really look[ing] nothing like any system [they are] supposed to“represent”' (Batterman and Rice 2014); recently, *toy models* -- that is, models so idealized and simplified that they border on being ‘stylized’ accounts of a single aspect of a target phenomenon -- have begun to receive renewed attention (Reutlinger, Hangleiter and Hartmann 2018). Finally, *exploratory models* have been credited with a range of epistemic functions, including that of exploring 'how-possibly' explanations (Gelfert 2016). What these recent accounts appear to have in common is a belief that, sometimes, in scientific inquiry 'less is more': by freeing models from specific empirical, representational, and predictive constraints, they can take on novel functions and may even provide insights that could not otherwise be easily obtained. And yet, there seems to be an air of paradox about this observation: how can such 'impoverished' models allow us to explain and understand the behaviour of *any* real-world systems? The key to this question, I argue, lies in recognizing that models frequently explore the *modal structure* of theories and phenomena; that is, they help us understand what is, and isn't, possible within a certain segment of the real world. This is why, for example, minimal models do not *just* generate a range of possible explanations, but -- like many toy models in economics -- may also allow for the derivation of impossibility theorems. It also helps explain why the same strategies that may be appropriate in early-

stage ('exploratory') research continue to remain useful once an underlying theory becomes available: exploring theoretical structure on the basis of counterfactual scenarios continues to be of use in furthering our understanding of phenomena in the actual world.

On not Opening the Black Box.
Transparency, Opacity, and the Pragmatics of Artificial Intelligence

Tuesday, February 26, 11.40 – 12.20
Room 24, Seminargebäude 106

Hajo Greif
Technical University of Munich
Warsaw University of Technology

The renaissance of Artificial Intelligence (AI) owes to two seemingly countervailing factors: more powerful and sophisticated computational resources on the one hand, and increasing abstention from claims to simulating human intelligence on the other. The first development is a simple fact, but has epistemologically relevant implications with respect to the computational complexity involved (the 'Black Box Problem'). The second development either manifests itself in a return to the original aim of AI of making computers solve problems that would require intelligence from humans, or in modeling prima facie simple, but embodied and environmentally situated activities. Accordingly, there appears to be a trend in AI towards more complexity on the level of computational abilities, and a trend towards more simplicity on the level of explanatory aims. The question to be discussed in this paper is: how may these developments be related? I will try to answer this question, first, by recourse to pragmatist views of modeling and simulation that highlight – and accept – the methodologically 'motley' and epistemically 'opaque' character of computer simulations (Winsberg, Humphreys). Second, I will use two distinct recent AI approaches as case studies: Behavior-based AI and Deep Learning.
Behavior-based AI starts from the assumption that the root of human cognitive abilities lies in embodied and environmentally situated skills of orientation, locomotion and exploration of objects in their surroundings. These skills are modeled in bottom-up fashion, using expressly simple and decentralized architectures and algorithms. These skills are supposed to be the foundation from which

capacities of abstract reasoning emerge, again in bottom-up fashion. Conversely, Deep Learning approaches are inspired by the structure and functions of the human nervous system but do not typically seek to explain them. Instead, partial formal analogues of human neuronal activities are used to detect patterns in data sets that are often too large and too complex to be tractable for human observers.

The instructive contrast between these two approaches can be characterized as follows: in Behavior- based AI, transparency is bought at the cost of simplifying model and target system – which gives rise to the 'scaling problem': will the model also explain more complex cognitive abilities, and still remain tractable? In Deep Learning, the ability to computationally capture complex problems is bought at the cost of epistemic opacity, often coupled with reduced explanatory aspirations. Moreover, a core principle of AI and computer science still cherished by Behavior-based AI is partly abandoned here: the notion of breaking down a complex problem into simple, solvable, and tractable arithmetical routines.

With respect to the 'Black Box Problem', opacity of the model will defeat any approach that seeks explanations of human cognition or other phenomena, as explanation requires epistemic transparency.

However, opacity of the model need not defeat an approach that is exclusively committed to producing workable applications. If the purpose of AI is the "simulation of cognitive processes" (Feigenbaum/Feldman 1963), some key features of these processes have to be represented by the simulation. If, however, AI intends "to construct computer programs which exhibit behavior that we call 'intelligent behavior' when we observe it in human beings" (ibid.), the means of producing that behavior do not serve as the target system of the model but as its, pragmatically justified, resource.

Against Mechanistic Imperialism in the Domain of Psychology

Monday, February 25, 16.30 – 17.10
Room 25, Seminargebäude 106

Bojana Grujicic
Humboldt-University of Berlin

Several proponents of ”new mechanistic philosophy“ have recently extended the framework of mechanistic account of explanation onto the domain of psychology, claiming that models in psychology proposed to explain our cognitive capacities are explanatory in virtue of being models of neural mechanisms, and as such they are incomplete – genuinely explanatory models in psychology are neural mechanism sketches (Piccinini & Craver 2011). Besides this descriptive claim, they put forth a normative claim too – psychologists should strive to provide mechanistic explanations, since that is the only way to uniquely determine the causal structure of the system. Unless they provide mechanistic explanations, psychologists are supplying us with merely descriptive or phenomenal models – the claim I label, following Weiskopf (2011), “mechanistic imperialism”.

Mechanists insist that to explain capacities our models need to pinpoint mechanisms on the lower level that are underlying the capacity in question – to capture the causal structure of the system we need to go about “vertically” in our explanatory endeavors. Staying on the “horizontal” level of functional models, traditionally employed in explaining capacities, will not suffice lest we provide merely descriptive models. I argue based on the very tenets these mechanists are ascribing to – namely, manipulationist account of causation, that we do not need to go vertically one level down in order to explain a capacity.

I argue that the thesis of mechanistic imperialism is incompatible with that of manipulationist account of causation. If causation is a matter of difference-making to Y by X, so that the value of variable Y would change if we changed the value of X by a suitable – “ideal”

intervention, as manipulationism has it (Woodward 2003) and mechanists in the debate ascribe to (Craver 2007), then any model capturing so-understood causal structure of the system has to be taken as explanatory – and not merely mechanistic models.
To analyze the claim of models in psychology being neural mechanism sketches, I take up face recognition model proposed by Bruce & Young (1986). A model is a mechanism sketch if it describes some of the internal details of the mechanism (Craver & Kaplan forthcoming). I test the model against the criteria for mechanistic explanation (Craver 2007, supplied by Woodward 2013), and show that it is not a mechanism sketch, since it does not invoke any neural implementational details – it a functional boxological model. However, the model is not merely descriptive, as it captures the causal structure of our cognitive systems. Focusing on the serial relation between three functional modules in the model – face recognition unit, person identity node and name generation unit, I will provide evidence corroborating that each functional module is a difference-maker for the next one in the row. Thus, playing by the rules of mechanists in relying on manipulationism, I will show that the model is explanatory, although it is a "horizontal" model.
If mechanists ascribe to interventionism, imperialism is out of the picture. If they want to remain imperialists, they need to ascribe to a different account of causation. They cannot have both.

Reduction and Neighboring Theories. A new classification of the inter-theoretic relations in physics

Tuesday, February 26, 15.50 – 16.30
Room 22, Seminargebäude 106

Rico Gutschmidt
University of Constance

It seems to be widely accepted that the complicated and miscellaneous inter-theoretic relations in physics do not fit into a single scheme of reduction and that the interesting work to be done is the investigation of special contexts. Accordingly, the notion of reduction usually does not refer to an eliminative relation between theories, but just describes mathematical and conceptual interdependencies.

However, I will argue in my paper that there are indeed examples of eliminative reduction in physics. Hence, I want to propose a definition for a relation of reduction that ensures that the reduced theory is not needed anymore for a complete description of the world. According to this definition, the reducing theory must be able to explain all the phenomena that can be explained by the reduced theory. However, this indirect reduction will be complemented with a direct part that compares the mathematical and conceptual structure of the theories in order to make sure that the indirect reduction is not just a coincidence. In a second step, I will argue that there are many pairs of theories that are directly connected in terms of their mathematical and conceptual structure but that do not fulfill the indirect condition of an eliminative reduction, i.e. the first theory is not able to explain all the phenomena that can be explained by the second. To distinguish such cases from the case of eliminative reduction, I will propose to call this relation 'neighborhood of theories'.

This new classification of inter-theoretic relations will then be applied to the different theories of gravitation. First, I will point out that Galileo's law of falling bodies and Kepler's laws of planetary motion are neighboring Newton's theory of gravitation. In addition, I will show that Newton's theory of gravitation is neighboring general relativity. Second, I will argue that Newton's theory of gravitation is able to explain both, the phenomenon of falling bodies and that of the planets' motion in the solar system, and in fact more precisely than Galileo's law of falling bodies or Kepler's laws of planetary motion respectively. Hence, in terms of my proposed definition, these theories are eliminatively reduced to Newtonian gravitation. However, I will finally point out that although Newton's theory of gravitation is neighboring general relativity, it is not eliminatively reduced to the latter. There are many phenomena explained by Newton's theory of gravitation but lacking an explanation by general relativity because the field equations are not even numerically solved for these cases. Hence, at least at the moment there is no reduction in the sense of my definition. Moreover, I will argue that there are fundamental limitations for explanations by general relativity without the systematic use of Newtonian concepts.

All in all, this shows that there are some examples of eliminative reductions but also many cases of neighboring theories without reduction. Accordingly, the differentiation between neighborhood and reduction proves to be a fruitful classification of inter-theoretic relations in physics.

Representations in Cognitive Science: An argument against naturalization

Wednesday, February 27, 11.40 – 12.20
Room 26, Seminargebäude 106

Ori Hacohen
Hebr. University Jerusalem

"It has become almost a cliché to say that the most important explanatory posit today in cognitive research is the concept of representation. Like most clichés, it also happens to be true." (Ramsey 2007, p. xi). Cognitive science and cognitive neuroscience have been dominated by explanations that appeal to components of the mind or the brain as internal representations or signals. This might be taken as evidence for a representationalist view of the mind- "that postulating representational (or 'intentional' or 'semantic') states is essential to the theory of cognition" (Fodor & Pylyshin 1988, p. 7). The mainstream representationalist view also holds that the representations posited in cognitive explanations are *natural* representations, that have their contents essentially and intrinsically. I will offer an argument against this view.

Conventional representations, such as public signs and linguistic symbols, do not arise naturally- their intentionality and their contents are defined by external cognitive agents and derived from these agents' intentions. The mainstream representationalist view aims to explain how mental representations are different- what it means to be a *natural* representation and how content can be intrinsic. The project of finding a naturalistic theory of content has been one of the central themes of philosophy of mind in the last decades. Most naturalistic theories subscribe to the idea that the occurrence of some natural process is a necessary condition for something to become a natural representation (evolution is the common example for such a process). I will argue that any commitment to the necessity of a prior process will contradict a more basic representationalist commitment- that representations are essential to cognitive explanations. The heart of the argument is that the occurrence of any prior process is itself *not* essential to cognitive explanations. Therefore, any view that defines representations by the occurrence of a prior process will find that the existence of representations is also not essential to these explanations. Finally, I will also offer some basic intuition for an alternative representationalist view. This view follows an ascriptionist line of thought that has traditionally been most identified with Dennett (1987), but Egan's recent work (2014) on the subject is much more closely related. On the alternative view, the representations featured in cognitive explanations are not natural and their contents are not intrinsic or essential. Instead, their intentionality is derived from the scientists that use these explanations.

Benefits and Limitations of Including Scientists' Accounts in Philosophical Analysis

Wednesday, February 27, 15.10 – 15.50
Room 22, Seminargebäude 106

Nora Hangel
University of Twente

Recently empirical philosophy has started to analyze interviews with scientists in order to inform philosophically relevant questions in social epistemology and philosophy of science (Wagenknecht, 2016: Hangel & Schickore, 2017). The questions concern how knowledge is generated, decisions concerning theory choice, multiple uncertainties, and questions of validation. Although social processes within science have been debated for many decades, the focus on the micro-structure of generating knowledge concerning science-in-the-making is still a newly evolving field.

To include scientists' accounts when individually and collaboratively generating knowledge, philosophy of science has drawn on ethnographic and sociological methods and expanded its toolbox to qualitative research methods. One challenge is finding a fruitful relation between descriptive and normative accounts from researchers and integrating them into a philosophical framework or argument. For instance, scientists describe what they do and refer to norms, but do their reflection counts as a normative account of their practice or merely a descriptive account of norms? As such, my work is a meta-scientific dialogue between methodological reflections of scientists and the philosopher's desire to account for empirically informed rational reconstructions of science.

From a naturalized approach from the inside of science I analyzed scientists' accounts and their reflections about their methods using qualitative data analysis. However, the conceptual level of the analysis enabled me to de- and re-contextualize scientists' accounts on the individual level and interpret them according to the relevant research contexts, e.g., procedures of validation. The interviews were reused from a previous collaboration of the author, involving a heterogeneous set of scientists from the USA, UK, and Germany. The interviewees were selected according to different career stages, academic ranks, and disciplines. For this presentation I focus on data from more than 50 experimentally working scientists from the natural sciences and social sciences.

I will argue first that qualitative methods enable us to identify epistemically relevant variables in the process of doing science. Second, analyzing interviews from scientists is a means of describing how cognitive, pragmatic, and social values influence scientifically relevant decisions. Third, this method enables us to develop a more accurate understanding of the actual issues relevant to scientists' decisions when generating knowledge. After discussing the limitations of this approach, I argue that for an empirically informed philosophy of science it is indispensable to develop reliable, systematic methods in order to keep in touch with problems relevant to scientists and philosophy of science.

Towards a Generalized Patchwork Approach of Scientific Concepts

Monday, February 25, 12.20 – 13.00
Room 26, Seminargebäude 106

Philipp Haueis
University of Bielefeld

Patchwork approaches hold that scientists subdivide their concepts often implicitly into several "patches" to describe and explain the investigated part of reality efficiently. For example: depending on the measurement procedure, the concept "hardness" picks out different properties, such that the meaning of "hardness" is rendered differently (Wilson 2006). These local applications and the procedures delineating them form different patches of "hardness". Patchwork approaches have been defended via detailed case studies of central concepts in mathematics and physics (Wilson 2006), chemistry (Bursten 2016), evolutionary biology (Love 2013, Novick 2018) and neuroscience (Author forthcoming). What the case-based approachs do not address, however, is whether there are discipline-independent reasons why scientific concepts develop a patchwork structure. It is therefore currently unclear whether data-driven bottom-up patchwork approaches can be generalized. It is consequently also unclear whether such practice-based approaches provide a genuine alternative to general philosophical accounts of scientific concepts such as essentialism (Ellis 2001) or holism (Diez 2002).

In this paper, I argue that patchwork approaches can indeed be generalized once we focus on the functional roles that concepts can fulfil across different scientific disciplines (Brigandt 2011). Concepts allow researchers to identify different phenomena (description) and provide a mechanism to correlate their descriptions with features of the world (reference). Concepts are building blocks for the systematic categorization of observed cases (classification) and for generalizations which describe unobserved cases (generalizability). They help scientists to determine why observed phenomena occur (explanation), when they occur and how to control them (prediction and control). An analysis of functional roles reveals which features are shared across discipline-specific patchwork approaches. For example: comparing cases from physics, neuroscience and chemistry reveals that measurement procedures are important determinants for how scientists use conceptual patchwork structures to refer to different properties, and that the scale of inquiry (e.g. spatial resolution) determines how concepts aid the systematic classification of functional kinds in a domain. Focusing on functional roles also provides a possible basis of comparing a generalized patchwork approach to essentialist or holist accounts of scientific concepts. Such an approach could capture the systematic relations between functional roles more parsimoniously, because it neither requires a universal reference relation to natural kinds to explicate how concepts generalize (essentialism) or a general theory to explicate how concepts allow prediction and explanation (holism).

How Strong is the Argument from Inductive Risk?

Tuesday, February 26, 11.40 – 12.20
Tagungsraum, Seminargebäude 106

Tobias Henschen
University College Freiburg

The so-called argument from inductive risk (AIR) says that
(1) any scientist s rejects or accepts hypotheses qua scientist,
(2) s accepts (rejects) hypothesis h iff s can assign a probability p to h and decides that p is (not) sufficiently high to warrant the acceptance of h,
(3) s's decision whether p is (not) sufficiently high presupposes value judgments,
(4) therefore, s makes value judgments qua scientist.
In this form, AIR goes back to Rudner (1956: 2). Hempel (1965: 92) speaks of the "inductive risk" of accepting (rejecting) false (true) hypotheses. Accordingly, the derivation of (4) from (1) – (3) has become known as "argument from inductive risk".
Jeffrey (1956: 237) objects to (1) that "the activity proper to the scientist is the assignment of probabilities […] to […] hypotheses", and not the acceptance or rejection of hypotheses. Levi (1969: 47) can be read as objecting to a purported ambiguity in (2) and (3): that the decision referred to in (2) is a decision about what to believe, while the decision referred to in (3) is a decision about how to act, that only the latter presupposes value judgments, and that the scientist qua scientist only needs to decide what to believe.
Despite these objections, theorists participating in the debate typically think that (1) – (4) are true. Rudner (1953: 4) anticipates Jeffrey's objection when suggesting that assigning probability p to hypothesis h is the same as accepting the hypothesis h1 that the probability of h is p, and that accepting h1 presupposes non-epistemic values. Jeffrey (1956: 246) later responds that assigning probability p to h is not the same as accepting h1. But Jeffrey's response is usu-

ally taken to leadinto a regress of assigning probabilities to probability assignments ad infinitum.

Douglas (2016: 614-5) and Wilholt (2009: 94) argue against Jeffrey that scientists should accept or reject hypotheses because they are responsible for the actions that are taken on the basis of these hypotheses. Wilholt (2009: 95-6) argues against Levi that his conception of a decision about what to believe “presupposes a sense of purity of epistemic activity that is exaggerated and unrealistic”. And many participants in the debate cite Hempel as a proponent of the truth of (1) – (4).

The paper will argue against

(a) Rudner that Jeffrey’s response doesn’t lead into an infinite regress: that a possible regress stops after justifying a probability assignment by a characterization of the probability distribution and the underlying experimental design.

(b) Douglas and Wilholt and with Schurz (2013: 324-331, 2014: 77) that a look at scientific practice suggests that scientists should accept or reject hypotheses only hypothetically.

(c) Wilholt that Levi’s conception of a decision about what to believe is accurate in the case of fundamental physics, and that there is no a priori reason why the same conception should not be applicable in the special sciences.

(d) many participants in the debate that Hempel is unlikely to endorse (1) or (4).

Crossing Domains: The role of the translator in the spread of scientific innovations

Tuesday, February 26, 11.00 – 11.40
Room 26, Seminargebäude 106

Catherine Herfeld
University of Zuerich

Scientific innovations are essential for progress in science. They are commonly considered successful when they find application in a broad variety of central problems. Before a new scientific idea is successfully applied, however, it usually has to be transferred within and across different, both preexisting but sometimes also newly forming, scientific domains. In this transfer process, a tension must be balanced between the novelty of an innovation on the one hand and its alignment with existing theoretical frameworks and practices on the other. By asking how scientific innovations are transferred and subsequently applied across different scientific domains, I analyze how this tension is resolved in practice. In particular, it is argued that the successful spread of a scientific innovation involves a process of 'translation' (Kuhn 2013 [1977]) of the innovation into the language of the target domain. This paper aims at further unpacking what this translation process entails. By drawing on Paul Humphrey's concepts of 'theoretical' and 'computational templates,' I analyze what Humphreys has termed the "construction process" in the case of applying a novel theoretical idea within and across scientific domains (Humphreys 2002, 2004).
I argue in line with Humphreys that the translation of a scientific innovation requires domain-specific knowledge in order to select appropriate idealizations and abstractions to justify the modification procedure, interpret the template, and finally apply it to a specific novel problem (Humphreys 2008, 174). But the focus on the transformative element of translations leads me to furthermore suggest that beyond subject-specific knowledge in the domain of applica-

tion, one also needs field-specific knowledge of the domain in which a theoretical template originated. To develop a theoretical into a computational template that can be applied to different domains, such field-specific knowledge enables the scientist to engage with the template in the first place; it allows the scientist to recognize the potential of a particular for a specific problem in her area, to modify and then apply it accordingly. I furthermore discuss the skills and expertise required on the side of the scientist for translating a novel piece of knowledge – a theory, a model, theorem, or an equation, for example – for its application to multiple problems in different domains. In the case of scientific innovations, translation involves adopting a novel idea while at the same time sustaining compatibility with previous research. Hence, such what I call 'translators' can be understood by drawing on the concept of "interactional experts" introduced by Collins and Evans (2002, 2007). Interactional experts have the ability – by engaging with experts of a particular area of specialization – to converse in a language that extends beyond the accustomed skill set and expertise of their own scientific domain. They are thereby able to engage with and scientists and their problems of other specialized scientific domains without themselves being part of that domain.

I support my analysis by discussing the particular case of the role that translators played in the diffusion of axiomatic choice theories in the second half of the 20th century across the social and behavioral sciences. Such theories reached a wide scope of application and can as such be considered as originating in a successful scientific innovation. I show that one crucial condition enabling this spread had been the presence of a translator having the skills to bridge the gap between the formal-mathematical theories of rational choice and the various areas of conventional scientific practice in which the theories eventually became applied. The analysis allows us to offer some general insights into the conditions under which scientific innovation disseminate across scientific domains, an under-researched area in philosophy of science. Furthermore, I discuss the implications of my finding for the design and organization of successful interdisciplinary research environments. Finally, the analysis contributes to our understanding of fruitful intra- and interdisciplinary research.

Computational Complexity as Evidence for the Epistemic Value of Deduction

Wednesday, February 27, 14.30 – 15.10
Room 24, Seminargebäude 106

Francisco Hernández Quiroz (UNAM)
Nancy Abigail Nuñez Hernández
(Mathematical Center for Advanced Studies)

Deduction is at the core of the majority of mathematical proofs. However, according to a pervasive philosophical tradition that goes back to logical positivism and still influences many philosophers, psychologists, and some other cognitive scientists, deduction has no epistemic value as a source of new knowledge because it adds nothing new to our knowledge. This paper challenges that philosophical tradition arguing that new knowledge is gained when a computationally complex deductive problem is solved. The amount of computational resources necessary to come to the solution of NP-complete problems reveals that it is very unlikely to know its solution just by knowing the axioms that imply it, as it is assumed by those who follow the philosophical tradition mentioned earlier. For instance, coming to know the solution of a SAT problem is hard even though the subject knows the axioms of propositional logic, so new knowledge is gained when that kind of problem is solved. By showing that deduction could be a source of new knowledge, this paper aims to shed new light on the epistemic value of deduction and to pave the way for a more robust conception of deduction.

Interventionism and Non-Causal Dependence Relations: New work for a theory of supervenience

Monday, February 25, 11.00 – 11.40
Room 25, Seminargebäude 106

Vera Hoffmann-Kolss
University of Cologne

1. Introduction

If Joanne stops her car, this can cause Alice to stop her car. However, Joanne's stopping her car is not a cause of Joanne's stopping her 15-year-old Toyota. Likewise, if a room has a temperature of 15°F, this can cause water pipes to freeze. However, the room temperature's being 15°F is not a cause of the temperature's being below the freezing point. More generally, causation is a relation between distinct events, whereas dependence relations between conceptually, mathematically or logically related events do not qualify as causal.

The aim of this paper is to discuss how interventionist theories of causation can meet the challenge of distinguishing between causal relations and non-causal dependence relations.

2. Interventionism and the Criterion of Independent Fixability

According to Woodward's interventionist account of causation, causes make a difference to their effects. A variable X is classified as causally relevant to a variable Y iff there is an intervention on the value of X which changes the probability distribution of Y (Woodward 2003). If the variables under consideration are not required to satisfy any further con- straints, logical or conceptual relations may be misclassified as causal. For instance, the value of the variable describing whether Joanne stops her 15-year-old Toyota depends partially on the value of the variable describing whether Joanne stops her car.

One can rule out many such cases by requiring that variables standing in causal re- lations to each other must satisfy the criterion of Independent Fixability (IF) proposed by Woodward: a set of variables V Satisfies IF iff all combinations of the values of the variables contained in V are metaphysically possible (Woodward 2015, 316).

However, IF does not rule out all problematic cases. For instance, the following two variables satisfy IF:

A: 1 if Person A is an aunt; 0 otherwise M: 1 if Person A is a mother; 0 otherwise

The probability distribution of M depends partially on the value of A. However, this correlation occurs for conceptual reasons only (positive values of A and of M presuppose that A is female), and there is obviously no causal relation between S and M.

3. Putting the Supervenience Relation Back to Work

The supervenience relation has come out of fashion in recent years, since many philoso- phers think that it should be replaced by something metaphysically more substantial, such as the grounding relation. I argue that even though this paradigm shift is well-justified in a number of contexts, the present context is an exception. I develop a criterion of the dis- tinction between causal and non-causal dependence relations, according to which varia- bles standing in causal relations to each other must not have overlapping supervenience bases. This criterion, I argue, can adequately cover the cases raising trouble for IF as well as further problematic cases.

Family Resemblances and Essentialism

Monday, February 25, 11.00 – 11.40
Room 23, Seminargebäude 106

David Hommen
Heinrich Heine University Düsseldorf

According to the classical-realist theory of meaning, multiple particulars fall under one and the same natural concept by virtue of

sharing at least one common property. Wittgenstein famously argues against the classical doctrine that most, if not all, natural concepts do in fact
not pick out a single character or set of characteristics that is the same in all the members of their extension. He observes that most concepts are "family concepts" which apply by virtue of "a complicated network of similarities overlapping and criss-crossing" (PI, § 66) among their instances.
Wittgenstein's family resemblance model is commonly taken to evince a nominalistic, anti-essentialist stance on the meaning of terms. In this talk, I try to show, however, that Wittgensteinian family resemblances, far from being purely conventional constructions, actually are compatible with a certain brand of essentialism, namely a broadly Aristotelian theory of species and genera, where genera are understood in terms of potentialities and species are actualizations of these potentialities. Aristotle's theory, in turn, informs contemporary theories of conceptual structure such as frames and conceptual spaces.

The Relevance and Weight of Scientific Evidence in Policy Decisions

Wednesday, February 27, 15.50 – 16.30
Room 26, Seminargebäude 106

David Hopf
Leibniz University Hannover

In modern society, many decisions of public interest are—or at least should be—informed by scientific research: think of common regulatory issues such as the approval of medical drugs which depend on clinical trials, but also of even more complex problems such as nuclear waste disposal or the response to anthropogenic global warming. In all these cases, answers to pressing questions cannot be given by a single study. Instead, they depend on a multitude of findings, that is, on the overall state of research concerning the issue at hand. In this paper, I address one of the principal problems of aggregating scientific evidence for the purpose of informing public policy: the question of what makes a piece of evidence relevant for decision-makers.

In her 2012 paper Weighing Complex Evidence in a Democratic Society, Heather Douglas introduces the issue of relevance as the first of two major challenges for the public use of scientific information. In the article, however, she focuses on the second problem, which she calls the "weight-of-evidence challenge": how do we assess the relevant evidence and arrive at a verdict? In the first part of my talk, I will give a succinct overview of her text, in which she discusses various approaches to evidence aggregation in light of the epistemic and democratic requirements of this task, which is complicated by the dissimilarity of available evidence. At the end of the talk, I will argue that her eventual conclusion is only partly correct: Douglas claims that the weight-of-evidence challenge should be addressed with an explanatory account; that is, aggregating evidence would come down to an issue of complex scientific inference. I respond that, while scientific inference is indeed part of the challenge, we also need to consider non-scientific, normative inference.

To substantiate this claim, I return to the challenge of relevance in the second part of my talk. I provide a schematic account of several different uses of the concept "scientific evidence" in the context of informing decision-making. The scientific inferences discussed by Douglas already depend on multiple types of evidence, such as primary research evidence, testimonial evidence, and secondary evidence. But the results of this inferential step—that is, research hypotheses being accepted or rejected—themselves become the evidence available to the decision-makers. On this second level, scientific evidence enters the action-guiding decision-framework underlying the policy decision at hand: evidential claims, together with normative rules, endorse specific courses of action. What marks a piece of evidence as relevant, then, is its direct or indirect support for or against these action-guiding inferences.

In the third and final part of my talk, I argue that this notion of relevance implies that the weight-of- evidence challenge also applies to the second level, that is, we also need to assess which normative inferences are supported by the available evidence. Lastly, I respond to several possible objections concerning the types of decisions under consideration, the disambiguation of the two levels, and why as well as how evidence should be weighed beyond hypothesis acceptance.

A Constructive Critique of Sugden's View on Economic Model

Tuesday, February 26, 16.30 – 17.10
Tagungsraum, Seminargebäude 106

Paul Hoyningen-Huene
Leibniz University Hannover

In a series of papers from 2000 on, Robert Sugden has analyzed the epistemic role of theoretical models in economics. His view is that these models describe a counterfactual world that is separated from the real world by a gap. This gap has to be filled if the model should have an epistemic function for our understanding of the real world. According to Sugden, this gap "can be filled only by inductive inference". The putative inductive inference that Sugden constructs leads "from the world of a model to the real world", based on "some significant similarity between these two worlds". In philosophy, the "significant similarity" that Sugden correctly adduces for the legitimacy of inductive steps has been spelled out as common membership to a natural kind. However, for Sugden's inductive step to be legitimate, the union of the appropriate set of models with the appropriate set of real target systems should form a natural kind, which is certainly not the case. For instance, with respect to causality model cities are utterly different from real cities, contrary to Sugden: models may at best represent the real causality.

In fact, the inferential step from models to reality is abductive, as Sugden correctly notes. However, he misunderstands abduction as a

sub-category of induction. Yet, abduction does not lead to generalizations as induction, but to risky explanatory hypotheses.
The abductive inference from a model to reality has the following form:
(i) x has property Z (empirical finding),
(ii) Situations of type A have property Z (model),
therefore
(H) x is a situation of type A.
If (H) is true, then (H) together with (ii) explain (i). However, the abductive step to (H) is risky, because it may also hold:
(ii*) Situations of type B have property Z, with B ≠ A.
Based on (i) and (ii*), one gets by abduction the alternative explanatory hypothesis
(H*) x is a situation of type B, with B ≠ A.
Thus, all one gets by an abductive step is a potential explanation (sketch). The only way to obtain the actual explanation is by showing that the model situation is sufficiently similar to reality and by excluding all alternative explanations. Thus, the real explanation is not distinguished from alternative explanations by an intrinsic property of high credibility, as Sugden assumes, but by its comparative advantage against competitors.
The upshot is that a theoretical model in economics (like Schelling's) never directly explains any particular empirical case (this resolves Reiss' "explanation paradox"). Instead, a model allows for abductive generation of a sketch of a potentially (perhaps surprising) explanatory hypothesis. In order to transform this potential explanation sketch into an actual explanation, the sketch must be elaborated and its empirical adequacy be shown. The latter crucially contains the exclusion of alternative potential explanations. This may be accomplished by showing that the empirical conditions necessary for plausible alternative mechanisms to work do not obtain.

Kant and Einstein on the Causal Order of Time

Tuesday, February 26, 16.30 – 17.10
Room 22, Seminargebäude 106

David Hyder
University of Ottawa

This paper aims to establish structural connections between the theory of time of Kant's Critique of Pure Reason and Einstein's Theory of Special Relativity. The connections in question derive from an internal connection between Kant's theory of relativistic kinematics, outlined in the latter's Metaphysical Foundations of Natural Science, and what Robert Palter called its "relativistic analogue"—the kinematic theory of Einstein's 1905 paper on special relativity. Since both theories also involve causal laws characterizing time-order, and both are internally related through their kinematics, these causal laws are internally related as well.

The talk has five parts:

(1) A presentation of Kant's theory of kinematics. This section shows that the Foundation's "Phoronomy" applies the Principle of Relativity to the space and time manifolds of the "Transcendental Aesthetic" to produce a family of "inertial frames", which replace absolute Newtonian space-time. The materials for this approach are located in texts of Euler, Kästner and Lambert, which Kant either owned or corresponded with the author about.

(2) A presentation of two causal principles, which, according to Kant, ground properties of time that are not fixed by the kinematics alone: the concepts of "later," "earlier," and "simultaneous." The first causal law (the "2nd Analogy of Experience") asserts that events that are "later" causally depend on events that are "earlier", but not vice versa. The second one (the "3rd Analogy") asserts that simultaneous events are bicausally connected, by distance-forces

acting instantaneously, in contrast to the first sort of causal relations, which act over a temporal interval.

(3) I then follow Robert Palter in arguing that the theory presented in (1) is a limiting case of the theory of Special Relativity. The parallelogram law that Kant derives involves a Euclidean triangle. If it is rendered hyperbolic, the kinematics obtained is that of Einstein's 1905 paper. I show how this is inevitable given a strict homology between Einstein's 1905 deduction of his kinematic parallelogram law, and the proof offered by Kant to obtain his own.

(4) I then show how the two causal laws of (2) become incompatible through this change in the space-time geometry. The conjunction of Kant's 2nd Analogy with his frame-construction produces a "Law of Mechanics", asserting that all causes of a present event must lie in the past in space. I show that the Principle of Locality is the dual of this Law, since it is obtained by combining the same law of causality with Lorentz-Einstein kinematics. I then show that this law is not cotenable with the dual obtained by mapping the 3rd Analogy, the Principle of Simultaneity, onto this same space-time, since the Locality forbids those instantaneous connections between distant points which the Principle of Simultaneity requires.

(5) In a final section, I connect the theory outlined above to Einstein's later objections to quantum mechanics, arguing that the contradiction derived in the first EPR paper (1935) rests on this same failure of cotenability, between what Planck had called, in the 1920s, the Nah- and Fernwirkungsprinzipien.

Time-sensitivity in Science

Wednesday, February 27, 16.30 – 17.10
Room 24, Seminargebäude 106

Daria Jadreškić
Leibniz University Hannover

This paper examines the role of time-sensitivity in science, a notion introduced in Daniel Steel's comment (2016) on Elliott and McKaughan (2014). Their discussion centers on the role of non-epistemic values in theory assessment and the epistemic status of speed of inference, and is based on two case studies: expedited risk assessments of the toxicity of substances and rapid assessment methods for wetland banking. I argue that: 1) speed supervenes on ease of use in the cases they discuss, 2) speed is an epistemic value, and 3) Steel's account of values (2010) doesn't successfully distinguish extrinsically epistemic from non-epistemic values. Finally, I propose an account of time-sensitivity. It is a feature of problems to be solved in their particular contexts, a feature recognized by an implicit or explicit judgment about a desired or expected time-frame of having a result, which gives rise to concerns about efficiency and influences methodological choices.

I start by arguing against Elliott's and McKaughan's view that the two tokens, speed and ease of use, independently of one another represent the same type in the cases they discuss, namely a non-epistemic value that sometimes takes priority over epistemic ones in assessing scientific representations. Besides the problem of labeling speed and ease of use as non-epistemic, I claim that in both cases speed supervenes on simplicity and ease of use, i.e. the methods are simple and easy to use in order to be fast and enable fast (soon and many) applications. Then I argue along the lines of Steel why speed ought to be considered an epistemic value, contrary to Elliott and McKaughan. I part from Steel in that I don't think that the epistemic/non-epistemic distinction suffices for explaining decision

making in science. After that I try to account for time-sensitivity by using Steel's distinction between extrinsically and intrinsically epistemic values (2010), where epistemic values are broadly construed. According to it, time-sensitivity might me a value manifested by social practices. I show that his distinction fails to distinguish between extrinsically epistemic values and non-epistemic values, especially when their influence on research is legitimate.
Time-sensitivity isn't captured well in either of the contrasting notions of value distinctions. We implicitly or explicitly assign a degree of time-sensitivity to problems, a judgment about when we want or expect to have results from a particular instance of research, but it is neither a feature exclusively external nor internal to science, but a requirement of efficiency which is both truth seeking and temporally constrained.

Dispositions in Biomedical Ontologies

Monday, February 25, 15.10 – 15.50
Tagungsraum, Seminargebäude 106

Ludger Jansen
Ruhr-University Bochum

Organisms come into existence, grow, and change. Some organisms are susceptible to certain diseases or poisons, others have developed a resistance to these. Biological species change and adapt to their environments. The world of biology features a wide variety of dispositions of different types. The paper will give an overview of the variety of phenomena and show how the formal ontological analysis of dispositions can help to represent biomedical knowledge in a structured way

On Evidentiary Standards for Dietary Advice

Wednesday, February 27, 15.10 – 15.50
Room 23, Seminargebäude 106

Saana Jukola
Bielefeld Universitiy

Nutrition science is a prime example of research at the interface with society. It is a field that does not only aim at satisfying our epistemic curiosity, but also at solving important societal problems. The importance of nutrition science in informing policy and practical decision-making is encapsulated in dietary guidelines. These guidelines, issued, for instance, by universities, governmental agencies, and health associations, are statements aimed at guiding public policy, informing debates on the effects of nutrition on the population at large, and instructing nutrition professionals on the basis of current scientific and medical evidence. Recently, the trustworthiness of these guidelines and the science that forms their basis has been questioned. As a group of Dutch nutrition scientists, medical doctors, social scientists and philosophers declared in a recent paper, "nutrition science appears to be in crisis and is currently confronted with a public reluctance to trust nutritional insights" (Penders et al. 2017, 2009).

A notable part of the critical discussion around nutrition advice is focused on the evidence the guidelines rely on. The critics of official nutrition guidelines argue that the lack of evidence from randomized controlled trials (RCTs) makes public health advice unreliable. This line of criticism relies on the ideals of evidence originating from evidence-based medicine. It holds that observational studies, i.e., mainly case-control studies and prospective cohort studies, cannot form a solid basis for practical recommendations because of their epistemic limitations. This paper evaluates this line of criticism against population-level nutrition guidelines and shows that it is problematic to criticize nutrition recommendations for not being

based on RCTs. The argument is two-fold. First, I argue that, due to practical, ethical, and methodological issues, it is difficult to conduct rigorous RCTs for acquiring evidence that is relevant for achieving the goals of population-level nutrition recommendations. Second, I will show that given the non-epistemic goals of the recommendations, the evidence assessment has to take into consideration the values of the target population and risks that follow from acting on the basis of the evidence. Consequently, the criteria of acceptable evidence should be adapted to the goals of the practice and the practical, ethical, and methodological constraints of the situation. Epistemic robustness and social robustness (Carrier & Krohn 2016; Carrier 2017) can serve as criteria for evaluating advice
that has to be given under uncertainty often faced while science-based practical guidelines are given.

Revitalizing Realism

Monday, February 25, 15.10 – 15.50
Room 24, Seminargebäude 106

Samuel Kahn
Wuhan University

In this paper I give a novel argument for realism. In particular, I maintain that if the KK-principle is jettisoned, then recent disputes about what I shall call meta-level arguments (like the no-miracles argument and the pessimistic meta-induction) can be set aside, and I argue that realism is nonetheless justified on the basis of what I call first-order considerations that can be extracted only from historiography.
The paper is divided into three sections. In the first, I set out my terms. I explain realism in terms of being justified or warranted in taking as true scientific theories literally construed insofar as these theories refer to unobservables. I then distinguish realism from what I call meta-realism, where the latter consists in being justified

or warranted in taking as true positive knowledge claims about scientific theories literally construed insofar as these theories refer to unobservables. This leads me to a discussion of the KK-principle, according to which knowledge implies knowledge that one knows (Kp—>KKp).

In the second section, I introduce the realist no-miracles argument (NMA) and its antirealist corollary, the pessimistic meta-induction (PMMI). I maintain that both of these arguments are problematic. On the one side, I rehearse recent criticisms from antirealists that expose flaws in realist attempts to articulate and, subsequently, patch up the NMA (the modus tollens response). On the other side, I point out that even sophisticated forms of the PMMI remain subject to a well-known realist retort: followed to its logical conclusion, such antirealism proves too much, devolving into radical skepticism (i.e., defeating knowledge claims about observables as well as unobservables) that risks becoming self-undermining .

In the third and final section, I use the foregoing exposition to make good on my thesis. I argue that the NMA represents an attempt to do too much: meta-realism is unnecessary for realists because jettisoning the KK principle enables realism to be based on first-order arguments (generally made by practicing scientists) and general epistemological considerations such as appeals to testimony and legitimate appeals to authority (generally in the case of laypeople), all of which are made “on the ground” and, thus, are not susceptible to any non-self-undermining PMMI. I conclude by noting that despite my critical attitude toward the NMA, my strategy does allow for a much more modest version of this argument to remain viable in special cases.

Network Explanations in Psychiatry: Interventions and causal relations?

Monday, February 25, 14.30 – 15.10
Room 25, Seminargebäude 106

Lena Kästner
Ruhr-University Bochum

Network approaches in psychiatry highlight the importance of combining behavioral, psychological, neurophysiological, genetic, and environmental factors into holistic explanations of mental disorders (e.g. Borsboom, Cramer & Kalis 2018). Such integrative explanations laudably emphasize that various factors may contribute to psychopathology. However, incorporating all of them into a single network may not be unproblematic.

First, it runs risk of blurring the distinctions between causes and realizers of mental disorders. Both are certainly relevant to mental disorders and should thus figure in our network models. But they are not relevant in the same way. This is important both clinically (in selecting treatment options) as well as philosophically (e.g. if we are interested in figuring out metaphysical relations). Yet, it is epistemically challenging to separate what is underlying or realizing a phenomenon from what is causing it. While I do not think we should limit network models to any one metaphysical relation, we should in principle be able to distinguish between them.

Woodward's (2003) interventionist account of causation provides the currently most promising and empirically adequate tool for picking out causal relations by difference-making. Interventionism may thus be hoped to distinguish causes from realizers and other relevant factors in network models (cf. Kendler and Campbell 2009, Campbell 2007). Problematically, however, relying on interventions alone to distinguish between casual and non-causal dependencies is begging the question. For applying interventionism requires that we exclude non-causal dependence relations from the start. In other

words: we have to know where non-causal dependencies obtain, rather than being able to find them by means of interventions.
In this talk, I will examine which tools may be used to analyze and distinguish between different relevance relations in network models. I introduce difference-making interventionism (DMI), which detects relevance in general rather than causation, and thus avoids question-begging. As such, DMI mirrors the empirical reality of psychiatry even more closely than Woodward's original interventionism. To disambiguate between causes and other difference-makers, DMI needs to be supplemented with additional heuristics. These might include resources familiar from discussions of interventionism in the context of mechanistic constitution and mental causation. As a starting point, I suggest we might employ (i) an adapted manipulability criterion (Craver 2007), (ii) heuristics based on multiple experiments (Baumgartner and Gebharter 2016, Baumgartner and Casini 2017, AUHTOR a), and (iii) considerations based on temporal order and scientific domain (author b, Baumgartner and Gebharter 2016).

Explanation and the Rise of Model Independence

Tuesday, February 26, 11.40 – 12.20
Room 22, Seminargebäude 106

Martin King
University of Bonn

Since the Higgs boson discovery in 2012, there have been no indications of physics beyond the standard model (BSM). Concrete BSM models have been pushed to the edges of their parameter spaces and as a result model-independent approaches, such as effective field theories (EFTs), have become increasingly popular in particle physics. The EFTs employed in new physics searches at the Large Hadron Collider (LHC) are what are known as bottom- up EFTs and are quite distinct from the top-down ETFs that have been more thoroughly treated in the philosophical literature. The aim of the paper is to examine the role of bottom- up EFTs in potentially explaining new physics.

The paper proceeds by first arguing that top-down EFTs can be understood as abstract and idealised versions of higher-energy (or UV-complete) theories.Similar points have been argued in philosophical work on EFTs and renormalisation group equations, such as (Batterman 2002), (Batterman and Rice, 2014), (Bain, 2013), and others. I will briefly present the Fermi theory of beta decay and make the case that claims about its being explanatory can be supported by an abstraction and idealisation process from the SM. The paper then contrasts this in three ways with a bottom-up EFT, in particular the Standard Model EFT (SMEFT). For the SMEFT, the UV-complete theory is not known and it is not known where the theory will break down and new physics will become relevant. And so unlike the first distinction is that there is no guarantee about the predictive, and hence explanatory, power of a bottom-up EFT.

The second distinction is that the SMEFT is not an abstraction or a idealisation of the SM, and cannot borrow its explanatory power. One constructs the SMEFT by expanding the SM Lagrangian with an infinite series of effective operators that parameterise the effects of BSM physics. Physicists make certain assumptions about UV physics in order to reduce the number of operators, but which operators are actually relevant is not yet known. The SMEFT is not optimised, per (Strevens, 2008), as it contains many irrelevant operators and cannot highlight explanatorily relevant features.

A third difference is that the SMEFT plays a very different role in the eventual explanation of new physics, namely, it is only a stepping stone on the way to an explanation. This can be seen in how it is used in LHC searches. Indications of new physics will result in non-zero coefficients for some set of the operators, which physicists can then use to constrain the structure of a BSM model that may explain the physics that underlies the deviation. While the SM serves as the UV-complete theory that grants Fermi theory its ability to explain beta decay, the SMEFT is probative, tentative, and used to constrain the structure of future BSM models. Thus, EFTs can differ significantly with respect to their ability to explain, depending on whether they are top-down or bottom-up.

A Mechanistic Conception of Metaphysical Grounding

Tuesday, February 26, 14.30 – 15.10
Room 23, Seminargebäude 106

Thomas Kivatinos
City University of New York

A dominant theoretical framework in philosophy of science employs the notion of mechanistic dependence to elucidate how “higher-level,” less fundamental phenomena depend upon and arise out of “lower-level,” more fundamental phenomena. To elucidate the same thing, literature in metaphysics employs the notion of grounding. As I argue, regardless of whether the notion of mechanistic dependence or the notion of grounding is used to theoretically portray how higher-level phenomena arise out of lower-level phenomena, what is captured by such portrayals is the same.

Thus, these notions pick out the same features of the world. With this as my basis, I identify the notion of grounding with the notion of mechanistic dependence, and thus, construct a mechanistic conception of grounding.

Since mechanistic dependence is understood in terms of mechanisms, my conception frames grounding in terms of mechanisms. Moreover, the contemporary notion of mechanisms is shaped by how mechanisms are represented via the mechanistic models and mechanistic explanations provided by science. Thus, because my conception grounding identifies grounding with mechanistic dependence and thereby frames grounding in terms of mechanisms, this conception suggests that the notion of grounding is to be tailored to and constrained by the mechanistic models and mechanistic explanations provided by science. This leads the mechanistic conception of grounding to reject a wide variety of conventional claims about grounding, and thus, to offer a treatment of grounding that is highly revisionary.

To reinforce the plausibility of the mechanistic conception of grounding, I discuss how grounding and mechanistic dependence are associated with explanation. Whereas mechanistic dependence is associated with mechanistic explanation, grounding is associated with grounding explanation. For each kind of explanation, some higher-level phenomenon P is explained by appeal to some low-level phenomenon that P arises out of. As I argue, these forms of explanation can be plausibly identified. This greatly supports the mechanistic conception of grounding. For if grounding explanations employ the notion of grounding and mechanistic explanations employ the notion of mechanistic explanation, and these forms of explanation can be identified, this suggests that these explanations employ the same notion. And, just as the notions of grounding and mechanistic dependence capture the same connection between higher-level and lower-level phenomena, grounding explanation and mechanistic explanation do so as well.

To argue that the mechanistic conception is to be preferred to standard conceptions, I argue that my conception offers a powerful defense of grounding from recent criticisms. Critics have argued that the notion of grounding is the product of exceedingly speculative metaphysical inquiry rather than science-based metaphysics. On the mechanistic conception of grounding, however, the notion of grounding turns out to be centered in science. Since the conception shows that grounding can be treated as mechanistic dependence, and since science is the primary basis for positing mechanistic dependence, science can be treated as the primary basis for positing grounding. Further, since the mechanistic conception tailors the notion of grounding to the mechanistic models and mechanistic explanations employed by science, the conception shapes the notion according to how scientific theories represent the world. Grounding is thus exonerated from the charge that it is untethered to science. This stands in stark contrast to standard conceptions of grounding which cannot offer any such defense to the relevant criticisms of the theoretical legitimacy of grounding.

Methodological Signatures in Early Ethology and the Recent Problem of Qualitative Terminology

Tuesday, February 26, 12.20 – 13.00
Room 25, Seminargebäude 106

Anna Klassen
Ruhr-University Bochum

What is the adequate terminology to talk about animal behaviour? Is vocabulary referring to mental or emotional states anthropomorphic and should therefore be prohibited or is it a necessary means to provide for an adequate description and should be encouraged? This question was vehemently discussed in the founding phase of ethology as a scientific discipline and still is relevant in today's debates in Applied Ethology concerning the assessment of animal welfare. Hence, a terminological and methodological question of biology became also a bioethical one. This multidimensionality can be grasped by using the concept of methodological signatures, developed by Köchy, Wunsch and Böhnert (Köchy, Wunsch, Böhnert 2016). It is designed to analyse animal research by examining parameters such as paradigmatic species or concepts of the human-animal-relationship. This facilitates a systematic comparison of different research programmes, especially in ethology.

In my talk, I first present the research of Konrad Lorenz (1903-1989) and Nikolaas Tinbergen (1907-1988), who are considered the founding fathers of ethology. Second, I analyse their methodological signatures, concentrating on the research techniques and theoretical notions. Thereby, I will show how the question of terminology is deeply embedded in a network of theoretical, practical and ontological concepts. Third, I establish a typology of core assumptions that are necessary to consider in discussing qualitative terminology in ethology. Fourth, I shed light on the possibility of using this typology to examine current frameworks of ethological research in animal welfare.

Understanding Climate Change with Process-Based and Data-Driven Models

Wednesday, February 27, 14.30 – 15.10
Room 26, Seminargebäude 106

Benedikt Knüsel
ETH Zurich

Understanding is an important epistemic aim of science (de Regt 2009). In climate science, process-based computer models are one of the essential tools to advance understanding, mainly by providing explanatory information. Using climate models for this purpose rests on two assumptions, namely that (a) the relationships are adequately represented in the model, and that (b) no important causal factor is missing from the model (Parker 2014). These assumptions are made based on the coherence of the models with background knowledge and specifically their rooting in scientific theory, which is also one of the key reasons for confidence in climate model projections (Baumberger et al. 2017). Hence, although climate models suffer from epistemic opacity and confirmation holism (Lenhard and Winsberg 2010), they can be useful for increasing the understanding of the climate system.

In recent years, increasing volumes of data have opened up pathways for new, data-driven methods (Pietsch 2016). Data-driven models like supervised machine learning can produce accurate predictions of complex phenomena without necessarily allowing understanding of the inner-workings of the model (Mayer-Schönberger and Cukier 2013; Pietsch 2015). It has been argued that causality is the reason for the predictive success of data-driven models (Pietsch 2016), which indicates that they can provide explanatory infor-

mation, too. Since data-driven models can be trained when understanding of the target system is insufficient for constructing process-based models, machine learning could be an interesting tool to advance understanding of ill-understood phenomena.
Using the example of attribution of climate change in temperature data, I discuss how these two modeling approaches can improve our understanding of the climate system and compare the kinds of understanding that they can provide. I argue that machine learning can yield explanatory information based on assumptions similar to (a) and (b), which requires that the analyzed dataset cover sufficiently many configurations of the target system (Pietsch 2016). However, in the case of data-driven models, assumption (a) generally holds only if assumption (b) holds, too. Furthermore, the lack of transparency of many machine learning algorithms makes it difficult to use background knowledge to affirmatively argue for assumptions (a) and (b). Hence, the type and level of epistemic opacity of machine learning tools poses a more serious problem for obtaining explanatory information than the epistemic opacity of process-based models. I suggest that this difficulty of data-driven models can be overcome to some extent by using hierarchies of data-driven models with respect to their opacity, whose outputs need to be interpreted in light of the relevant background knowledge.
Finally, using the example of damages from hurricanes on human settlements, I show that another set of data-driven methods, unsupervised machine learning tools aiming to create homogeneous clusters within a dataset, can provide unification. Even if such unificatory information is considered non-explanatory, as Gijsbers (2013) has argued based on Lipton (2009), I show that it can advance understanding of complex targets. This is possible without assumptions such as (a) and (b), but it again requires that the dataset cover sufficiently many configurations of the target system.

Genealogy as a Scientific System of Order

Tuesday, February 26, 11.00 – 11.40
Room 25, Seminargebäude 106

Michael Koerner

In my talk I will define genealogy as a scientific system of order, show how it is combined with typological systems of classification and compare examples of its use in biology, linguistics, archeology and sociology.

Each order is built to prioritize one point of view over multiple others. The task of a scientist is to choose the order that best assists his or her research. Therefore, the system should rest on categories that are supposed to be of the highest theoretical value. The most common order in that regard is the typology, the classification by explicitly defined types. It is exemplified by the periodic table or the spectral classes of stars, which put a property like the number of protons or the emissons of ionized matter at the center. These properties are causally and statistically linked to a number of other interesting properties, and thus deserve to structure the way in which phenomena are presented.

Genealogical systems of order rest on the assumption, that for some research interests *descent* is the category of the highest theoretical value. This seems to be the case any time when objects can be concieved of as reproductions that replace each other. The resulting system differs logically from typologies, as pointed out by Michael Ghiselin (1997). It uses proper names instead of general ones, leading to the absence of necessary properties. As a consequence it doesn´t allow inference of properties across levels in the way a typology does. To put it ontologically: A genealogy´s subdivisions are parts of concrete objects, while a typology´s subdivisions are logical classes.

As soon as genealogies are used to explain changes in meaning over time, they use synchronic systems to attribute meaning. These systems are typological pyramids of purposes with continua-

tion of a perishable structure on top and layers of subordinate purposes beyond. They formulate conditions of equivalence for objects that replace each other. This allows to judge the ability of varying objects to contribute to structural continuation.
Combinations of genealogy and typology have led to progress in a wide range of sciences. I will argue that Charles Darwin, the linguist August Schleicher, the archeologist Oscar Montelius and the sociologist Max Weber have all used genealogy in a way that still influences their disciplines. The following table sums up the analogies I want to draw:

Non-Causal Explanatory Asymmetries

Monday, February 25, 11.40 – 12.20
Room 25, Seminargebäude 106

Daniel Kostic
University Bordeaux Montaigne

Any good and successful explanation has to be asymmetric, otherwise, it's circular. In causal explanations, the explanatory asymmetry simply follows the direction of causation, i.e. we generally tend to think that causes explain their effects, and not the other way around. For example, the changes in the air temperature cause the mercury
To expand and thus to climb up the glass column in a thermometer, but it seems absurd to say that expanding of the mercury causes the changes in air temperature. If the changes in air temperature are true causes of the expending of the mercury in the thermometer, then this kind of asymmetry will have to be preserved across all the counterfactuals related to that explanation. That is why the counterfactual information and explanatory asymmetries are central in distinguishing good from bad explanations.
But in topological explanations in neuroscience, it is not immediately obvious what can ground the explanatory asymmetry.
I show that there are two ways to think about non-causal direction-

ality in describing counterfactual dependency relation and how they can ground the explanatory asymmetry, i.e. the “vertical” and the “horizontal”.

By “vertical”, I mean counterfactual dependency relation which describes dependency between variables at different orders in the mathematical hierarchy, e.g. a derivation of a scaling exponent in the Kleiber’s law, from organism’s dimensionality (Saatsi and Pexton 2013).

On the other hand, by “horizontal” I mean the counterfactual dependency relations that are at the same order in the mathematical hierarchy. I show, using the examples of network controllability, that such “horizontal” counterfactual dependency relation can obtain between topological properties and a network representation of the brain.

In the vertical case, the directionality seems to be straightforward to understand, it follows the direction of the derivation of a mathematical property from a more abstract mathematical structure.

But in horizontal cases, such directionality is not easy to distinguish. For example, there is a sense of directionality between the changes in the brain to the changes of the variables in a network model, a bottom-up directionality. The problem is that not all of these changes will be explanatory of the target system. In the Watts and Strogatz Model (1998), adding a node to a cluster would change the value of the clustering coefficient variable, but it won’t affect the dynamics of the system or the global topology of the network. It seems plausible that only a change that involves the introduction of long-range connection would change the topology from regular to small-world and thus enable more efficient signal processing in the brain.

In “horizontal” cases such directionality may be conceived in terms of constraining dependency relations between topological structures and the network representation of the brain dynamics. In this sense, even though the topological and dynamic variables are at the same organizational level and the same order in the mathematical hierarchy, the constraining relations between them give the explanation its directionality.

Individuation Practices in Studies of Host-Parasite Systems

Tuesday, February 26, 12.20 – 13.00
Room 23, Seminargebäude 106

Nina Kranke

Traditionally, philosophers of biology have analyzed questions about individuality in terms of metaphysics and favored theory-centered approaches. More recently, however, the interest in studying scientific practices and discussing epistemological questions surrounding individuality has increased (e.g. Pradeu 2012; Kovaka 2015; Chen 2016). Following these practice-based epistemological approaches I analyze individuation practices in recent studies of host-parasite systems focusing on helminths (i.e. different species of parasitic worms) and their hosts.
My analysis of different studies suggests a pluralistic account of individuation practices and corresponding notions of individuality. In some studies, the host is seen as an immunological individual that causally interacts with the parasite (cf. Pradeu 2012). In these cases, the researchers are usually interested in the host's immune response and/or in the parasite's strategies to circumvent the host's immune system. Other researchers aim at understanding how the parasite influences its host (e.g. its behavior) and how changes in the host's genotype alter the parasite's phenotype. In these studies, the helminth is understood as an extended phenotype of its hosts, or vice versa. From this perspective, the host and the parasite together form an evolutionary individual (cf. Ereshefsky & Pedroso 2015). Some authors conceptualize helminths as parts of the "multibiome" (Filyk & Osborne 2016), the host's intestinal ecosystem. These researchers are interested in the interactions among members of the multibiome as well as between the multibiome and

its host. Here, helminths are seen as constituents of an ecological individual (cf. Huneman 2014). In the context of Darwinian Medicine, helminths are understood as integrated parts of the host organism that contribute to the proper functioning of its immune system. In this case, the researchers are interested in investigating the contribution of helminths to human health.

From my analysis I conclude that different individuation practices are not necessarily linked to certain fields nor to the researchers' disciplinary identity or personal preferences. They also seem quite independent form the organisms that are studied. Instead, the case studies show that different individuation practices correspond to different epistemic aims (cf. Love & Brigandt 2017). The four above mentioned notions of individuality differ in their degree. While ecological individuality is a rather weak type of individuality, the degree of integrity and cohesion is much higher in individual organisms.

Recent work in history of science also allows a comparison between individuation practices in parasitology in the late 19th and early 20th century with more recent practices. While a plurality of individuation practices could already be found during this period, host and parasite were generally seen as separate entities that are causally connected (Osborne 2017; Love & Brigandt 2017). My analysis suggests that contemporary researchers use a wider range of individuation practices. In addition to treating host and parasite as separate organisms or separate units of research, contemporary research also conceptualizes host-parasite systems as wholes. I hypothesize that this expansion of individuation practices is a result of the incorporation of evolutionary and ecological perspectives into studies of host-parasite systems in the course of the 20th century.

How our Mind Enables and Constrains the Scientific Theories we Formulate

Monday, February 25, 16.30 – 17.10
Room 22, Seminargebäude 106

Alexander Krauss
London School of Economics

The paper addresses the broad question of how our cognition and senses both enable and constrain the particular theories about the world we are able to develop – and outlines the broader implications of these constraints on our theories. In doing so, it provides a new cognitive account to one of the central, long-standing debates of philosophy of science about what the boundary is between the scientific and the non-scientific. This topic of demarcating the sciences has been widely discussed by historians, philosophers and sociologists of science. However, they have often taken theoretical, conceptual and philosophical approaches. I provide an empirical account that is grounded in the actual limitations and evolution of our cognition and senses.

Activity Causation

Monday, February 25, 15.50 – 16.30
Room 26, Seminargebäude 106

Beate Krickel
Ruhr-University Bochum

Many defenders of the new mechanistic account base their views on an ontology consisting of entities (objects, parts) and activities (operations, functions). The notion of an activity is supposed to capture the dynamic, active, temporally extended, and most importantly causal nature of mechanisms. It remains unclear how the notion of an activity is related to the notion of causation, and whether we

can use the former in order to make sense of the latter.
I will provide an account of causation in terms of activities that starts from Salmon's and Dowe's process theories of causation by analogously invoking the notion of an active entity-involving occurrent (i.e., higher-level processes) and that of a mechanistic interaction (i.e., higher-level causal interactions). I will show how activity causation can account for higher-level causation, and how one can solve common problems of process theories such as the relevance problem and the omission problem.

Digging the Cannels: On how to separate nature and culture

Monday, February 25, 12.20 – 13.00
Room 24, Seminargebäude 106

Maria Kronfeldner
Central European University

There is a broad, so-called interactionist consensus in contemporary philosophy of the life sciences. It states that there are plenty of interactions between nature, culture and the environment, be it at the level of ontogenetic development, epigenetic inheritance or phylogenetic evolution (see, for instance, Tabery 2014 and Schaffner 2016 for the developmental level, Jablonka and Lamb 2005 for the epigenetic level, as well as Richerson and Boyd 2005 and Lewens 2015 for the evolutionary level.)
Sometimes this interactionist consensus has been combined with the claim that this challenges the very distinction between nature and culture (see, for instance, Lock 2013 or Meloni 2016). The most explicit attack against the belief in nature and culture stems from turn-of-the century versions of developmental systems theory, e.g. in Griffiths and Gray (1994, 2001). "So-called channels," Griffiths and Gray (2001: 196) claimed, are "not generally independent of one another."

In this talk, I argue that this is wrong. Though it might well be true that the epigenetic channel is quite dependent on the genetic channel, it is clearly wrong that culture (i.e., the cultural channel) is generally dependent on nature (i.e., the biological channel).
The interactions between nature and culture, be it at the developmental, epigenetic or evolutionary level, still allow for diggin' the channels, i.e. for distinguishing a biological and cultural channel of inheritance as independent. These channels are empirically discernable separate sub-systems of the sum total of developmental resources traveling between individual organisms and influencing development and evolution.
To argue for that I show that the two channels are (a) near-decomposable, (b) show different temporal orders, (d) and can change independently. In other words, culturally inherited developmental resources is independent of nature in three senses: they are (a) near-decomposable from biologically inherited developmental resources; they change in a quite different manner and (c) does so autonomously.
I will then discuss why the latest developments in epigenetics, in particular 'parental effects' are not providing a challenge for the distinction between nature and culture as different channels of inheritance.

On the Exploratory Function of Agent-Based Modelling

Tuesday, February 26, 15.10 – 15.50
Tagungsraum, Seminargebäude 106

Meinard Kuhlmann
University of Mainz

Thomas Schelling's (1971) famous model of segregation is an early example of agent-based modelling (ABM) which began to flourish in the 1990s with large-scale Sugarscape and various models for the study of cooperation as well as social dilemmas. Hamill and Gilbert

(2016, p. 4) characterize an agent-based model as “a computer program that creates an artificial world of heterogeneous agents and enables investigation into how interactions between these agents, and between agents and other factors such as time and space, add up to form the patterns seen in the real world.” The use of computer simulation and the high degree of idealization involved are in fact often seen as the hallmarks of agent-based modelling. Thus it is not surprising that ABM is confronted with a number of objections. One objection is that agent-based models are so highly idealized that they fail to represent the real world in any reasonable sense. Artificial worlds may be fun to play with but they are no serious description of our real world. Another objection is that they only provide how-possibly explanations, i.e. at best they show how observed patterns may have come about but there is no conclusive evidence that this is really what happens. Moreover, a completely different model may well yield the same result.

I will show that focussing on an often neglected, but crucial function of agent-based modelling not only brings the distinctive character of ABM to the fore but is apt to rebut these objections. Building on Gelfert’s (2016, ch. 4) account of the exploratory uses of scientific models my main thesis is that the crucial function of ABM is to explore how observed facts about social and other systems causally depend on certain structural properties of their interactive organisation, in the absence of an accepted underlying theory. One core step towards this goal is robustness analysis (cf. Kuorikoski et al 2010), albeit in a somewhat different manner than usually. The idea is to study how the agents interact in different models, all of which reproduce the observed facts. Even though the models may (and actually should) be quite different, there are often common very general properties in how the agents interact across different models. For instance, in ABM of financials markets one crucial structural feature turns out to the very possibility of switching strategies, depending on the success of other traders. If the observed facts are only robustly reproduced when such a structural feature obtains in the model, this is a strong indication that it is not merely a matter of

how-possibly but a real causal feature.
My approach is apt to show that the lack of representation of highly idealized agent-based models does not undermine their value because representation is not their primary function anyway. In a last stept I will briefly compare my account with two related approaches, namely concerning minimal models (Rice and Batterman 2014) and toy models (Reutlinger et al. 2018).

The case against factorism

Monday, February 25, 11.00 – 11.40
Room 22, Seminargebäude 106

Gijs Leegwater (University of Rooterdamm)
Fred A. Muller (University of Rotterdamm)

We discuss the case against Factorism, which is the standard assumption in quantum mechanics that the labels of the single-particle Hilbert-spaces in direct-product Hilbert-spaces of composite physical systems of similar particles refer to particles, either directly or descriptively. We mount a defence of descriptive Factorism, by introducing the concepts of snapshot Hilbert-space and Schrödinger-movie

The History and Interpretation of Black Hole Solutions

Tuesday, February 26, 14.30 – 15.10
Room 22, Seminargebäude 106

Dennis Lehmkuhl
University of Bonn

The history and philosophy of physics community has spent decades grappling with the interpretation of the Einstein field equations and its central mathematical object, the metric tensor. However, the community has not endavoured a detailed study of the solutions to

these equations. This is all the more surprising as this is where the meat is in terms of the physics: the confirmation of general relativity through the 1919 observation of light being bent by the sun, as well as the derivation of Mercury's perihelion, both depend much more on the use of the Schwarzschild solution than on the actual field equations. Indeed, Einstein had not yet found the final version of the field equations when he predicted the perihelion of Mercury. The same is true with respect to the recently discovered black holes and gravitational waves: they are, arguably, tests of particular solutions to the Einstein equations and how these solutions are applied to certain observations. Indeed, what is particularly striking is that all the solutions just mentioned are solutions to the vacuum Einstein equations rather than to the full Einstein equations. This is surprising given that black holes are the most massive objects in the universe, and yet they are adequately represented by solutions to the vacuum field equations.
In this talk, I shall discuss the history and the diverse interpretations and applications of two of the most important (classes of) solutions: the Schwarzschild solution and the Kerr solution. I will address especially the history of how the free parameters in these solutions were identified as representing the mass and angular momentum of isolated objects, and what kind of coordinate conditions made it possible to apply the solutions in order to represent point particles, stars, and black holes.

On Metaphysically Necessary Laws in Physics

Wednesday, February 27, 11.00 – 11.40
Room 22, Seminargebäude 106

Niels Linnemann
University of Geneva

Fine (2002) argues that natural necessity can neither be obtained from metaphysical necessity via forms of restriction nor of relativi-

zation – and therefore pleads for modal pluralism with respect to natural and metaphysical necessity. Aiming at applying Fine's view to the laws of nature, Wolff (2013) provides illustrative examples to this effect with specific recourse to the laws of physics: On the one hand, Wolff takes it that the equations of motion can count as examples of physical laws that are only naturally but not metaphysically necessary. On the other hand, Wolff argues that a certain conservation law
obtainable via Noether's second theo- rem is an instance of a metaphysically necessary physical law. I show how Wolff's example for a putatively metaphysically necessary conservation law fails but argue that so-called topological currents can nevertheless count as metaphysically nec- essary conservation laws carrying physical content. I conclude with a remark on employing physics with respect to metaphysics, and metaphysical notions within physics.

Social Emergence and Unpredictability

Wednesday, February 27, 11.40 – 12.20
Room 25, Seminargebäude 106

Simon Lohse
Leibniz University Hannover

In recent years, the debate between individualists and holists in the social sciences has often been framed in terms of reduction and emergence. So far the discussion has focused predominantly on the possibility and the existence of emergent causal powers or downward causation in the social world (e.g. Sawyer, 2005; Elder-Vass, 2010). In this talk, however, I will attempt to shed some light on another aspect of social emergentism: I will discuss theories of social emergence that are based on the idea of the unpredictability of social systems and analyse related problems. After introducing the core idea of in-principle-unpredictability as a mark of strong emergence (and its epistemic rationale), I will examine and criticise three arguments that have been put forward to defend the idea that social systems are – in principle – unpredictable and therefore emergent. First I will discuss Niklas Luhmann's (1995) notion of intransparent historical systems as the basis of unpredictability in the social sciences. Next I will analyse the possibility of transferring C.D. Broad's idea (1980[1925]) of epistemic disconnected, and hence emergent, micro-macro-laws to the social sciences. Third I will scrutinize Achim Stephan's idea (2011) of grounding the unpredictability of social systems in their (alleged) deterministic-chaotic nature. I will show that

(a) methodological individualists have the explanatory resources to counter Luhmann's argument,

(b) that the current state of social scientific knowledge makes it very hard to make a plausible case for the epistemic disconnectedness of micro-macro-laws in the social sciences, and (c) that the case for deterministic chaos in the social sciences is hitherto a mere possibility without much empirical warrant.

In the last part of my talk I will attempt to add a pragmatic dimension to these results and to sketch some consequences for the viability of reductionist approaches in the social sciences. I will conclude my talk with a brief recommendation for social emergentists who aim to make a case for the unpredictability of social systems, namely that they should put more effort in a serious empirical corroboration of their case (as opposed to getting tied up over the conceptual case for the in-principle-possibility of social emergence due to unpredictability).

The Consequences of Consequentialism for Values and Science

Tuesday, February 26, 15.50 – 16.30
Room 26, Seminargebäude 106

Charles Lowe
University Osnabrück

Recent years have seen a proliferation of arguments meant to show that social, ethical, and other non-epistemic values may legitimately influence, inform, or constrain nearly all facets of scientific practice. However, even the most enthusiastic supporters of such claims acknowledge that not every type of value-based influence is legitimate, and thus that a principled criterion is required whereby legitimate and illegitimate influences may be distinguished.

According to one prominent school of thought that Daniel Steel (2017) has recently termed aims approaches, value influences are legitimate when they promote the aims of scientific inquiry, where the aims in question may be both epistemic and non-epistemic in nature

and the former do not necessarily trump the latter. Steel argues that adopting such a criterion is problematic because the actions required to promote some non-epistemic aim may entail a corruption of science. This issue is supposedly avoided by so-called epistemic constraint approaches, which permit value influences only "so long as they do not override certain standards of adequate science."

In this talk, I show that the problem Steel identifies for aims approaches is a consequence of the broadly consequentialist criteria they employ and is largely analogous to well-known issues faced by consequentialist views in moral philosophy. More crucially, I also argue that, pace Steel, epistemic constraint approaches face similar issues because they tend to rely on consequentialist criteria for determining which kinds of deviation from standard scientific practice constitute a corruption of science or otherwise problematic overriding of standards of adequate science. To illustrate this latter point, I engage with what Steel himself recognizes as one of the most sophisticated attempts to present an epistemic constraint approach, Heather Douglas' (2014) "Moral Terrain of Science".

Douglas is concerned with developing a framework for understanding how scientists might best balance their various duties to good scientific practice, the scientific community, and the broader society within which they live and work. To illustrate the framework's usefulness, she uses it to analyze a recent controversy concerning the suggested withholding of studies about a particularly dangerous strain of flu, for fear that they might be misused to create biological weapons. On her account, such value-motivated deviations from standard scientific practice may be legitimate so long as they do not result in the undermining of "the value of science to society and what it takes to do science with such a value."

In order to explore whether or not epistemic constraint approaches can really avoid the problems attributed to aims approaches, I spell out and assess two possible readings of Douglas' clearly consequentialist criterion. The first of these, which is closely analogous to act consequentialism, can in fact not avoid legitimizing value influences that may result in a corruption of science. The second, more clearly related to a kind of rule consequentialism, seems to fair better concerning the issue identified by Steel, but may bring with it other unwelcome consequences for supporters of non-epistemic values in science.

Meeting in the Middle:
Adapting Qualitative Methods to Philosophical Questions

Wednesday, February 27, 14.30 – 15.10
Room 22, Seminargebäude 106

Miles Macleod
University of Constance

While there has been a recent uptake in interest in qualitative methods in the philosophy of science (see Wagenknecht et al., 2015) many philosophers still remain to be convinced that these methods have an informative role to play. Finding a proper place for qualitative methods requires critical reflection on what is considered relevant to philosophical explanation and prescriptions. At the same time it is also clear that such methods cannot simply be imported from other fields. Rather they need to be adapted to fit philosophical goals and interests. Qualitative methods and philosophy of science need to find a common ground. To explore these issues we rely on experiences gathered from ethnographic studies of model-building practices in systems biology and bioengineering sciences. We illustrate cases in which a qualitative analysis of problem-solving practices is essential for understanding and rationalizing methodological choices within these fields. The rational basis underlying the decisions made only become clear once it is understood what the constraints on problem-solving within the field are and how individual modelers can respond to them. Fine-tuned interview and observational studies of these practices reveal in this case underlying cognitive motivations for the choices made. For instance systems biologists choose to model systems at a particular degree of abstraction and scale insofar as these scales keep the debugging processes they have to rely on to build such models manageable.

However on a purely epistemic account the models produced seem to fail the predictive goals systems biologists ordinarily express, since they lack appropriate scale and complexity. Ultimately the decision to build such models is rooted in cognitive considerations as well as epistemological ones, but such cognitive considerations only become visible after careful empirical investigation. Philosophical investigations relying on publications alone may fail to fully understand why the field pursues practices which would otherwise seem inefficient on rational epistemological grounds.

In general we hypothesize that qualitative methods are of a specific use when contextual factors or information outside of typical philosophical argumentation is relevant to a philosophical point of understanding, explanation or guidance. Such factors may be institutional, social, epistemological or cognitive. None of these factors are necessarily extractable from publications alone. At the same time, qualitative methods, like ethnographic methods in particular, tend to privilege the role of observable social and material factors over the role of the more technical cognitive or methodological factors which might be particularly relevant for understanding the methodological choices and epistemic structures philosophers are interested in. Any methodological approach needs to find a way to reformulate its aims in terms of narrower philosophical concerns by zooming on just those aspects it considers important.

However such an approach also needs to preserve the benefits which flow from the objectivity and openness of a qualitative approach, for finding relevant factors that would otherwise be invisible.

What is a Model-Narrative?

Tuesday, February 26, 15.50 – 16.30
Tagungsraum, Seminargebäude 106

Rui Maia
Bielefeld University

The epistemic roles of narratives in scientific activity have been subject of renewed interest in the current philosophy of science literature (Norton Wise, 2011; Currie, 2014; Kelly and Russo, 2017). Many scholars have observed that scientific researchers make use of narratives in a variety of ways, including in reasoning, in disciplinary interactions and in more specific scientific practices (Morgan and Norton Wise, 2017). Indeed, one of the the main practices in which narratives seem to play an important role is modeling (Hartmann, 1999; Morgan, 2012). This is in an sense surprising since this practice is often associated with formal methods. Yet, it is also very interesting since if we can understand what exactly the role of narratives in more formal settings is, the hope is that we will be able to more easily understand their role in more general practices.

Nonetheless, to be able to understand what epistemic role a narrative plays, one must arguably have an idea of what a narrative is. Here, however, we encounter some difficulties. For scholarly research has often focused on the former instead of the latter (Morgan, 2001; Grüne-Yanoff and Schweinzer, 2008). My aim in this paper is to try to invert this focus. Yet my goal is much more circumscribed. I am interested in narratives as they are used in modeling,

but my scope is limited to the discipline of economics.
My first move to propose a definition of what I call model-narratives. In a nutshell, I claim that a model-narrative is a narrative which is told in conjunction with a model and represents the same causal relations in the same target system as the model. A narrative tout court is a textual representation of an event or a series of events, where an event is a causal transition between states of affairs and series of events are causally related. Put this way the connection between models and model-narratives becomes apparent.
Yet I also want to make it clear that model-narratives are to some extent independent from models. This is my second move. I claim that the representation relation going on in models is different from the one going on in model-narratives. And the reason is that they are two different kinds of representational vehicles which license different sorts of inferences about the targets they represent (Suárez, 2004). The model achieves an analytical representation of the target system, in which we make inferences about certain formal properties and relations of interest (Contessa, 2007). The model-narrative achieves a material representation of the target system, in which we make inferences about certain material aspects in virtue of which the causal relations of interest hold (Cartwright, 2017). Thus recognizing the nature of this distinction is important for any analysis of the role and epistemic worth of model-narratives.

Institutions and Scientific Progress

Tuesday, February 26, 11.40 – 12.20
Room 26, Seminargebäude 106

Chrysostomos Mantzavinos
University of Athens

Scientific progress has many facets and can be conceptualized in different ways, for example in terms of problem-solving, of truth-likeness or of growth of knowledge. The main claim of the paper is that the most important prerequisite of scientific progress is the institutionalization of competition and criticism. An institutional framework appropriately channeling competition and criticism is the crucial factor determining the direction and rate of scientific progress, independently on how one might wish to conceptualize scientific progress itself. The main intention is to narrow the divide between traditional philosophy of science and the sociological, economic and political outlook at science that emphasizes the private interests motivating scientists and the subsequent contingent nature of the enterprise.

The key is to focus on the complex institutional matrix that defines the way that scientists interact in their daily activities and which crucially shapes their outcomes. Scientific activity is undertaken by imperfect biological organisms with a limited cognitive capacity in interaction with artefacts in a specific social context. The scientific enterprise is a social process (Hull, 1988), and it consists of the attempt of the participants in this process to provide answers to puzzles and solutions to theoretical problems (Mantzavinos, 2013, 2016). The scientific enterprise is embedded in the institutional framework of the society consisting of informal and formal institutions. What we call "science" is not a means toward the accomplishment of anything. It is, instead, the institutional embodiment of the processes of constructing and criticizing solutions to theoretical problems that are entered into by individuals in their several abilities and skills. Individuals are observed to cooperate with one another, to compete with one another, to devise representational vehicles for solving problems, to experiment and criticize one another. The network of relationships that emerges and evolves out of this process is called "science". It is a setting, an arena, in which scientists attempt to accomplish their own purposes, whatever these may be.

The talk of "the aim of science" is not simply a false abstraction, it is a seriously misleading oversimplification. Only an agent can have an aim. But the order of activities that we call "science" is not a deliberate arrangement made by somebody, a taxis, to use the classical

Greek word. It is a kosmos, a grown order exhibiting orderly structures which are the product of the action of many individuals, but not the product of a human design. It constitutes an arena of activities which has not been made deliberately – therefore, it cannot legitimately be said to have a particular aim.
The debate about the ways that progress is tied to the aim of science is hence largely misplaced. The only plausible question, on the contrary, is whether and how the diverse aims of individual scientists and scientific organizations produce outcomes in a process of social interaction that are appraised positively with reference to diverse values.

Dark Matter = Modified Gravity? Scrutinising the spacetime-matter distinction through the modified gravity/ dark matter lens

Tuesday, February 26, 15.10-15.50
Room 22, Seminargebäude 106

Niels Martens (RWTH Aachen University)
Dennis Lehmkuhl (University of Bonn)

When applying the laws of gravity to the luminous matter that we observe around us in the universe, one obtains an evolution of that matter which is not empirically adequate---at the scale of galaxies and galaxy clusters as well as at the cosmological scale. We face a dilemma between two options that seem to be obviously distinct: either the matter sector needs to be complemented with non-luminous (i.e. dark) matter (DM), or the gravity/spacetime sector needs to be modified (MG) (or perhaps a bit of both).
In this paper, we investigate what criterion, if any, is supposed to conceptually distinguish DM theories from MG theories. In doing so, we not only draw upon literature on the broader distinction between matter on the one hand and spacetime/gravity/geometry on the other, we also move in the other direction by pointing out the

implications of the uncovered ambiguities inherent in the DM/MG dichotomy for this broader distinction. More specifically, we compare Khoury and Berezhiani's Superfluid Dark Matter with Hossenfelder's Lagrangian formulation of Verlinde's emergent gravity. We extract from the literature a family of candidates for being necessary and/or sufficient criteria for an object being (dark) matter, as well as a similar family of criteria that determine whether an object is a (modified) spacetime. Both of the above theories score (almost) maximally with respect to both families of criteria: both theories are (almost) as much of a dark matter theory as possible, as well as being as much of a modified spacetime/gravity theory as possible.
This case study is a first sign that the distinction between modified gravity and dark matter theories---and by extension the spacetime–matter dichotomy---is much less clear than usually assumed, even before reaching the regime where quantum gravity reigns. This blurring severely undermines the current animosity between dark matter advocates and modified gravity advocates, as well as the substantivalism-relationalism debate (where both camps agree that spacetime and matter are clearly conceptually distinct).

What is the Meaning of Causal Economic Claims?

Monday, February 25, 16.30 – 17.10
Room 26, Seminargebäude 106

Mariusz Maziarz
Polish Institute of Economics

What is the meaning of causal claims put forth by economists? What is the definition(s) of causality accepted by economists? To address these questions, I have systematically reviewed research published in three top economic journals (American Economic Review, Journal of Political Economy, and Quarterly Journal of Economics) between 2005 and 2015, and – after choosing the research that put forth causal implications, described the methods of causal inference employed currently by the mainstream economists. My Cologne presentation focuses on studying the meaning of causal claims voiced by economists by means of referentialist semantics. The main conclusion is that economists as a group do not stick to a single definition of causality but are causal pluralists.

One or Two? A process perspective on pregnant individuals

Monday, February 25, 11.00 – 11.40
Room 24, Seminargebäude 106

Anne Sophie Meincke
University of Southampton

How many individuals are present where we see a so-called pregnant individual? It seems there are exactly two possible answers to this question: a pregnant individual 'is', as it were, either one or two individuals. The standard answer is the latter, mainly championed by scholars endorsing the predominant Containment View of pregnancy, according to which the foetus, or 'foster', resides in the gestating organism like in a container (Oderberg 2008, Smith & Brogaard 2003). The first answer has recently found some potential support in the Parthood View, according to which the foster is a part of the gestating organism (Kingma forthcoming, Kingma 2018). Here I propose a third answer: a pregnant individual is a bifurcating hypercomplex process and, hence, neither two individuals nor one individual but something in between one and two. The Process View, by acknowledging the processual nature of organisms (Meincke 2019a, Meincke 2019b, Meincke 2018, Nicholson & Dupré 2018, Dupré 2012), overcomes the difficulties the Parthood View encounters when combined with the widely-shared view that organisms are substances.

I proceed in two steps. First, I assess the Parthood View of pregnancy. I argue that it rightly opposes the Containment View by stressing the functional integration of the foster in the gestating organism,

but that it (i) operates with a notion of parthood that is either inappropriate or too vague, and (ii) struggles to maintain, in a substance ontological framework, the natural assumption that fosters are entities that continue to exist through and after birth. Second, I present the Process View of pregnancy. I expound the central concept of a bifurcating hypercomplex process and argue that the Process View (i) provides a plausible interpretation of the specific sense in which a foster is a part
of a gestating organism, while at the same time (ii) allowing us to maintain the assumption of the foster's continued existence through birth in a way that does justice to the temporal dynamics of the process of pregnancy.

Moral Modeling

Tuesday, February 26, 15.10 – 15.40
Room 26, Seminargebäude 106

Christoph Merdes
ZiWiS (FAU)

The relationship between intuitions and considered judgment on the one hand and high-level theories of normative ethics such as utilitarianism on the other is one of the central problems of metaethics. The case bares at least metaphorical resemblance to the relationship between theory and data in the empirical sciences. However, as practitioners of applied ethics like to point out, even if one is convinced of one particular high-level theory, nothing follows algorithmically for a given ethical problem (O'Neill, 1987). The mirror image of this problem is that intuitions, even if acceptable as ethical data, cannot directly refute high level theories. The solution I suggest is to employ models as the mediating agent between theory and intuition, following the analysis of models as mediators in the philosophy of science (Morgan and Morrison, 1999).

Spelling out a model-based account of this relationship requires us to commit to some metaethical assumptions, which I shall not defend here. Mainly, it is assumed in the following that intuitions provide data in the following sense: It is possible for a moral agent to form a propositional attitude on the basis of moral intuition. Intuition is viewed as a generally reliable, but fallible measurement device, the development of which is generally guided by, but not reducible to, the agent's moral convictions. Second, cognitivism is assumed, meaing that normative sentences are truth-apt. It is not necessary to accept realism – or antirealism – since either would be compatible with the suggested account.
Let me consider a brief examples to explore the idea of models as media- tors between ethical theory and immediate intuition. Consider the approach to normative political philosophy championed by Nozick (1974): He subscribes to what he understand to be Kantian ethics, based on the autonomy of the individual. However, when it comes to the analysis of the consequences of his fundamental ethical theory in his version of the state of nature, Nozick employs Lockean basic rights to life, liberty and property as his normative standard. How are we supposed to make sense of this argumentative move?
The reconstruction suggested by the analogy to models in science – not stated as such by Nozick of course – is that he utilizes Lockean rights as a model of Kantian ethics. Compared to the notoriously difficult to interpret categorical imperative, Lockean rights provide a more easily applicable standard of evaluation for action; they enable Nozick to compare behavioral patterns in the state of nature with a normative standard and confront that standard directly with intuition. As a matter of course, Lockean rights are not implied directly by Kantian theory. But that is perfectly in line with an account of models that reconstructs them as entities that are semiautonomous both from data (intuition) and theory. However, the choice of the model – Lockean basic rights – is still guided both by deontological ethical theory and by intuitions on the domain of application. Similar to scientific models, moral models are bound to specific do-

mains and applications; fitting a moral theory to a different set of intuitions from another domain likely results in a very different kind of moral model; Lockean basic rights may cover much ground in an account of the state of nature, but be largely inappropriate in the domain of biomedical ethics.
Reconstructing ethical principles as models can also be understood as an explication of the idea of mid-level principles much of biomedical ethics relies on (Beauchamp and Childress, 2001): Practitioners in this area often find high- level theories hard to apply, and intuitions too spurious and arbitrary. There- fore, they prefer to utilize principles such as beneficence and respect for autonomy. However, the actual logical relationship between the levels is not properly understood, begging important epistemological questions; a reconstruction by analogy to scientific models suggests a well-explored set of answers. Whether the account is metaethically fully satisfactory remains to be investigated.
Some normative ethicists may even deny that they are consistent under all conditions of application.

Towards a Process Ontology of Pregnancy: links to the individuality debate

Tuesday, February 26, 11.00 – 11.40
Room 23, Seminargebäude 106

Hannah O'riain
University of Calgary

Pregnancy is a neglected but useful case study for investigating biological individuality. Existing accounts of individuality in pregnancy use substance ontology to define the conceptus as a separate individual (as in Smith and Brogaard's container model, 2003), or as part of its host (Kingma's part-whole claim, 2018, forthcoming). Substance ontology frames the world in terms of static entities; if the biological world is ever-changing, yet composed of substances, persistent personal and organismal identities are puzzling. I argue these substance-based accounts are unsatisfactory because they must distort physiology and avoid answering important questions to provide a definitive ontology. While Kingma's part-whole account is built on more correct physiology than Smith and Brogaard's container model, she still struggles to address whether the foster is part of its gravida before implantation and after birth. She is tentative in proposing the part-whole account because these open questions have bearing on theproduction of a definitive ontology. Kingma recognizes that the metaphysical account we accept has practical consequences – in this case for the autonomy of pregnant women. She and I both argue that we should investigate our meagre sample of ontological accounts of pregnancy critically, and consider replacing them if they are biologically inaccurate and socially harmful.

Nicholson and Dupré (2018) provide a way out of the persistent identity puzzle, criticizing both substance- based conceptions of organisms, and monist approaches to ontology. I apply these critiques to pregnancy. Using Nicholson and Dupré's lens (2018), I resolve several difficulties that substance-based views of individuality encounter in the pregnancy case. Process ontologies are populated by individuals that are more like whirlpools or markets than tangible objects: usefully stable entities that are actively sustained (Dupré, 2014). In this vein, I discuss how there are no useful, clear boundaries between the conceptus and pregnant organism: pregnancy is a complex, intertwined relationship of hierarchical biological processes, including metabolic activities and life cycles. Implantation, birth, and breastfeeding are some of the biological processes that complicate our efforts to carve the world into distinct, static individuals according to any monolithic account of biological individuality.

A process account of organismal and personal identity will provide better tools for biologists and philosophers investigating individuation. Dupré's concept of nested hierarchies of processes allows us to zoom in or concentrate on stabilities that importantly form individual entities, be they framed as parts, wholes or background setting, according to our research question. In the pregnancy case, this clarifies the puzzle of how a foster could be both a part of its gravida and a meaningful individual. Future work to create a satisfactory account of individuality in the context of mammalian ovulation, gestation and lactation would bring up useful themes, empirical grounding and new approaches for understanding biological individuality and organismality in philosophy of biology more broadly; for example, in philosophical conversations about genes, development and species transitions in evolutionary biology. For now, I conclude that individuation in pregnancy deserves careful consideration, and that our ontological investigations of pregnancy ought to include more processual understandings.

Particle Identification through Time of Flight Measurements: Testing Bell's hypothesis on position observations in quantum physics

Monday, February 25, 14.30 – 15.10
Room 23, Seminargebäude 106

Andrea Oldofredi
University of Lausanne

In the paper "On the impossible pilot-wave" Bell showed that observables, Hermitian operators associated to the properties of quantum systems, are emergent notions in the de Broglie-Bohm's theory. In particular, he explained how spin is reduced to the interaction among

particles involved in a given spin-measurement, thus, it should not be considered an intrinsic property of quantum systems. This result has been subsequently generalized to every other observable associated to magnitudes of physical systems, so that only positions are treated as genuine properties of quantum particles. In this regard, Bell concluded that "in physics the only observations we must consider are position observations, if only the position of instrument

pointers. It is a great merit of the de Broglie-Bohm picture to force us to consider this fact”.
This claim had an important resonance and may be translated affirming that in measurement situations every relevant piece of information obtainable about physical systems can be reduced to the spatial arrangement/location of the systems in question. Many authors based on it a radical metaphysical hypothesis concerning particles’ identification: not only observables are not in general genuine properties of quantum particles, but also particles’ identity is an emergent feature dependent on their dynamical behavior, i.e. the different species recognized via experimental observations are just manifestations of different dynamical behaviors. Hence, in order to recognize to what species a given particle belongs to, it is necessary and sufficient tolook at its trajectory.
In this talk I test the validity of Bell’s claim taking into account Time Of Flight (TOF) measurements, an important method used for particle identification in High Energy Physics (HEP) experiments. Looking into the mechanisms of this technique, it will be stated that spatial information is necessary but not always sufficient to recognize particles’ species, so that one necessarily needs further information provided by time in addition to the mere spatial locations of physical systems. Thus, from experimental practice there are counterexamples to Bell’s thesis. Secondly, I aim to analyze the interpretation of TOF measurement and the notions involved in it such as “time of arrival” and “time of flight’ of quantum systems in spatial regions. I will argue that these expressions, which should be literally interpreted in order to explain what we observe in HEP experiments, receive a more adequate treatment in trajectory based approaches to quantum physics. Therefore, TOF measurements may be helpful case studies to shed light on the ontology of quantum physics. In the third place, I will clarify the different meanings that time has in the context of quantum theory following Busch’s distinction.
It will be stated that in TOF measurements one can meaningfully speak about an observable time as a genuine property (or magnitude) of physical systems. Finally, nonetheless, it will be argued that

Bell's claim can be saved endorsing a relationalist view of space-time; in this regard two examples taken from the recent literature in philosophy of physics will be discussed: Esfeld's minimalism and Barbour's shape dynamics.

Is Everything Fine if Natural Kinds are Nodes in Causal Networks?

Monday, February 25, 11.40 – 12.20
Room 26, Seminargebäude 106

Yukinori Onishi (The Graduate University for Advanced Studies)
Davide Serpico (The Graduate University for Advanced Studies)

It is well known that traditional essentialism about natural kinds is untenable in light of the variability characterizing the biological world. Richard Boyd's Homeostatic Property Cluster theory (HPC, henceforth) has been widely adopted as the best theoretical framework for settling the issue. According to HPC, property clusters typically associated with natural kinds are sustained by a homeostatic mechanism shared by each kind's member. This accounts for both the similarity and the variability among different individuals (see Boyd 1991). However, the notion of homeostatic mechanism remains unclear.

Given this ambiguity, some scholars have interpreted the notion of homeostatic mechanism by referring to the notion of mechanism mostly employed in philosophy of mechanistic explanation (Strict Mechanisms, hereafter). According to this notion, a mechanism is a structure performing a function in virtue of its component parts, component operations, and their organization (see Bechtel & Abrahamsen 2005). By interpreting Boyd's homeostatic mechanisms as Strict Mechanisms, Craver (2009) has questioned the suitability of HPC in carving nature at its joints. Craver draws on the apparent impossibility to make clear-cut, unambiguous distinctions between mechanisms: indeed, parts are always parts relative to a decomposition framed by reference to some property or activity displayed by the whole they belong to. Furthermore, a mechanism is always a mechanism for a given phenomenon that plays some role in our explanatory practices (Glennan 1996; Craver 2015). Craver argues that, as far as mechanistic explanation and the identification of mechanism used for it involve these pragmatic elements, the HPC account equipped with the notion of Strict Mechanism is unable to identify objective kinds in nature.

Craver's criticism has led some scholars to reject the notion of homeostatic mechanism. Slater (2014), for example, discards any causal notion of mechanism and proposes a very slender characterization of natural kinds, which he calls Stable Property Cluster account (SPC). While such an account might be welcomed for its light metaphysical load, it also means that it has given up answering some of the questions traditionally asked with natural kinds, and this move may be justifiable given the difficulty that current theories of natural kinds face. Recently, however, Khalidi (2015) has proposed an account of natural kinds that replaces the notion of homeostatic mechanisms with a looser notion of mechanism (Loose Mechanisms, hereafter). According to this view, natural kinds are nodes in causal networks, which is characterized by causal graphs. Khalidi's account seems to point to a nice middle ground between HPC (equipped with Strict Mechanism) and Slater's SPC account, but does the change in the notion of mechanism really save HPC from difficulty?

In this presentation, we shall analyze whether Khalidi's causal account, involving the notion of Loose Mechanism, is free from Craver's challenges and argue that, while Khalidi contributes greatly to the advancement of the natural kinds theory, his account still fails to address Craver's challenges against the original HPC.

Varieties of Error and Varieties of Evidence in Scientific Inference

Wednesday, February 27, 14.30 – 15.10
Room 23, Seminargebäude 106

Barbara Osimani
MCMP

The Variety of Evidence Thesis, that is the thesis according to which, pieces of evidence coming from independent lines of investigation are more confirmatory, ceteris paribus, than e.g. replications of analogous studies, has been shown to fail by [Bovens and Hartmann(2003)] and [Claveau(2013)]. However, the results obtained by the former only concern instruments whose evidence is either fully random or perfectly reliable; instead in [Claveau(2013)], unreliability is modelled as deterministic bias. In both cases, the unreliable instrument delivers totally irrelevant information (disconnected from reality). We present here a model which formalises both reliability, and unreliability, differently. Our instruments are either reliable, but
affected by random error, or they are biased but not deterministically so.

More importantly, Bovens and Hartmann's results are counterintuitive in that in their model a long series of consistent reports from the same instrument does not raise suspicion of "too-good-to-be-true" evidence. This happens precisely because they neither contemplate the role of systematic bias, nor unavoidable random error of reliable instruments in hypothesis update. In our model the VET fails as well, but the area of failure is considerably smaller than for [Bovens and Hartmann(2003)] and for [Claveau(2013)]. Furthermore it affects borderline cases where the ratio of false to true positive reports for the two instruments become favourable for the biased one.

Against Phylogenetic Conceptions of Race

Tuesday, February 26, 11.40 – 12.20
Room 25, Seminargebäude 106

Kamuran Osmanoglu
University of Kansas

Biological racial realism (BRR) continues to be a much-discussed topic, with several recent papers presenting arguments for the plausibility of some type of "biological race." In this paper, the focus will be on the phylogenetic conceptions of race, which is one of the most promising views of BRR, that define races as lineages of reproductively isolated breeding populations. However, I will argue that phylogenetic conceptions of race fail to prove that races are biologically real. I will develop and defend my argument against the phylogenetic views of race by relying on current research in population genetics, human evolution, and social sciences. Ultimately, I will argue that (i) race is not a biologically legitimate category and (ii) philosophers should direct their resources to understand problems that arise due to racialization, and thereby they should find solutions to those problems.

When Glaciers Prophesy: Building a case for predictive historical science

Wednesday, February 27, 15.10 – 15.50
Room 26, Seminargebäude 106

Meghan Page
Loyola University Maryland

Models of "good science" often appeal to successful predictions and observable empirical results. This poses a problem for historical sciences, such as archaeology, evolutionary biology, and geology, that investigate historical events. It is difficult to replicate evolutionary stories in a laboratory, and the past that is no longer accessible for direct observation (e.g. we can't watch dinosaurs eat to determine their palate.) These structural differences between historical science and experimental science have led to doubts whether claims about the past, even those made by experts, can be successfully verified by science.

Carol Cleland offers a powerful defense of historical science by appealing to what David Lewis describes as "the asymmetry of overdetermination." The asymmetry of overdetermination is a causal asymmetry---an event is usually underdetermined by any particular cause (e.g. tossing a baseball towards a window is not a guarantee that the window will break) but causes are epistemically overdetermined by their effects (if the baseball does break the window, it will leave a host of traces to prove that it did.) The widespread traces left by

events on the world act as a breadcrumb trail---by uncovering enough of these traces, scientists navigate a path to an explanation through the search for a common cause.

According to Cleland, both models of science, experimental and historical, are justified by the asymmetry of overdetermination. Because causes do not uniquely determine their effects, experimental scientists repeatedly test their hypotheses to isolate relationships between variables; scientists must verify they are tracking regularities and not accidents, and to do this they must isolate individual causal relationships from the complex web of total causes that converge at any particular event. In contrast, historical scientists trace a specific path from effect to cause. Given that any actual event leaves a great number of effects, scientists can rely on these traces to distinguish between competing causal explanations.

While Cleland's picture is compelling and accommodates many historical research programs, it fails to account for the specific role of historical science in making claims about the future. This is contrary to practice, considering, for example, that some of the best evidence we have concerning the relationship between CO_2 emissions and abrupt global climate change comes from historical sciences

such as glaciology and paleoclimatology.
In this paper, I present a case-study concerning the introduction and verification of Walter C. Broecker's hypothesis that there are alternating modes of operation in the meridional overturning circulation. Broecker's historical work interpreting ice core data led him to hypothesize that there are differing modes of circulation in oceanic deep currents that, if switched, can lead to abrupt changes in climate. A number of predictions that follow from Broecker's hypothesis (some historical, some not) have have proven accurate, offering support for his claim. I use this case as a reductio against Cleland's view. If Cleland is right, historical science is only justified in making claims about the past. But historical science often offers successful predictions about both regularities and future events. Therefore, Cleland's view is problematic.

Rhetorics of Empiricism and Disciplinary Purity: Alchemy and "protochemistry" in enlightenment Germany

Wednesday, February 27, 12.20 – 13.00
Room 24, Seminargebäude 106

Alan Park
Harrow School

The antiscientific nature of alchemy can be observed due to the influences of several entangled subjects. The separation of these distinct fields appears to occur in the 17th and 18th century, and it appears that alchemy falls out of scientific premises after this period, while Chemistry establishes itself as a scholarly and an empirical subject. It can be understood that the scientific movements by the alchemists in this period had made Chemistry a separate subject to alchemy, leaving all the theology and philosophy behind. Therefore, alchemy had no place inside the boundaries of science after this specific period; the image we have of alchemy now is the leftover from what Chemistry left behind in alchemy, and by that we can conclude that alchemy is indeed antiscientific after the 18th century. However, alchemy before the separation cannot be considered unscientific, because Chemistry was a subset of it and encompassed Chemistry within. As Principe wrote, we cannot consider alchemy to be part of modern science, because that would be incorrect; however, it should be part of natural philosophy and the history of science.

In accordance with the writings of many alchemists who were favourable of the experimental practices of alchemy, this paper concludes that alchemy was indeed scientific until the divide, which separated alchemy into two subjects, science and philosophy. This paper also proposes that the turning point of this event was Paul's "reduction to pristine state", and its influences on the 17th and 18th century alchemists which led to Chemistry's development as an individual subject, separate from alchemy. The consideration of alchemy as "protochemistry" may be wrong, but the development of the subject was done by alchemists who offered scientific suggestions towards philosophical questioning, and it cannot be hidden that alchemy occupies a vast amount in the history of science. This paper concludes that alchemy should not be considered antiscientific before the Enlightenment, but may be after this period when Chemistry developed itself as an independent subject assisted by the scientific alchemists who preferred the usage of experiments to prove theories rather than rely on theological and metaphysical elements to explain their ideas, leaving the disliked elements behind. In addition, it also claims that the divide in alchemy had given it a worse reputation than before, and had been increasingly become worse until the modern centuries when it was seen and accepted as an important section in the history of science and Naturphilosophie. This paper considers alchemy to have laid the foundations of Chemistry and had given way to its separation in the period of Enlightenment, where it no longer considered scientific material. The argument that alchemy is antiscientific is only valid for after this period, and thus we can reject the statement that alchemy was unscientific overall.

Descriptions for Explanation and Prediction of Conserved and Variable Mechanisms

Tuesday, February 26, 16.30 – 17.10
Room 24, Seminargebäude 106

Viorel Pâslaru
University of Dayton

I examine the differences between descriptions of mechanisms used for explanations and in formulating predictions involving conserved mechanisms and propose how to address one of their limitations.
The new mechanistic philosophy defined itself relative to the explanatory practice of biologists and by contrast to the DN model. Accordingly, the main function of mechanism description is to provide explanations of phenomena under scrutiny. The role of mechanisms in formulating predictions is addressed only briefly, despite prediction being very important in scientific practice and despite claims by various scientists that descriptions of mechanisms are crucial for successful predictions. I argue that the available contributions by philosophers examining mechanisms could be summarized by two claims. First, descriptions of mechanisms are used both for explanation and prediction. This view is held by (Machamer, Darden, and Craver 2000, Glennan 2002, Woodward 2002, Illari and Williamson 2012, Casini et al. 2011, Gebharter and Kaiser 2014). These philosophers assume highly conserved mechanisms (Steel 2008) and there does not seem to be any difference in descriptions of mechanisms used for either of the two purposes. The second claim is that predictions are necessary to develop explanations. Adequate mechanistic explanations are formulated based on accurate descriptions of the mechanism responsible for the phenomenon under scrutiny. If the description is accurate, it will generate correct predictions. And so generating and testing predictions by means of top-down and bottom-up experimental strategies (Bechtel and Richardson 1993, Craver 2007) are means to verify the adequa-

cy of explanations and to correct them, if needed.

I examine explanatory practice of ecologists and argue that it supports the second claim concerning the necessity of predictions for formulating explanations. However, mechanistic explanations of ecologists show that the relationship between mechanistic explanation and prediction is not always symmetrical, as the first claim states, but is rather more complex in the following ways. A) Some predictions require descriptions of mechanisms that do not represent satisfactory explanations. This is the case when an ecological phenomenon is explained by describing a mechanism consisting of populations and their traits are used as the basis of predictions. By contrast, the acceptable mechanistic explanation refers to a lower-level mechanism consisting of organisms, their traits, organization and activities. B) Despite this explanatory virtue, this description of the lower-level mechanism cannot be used to make novel predictions. Instead, it is the population-level description that generates novel predictions. C) Accuracy of predictions is increased not by offering a more detailed description at the lower-level mechanism, but by completing the causal structure at the population level by incorporating an environmental factor. D) Even if population-level descriptions generate predictions, ecologists find it necessary to complement those descriptions with a lower-level mechanistic explanation. Thus, contrary to the new mechanistic philosophy, descriptions of mechanisms used for explanations are different from those used for prediction.

Ecologists too focus on highly conserved mechanisms, yet many of ecological mechanisms are variable, not conserved. To predict the behavior of variable mechanisms, both types of descriptions are necessary, as shown by the case of invasive species.

Disease as Essence Destruction: The case of (lung) cancer

Monday, February 25, 14.30 – 15.10
Tagungsraum, Seminargebäude 106

François Pellet
University of Muenster

In the contemporary literature about the nature of disease, we distinguish between three groups of theories of disease, which may be labeled “axiologism about disease”, “dysfunctionalism about disease” and “hybridism about disease”. These three groups of theories of disease are distinguished with respect to the intuition(s) that we have about what disease is.
Axiologism about disease accounts for the intuition that (i) saying that e.g. cell growth is cancerous is making a specific negative value judgement toward cell growth, where the value at issue is intuitively a lethal one (like death), by contradistinction with a vital value like health and life.
Dysfunctionalism about disease takes into consideration the intuition that (ii) saying that cell growth is cancerous is saying that cell growth is biologically dysfunctional.
While many theories of disease have further analyzed either intuition (i) or (ii), it is obvious that a complete theory of disease should coherently analyze both intuitions (i) and (ii). Thence, a third and last group of theories of disease coined “hybridism about disease” has quickly arisen.
In this talk, after presenting the above three groups of theories of disease, I argue for a certain hybrid theory of disease called “essentialism about disease” with (lung) cancer as a case study, according to which x is diseased, iff (i) x is a healthy processual part of an organism, and (ii) x has a specific lethal value i.e. that x hosts properties destroying x’s essence.
Along condition (i), I argue for a highly fine-grained individuation of the disease host; only a healthy processual part of an organism can

be a disease host: e.g. (lung) cancer has as its host cell growth in (the lung's) tissues; etc.
Along condition (ii), I argue, first, that health is a specific vital value (like life), where for x to have a vital value is for x to be (a processual part of) a good organism, and to possess all the properties essential for being (a processual part of) a good organism: e.g. if cell growth is getting more and more of its essential properties, then cell growth is being more and more healthy i.e. through the maturation, differentiation and division of the specialized cells (e.g. respiratory epithelial cells) in the lung's tissues (cell cycle), where it can be said the biological function of cell growth in the lung's tissues. If cell growth is not healthy, then it is diseased i.e. that more and more of its essential properties are being destroyed (e.g. through a specific epithelial dysplasia followed by an uncontrolled cellular proliferation i.e. the development of a lung carcinoma (in situ)); cell growth in the lung's tissues is, thus, biologically dysfunctional.
Second, I argue that the essential properties of x are all the constitutive parts of a whole x, and a part of the essence of x is a single constitutive part of x.
To conclude, I show how essentialism about disease coherently unifies both intuitions (i) and (ii) by analyzing them through the notion of essence destruction.

Relations between Psychotherapeutic Practice and Models of Mental Disorders

Monday, February 25, 15.50 – 16.30
Room 25, Seminargebäude 106

Julia Pfeiff
Leibniz University Hannover

In my paper, I will be concerned with a widely-used explanatory model of Obsessive Compulsive Disorder (OCD) that was originally developed by Paul Salkovskis, a renowned researcher in clinical psychology and psychotherapist, in 1985. I will investigate how practical aims of psychotherapists in explaining their patient’s mental disorders have influenced – and continue to influence – the content and form of this model.

I will start by pointing out that several features of Salkovskis’ explanatory model differ crucially from those of other explanatory models of mental disorders that can be found, for example, in psychiatry. To illustrate this point, I will compare it with another explanatory model of OCD. I will argue that Salkovskis’ model differs from it mainly by employing folk-psychological vocabulary, by employing a particular conception of functionality, and by normalizing (compare Bolton, 2007) the patient's experience.

Having thereby set the stage, I will argue for my main thesis, namely that these features of Salkovkis’ model are due to several pragmatic aims which mental health professionals have when explaining mental disorders to their patients within psychotherapy.

To do so, I will identify several practical aims psychotherapists pursue when explaining mental disorders to their patients. This part of my paper is based on results from a study on explanations in clinical psychology during which I interviewed six cognitive-behavioral psychotherapists about their explanatory practices. From these findings, I will infer that the primary aims of these practices within psychotherapy are tied to the overarching goal of motivating one’s patient forstructured psychotherapeutic treatment. To achieve this goal, the mental health professional has to achieve several subgoals such as, e.g., shifting the blame away from the patient and enabling her to cope better with her disorder.

Secondly, I will reconsider the noteworthy features of this model described in the first section, arguing that they are surprisingly very well aligned with the pragmatic aims of explanatory practices that I identified before.

In a third section, I will argue that this influence from explanatory practices within psychotherapy on the structure and content of the explanatory model is due to the fact that the author based his model on so-called “clinical” evidence, that is, on observations which he

made within clinical practice.
Finally, I will generalize this thesis, arguing that specific aims arising within psychotherapeutic practice exert considerable influence on the content and structure of explanatory models of mental disorders more generally. Lastly, I will discuss potential implications of this broader claim.

Stem Cell Concepts: Broadening the scope of philosophy of science debate

Monday, February 25, 16.30 – 17.10
Tagungsraum, Seminargebäude 106

Anja Pichl
Bielefeld University

After twenty years since the first cultivation of human embryonic stem cells, significant progress towards a better understanding and handling of stem cells and steps towards clinical application have been made, but key issues concerning their conceptualization, functioning and controlled biomedical applicability still remain obscure. This talk reconsiders debates on stem cell concepts and stemness among stem cell scientists and philosophers of science and attempts to broaden them towards an understanding of science in its societal context and towards a critical reflection of its methodological basis.
I'll start by characterizing the older 'state vs. entity' debate (Zipori 2004, Lander 2009, Leychkis et al. 2009) and more recent ontological distinctions (Laplane 2016) and Fagan's minimal stem cell model (Fagan 2013). Drawing on Fagan's focus on evidential constraints arising from the stem cell concept itself and how they are handled in scientific practice as well as scientific insights into the context-dependence of stem cell identity and functioning, usually referred to as cellular plasticity, I'll argue that the classical view of stem cells as clearly identifiable entities with certain intrinsic properties is in tension with current best scientific and philosophical understanding.

That it lives on notwithstanding the outlined epistemic problems will be explained by referring to two constituents of the field of stem cell research: methodological reductionism and clinical goals. To conceive of stem cells as the source of (tissue- and organism-level) processes of development and regeneration can be traced back to basic essentialist and reductionist commitments of the life sciences criticized by many philosophers of science (recently Dupré and Nicholson 2018). I'll investigate their form in stem cell research and how they give rise to some epistemic problems encountered by working scientists like those of extrapolating results from one stem cell system to others (Robert 2004, Flake 2004). Clinical goals have been shown to be constitutive for the field of stem cell research (Fagan 2013). Therapeutic visions to a large part still depend on the idea of isolating and purifying stem cells for medical use and thus contribute to favouring the entity view.
I conclude that the therapeutic goals, together with reductionist commitments (1) have a misleading influence on the understanding of stem cells and the choice of concepts and methods in studying them and set too narrow limits on the scope and depth of stem cell research, and (2) depend on and contribute to unreasonable expectations of stem cell therapeutic applications which in the end puts patients and science's reputation at risk. (3) "Stem cells are not cells"
(Fagan forthcoming) will be explained in its meaning and consequences for research and application.

Features of Bayesian Learning based on Conditioning using Conditional Expectations

Tuesday, February 26, 14.30 – 15.10
Room 25, Seminargebäude 106

Miklos Redei (London School of Economics)
Zalan Gyenis (Jagiellonian University)

General features of Bayesian learning are investigated, where Bayesian learning is understood as inferring probabilities from other probabilities (evidence) by conditioning based on the theory of conditional expectations due to Kolmogorov. The Bayes Blind Spot of a Bayesian Agent with a prior is defined as the set of probability measures that are absolutely continuous with respect to the Agent's prior but which cannot be obtained as a result of a single conditioning of the prior. It is shown that the Bayes Blind Spot is typically a very large set. Open problems about the size of the Bayes Blind Spot are formulated. The results presented highlight the significance of prior in Bayesianism from a new perspective which becomes available only if one uses Kolmogorov conditioning.

What Kind of Realism – if any – is Whitehead's Organic Realism?

Monday, February 25, 15.50 – 16.30
Room 24, Seminargebäude 106

Aimen Remida
University of Düsseldorf

Whitehead uses the label "organic realism" as a suggestion for describing – in the language of physical science – the new outlook that emerges by abandoning old materialism and defines it as "the displacement of the notion of static stuff by the notion of fluent energy" (A.N. Whitehead, 1978 : 309). In order to determine the genuine nature of this Whiteheadian realism, one should investigate the general features of Whitehead's metaphysics on the light of the recent debates on basic metaphysical positions in general and the question of realism in particular. There are two opposite paradigms within contemporary investigations of general metaphysical orientations: (i) the so-called Object-Oriented-Ontology (OOO) (cf. G. Harman) and (ii) Process Metaphysics (cf. N. Rescher). The opposition between these two paradigms is insofar justified, as the first is denoted by a certain rejection of the centrality of the ideas of flow, process and relatedness, which characterizes the second. In fact, the (OOO) calls for a return to the focus on objects, which criticizes the overemphasis on subjectivity of almost all post-Kantian metaphysics.

Whitehead's metaphysics presents a challenge for any attempt of classification into the dichotomy of the two paradigms mentioned above. For it includes heterogeneous elements, upon which the argumentations for and against each position could rest. On the one hand, and this is the usual interpretation, Whitehead's philosophy of organism is among the most recognized examples of modern process philosophy, especially as established in his Process and Reality. On the other hand, the crucial role of the notions of "actual entity", "eternal object" and "superject", as fundamental elements of the Whiteheadian metaphysics as well as the general outline of the correspondingly built cosmology, manifest several common features with the very spirit of (OOO). In this paper, I argue that Whitehead's organic realism is at the same time a process metaphysics and an object-oriented-ontology, so that one could speak about an object-oriented process metaphysics, as a coherent Whiteheadian position.

Objectivity as Independence

Tuesday, February 26, 16.30 – 17.10
Room 25, Seminargebäude 106

Alexander Reutlinger
LMU Munich

Objectivity is often taken to be an epistemic virtue in the sciences – by scientists and non-scientists alike. However, the notion of objectivity is surprisingly unclear. In this talk, I will argue for a novel approach to objectivity – the subjunctive independence account of objectivity. According to this account, scientific objectivity is a kind of independence. That is, roughly put, some fact A is objective if and only if A obtains independently of something else, another fact B.

To make this idea precise, I will first define independence as follows: fact A is subjunctively independent of another fact B if and only if (1) if B were the case, then A would be the case, and (2) if B were not the case, then A would still be the case (building on Skyrms 1980; Lange 2000; Woodward 2003).

Based on this definition of independence, I will characterize objectivity as follows: some fact A is objective in relation to another fact B if and only if A is subjunctively independent of B. In the case of scientific objectivity, A-facts typically are empirical hypotheses (or models), while B-facts typically regard different possible cognitive states of scientists or different methods. That is, some empirical hypothesis H is objective if and only if the following subjunctive conditionals hold: (i) if the scientists testing H were to differ in certain kinds of cognitive states, H would still be regarded as true (or accepted, depending on one's realist commitments), or (ii) if different methods were used to test H, then H would still be regarded as true, (or accepted).

My account of objectivity is inspired by the key idea of Nozick's "invariance" version of an independence account of objectivity (Nozick 2001). However, Nozick's account suffers from at least two short-

comings: first, Nozick does not provide a general explication of invariance. He merely illustrates the notion of invariance by way of example. Second, Nozick's elaborate examples of objectivity are exclusively examples from physics. I try to improve the independence account by using the notion of subjunctive independence to make precise the idea of independence (in response to the first of Nozick's shortcomings) and by discussing a more diverse diet of case studies (in response to the second shortcoming). Moreover, I will point out that if one adopts the subjunctive independence account, objectivity turns out to be relational, contrastive, and gradual.
In a second step, I will argue that the subjunctive independence account applies to typical examples of scientific objectivity discussed in the recent literature in the history and philosophy of science. The examples include: objectivity as intersubjective agreement, objectivity as reproducibility, aperspectival and mechanical objectivity, objectivity as value freedom, objectivity as robustness, and ontological objectivity. I take it that capturing such a wide range of examples counts in favor of the subjunctive independence account.
In a final step, I will respond to challenges stemming from pluralist and eliminativist views in the recent literature on objectivity (for instance, Douglas 2004; Hacking 2015; Ludwig 2017).

How Far do Evolutionary Explanations Reach? On the application of evolutionary explanations to explain non-biological phenomena

Monday, February 25, 15.50 – 16.30
Room 22, Seminargebäude 106

Thomas Reydon
University of Hannover

Both in academic and in public contexts the notion of evolution is often used in an overly loose sense. Besides biological evolution, there is talk of the evolution of societies, cities, languages, firms, industries, economies, technical artifacts, car models, clothing fashions, science, technology, the universe, and so on. While in many of these cases (especially in the public domain) the notion of evolution is merely used in a metaphorical way, in some cases it is meant more literally as the claim that evolutionary processes similar to biological evolution occur in a particular area of investigation, such that full-fledged evolutionary explanations can be given for the phenomena under study.

Such practices of "theory transfer" (as sociologist Renate Mayntz called it) from one scientific domain to others, however, raises the question how much can actually be explained by applying an evolutionary framework to non-biological systems. Can applications of evolutionary theory outside biology, for example to explain the diversity and properties of firms in a particular branch of industry, of institutions in societies, or of technical artifacts, have a similar explanatory force as evolutionary theory has in biology? Proponents of so-called "Generalized Darwinism" (e.g., Aldrich et al., 2008; Hodgson & Knudsen, 2010) think it can. Moreover, they think evolutionary thinking can perform a unifying role in the sciences by bringing a wide variety of phenomena under one explanatory framework.

I will critically examine this view by treating it as a question about the ontology of evolutionary phenomena. My starting premise (which for the moment I will simply assume) is that for an explanation of a particular phenomenon to be a genuinely evolutionary explanation, the explanandum's ontology must match the basic ontology of evolutionary phenomena in the biological realm. This raises the question what elements this latter ontology consists of. But there is no unequivocal answer to this question, as there is ongoing discussion about the question what the basic elements in the ontology of biological evolutionary phenomena are and how these are to be conceived of (e.g., the units of selection debate, the debate on evolutionary individuality, etc.). I will argue that this situation forces researchers to devise specific evolutionary ontologies of the phenomena under study. However, biological evolutionary theory does impose some restrictions on which ontologies are acceptable. By examining concrete attempts to formulate evolutionary explanations of non-biological phenomena, I will illustrate how the ontology of biological evolution constrains evolutionary explanations outside biology.

The Subset Understanding of Multiple Realization: Nothing but advantages

Wednesday, February 27, 11.00 – 11.40
Room 25, Seminargebäude 106

Christian Sachse
University of Lausanne

A major argument to adopt Sydney Shoemaker's subset approach for the notion of functional properties is to avoid the epiphenomenalist threat qua making maximally explicit what token identity precisely means for higher-level, functionally defined properties. However, following objections by Jaegwon Kim, Larry Shapiro and others, such important metaphysical advantages come at a high price: it excludes the multiple realization of functional properties and consequently the explanatory autonomy of special sciences, like biology. This paper aims at challenging that criticism. More precisely, it aims at 1) making the subset approach compatible with recent developments on multiple realization (notably those by Tom Polger, Larry Shapiro, Ken Aizawa & Carl Gillett) and 2) integrating that achievement into a framework capable of defending the explanatory autonomy of biology in a metaphysically sharp way.

Extended abstract of the major objection

Multiple realization in the subset approach means that while two tokens b1 and b2 come under one functionally defined biological type B by sharing the same functional disposition (ci) (cf. Shoemaker, 2001, pp. 78-79), b1 and b2 come under different physical types P1 and P2 when differing in some other, non-functional, disposition (c1-cn). However, given that ci is in each case a subset among the complete causal profiles of b1 and b2 when described by the physical types P1 and P2, respectively, physics may in principle construct one unifying type P as well, one referring only to that very ci in both tokens as does B (cf. Kim, 2010, pp. 111-112; Shapiro, 2000, p. 647), which would actually mean the denial of multiple realization.

Extended abstract of key issues of the reply

B always refers to the very same functional disposition ci in b1 and b2, but given the physical differences, that shared disposition ci actually has different manifestation conditions in each token. A silent gene mutation may serve as illustration: two physically different DNA sequences lead to the production of identical proteins only qua slightly different causal paths. Importantly, following recent developments on multiple realization by Aizawa, Gillett, Polger & Shapiro, multiple realization here is (and generally should be) some kind of double difference: what realizes a functional similarity are physical different tokens put into physically different contexts, and necessarily so.

When typing b1 and b2, physics would not construct one unifying type P since that would mean to make abstraction from the required different manifestation conditions; that would mean to give up its goal of ideally exceptionless types that result only from a perfect similarity of all tokens of one type. Put differently, even if it is possible for physics to construct one unifying type P about only ci as well,s it would not do so in principle unless constructing types as do special sciences like biology.

How Physical Practice Employs the 'Physical Possible'

Wednesday, February 27, 12.20 – 13.00
Room 22, Seminargebäude 106

Kian Salimkhani
University of Bonn

The received view takes 'physically possible' worlds rather formally as being about having the same physical laws as the actual world (e.g., Bradley and Swartz 1979) or satisfying the physical laws of the actual world (e.g., Carroll 1994). We provide a survey on how a notion of 'physical possibility' can be viewed to be employed and thereby constrained in the actual practice of physics' research. We argue that the term 'physically possible' helps to explain physical research with respect to the following points: (1) It allows for giving (theoretical) explanations of why a certain state of affair holds. (For example, a state of affair is explained if it is revealed as physically necessary under certain assumptions which constrain what is physically possible.) (2) It serves as a criterion for whether a mathematical feature of a model counts as physically significant or not. (For example, a singularity in some model of general relativity is taken as a physical property of the world only if the singularity exists as well in arbitrary neighbouring (physically possible) models.) (3) It allows for formulating new physical theories based on both theoretical reasoning and new empirical evidence; modal notions come into play when the previously held necessity (fundamentality) of some physical structure is challenged and the modal status changes to mere contingency. For example: mass is necessarily conserved in Newtonian mechanics while it is not anymore in special relativity. The results from this survey should be relevant for any sort of discussion of the physical possible.

A Re-evaluation of E. J. Lowe's Account of Laws of Nature

Tuesday, February 26, 15.10 – 15.50
Room 23, Seminargebäude 106

Petter Sandstad
University of Rostock

Lowe's account of laws of nature is unduly neglected, and with one addition to his theory it is able to answer all major objections.

"The form of a law, in the simplest case, is just this, on my view: substantial kind K is characterized by Fness, or even more simply, K is F." (Lowe 2006: 132) For instance, Common salt is Water-soluble. One

part of it is anti-Humean, by understanding laws of nature to be connections between universals; an aspect shared with the Armstrong-Dretske-Tooley account (A-D-T). Another part is more Humean, because Lowe does not accept metaphysical necessity as what connects universals into laws of nature; unlike A-D-T, Lowe's account is therefore invulnerable to the criticism of Schrenk (2010).

Still Lowe's account shares two main objections with A-D-T. First, the inference problem is perhaps less problematic for Lowe. He thinks laws are, in several respects, contingent. First, laws of nature (often) involve physical necessity, which does not hold across all possible worlds. Second, Lowe is not a Platonist on universals, and therefore it is contingent which universals exist (if there had been no salt, then there would be no laws about salt). Third, Lowe accepts that laws often allow for exceptions (Lowe 2009), and therefore do not support counterfactuals and cannot be understood as universal quantifications.

Second, however, the identification problem is perhaps an even greater problem for Lowe: What exactly is it for a kind-universal to be characterized by a property-universal? The relation cannot be the exact same relation as for a substance to be characterized by a trope—since the latter case concerns particulars, while the former concerns universals (Johansson 2006: 515). Here we must add to Lowe's view, by interpreting the relation between kind-universals and property-universals as the per se connections of Aristotle's Posterior Analytics.

Third, a problem idiosyncratic to Lowe: Many laws of nature seem to relate two or more property-universals, yet Lowe prima facie disallows this (Johansson 2006: 516–517). For instance, the combined gas law says that the ratio between the pressure-volume product and the temperature of a system remains constant. However, Lowe (2015), in a posthumous paper, provides an answer to this objection: Such laws contain a reference to the kind which the law applies to, only that this reference is suppressed in such law's mathematical formulation because the kind serves no computational role (Lowe 2015: 81). In the same paper, Lowe (2015:67) also accepts that there are both determinates and determinables within the category of property-universals, thus answering another criticism (Johansson 2006: 516).

Grounding Numerals

Wednesday, February 27, 16.30 – 17.10
Room 25, Seminargebäude 106

Mario Santos-Sousa
UCL

The study of our innate numerical capacities has become an active area of recent cognitive research. Given that these capacities appear to be very limited, it is widely assumed that the use of external aids—such as numeral systems—expands our natural ability to reason about numbers. In fact, people have identified arithmetic as an important case of the use of external aids in thinking, but the question of how these 'thinking tools' acquire numerical content remains unsettled. After all, written numerals (say) are material inscriptions that—under some suitable interpretation—could stand for anything whatsoever. What constrains the range of available interpretations so that these otherwise meaningless symbols can achieve their representational aims?

Extant accounts either pull the relevant content out of thin air or make it parasitic on some antecedently available interpretation. On pain of circularity or regress, we have to explain how numerals come to represent what they do without relying on some prior—and mysterious—grasp of their intended interpretation.

I will start with the recognition that numeral symbols, in and of themselves, do not represent anything at all. In isolation, they are

representationally idle. It is only by being embedded in broader systems of representation that these symbols acquire numerical content. Numeral systems, I suggest, have distinctive features that relate individual symbols to one another and thereby constrain their representational content.
This, however, still doesn't uniquely determine the system's representational target. Our familiar decimal base system, for instance, can stand for linear sequences but it can also stand for circular ones, depending on the case at hand. Thus, I will further argue that systems of numerical representation, in turn, need to be grounded in specific cultural practices, which govern their use and are carried out by agents naturally equipped to exploit some of their distinctive (structural) features.
I will illustrate these claims by means of a case study involving different numeral systems (such as tallies and positional systems) and the practices in which they are deployed (most notably, counting and calculation).

Axiomatization as an Act of Mathematics Studies: Or the marvelick tradition and formalized mathematical theories

Wednesday, February 27, 15.10 – 15.50
Room 25, Seminargebäude 106

Deniz Sarikaya
University of Hamburg

We want to discuss in how far we can adapt the framework of theory choice as it was debated and developed in Philosophy of Science for Philosophy of Mathematics. There are several criteria which are included in such debates, among them: Simplicity, Power, Consistency and those criteria where debated also in the Philosophy of Mathematics. We want to focus on the question what adequacy to the data should mean in the context of mathematical theories.
David Hilbert famously suggested to formalize the "inhaltliche

Mathematik" to deal with problems of consistency proofs and Russell's antinomy. This approach led to the branch of mathematical logic / metamathematics and provided a currently mostly accepted framework for all mathematics, i.e. first order logic and set theory. We will read the act of formalization as the logical (or first order) model or representation of a mathematical theory. While scientists study the real world, which means there is a concrete reality the models/theories need to fit, the mathematician can deal with in principle arbitrary systems. We understand Hilbert's idea as an act of mathematics studies, trying to find adequate models for mathematical practice. We want a new axiomatization to do several things:

1. Fit to our intuitions of the mathematical concepts
2. Have the right consequences
3. Offer techniques and results analogue to existing fields
4. Offer results analogue to existing fields

We will give different cases in which different of those aspects came into account. The first two points can be seen in foundational debates in the (Philosophy of) Set Theory. One might think that our current axiomatization for set theory ZFC is not a good base to do set theory and we should rather look for new axioms, since we lack the power to decide some questions, which occurred in the mathematical practice, like the Continuum Hypotheses. Gödel had this position and wanted to justify new axioms (partly) by inductive arguments (see f.i. Gödel, Kurt: "What is Cantor's continuum problem?", The American Mathematical Monthly 54(9) (1947), pp. 515–525)

This quasi-empirical component, or inductive reasoning is debated in the Philosophy of set theory. 3 and 4 will be illuminating in a case study of (topological) infinite graph theory. We analyse the conceptualization of basic notions of infinite (topological) graph theory and will mainly focus on infinite cycles. We show in how far different definitions of "infinite cycle" were evaluated against results from finite Graph Theory. There were (at least) three competing formalizations of infinite cycles focusing on different aspects of finite ones.

For instance, we might observe that in a finite cycle every vertex has a degree of two. If we take this as the essential feature of cycles, we can get to a theory of infinite cycles. A key reason for the rejection of this approach is that some results from finite Graph Theory do not extend (when we change syntactically “finite graph, finite cycle” etc. to “infinite graph, infinite cycle” etc.)

Humeanism, Best System Laws, and Emergence

Tuesday, February 26, 15.50 – 16.30
Room 23, Seminargebäude 106

Olivier Sartenaer
University of Cologne

To this day, ontological emergence has been almost exclusively debated within non-humean power-based or law-based metaphysics. Typical discussions involve dispositional essentialists or nomic necessitarians, who usually wonder about whether a case can be made that irreducible causal powers, or irreducible governing laws, can happen to come into being under some specific circumstances. Be they enamored with emergence or rather partisans of reductionism, participants generally share a basic common ground in some version of the Eleatic principle, according to which "to be is to have determinative power". That such debates have been overwhelmingly played out in a non-humean arena is no real surprise. For one thing, contemporary humeanists themselves never felt that attracted by emergence - consistently with Lewis's own dismissal of "suchlike rubbish" -, as it seems supervenience has always been all they really needed.

Yet, advocating the irreducibility of some worldly entities and, with it, the possible autonomy of some special sciences, doesn't prima facie appear to be an endeavor that ought not to be pursued in a humean setting. What seems a good indication that such a claim is not totally unreasonable certainly is the fact that, at present, it is rather standard to trace the very birth of emergentism in the works of a philosopher, John Stuart Mill, who also happened to embrace a broadly humean worldview. Unfortunately, although Mill is usually seen as the main progenitor of emergence, there hasn't been many attempts to exactly explicate the way in which he construed the notion. As a result, Mill's view is often unapologetically conflated with other distinct accounts of emergence under the unfortunate umbrella label of "British Emergentism". However, as I will endeavor to show in this paper, Mill's view on emergence turns out to be rather idiosyncratic, and actually provides us with some unexpected resources that allow, pace Lewis, for somehow reconciling humeanism with (ontological) emergence.

Digging out Mill's philosophy of emergence and emphasizing the extent to which it happens to conflict with its standard contemporary interpretation can certainly have some historical interest, but it is not what will primarily keep me busy here. The main purpose of the present paper is rather to use Mill's scattered insights on emergence as guides towards the establishment of a peaceful coexistence between humeanism and emergence. As a secondary objective, I also show that some peculiar form of ontological emergence allows for conceiving the autonomy of the special sciences in an interesting way, consistently with the reductionist ideal of a unified, all-encompassing science. Incidentally, such a conception will be claimed to strengthen a recent variant of the humean Best System Account of lawhood, known as the "Better Best System Account", for which, it has been recently contented, John Stuart Mill is to be considered the "patron saint".

Using Agent-based Models to Explain Scientific Inquiry: current limitations and future prospects

Wednesday, February 27, 15.50 – 16.30
Room 22, Seminargebäude 106

Dunja Šešelja

LMU München MCMP

Computational modeling has in recent years become an increasingly popular method for the study of social aspects of scientific inquiry. In particular, agent-based models (ABMs) have been used to simulate scientific inquiry, allowing for the examination of various socio-epistemological issues: from tensions between individual and group rationality, to different social mechanisms that impact the efficiency of inquiry, to different research strategies, etc. A common feature of ABMs developed in philosophy of science is that they are simple, 'thin' representations of scientific inquiry. The primary appeal of such models is that they allow for an easy insight into possible causal mechanisms underlying the phenomenon in question. Nevertheless, such simplicity comes at a price: the model will in turn be highly idealized, making it difficult to determine its relation to the real world. More precisely, the more idealized a model is, the harder it gets to exactly determine target phenomena it represents.

Despite their highly idealized character, many of the ABMs proposed in the literature have been motivated by concrete episodes from the history of science, suggesting potential explanations of the given cases (Zollman 2010; O'Connor and Weatherall 2017; Weatherall, O'Connor, and Bruner 2018;

Holman and Bruner 2015). This has had two significant consequences for the reception of ABMs of science. On the one hand, these models have been considered to be primarily aiming at explaining real- world phenomena or at least providing 'how-possibly explanations' or 'proofs of principle' that should be applicable to the given cases. On the other hand, the lack of robustness analyses of the given findings has cast doubt on their link to real-world phenomena, and hence on the relevance of these results for actual scientific inquiry (even in a how-possibly way). As a result, it has been suggested that the vast majority of ABMs developed in philosophy of science are currently only exploratory, rather than explanatory (Frey and Šešelja 2018b) and that they need to be 'thickened' and

enhanced by empirical data to provide insights into actual scientific inquiry (Martini and Pinto 2016).
In this talk I examine different ways in which ABMs of science can become a more reliable method for providing normative accounts of scientific inquiry. To this end, I discuss the usefulness and limitations of
a) derivational robustness analysis which consists in the employment of different models for the study of the same research question, and b) the import of historical data on concrete case-studies as a method of empirical calibration of ABMs. I illustrate this approach by using two structurally different ABMs of science: the ABM by Frey and Šešelja (2018a, 2018b) inspired by Zollman's (2010) model, and the argumentation-based ABM (ArgABM) (Borg et al. 2018, 2017) to represent a case-study from the history of medicine: the research on peptic ulcer disease (PUD).

Natural Concepts in a Brain-Based Feature System

Monday, February 25, 12.20 – 13.00
Room 23, Seminargebäude 106

Corina Strößner (Heinrich Heine University Duesseldorf)
Henk Zeevat (Heinrich Heine University Duesseldorf)

Binder et al (2016) develop a system of 64 "brain-based semantic features", the activation of which correlates strongly with activation of a specific area on the cortex. While the meaning of "semantic feature" remains unelucidated, the paper shows that an association matrix of their 64 features gives comparable results to classical distributional semantics that analyses a word meaning by its textual context (cf. Lenci 2018).
BLIND tries to give a stronger interpretation to "semantic features" by exploring the possibility to move from the features to a symbolic representation as attribute value structures, i.e. Barsalou (1992) frames. The features provide the fragments from which concepts

are build. By that means conceptual learning and formation is based on cognitively salient aspects. We associate concepts to the basic features to a certain degree, which can mean two things:

- The intensity of cognitive association: Dogs and tigers are both dangerous, but tigers far more so. This interpretation calls for non-binary features with real numbers as values, i.e. dimensions in the sense of Gärdenfors (2000).
- Linguistic evocation: The word "fall" sometimes occurs in the context of "love", but much more often in the context of downwards direction. Here, association ties with the ambiguity found in natural language words.

The resemblance of brain-based association matrices to those from distributional semantics suggests a close connection between the interpretations, but this needs further discussion.

How would a representation of a concept look like? Many of the features in Binder et al. (2016) are concerned with perceptual categories like smell, motion or colour. For a natural kind this gives the boring result that a rabbit has a rabbit smell, rabbit colour, rabbit m ovement and so on, which opens the approach to the objection that the features of are only determined after concept is known. The and there is a gain of learning the frame, because in a very partial observation like rabbit smell in a dark forest and we can derive the unobserved values. Thus, what makes such concept useful is the co-occurrence of values on different attributes.

Is this, however, our rabbit concept? How does this fit to the essentialist intuition that a rabbit is different from a rabbit robot that has been designed to look, smell and move like a rabbit? A set of answers to these questions will be explored in the talk.

Class Selection in Inheritance Inference

Monday, February 25, 11.40 – 12.20
Room 23, Seminargebäude 106

Paul Thorn
Heinrich Heine University Duesseldorf

We present results from a simulation-based study of inheritance inference, investigating whether the performance of inheritance inference varies, depending on the criteria that are used in selecting 'acceptable' classes.
In executing an inheritance inference, one reasons from a premise stating that a given property is 'typical' among a class of individuals, and concludes that the property is typical among a subclass of the class:

Property ☐ is typical
of C.

☐ is typical among SC.

Within our study, the reliability of an inference type is identified with the inference type's tendency to deliver true conclusions, given true premises. To keep things simple, we say that a property is "typical" of a class (or subclass) iff the relative frequency of the property among the class (or subclass) meets or exceeds a given bound r.
One approach to inheritance inference (typical in the field of non-monotonic reasoning) proceeds by treating any atomic property as determining an acceptable class. We call atomic properties "unfitted" classes. A second approach (which had previously not been considered in the context of inheritance inference) identifies acceptable classes with the cells of a partition (of size k) of the domain that satisfies the condition of maximizing the similarity of objects

that are assigned to the same class. We call the classes of the latter sort “fitted” classes.

In addition to ‘regular’ inheritance inference, we investigated inheritance inference in the case of ‘exceptional subclasses’ (i.e., cases where there is some property □ (□≠□), wh
but □ is not typical among SC).

Figures 1 and 2 present some results of our study. Each bar represents a mean value for 1,000,000 randomly generated environments, with objects characterized in a language with 8 atomic predicates/properties. For both figures, error rates are the relative frequency of cases not satisfying the ‘conclusion condition’ for an inheritance inference among the cases that satisfy the ‘premise conditions’.

As illustrated by Figures 1 and 2, our study shows that inheritance inference based upon fitted classes is far more reliable than inheritance inference based on atomic properties. The two approaches also produce different results in the case of exceptional subclasses: In the case of exceptional subclasses, inheritance inference based upon atomic properties is horrendously unreliable. Conversely, inheritance inference based upon prototype-based classes generally suffers only a small decrease in reliability, in the case of exceptional subclasses. The results of the study address a long-running debate in the field of non-monotonic reasoning, concerning whether inheritance inference is reasonable in the case of exceptional subclasses: The matter depends upon the criteria used in selecting acceptable classes!

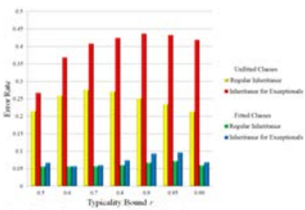

Figure 1: Error Rate as a Function of r (with k = 8)

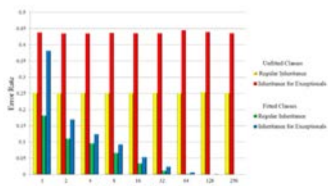

Figure 2: Error Rate as a Function of k (with r = 0.9)

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What Fish is This? Process ontology and biological identity

Monday, February 25, 11.40 – 12.20
Room 23, Seminargebäude 106

Rose Trappes
Bielefeld University

Recent years have seen a push for the development of a process ontology of biology (Dupré 2012; Dupre and Nicholson 2018). One of the difficulties faced by a process ontology is establishing the identity of processes as they change over time. Process ontologists reject the idea of entities as substances with definite essences or stable properties, so they cannot appeal to genes, morphology, or other potentially identity-determining properties. Instead, they must determine identity based on the flux of biological processes. Thomas Pradeu has recently proposed genidentity as a concept for conceiving identity across time from a process perspective (Guay & Pradeu 2016; Pradeu 2018).

Pradeu builds on David Hull's use of the concept of genidentity. Genidentity is the concept of identity over time as the continuity of change. Specifically, Hull conceived of biological genidentity as causal continuity in the gradual change of internal organisation, with rapid or abrupt disruptions of internal organisation determining the end of one biological entity and the start of another (Hull 1978, 1992). Pradeu adds his own interpretation to the somewhat fuzzy idea of "internal organisation" by referring to processes like biochemical and immunological interactions (Pradeu 2018). Importantly for its suitability for a process ontology, genidentity does not refer to stable properties to determine the identity of a biological entity. Rather, it is based on the rate of change of biological processes.

Pradeu's proposal represents a much-needed attempt to formulate a coherent concept of biological identity in process terms. More problematically, Pradeu claims that the role of genidentity is not ontological so much as epistemological. He argues that it allows scientists to determine biological identity over time in complex situations, such as the many forms of reproduction, or the formation of symbiotic relationships (Pradeu 2018). While an approach based on genidentity may serve some of these complex situations well, tracking the causal processes and the disruption of internal organisation is typically not possible or at least not feasible. In contrast, more "thing-like" approaches that focus on genetic makeup, morphological traits, and behavioural types often work quite well in determining identity over time.

To take a recent example, the Indian hump-backed mahseer, a prized but endangered mega-faunal game fish, was recently identified as the fully-grown version of juveniles previously identified as Tor remadevii (Pinder et al. 2018). The authors took a thing-like approach, using morphological, molecular, and photographic evidence to determine identity over time. Tracking developmental processes of the fish was both unnecessary and, given the small populations of the fish remaining and the lack of knowledge about their life cycles, unfeasible. In cases like this – and there are many throughout the biological sciences – genidentity does not seem the right approach for determining identity over time.

Thus, I argue that Pradeu's insistence on the epistemological benefits of the concept of genidentity is misplaced. Nevetherless, I argue that genidentity may be of ontological significance for biological scientists. Specifically, using a number of case studies I examine whether genidentity could in theory and does in practice inform a process ontology that underlies more thing-like epistemological approaches to biological identity over time.

Are quantum phenomena an impasse for Bogen and Woodward's account of science?

Monday, February 25, 15.10 – 15.50
Room 23, Seminargebäude 106

Frida Trotter
University of Lausanne

The distinction between data and phenomena by Bogen and Woodward (B&W henceforth) provides a comprehensive description of the scientific activity and, more specifically, of the relation between theory and world. In their account, the scientific understanding of the world is characterized by the relationship among theory, phenomena and data. Theories both explain and make predictions about phenomena. These latter are objects, facts, events with theory-independent ontological status. Data constitute evidence for the existence of phenomena and for the validity of theories. Observation and experimental practice concern the production and assessment of data. (Bogen & Woodward, 1988) (Woodward, 1989)

In quantum mechanics, the connection between theory and explananda has some unique features. Quantum phenomena are caused by the particular behaviour of quantum systems, which is defined in the theory through the notions of state and of observable. An example is the case of entangled systems. In this case, the phenomenon predicted and explained by QM is the occurrence of robust correlations among measurement outcomes in experiments carried out on entangled systems. These phenomena are highly accurately predicted by the theory, and explained by it, as consequences of the state in which the systems are before interacting with the measuring apparatus. The only empirical evidence available amounts to the records of these correlated outcomes, but it is not possible, by definition, to observe the systems while they are still entangled. Hence, our knowledge of what takes place before measurement has almost entirely theoretical nature. In cases like this, such theoretical, formal description of the quantum phenomenon is all we have epistemic access to about that event.

The state of two or more entangled systems is specified as a particular correlation among the states of the systems involved. Due to the relation in which such systems are when entangled, their individual states cannot be specified separately from one another. Although the measuring apparatus interacts locally with the entangled systems, it is their state as a whole, i.e. inasmuch as it includes the state of each system involved, that changes as a consequence of such interaction. We can verify the truth of this description of the situation through the notion of observable, which is one of the places in which the complexity of the characterization of quantum phenomena emerges evidently. An observable is a property of the system that represents quantities that can be measured experimentally (e.g. energy, spin, position etc.). Formally, they are defined as Hermitean operators on a multi-dimensional abstract space (Hilbert space), the expectation values of which are real numbers, and hence they can represent measurement outcomes. (Griffith, 1995) (Allday, 2009) (Norsen, 2017)

The claim of my presentation is that since B&W's definition of phenomenon requires that this latter is an aspect of the world that is independent of what the theory can say about it, their account seems to fail to capture the particular features of quantum phenomena. The physical features of the latter seem to display in fact an almost complete correspondence with their mathematical description in abstract, non-physical space. Hence, as the notion of phenomenon plays a pivotal role in B&W's account of science, it seems that this latter is incompatible with quantum mechanics: the subdivision between theory, phenomena and data as it appears in their account cannot satisfactorily represent the structure of quantum mechanics and its relation with the world.

The general structure of my intervention will be as follows. After having presented B&W's tripartition among theory, phenomena and data, I will argue why their definition of phenomenon seems unable to encapsulate the nature of quantum phenomena characterized in terms of states and of observables. I will then assess the consequent two possibilities that either B&W's account is correct, and quantum phenomena are thus to be considered as spurious, or quantum phenomena are full-fledged stable features of the world, and B&W's account has the limit of being unable to encompass their particular status. I will conclude with final remarks on the special character of quantum phenomena.

How Evolutionary Game Theory Explains

Monday, February 25, 15.10 – 15.50
Room 22, Seminargebäude 106

Walter Veit
University of Bristol

Evolutionary explanations are often faced with the criticism of providing nothing more than a 'just-so stories', a historical account that has no evidence in its favour. For Charles Darwin, it was very important to collect plenty of evidence for his theory of natural selection. Biologists to this day continue to accumulate corroborating evidence. When biologists try to explain the occurrence of a certain behaviour or a phenotype in general, they often start by hypothesizing how the trait could be adaptive. This research program is often criticized as a sort of Panglossian adaptationism, i.e. assuming the adaptiveness of a trait without further evidence. I shall not concern myself here with the question of whether the adaptionist research program is a fruitful one, but with the question of how evolutionary game theory (EGT) models, which are often employed in such adaptationist theorizing serve as explanatory devices.

This paper argues for the explanatory power of EGT models in three distinct but closely related ways. First, following Sugden and Aydinonat & Ylikoski I argue that EGT models are created parallel worlds i.e. surrogate systems in which we can explore particular (evolutionary) mechanisms by isolating everything that could be interfering in the real world. By specifying the pool of strategies, the game and the fitness of the strategies involved, EGT explores potential phenomena and dynamics emerging and persisting under natural selection. Given a particular phenomenon, e.g. cooperation, war of attrition, costly signalling, EGT enables the researcher to explore multiple 'how-possibly' explanations of how the phenomena could have arisen and contrast them with each other, e.g. sexual selection, kin selection and group selection. Secondly, I argue that by eliminating 'how-possible' explanations through falsification, we can arrive at robust mechanisms explaining the stability and emergence of evolutionary stable equilibria in the real world. In order for such falsification to be successful, it requires deliberate research in multiple scientific disciplines such as genomics, ethology and ecology. This research should be guided by the assumptions made in the applications of particular EGT models, especially the range of parameters for payoffs and the strategies found in nature. Thirdly, I argue that in order to bridge the gap between the remaining set of 'how-possibly' explanations to the actual explanation requires abduction, i.e. inference to the best explanation. Such inference shall proceed by considering issues of resemblance between the multiple EGT models and the target system in question evaluating their credibility. Together these three explanatory strategies will turn out to be sufficient and necessary to turn EGT models into a genuine explanation.

Can conventionalism save the Identity of Indiscernibles?

Monday, Feburary 25, 11.40 – 12.20
Room 22, Seminargebäude 106

Tina Wachter
University of Hannover

According to the Leibnizian Principle of the Identity of Indiscernibles (PII) there cannot exist two objects with exactly the same properties. Either there is only one object or there has to be some discerning property. One of the first counterexamples against this Leibnizian Principle is Kant's description of a possible world containing only two identical drops of water and otherwise empty. Kant argues that this world would violate the PII because numerically there exist two entities, but which have all the same properties, and therefore cannot be identified as certain individuals; Max Black's argument of two iron spheres is the modern version of this example. In both cases the argument, conclude that the which-is-which-identity cannot be preserved for two such objects, because we have no ability to distinguish them while taking their possible worlds' descriptions seriously, namely that there are two objects with identical properties.
One possible counterargument was formulated by Ian Hacking, based on Poincaré's Conventionalism. Hacking argues that every possible world description can be re-described in such a way that the PII is preserved. Concerning the possible world with two identical objects, which violate the PII when we accept their description as containing two numerically distinct entities that are identical in all their properties, Hacking would modify its description for the sake of the PII. Although Hacking can preserve the PII by changing the given description, this conventionalist approach leads to further questions concerning the ontological status of geometry and spacetime, and whether we can change such descriptions arbitrarily as he suggests and still be speaking about the same scenarios.
I argue that Hacking is misguided in discussing such examples by simply changing the given description to save the PII. He modifies the original setting essentially until it meets his expectations or favoured theory, instead of dealing with it the way it was originally described. Moreover, there are actual world counterexamples which show that the PII is violated already, e.g. a two-particle quantum mechanical situation in which both particles are permutation invariant and situated in mixed states. In such cases we cannot label the particles involved anymore, or hold on to their which-is-which-

identity, because they are in superposition states and no longer describable with two separate states. So, this example shows that there already exists a situation where the PII is violated. Moreover, Hackings solution of simply re-describing the circumstances in a way that preserves the PII would not work here. I argue that for this two-particle situation it is not possible to give another description besides the mixed state expression in which both particles exist in this superposition state, because it is already the only consistent description of the given situation.

From Theory Reduction and Reductive Explanation to Inter-level Scientific Practices: The Spemann-Mangold organizer and molecular developmental biology

Wednesday, February 27, 12.20 – 13.00
Room 23, Seminargebäude 106

Marcel Weber
University of Geneva

The relationship between classical genetics and molecular biology has been widely discussed, mostly under the rubric of "reduction", a supposed inter-theory explanatory relation. By contrast, classical experimental embryology and its relation to molecular developmental biology has been largely ignored. In this paper, I present an analysis of the case of the Spemann-Mangold organizer, which was discovered by transplantation experiments on amphibian embryos in the 1920s. What kind of knowledge did this classical approach produce, and how is it related to more recent advances about the molecular mechanisms of development?

The Spemann-Mangold experiment involved a transplantation of embryonic tissue removed from the blastopore lip of newt blastulae. When grafted to the ventral part of another embryo at about

the same stage, this material induced a whole new body axis and resulted in a secondary embryo attached to the larger embryo. According to the standard interpretation at the time, the blastopore lip tissue has the potential of organizing dividing embryonic cells such that they will form a new body axis, hence the term "organizer". However, the exact explanation of this phenomenon and its implications for normal development remained largely controversial until the 1980s. In particular the finding that many substances including dead tissue can have the same or similar effects called the whole organizer concept in question.
It was eventually shown by molecular studies that the organizer tissue secretes numerous growth factor antagonists that prevent the induction of epidermis in embryonic tissues that had previously been committed for the neural pathway. Classical embryologists had always thought that it was the other way around, i.e., that the neural pathway was induced while epidermis was the default state. Thus, it is unclear if we can say that molecular biologists identified the molecular realizers of a previously known causal role, as current metaphysical thinking would have it.
We could argue at length whether the classical embryologists' knowledge was explanatory and whether it has more recently been reduced to the molecular level, however, it would be a mistake to focus exclusively on its explanatory achievements. Taking a practice-oriented approach, I will show that the most important contribution was due to the fact that this kind of knowledge about the effect of certain manipulations such as the Spemann-Mangold experiment could be successfully integrated into the investigative strategies of molecular biology during the 1980s and 90s, strategies which led to the identification of numerous genes and proteins that specify the main body axes in early development. Molecular developmental biologists thus created a kind of inter-level experimental practice combining techniques and investigative strategies from classical embryology and from molecular biology. I show here that within these inter-level practices the classical experimental techniques played the role of measurements that were used to determine the

biological activities of embryonic tissues as well as of isolated molecules.

Scientific Definitions and a New Problem for Pyrrhonian Scepticism

Wednesday, February 27, 11.00 – 11.40
Room 24, Seminargebäude 106

Benjamin Wilck
Humboldt University Berlin

My paper identifies a previously unnoticed problem with the application of Pyrrhonian scepticism to scientific principles, in particular geometrical definitions. In the Outlines of Scepticism (book I, sections 8–10), Sextus Empiricus defines the sceptical method as an ability to suspend belief about any given proposition by constructing pairs of opposing and equally convincing arguments. In Adversus Mathematicos (= M) I–VI, Sextus nonetheless presents a series of straightforward refutations of scientific doctrines rather than oppositions of arguments and counterarguments. That's why commentators have thought that the method deployed in M I–VI is not Pyrrhonian scepticism, but is rather negative dogmatism (Pappenheim, 1874: 16–17; Apelt, 1891: 258–259; Zeller, 1923: 51n2; Janáček, 1972; Russo, 1972: viii n2; Pellegrin et. al., 2002: 23–24; cf. Barnes, 1988: 76–77; Desbordes, 1990: 169). Recently, however, it has become widely accepted among scholars that the apparent lapse from Pyrrhonian scepticism into negative dogmatism, which we find in M I–VI, can be rectified by supplementing additional arguments opposing Sextus' refutational arguments (Blank, 1998, I–IV; Desbordes, 1998: 168; cf. Barnes, 1988: 72–77; Morison, 2004: section 5).
Against this I present a counterexample. While the aforementioned strategy accounts for scientific theorems, which are usually accompanied by a proof, it fails in the case of definitions, for which there is no proof nor justification of some other sort. Sextus' attack on geometry in M III, Against the Geometers, serves as an exemplary model of this difficulty, as Sextus there launches an overall attack on the art of geometry, directed primarily towards definitions. In particular, my

paper studies one single example of Sextus' attacks on particular geometrical definitions, namely the argument at M III.95–97 against Euclid's definition of the straight line as "the line which is placed equally with its parts." The argument is presented as a deductive counterargument to the given definition of the straight line, whereas Sextus does not provide any positive argument in favour of that definition. I argue that we will not even be able to supply any such argument, as there simply is no deductive argument that matches the counterargument at M III.95–97 in argumentative strategy.
All the arguments in M I–VI against particular scientific definitions turn out not to be applicable to particular scientific definitions; thus, they cannot be instances of Pyrrhonian scepticism. Neither the standard (Annas and Barnes, 1985: 24; 39; 82–83; 98; 102; 121–122; cf. Striker, 1983: 100; Hankinson, 1995: 159) nor the most recent (Morison, 2011) interpretations of Pyrrhonian scepticism give a satisfying account of Sextus' arguments against particular definitions. Hence, although Pyrrhonian scepticism is supposed to be applicable to all kinds of proposition or belief, there turns out to exist one type of proposition to which it does not apply, namely scientific definitions.

Program Explanations and Mathematical Realism

Tuesday, February 26, 15.10 – 15.50
Room 24, Seminargebäude 106

Krzysztof Wójtowicz
University of Warsaw

Mathematical explanations are an important (sub-) category of scientific explanations. The central claim is that mathematical theorems explain the phenomena by appealing to some abstractly specified properties of the system in question – not by describing the causal nexus or the detailed mechanisms. One of the interesting approaches is the programming account: mathematical theorems are interpreted as imposing some modal constraints on the world, i.e. "programming" it (so the proposal is based on the more general idea of program explanation). One of the interesting questions to discuss is the problem of the necessary background assumptions: what do we really need in order to discover and justify these constraints?

In the talk I will focus on examples from discrete mathematics, as the problem can be formulated in a clear way (in particular, the problem of idealisations can be – in principle – dismissed). The first example concerns the efficiency of algorithms: sometimes they cannot be improved, and we seek the explanation of this fact rather in results from complexity theory, which provide theoretical bounds for complexity – not in the particular causal stories. The second example is the consistency of PA. No inconsistency has been found yet, and the explanation has a mathematical character (we have a consistency proof within ZFC, and this explains the failure of the possible search of inconsistency). Similarly, the explanation, why nobody has ever found a quick (i.e. polynomial algorithm) for checking whether some formula of propositional calculus is a tautology is mathematical (and the P=NP? problem is involved). In many cases, we need some strong resources: ZFC or perhaps even more (as it is not clear, what assumptions are necessary in order to solve the P=NP? problem), which means, that discovering the properties programming the physical processes requires the use of strong mathematical theories.

The problem becomes acute from the point of view of mathematical realism: if we interpret mathematical theories as expressing objective truths (in particular: as describing some well-established mathematical reality), it is important to investigate, how strong the necessary assumptions are. (The results from reverse mathematics are also of importance, but this topic can only be briefly mentioned here).

Mathematical theorems play the role of programming properties of the physical world, providing in particular explanations for physical (biological, medical, chemical) phenomena – in an abstract form.

We need mathematical assumptions in order to prove these theorems – and these assumptions might be fairly strong. This applies even to the case, when the theorems concern rather simple situations and processes (not involving complicated idealisations). This means, that – in a sense – the modal constraints are identified via theorems, which in turn require assumptions, which can only by justified by highly abstract conceptual analysis. The examination of their status is crucial – in particular, from the point of view of mathematical realism.

On Telic and Instrumental Values in Framing Human Control over Nature

Tuesday, February 26, 14.30 – 15.10
Room 26, Seminargebäude 106

Li-An Yu
Bielefeld University

I aim to add a new dimension to the debates on values and science, which is useful for understanding and characterizing policy-driven scientific research and science-based policy-making against the background of multifarious relations among values, science, society, and nature at large (Longino, 1990; Lacey, 1999; Jasanoff, 2010; Kitcher, 2010; Schieke et al. 2011; Steel, 2015; Biddle et al. 2017). In order to deal with values involved in science and society, I propose an axiological framework for understanding and characterizing human control in terms of telic and instrumental values. The former encompass basic values for structuring action such as justice, utility, harmony with nature, and also truth (see below), and the latter are instrumental for achieving such goals. As science is constituted by a particular set of values of control, my objective is to elaborate variants of human control by a comparative study of cultural values in Western and East Asian traditions. Thus, we can see alternatives to the existing distinctions among values in the contemporary epistem-

ic and ethical debates about anthropogenic climate change.
Being responsible for the terrestrial climate, or human flourishing (Kitcher, 2010) can be distinguished from other forms of responsibility by a particular cultural value commitment in science, that is, by assuming that scientific truth is actually a telic value which is implemented in technological utility. Truth of this sort is a special form of gaining human control. This connection arises from Baconian philosophy, where understanding is tied up with practical use. Gathering scientific truths tends to enhance options for intervening in the course of nature, and is a presupposition for improving our living conditions – as well as coping with the side-effects of this endeavor. Any science-based action for combating the terrestrial climate change in order to achieve human flourishing requires this value commitment.
By contrast, we would not expect an individual or collective ethical agency to endorse taking action by doing research, if the relationship between truth and utility was not accepted. Yet, there are other possible legitimate telic values emphasized in different cultures, and they can exhibit a tension with the Baconian value commitment, such as liberation from sufferings and harmony with nature in East Asian culture. The former would not suggest maximizing utility as a legitimate telic value in response to the changing external conditions. The latter rather places emphasis on local adaptation measures than on controlling the climate on a global scale. In order to formulate a widely acceptable climate policy, a community needs to consider additional telic values, and accomplish a trade-off among them when they are in conflict with each other. The expertise of the socially responsible philosopher of science (Kitcher, 2011; Kourany, 2011) does have a role to play in this endeavor. On the whole, I seek to reexamine and reformulate the epistemic and ethical debates on climate modelling, climate change denialism, global and intergenerational justice and adaptation measures.

Idealization and Understanding with Diagrammatic Biological Models

Wednesday, February 27, 11.40 – 12.20
Room 23, Seminargebäude 106

Martin Zach
Charles University

It has long been argued that idealized model schemas cannot provide us with factive scientific understanding, precisely because these models employ various idealizations; hence, they are false, strictly speaking (e.g. Elgin 2017, Potochnik 2015). Others defend a middle ground (e.g. Mizrahi 2012), but only few espouse (in one way or another) the factive understanding account (e.g. Reutlinger et al. 2017, Rice 2016).

In this talk, and on the basis of the model schema of metabolic pathway inhibition, I argue for the conclusion that we do get factive understanding of a phenomenon through certain idealized and abstract model schemas.

As an example, consider a mechanistic model of metabolic pathway inhibition, specifically the way in which the product of a metabolic pathway feeds back into the pathway and inhibits it by inhibiting the normal functioning of an enzyme. It can be said that such mechanistic model abstracts away from various key details. For instance, it ignores the distinction between competitive and non-competitive inhibition. Furthermore, a simple model often disregards the role of molar concentrations. Following Love and Nathan (2015) I submit to the view that neglecting concentrations from a model is an act of idealization. Yet, models such as these do provide us with factive understanding when they tell us something true about the phenomenon, namely the way in which it is causally organized, i.e. by way of negative feedback (see also Glennan 2017). This crucially differs from the views of those (e.g. Strevens 2017) who argue that idealizations highlight causal irrelevance of the idealized factors. For

the phenomenon to occur, it makes all the difference precisely what kind of inhibition is at play and what the molar concentrations are.
Finally, I will briefly distinguish my approach to factive understanding from those of Reutlinger et al. (2017) and Rice (2016). In Reutlinger et al. (2017), factive (how-actually) understanding is achieved by theory-driven de-idealizations, however, as such it importantly differs from my view which is free of such need. Rice (2016) suggests that optimization models provide factive understanding by providing us with true counterfactual information about what is relevant and irrelevant, which, again, is not the case in the example discussed above.

The Black Box Problem and the Norms of Explainable AI

Tuesday, February 26, 12.20 – 13.00
Room 24, Seminargebäude 106

Carlos Zednik
University Magdeburg

Machine Learning (ML) methods are a major catalyst for progress in Artificial Intelligence (AI). Unfortunately, computers programmed using ML methods such as Deep Learning (DL) are increasingly opaque: it is difficult to “look inside” these systems so as to know how or why they do what they do. As a consequence, these systems are less likely to be trusted and understood, as well as more difficult to fix. The challenges posed by the opacity of ML-programmed computers is known in AI as the Black Box Problem.
The nascent Explainable AI research program is dedicated to solving the Black Box Problem. As is the case for many nascent research programs, however, several foundational issues remain unresolved. In particular, it remains unclear what it takes to “explain” an ML-programmed computer, and how explaining in this context relates to other epistemic achievements such as describing, predicting, understanding, and intervening. This talk aims to articulate the

norms of Explainable AI, and deploy these norms to evaluate recent research. To this end, inspiration will be sought in philosophical work on scientific explanation in psychology and neuroscience, most notably work that centers on David Marr's levels of analysis account. Indeed, the challenge of rendering transparent an ML-programmed computer for a particular user is akin to answering specific kinds of questions about a computational system at a particular level of analysis: "what" and "why" questions at the computational level, "how" questions at the algorithmic level, and "where" questions at the implementational level.

A first, "analytic", branch of Explainable AI aims to solve the Black Box Problem by deploying experimental methods and mathematical analyses not unlike the ones being deployed in psychology and neuroscience. For example, visualizations have been used to characterize the sensitivity of specific nodes and layers of DL-trained networks to specific kinds of inputs. This kind of research seeks to illuminate the flow of information through the network, thereby answering "how" questions at the algorithmic level. Alas, much has yet to be done to render this flow of information understandable to human observers; accordingly, much might still be gained by co-opting special-purpose analytic techniques from psychology and neuroscience.

A second, "synthetic", branch of Explainable AI aims to develop new ML-programmed computers that are not only tasked with solving a particular problem, but also with issuing a human-understandable "explanation" of the machine-generated solution. This kind of research is well-suited for answering "what"- and "why"-questions at the computational level. However, attention must be paid to the threat of "just-so stories": Although a particular "explanation" may seem plausible to and be understood by a particular human observer, it need not reflect the causally-relevant variables that actually generate the machine's output. Thus, more work needs to be done to supplement this approach with methods for answering "how" and "where" questions.

Overall, by analyzing the notion of ‘opacity’ in Marrian terms, it is possible to better understand the nature of the Black Box Problem, as well as to better evaluate recently proposed solutions from Explainable AI. Although this research program has made a good start, much work has yet to be done to satisfy the relevant explanatory norms and to thereby render ML-programmed computers transparent.

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:

Monday, Feb. 25:	08:00 - 09:00 in building 105, next to the main entrance 09:30 - 17:30 in building 106, Room S21
Tuesday, Feb. 26:	08:45 - 17:30 in building 106, Room S21
Wednesday, Feb. 27:	08:45 - 17:30 in building 106, Room S21

Conference venue

The conference is located in buildings 105 (Hörsaalgebäude) and 106 (Seminargebäude) at the University of Cologne. The address of the conference venue is Universitätsstraße 35 & 37, 50931 Cologne.
To reach the conference venue from the main station, take the underground U16 (Bonn Hbf/Bonn Bad-Godesberg/Wesseling) or U18 (Bonn Hbf/Brühl/Klettenbergpark) from below main station and exit at Neumarkt. From Neumarkt take the tram U9 (Sülz/Universität) to Universität. You will need about 30 minutes in total.

Conference rooms

The parallel sessions and symposia will be held in the Seminargebäude (106), Rooms 22-26 (2nd floor) and Tagungsraum (ground floor). The plenary lectures as well as the GWP meeting will take place in the Hörsaalgebäude (105), Hörsaal C (1st floor).

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. Merely the main canteen (“Mensa”) is not handicapped accessible, but there are several different possibilities for lunch. For support just contact our crew at the registration and information desk.

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 (UniKoeln-WEB) free of charge. Please contact our assistants at the information desk for a username and password.

Luggage room

You can leave your luggage at the registration and information desk in Room S21 during the above mentioned service times.

ATM

The nearest ATM (Sparkasse) is located at the Philosophikum (building 103), at the side facing the Universitätsstraße. A second ATM (Sparkasse) is located at the south entrance of the main canteen

("Mensa"). There is also an ATM at Kiosk Pur (Zülpicher Str. 174, intersection of Zülpicher Straße and Universitätsstraße).

Coffee and refreshments

Coffee and tea will be served during the refreshment breaks. All refreshments are served in the Seminargebäude (106), 2nd floor. There are also several cafeteria as well as a canteen at the university campus where you can pick up some drinks and sandwiches:

- PhilCafé on the ground floor of the Philosophikum (building 103), 08:00 – 15:00
- Bistro-Uni-E-Raum in the basement of the main building (building 100), 07:30 – 16:00
- Kiosk Pur (Zülpicher Str. 174, intersection of Zülpicher Straße and Universitätsstraße), open all day
- Italian café (outdoor) next to building 105 (Hörsaalgebäude), open all day
- Kiosk on the ground floor of the Philosophikum (building 103), 07:45 – 19:00

Lunch

There are several possibilities for lunch close to the university. Especially Zülpicher Str. and Kyffhäuser Str. offer many (fast food) restaurants like

- Thai fast food restaurant Khun Mae, Kyffhäuser Str. 38
- Falafel fast food restaurant Habibi, Zülpicher Str. 38
- Pizza Pazza, Weyertal 34 (limited seating)

There are further possibilities to have lunch on the campus:

- Bistro-Uni-E-Raum in the basement of the main building (building 100), 07:30 – 16:00
- PhilCafé on the ground floor of the Philosophikum (building 103), 08:00 – 15:00 (sandwiches only)
- Main canteen ("Mensa"), 11:30 – 15:00 (Does not take cash! You need Lunch tickets, which are available at the registration and information desk)

Dinner

For traditional/local food consider:

- Brauhaus Päffgen, Friesenstraße 64-66
- Brauerei zur Malzmühle, Heumarkt 6
- Brauerei Heller, Roonstr. 33 (vegetarian & vegan options)
  (all three serve their own beer)

Further (fairly arbitrary) recommendations are:

- Café Feynsinn, Rathenauplatz 7
  (There are a few nice restaurants around Rathenauplatz)
- Pizzeria 485 Grad, Kyffhäuser Str. 44 (Tuesday & Wednesday)
- Manni's Rästorang, Kyffhäuser Str. 18
- Indian restaurant Thali, Engelbertstr. 9
- Funkhaus, Wallrafplatz 5
- Il Mezzogiorno, Breite Straße 102
- Mumbai Palace, Am Malzbüchel 1
- Burger: Marx und Engels, Hohenzollernring 21-23
- Bay Area Burrito Company, Friesenwall 16-18

Furthermore a nice area for restaurants is the “Belgian Quarter” e.g. on Aachener Str. and Brüsseler Str.

For vegetarian and vegan food consider in particular

- Well Being 1 (Nähe Rudolfplatz) Am Rinkenpfuhl 57 (vegetarian and vegan)
- Edelgrün, Venloer Str. 233 (vegan only)

Tourist information

A tourist information is located in front of the main entrance of the main Cathedral (Kardinal-Höffner-Platz 1). See also <https://www.koelntourismus.de/>.

Police and medical assistance

If you need to call the police or need an ambulance, the emergency number is 112.

On arrival

Transportation between Cologne/Bonn Airport and Cologne main station takes about 15 min with the city trains S12 and S19, or the regional trains RE6 and RE8. Buy a zone 1b single fare ticket for 3,00 EUR – it is valid up to 90 minutes after stamping and will take you to the university too. Taxi costs for the route will be about 33 EUR.

Public transportation in Cologne

If you need to use buses or trams (the latter are identical with the underground in Cologne) you might want to buy a single ticket (3,00 EUR), a 24h ticket (8,80 EUR), or a 4er ticket (12,00 EUR), all for the zone 1b. There is also a 7-day ticket for zone 1b (26,30 EUR). For short distances (up to 4 stations) you can use a short distance ticket (2,00 EUR). The tickets can be purchased in the buses and trams (chip and pin card or coins only); train stations and some tram stations have ticket machines (chip and pin or coins only) as well. You can find information about routes, timetables, and prices at the website of the Kölner Verkehrs-Betriebe: https://www.kvb.koeln/.

To reach the conference venue from the main station, take the underground (below main station) U16 (Bonn Hbf/Bonn Bad-Godesberg/Wesseling) or U18 (Bonn Hbf/Brühl/Klettenbergpark) and exit at Neumarkt. From Neumarkt take the tram U9 (Sülz/Universität) to Universität. You will need about 30 minutes in total.

Taxi

You can phone up and book a taxi from a taxi office; call (24h): +49 (0)221 2882 or book at: <https://www.taxiruf.de/>. A taxi from the university to the main station costs about 15 EUR.

Speakers A – Z