FIRST CONFERENCE OF THE EUROPEAN PHILOSOPHY OF SCIENCE ASSOCIATION

Programme & Book of Abstracts

November 14-17, 2007
Faculty of Philosophy
Complutense University
Madrid, Spain

Programme
& Book of Abstracts
EPSA07

Edited by:

Iñaki SAN PEDRO
Albert SOLÉ

First Conference of the European Philosophy of Science Association

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Faculty of Philosophy,
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Preface by Matti Sintonen

Philosophy of science has been a growth area for decades, and it requires no clairvoyance to see that it will be of increased importance in years to come. In recognition to this a number of European philosophers of science got together in London in September, 2006, to establish The European Philosophy of Science Association (EPSA).

The petition to establish EPSA was signed in Helsinki, Finland, in November, 2006. The Statutes then accepted specify the goal: EPSA is an Association to advance and promote the European tradition in philosophy of science. More specifically, the statutes state that EPSA promotes and advances philosophy of science in Europe; that it furthers contacts among philosophers of science in Europe; that it ensures that information relating to philosophy of science in Europe is regularly circulated amongst members of the academic communities in Europe; that it promotes international philosophical and scientific exchanges on all levels; and that it supports, on an international level, the progress of philosophical studies and their communication to the world of learning and to the educated public.

The most important concrete way of advancing and promoting philosophy of science in Europe certainly is a regular conference where exchange of new ideas can be carried out, information about events and developments can be given and received, and philosophy of science more generally can be propagated. This is to be a forum where the developments of the past years are laid down, the State of Art is presented, and prospects for the future are discussed.

We, the provisional Steering Committee, expected that the new Association would be welcomed by the community – but we were positively surprised by the great number of submissions.

The forms EPSA will take in future are in the shaping. I would like to emphasize, on behalf of the Steering Committee, that we wish the enterprise of EPSA to be open to everyone who share its goals. Moreover, it is of utmost importance for a budding organization such as this that members and friends contribute to the EPSA cause by actively developing its activities.

The Steering Committee unanimously chose Complutense University, Madrid, for its first venue. I take the opportunity of expressing our gratitude to the University. At the same time I wish to thank and congratulate the Local Committee, headed by professor Mauricio Suarez, and the Programme Committee, chaired by professors Mauro Dorato and Miklos Rédei, for the excellent work they have done.

It is a great pleasure for me, on behalf of the Steering Committee, to welcome you all to the first EPSA Conference here in Madrid. Have a productive and enjoyable conference!

Matti Sintonen
President
European Philosophy of Science Association

Preface by Mauricio Suárez

It is an honour for Complutense University and for Spain to host the Founding Conference of the *European Philosophy of Science Association* (EPSA). The Conference is meant to be the first of many biennial occasions for philosophers of science working within Europe to meet regularly and exchange ideas and information. EPSA aims to provide Europeans with an analogous organisation to the North American *Philosophy of Science Association* (PSA). Like PSA, the Association is born out of a desire for collegiality and inclusion among all philosophers of science working within Europe, regardless of nationality.

The conferences are thus intended in a spirit of friendly and admiring competition towards its North American cousins. But we also want to preserve what we take to be particularly European practices and spaces. Thus one significant difference is the desire to hold the biennial conferences in Academic environments as opposed to large convention hotels. This has, admittedly, some disadvantages. First, venue facilities, such as available rooms for parallel sessions, will typically be limited. As a consequence registration will tend to be restricted too. However, EPSA intends its conferences to be small, highly selective occasions. For instance its aim is never to have more than 5 concurrent parallel sessions. Second, delegates will need to travel every day from their hotels to the venue and back. However, using public transport in large cities in order to get to work is a standard European practice, which among other things allows people to get a taste for the pace and rhythm of the city. Besides European Universities are among the oldest in the world, so venues will tend to be significant historic buildings and sites, with interest for delegates to visit in any case.

More locally, we hope that EPSA07 will help further the internationalisation of Spanish philosophy of science, which has been improving at a steady pace for two decades now. This partly reflects the excellent level that the sciences themselves have reached over this period. For instance the state of physics in Spain is nowadays as healthy as any in Europe, with plenty of first rate physicists publishing in the top journals, and similarly for the medical, biological, geological and social sciences. There has also been a sustained effort by the public authorities to open up the Spanish University system to free competition, including bringing in researchers educated abroad who in turn will train young scholars. All of this is having a very nicely beneficial influence upon the state of philosophy of science in this country, with several societies being prominently active – particularly the Spanish Society for Logic, Methodology and Philosophy of Science, and the Spanish Society for Analytical Philosophy.

EPSA is an association that looks to the future, and so do we. All of us at Complutense are willing to help out with anything you might need. We welcome you to our University and the Faculty and we hope that you will enjoy your time among us, as well as the city and generally your stay in Spain.

Mauricio Suárez,
Chair of the Organising Committee, EPSA07

EPSA07: Conference Programme

WEDNESDAY 14 NOVEMBER

15:00: **Registration Opens** ***Foyer of the Faculty of Philosophy, UCM***

16:00 – 17:00 **Opening of the Conference** ***Room: Paraninfo***

Carmen Acebal Sarabia (ViceRector of Research, Complutense University)

Juan Manuel Navarro Cordón (Dean, Faculty of Philosophy, Complutense University)

Mauricio Suárez (Chair, Organising Committee, EPSA07)

Matti Sintonen (President, EPSA)

17: 00 – 17:30 Coffee Break

17:30 – 19:30 Parallel Sessions I

I: General Philosophy of Science (Causation)

Room: Paraninfo

Chair: Carl Hoefer (Autonomous University of Barcelona)

17.30 –18.00 Max Kistler (Université Pierre Mendès-France, Grenoble and Institut Jean Nicod, Paris) “Mechanistic Explanation and Causation” (1)

18.00 – 18.30 Alex Broadbent (University of Cambridge) “The Difference between Cause and Condition” (2)

18.30 – 19.00 Robert Kowalenko (University of Hertfordshire) “A Curve-Fitting Approach to Ceteris Paribus Laws” (3)

19.00 – 19.30 Mathias Frisch (University of Maryland) “Causation and Physics” (4)

II: Formal Methods in the Philosophy of Science (Philosophy of Mathematics)

Room: A217

Chair: Miklós Rédei (London School of Economics)

17.30 – 18.00 Mark van Atten (IHPST(CNRS/Paris I/ENS)) "Phenomenology and Transcendental Argument in Mathematics: Brouwer's 'Bar Theorem'" (5)

18.00 – 18.30 José Ferreirós (University of Seville) "Mathematical Knowledge and the Interplay of Practices" (6)

18.30 – 19.00 Mario Santos-Sousa (Autonomous University of Madrid) "Natural Mathematics: A Pluralistic Approach to Mathematical Cognition" (7)

III: Philosophy of Natural Sciences (Geometry and Matter)

Room: Salón de Grados

Chair: Oliver Pooley (Oxford University)

17.30 – 18.00 Norman Sieroka (ETH Zurich) "Dynamic Agents and Geometrisation: A Weylian Approach towards Theories of Matter" (8)

18.00 – 18.30 Dennis Lehmkuhl (University of Oxford) "Geometrization(s) of Matter" (9)

18.30 – 19.00 Eric Audureau (CEPERC/CNRS/University of Provence) and Gabriella Crocco (CEPERC/CNRS/University of Provence) "Relativity Theory and Poincare's Conception of Space" (10)

19.00 – 19.30 Adán Sus (Autonomous University of Barcelona) "Absolute Objects and General Relativity: Dynamical Considerations" (11)

V: Historical, Social and Cultural Studies of Philosophy of Science (Gender and Values in Science)

Room: Sala de Juntas

Chair: Berna Kilinc (Bogazici University)

17.30 – 18.00 Maria Rentetzi (National Technical University of Athens) "Rose Rand: Between two Different Gendered Cultures of Physics and Philosophy in Interwar Vienna" (12)

18.00 – 18.30 Hans Puehretmayer (University of Vienna) "Beyond Judgemental Relativism: Combining Feminist Standpoint Theories and Critical Realism" (13)

18.30 – 19.00 Kristina Rolin (Helsinki School of Economics) "Science as Collective Knowledge" (14)

THURSDAY 15 NOVEMBER

9:00 – 11:30 Parallel Sessions II

I: General Philosophy of Science (Realism)

Room: Paraninfo

Chair: Stathis Psillos (University of Athens)

9.00 – 9.30 Panu Raatikainen (University of Helsinki) “Theories of Reference and the Philosophy of Science” (15)

9.30 – 10.00 Fabrice Pataut (IHPST) “Verifiability, Scientific Realism and Constructive Empiricism” (16)

10.00 – 10.30 Wang-Yen Lee (University of Cambridge) “The Probative Force and Dialectical Value of Structure-Oriented Second-Order Abductive Arguments for Scientific Realism” (17)

10.30 – 11.00 Axel Gelfert (National University of Singapore) “Coherence and Indirect Confirmation between Scientific Models: A Case Study and its Epistemological Implications” (18)

11.00 – 11.30 Andreas Karitzis (University of Athens) “Defending Realism: Can Ontology Do the Trick?” (19)

II: Formal Methods in Philosophy of Science (Mathematics and Logic)

Room: A217

Chair: Wolfgang Spohn (University of Konstanz)

9.30 – 10.00 Demetra Christopoulou “How to Deal with Ianus’ Face of Natural Numbers” (20)

10.00 – 10.30 Georg Schiemer (University Vienna) “Frege and Peano on Quantification and Logical Scope” (21)

10.30 – 11.00 Jean-Pierre Marquis (University of Montréal) “Mathematical Forms and Forms of Mathematics: Homotopy Types” (22)

11.00 – 11.30 Charlotte Werndl (University of Cambridge) “Mathematical Definitions that Capture Real-World Phenomena or Features: On the Formation and Justification of Definitions” (23)

III: Philosophy of Natural Sciences (Principles in Physics)

Room: Salón de Grados

Chair: Mauro Dorato (University of Rome III)

9.00 – 9.30 Maarten Van Dyck (Ghent University) “The Historical A Priori: The Case of Inertia” (24)

9.30 – 10.00 Michael Stöltzner (University of Wuppertal) “Can the Principle of Least Action be Considered a Relativised a Priori?” (25)

10.00 – 10.30 Chrysovalantis Stergiou (National Technical University of Athens) “Some Remarks on Causal Processes in Classical and Local Quantum Physics” (26)

10.30 – 11.00 Laura Felline (University of Rome III) “Structural Explanation: From Relativity to Quantum Mechanics” (27)

11.00 – 11.30 Richard Healey (University of Arizona) “Gauge Symmetry and the Theta-Vacuum” (28)

IV: Philosophy of Social Sciences (Psychology & Rationality)

Room: Sala de Juntas

Chair: Claude Debru (ENS)

9.00 – 9.30 Caterina Marchionni (Erasmus University Rotterdam) and Jack Vromen (Erasmus University Rotterdam) “Ultimate and Proximate Explanations of Cooperative Behaviour: Plurality or Integration?” (29)

9.30 – 10.00 Athanasios Raftopoulos (University of Cyprus) “Ambiguous Figures and Representationalism” (30)

10.00 – 10.30 Corrado Sinigaglia (University of Milan) “The Shared Space of Actions: Mirror Neurons and Motor Intentionality” (31)

10.30 – 11.00 Antonio Zilhao (University of Lisbon) “Incontinence, Honouring Sunk Costs and Rationality” (32)

11.00 – 11.30 Simone Gozzano (University of L'Aquila) “Multiple Realizability and Identity’’ (33)

11:30 – 12:00 Coffee Break

12:00 – 13:15 **Plenary Lecture** ***Room: Paraninfo***

Chair: Maria Carla Galavotti (University of Bologna)

Anne Fagot-Largeault (Collège de France): “Styles in Philosophy of Science”

13:15 – 15:00 Lunch Break

15:00 – 17:00 Parallel Sessions III

I: General Philosophy of Science (Models and Representation)

Room: Paraninfo

Chair: Xavier de Donato (UNAM, Mexico)

15.00 – 15.30 Tarja Knuuttila (University of Helsinki) “Some Consequences of Pragmatism: Whatever Happened to the Notion of Representation in the Philosophy of Science” (34)

15.30 – 16.00 Demetris Portides (University of Cyprus) “Idealization and Abstraction in Scientific Modelling” (35)

16.00 – 16.30 Uskali Maki (University of Helsinki) “Models and the Locus of their Truth” (36)

16.30 – 17.00 Marion Vorms (IHPST) “Understanding Theories: Formats Matter” (37)

II: Formal Methods in Philosophy of Science (Causation and Probability)

Room: A217

Chair: Mathias Frisch (University of Maryland)

15.00 – 15.30 Bert Leuridan (Ghent University) “The Need for Causal Modellling in Philosophy of Science” (38)

15.30 – 16.00 Alessio Moneta (Max Planck Institute of Economics) “Can Graphical Causal Inference Be Extended to Nonlinear Models? An Assessment of Nonparametric Independence Tests” (39)

16.00 – 16.30 Sun Demirli (Bogazici University) “Does Lewis’ Account of Chance Bear on Scientific Ontology?” (40)

16.30 – 17.00 Jan Sprenger (University of Bonn) “Statistics do not Require Frequentist Justifications” (41)

III: Philosophy of Natural Sciences (Space and Time)

Room: Salón de Grados

Chair: Carl Hoefer (Autonomous University of Barcelona)

15.00 – 15.30 Oliver Pooley (Oxford University) “Background Independence” (42)

15.30 – 16.00 Hanoch Ben-Yami (Central European University) “Backward Light-cone Simultaneity, with Special Application to the Twin Paradox” (43)

16.00 – 16.30 Laszlo E. Szabó (Eötvös University) “Empirical Foundation of Space and Time” (44)

16.30 – 17.00 Steven Savitt (University of British Columbia) “The Transient Nows” (45)

V: Historical, Social and Cultural Studies of Philosophy of Science (Case Studies)

Room: Sala de Juntas

Chair: Mieke Boon (University of Twente)

15.00 – 15.30 Nathalie Gontier (Free University of Brussels, VUB) “Philosophy of Anthropology and the Gradualism versus Punctuated Equilibrium Debate” (46)

15.30 – 16.00 Sabine Plaud (University of Paris I) “On Photographs and Phonographs: The Influence of Some Technical Innovations on Ernst Mach’s and Ludwig Wittgenstein’s Conceptions of Pictures” (47)

16.00 – 16.30 Floriane Blanc (LEPS) “Analyzing an Aspect of the Inaugural Lectures of the Paris Museum of Natural History: An Appropriate Concept of Representation” (48)

16.30 – 17.00 Geerdt Magiels (Free University of Brussels) and Gustaaf Cornelis (Free University of Brussels) “Dr Jan Ingen Housz, The Forgotten Discoverer of Photosynthesis” (49)

17:00-17:30 Coffee Break

17:30 – 19:30 Parallel Sessions IV

I: General Philosophy of Science (Structuralism)

Room: Paraninfo

Chair: Dennis Dieks (Utrecht University)

17.30 – 18.00 F.A. Muller (Erasmus University Rotterdam/Utrecht University) “The Concept of Structure” (50)

18.00 – 18.30 Holger Lyre (University of Bonn) “Structural Realism: Intermediate View and Laws of Nature” (51)

18.30 – 19.00 Angelo Cei (University of Leeds) “A Form of Ramseyan Humility? David Lewis’s version of the Ramsey Sentence and the debate on Structural Realism”(52)

19.00 – 19.30 Juha Saatsi (University of Leeds) “Whence Ontological Structural Realism?” (53)

IIIa: Philosophy of Natural Sciences (Reductionism)

Room: A217

Chair: Henk De Regt (University of Amsterdam)

17.30 – 18.00 Peter Fazekas (Budapest University of Technology and Economics) “Different Models of Reduction and the Inevitability of Bridge-Laws” (54)

18.00 – 18.30 Markus Eronen (University of Osnabrück) “Reductionism and Problems of Explanatory Pluralism” (55)

18.30 – 19.00 Jens Harbecke (University of Lausanne/University of Bern) “Conservative and Eliminative Reduction: Exploring the Spectrum” (56)

19.00 – 19.30 Simon Bowes (University of Sussex) “Natural Kinds and Reduction in the Cognitive Sciences” (57)

IIIb: Philosophy of Natural Sciences (Quantum Theory)

Room: Salón de Grados

Chair: Pedro Sánchez and Albert Solé (Complutense University)

17.30 – 18.00 Robin Hendry (Durham University) “The Chemical Bond: Structure, Energy and Explanation” (58)

18.00 – 18.30 Peter Vickers (University of Leeds) “Bohr’s Theory of the Atom: Content, Closure and Consistency” (59)

18.30 – 19.00 Hernán Pringe (University of Pittsburgh) “Cassirer and Bohr on Intuitive and Symbolic Knowledge in Quantum Theory” (60)

19.00 – 19.30 Brigitte Falkenburg (University of Dortmund) “Wave-particle Duality in Physical Practice” (61)

V: Historical, Cultural and Social Studies of Science (Carnap/Commerce)

Room: Sala de Juntas

Chair: Friedrich Stadler (Vienna Circle Institute)

17.30 – 18.00 Thomas Uebel (University of Manchester) “Carnap, Explication and Ramseyfication” (62)

18.00 – 18.30 Paul Franco (University of Pennsylvania) “The Constitutive A Priori and the Quine/Carnap Debate” (63)

18.30 – 19.00 Lieven Decock (Free University of Amsterdam) “Carnap and Quine on the Analytic-Synthetic Distinctions” (64)

19.00 – 19.30 Gurol Irzik (Bogazici University) “Is Science Being Commercialised? A Manifesto for Philosophers of Science” (65)

<u>FRIDAY 16 NOVEMBER</u>

9:00 – 11:30 Parallel Sessions V

I: General Philosophy of Science (Nature of Science)

Room: Paraninfo

Chair: Matti Sintonen (University of Helsinki)

9.00 – 9.30 Jan Faye (University of Copenhagen) “Interpretation in the Natural Sciences” (66)

9.30 – 10.00 Daniel Sirtes (University of Basel) and Marcel Weber (University of Basel) “Scientific Significance Scrutinized” (67)

10.00 – 10.30 Jeremy Kessler (University of Cambridge) “Analogy by Exemplar: A Kuhnian Alternative to Hesse’s Account of Analogy in Science” (68)

10.30 – 11.00 Thomas Reydon (Leibniz University of Hannover) “Natural Kinds as Tools for Philosophers of Science” (69)

11.00 – 11.30 Justin Biddle (Bielefeld University) “The Ambiguously Social Character of Longino’s Theory of Science” (70)

II: Formal Methods in Philosophy of Science (Statistics and Confirmation)

Room: A217

Chair: Stephan Hartmann (Tilburg University)

9.00 – 9.30 Wolfgang Spohn (University of Konstanz) “Measuring Ranks by the Complete Laws of Iterated Contraction” (71)

9.30 – 10.00 Francesco Calandra (University of Trieste) and Gustavo Cevolani (University of Bologna) “Belief Revision and Truth-Approximation” (72)

10.00 – 10.30 Theo Kuipers (University of Groningen) “Bridging the Gap Between Belief Revision and Truth Approximation” (73)

10.30 – 11.00 Carl Wagner (University of Tennessee) “Old Evidence and New Explanation” (74)

11.00 – 11.30 Roberto Festa (University of Trieste), Vincenzo Crupi (University of Trieste) and Carlo Buttasi (University of Trieste) “The Grammar of Confirmation” (75)

IIIa: Philosophy of Natural Sciences (Biomedicine, Ecology)

Room: A25

Chair: Sophia Efstathiou (UCSD / London School of Economics)

9.00 – 9.30 Giovanni Boniolo (IFOM) and Marcello D'Agostino (University of Ferrara) "Biomedical Networks and their Logics" (76)

9.30 – 10.00 Ulrich Krohs (University of Hambrug) "Epistemic Consequences of two Different Strategies for Decomposing Biological Networks" (77)

10.00 – 10.30 Astrid Schwarz (Technical University Darmstadt) "Commuting Concepts and Objects in Scientific Ecology" (78)

10.30 – 11.00 Werner Callebaut (Konrad Lorenz Institute for Evolution and Cognition Research) "Contigency and Inherency in (Eco)EvoDevo" (79)

11.00 – 11.30 Julian Reiss (Erasmus University Rotterdam) "Is There a Role for Clinical Expertise in Evidence-Based Medicine?" (80)

IIIb: Philosophy of Natural Sciences (Philosophy of Physics)

Room: Salón de Grados

Chair: Isabel Guerra (Complutense University)

9.00 – 9.30 Henrik Zinkernagel (University of Granada) "Causal Fundamentalism in Physics" (81)

9.30 – 10.00 Elena Castellani (University of Florence) "Dualities and Intertheoretic Relations" (82)

10.00 – 10.30 Matteo Morganti (London School of Economics/IHPST) "Individual Particles, Properties and Quantum Statistics" (83)

10.30 – 11.00 Daniel Parker (Virginia Tech) "Was There an Ice Cube There, or Am I Just Remembering It?: Reposing the Question of the Veracity of Memory" (84)

11.00 – 11.30 Dennis Dieks (Utrecht University) "Structuralism, Symmetry and Identical Particles" (85)

IV: Philosophy of Social Sciences (Philosophy of Economics)

Room: Sala de Juntas

Chair: Uskali Maki (University of Helsinki)

9.00 – 9.30 Ghislaine Idabouk (University of Paris VII) "Randomness, Financial Markets and the Brownian Motion: A Reflection on the Role of Mathematics, its Interactions with Economics and the Ideological Implications in the Financial Theory of the late 20th Century" (86)

9.30 – 10.00 Aki Lehtinen (University of Helsinki) "Farewell to Arrow's Theorem" (87)

10.00 – 10.30. Stuart Yasgur (London School of Economics) “The Money Pump and the Justification of the Transitivity Condition” (88)

10.30 – 11.00 Menno Rol (University of Groningen) “Explanatory Progress and Tendencies in Economics” (89)

11.00 – 11.30 Hans Radder (Free University of Amsterdam) “Mertonian Values, Scientific Norms and the Commercialisation of Academic Research” (90)

11:30 – 12:00 Coffee Break

12:00 – 13:15 **Plenary Lecture** ***Room: Paraninfo***

Chair: Stathis Psillos (University of Athens)

Ilkka Niiniluoto (University of Helsinki): “Theory-Change, Truthlikeness and Belief-Revision”

13:15 – 15:00 Lunch Break

15:00 – 17:00 Parallel Sessions VI

Ia: General Philosophy of Science (Simulation)

Room: Paraninfo

Chair: Martin Carrier (University of Bielefeld)

15.00 – 15.30 Erika Mattila (London School of Economics) “Explanatory and Predictive Functions of Simulations” (91)

15.30 – 16.00 John Michael (University of Vienna) “Simulation as an Epistemic Tool between Theory and Practice” (92)

16.00 – 16.30 Johannes Lenhard (Bielefeld University) “The Platform Concept of Simulation Modelling” (93)

16.30 – 17.00 Claude Debru (ENS) “Neurophilosophy of Sleep and Dreaming” (94)

Ib: General Philosophy of Science (Experiment and Observation)

Room: Sala de Juntas

Chair: John Worrall (London School of Economics)

15.00 – 15.30 Valeriano Iranzo (University of Valencia) “Severe Tests and Use-Novelty” (95)

15.30 – 16.00 Theodore Arabatzis (University of Athens) “Rethinking the Theory-Ladeness of Observation: Implications for the New Experimentalism” (96)

16.00 – 16.30 Tuomo Tiisala (University of Chicago) “Hacking’s Verificationism” (97)

16.30 – 17.00 Ioannis Votsis (University of Düsseldorf) “Making Contact with Observations” (98)

IIIa: Philosophy of Natural Sciences (Biology & Values)

Room: A217

Chair: Samir Okasha (University of Bristol)

15.00 – 15.30 Fabrice Gzil (IHPST/Collége de France) “Animal Models of Alzheimer’s Disease and Cognitive Ageing” (99)

15.30 – 16.00 Sophia Efstathiou (UCSD, visiting LSE) “Articulating ‘Race’ in Genetic Terms” (100)

16.00 – 16.30 Emmanuel d’Hombres (University of Paris VII) “Differentiation as a Modality of Evolution: From Biology to Sociology and Back” (101)

16.30 – 17.00 Torsten Wilholt (Bielefeld University), “Values in Science and the Problem of Bias” (102)

IIIb: Philosophy of Natural Sciences (Quantum Probability)

Room: Salón de Grados

Chair: Iñaki San Pedro (Complutense University)

15.00 – 15.30 Patrick Suppes (Stanford University) “Upper Probabilities, Entanglement and Decoherence” (103)

15.30 – 16.00 Giovanni Valente (University of Maryland) “Is There a Stability Problem for Bayesian Noncommutative Probabilities?” (104)

16.00 – 16.30 Gerd Grasshoff (University of Bern), Samuel Protmann (University of Bern) and Adrian Wüthrich (University of Bern), “Minimal Assumption Derivation of a Bell-type Inequality” (105)

16.30 – 17.00 Tomasz Placek and Leszek Wronski (Jagiellonian University) “On the Infinite EPR-like Correlations” (106)

17:00 – 17:30 Coffee Break

17:30 – 19:30 **General Assembly of EPSA** ***Room: Paraninfo***

SATURDAY 17 NOVEMBER

9:00 – 11:30 Parallel Sessions VII

I: General Philosophy of Science (Prediction, Chance, and Error)

Room: Paraninfo

Chair: Theo Kuipers (University of Groningen)

9.00 – 9.30 Gerhard Schurz (University of Düsseldorf) "Universal vs. Local Prediction Strategies: A Game Theoretical Approach to the Problem of Induction" (107)

9.30 – 10.00 Sorin Bangu (University of Western Ontario) "The Principle of Indifference and Statistical Tests: A Critique of Gillies' Eliminative Strategy" (108)

10.00 – 10.30 Cornelis Menke (Bielefeld University) "On the Explanation of Predictive Success due to Chance" (109)

10.30 – 11.00 Marcello D'Agostino (University of Ferrara) and Corrado Sinigaglia (University of Milan) "Forecasting Accuracy and Subjective Probability" (110)

11.00 – 11.30 Jacques Dubucs (IHPST – CNRS) "Intended Models" (111)

IIIa: Philosophy of Natural Sciences (Biology)

Room: A25

Chair: Giovanni Boniolo (IFOM)

9.00 – 9.30 Andrés L. Jaume (University of Salamanca) "Are all Biological Functions Adaptations?" (112)

9.30 – 10.00 Marshall Abrams (University of Alabama at Birmingham) "Radical Pluralisms about Units of Selection" (113)

10.00 – 10.30 Predrag Sustar (University of Rijeka) "Functions in the Morphospace" (114)

10.30 – 11.00 Johannes Persson (Lund University) "Mechanism-as-activity and the Threat of Polygenic Effects" (115)

11.00 – 11.30 Samir Okasha (Bristol University), "On the Significance of R. A. Fisher's Fundamental Theorem of Natural Selection" (116)

IIIb: Philosophy of Natural Sciences (Quantum Mechanics)

Room: Salón de Grados

Chair: Henrik Zinkernagel (University of Granada)

9.00 – 9.30 Alexei Grinbaum (CEA-Saclay) “Reconstruction of Quantum Theory” (117)

9.30 – 10.00 Aristidis Arageorgis (National Technical University of Athens) “Holism and Nonseparability by Analogy” (118)

10.00 – 10.30 George Darby (University of Leeds) “Is Quantum Vagueness Vagueness?” (119)

10.30 – 11.00 Michael Seevinck (Utrecht University) “On the Merits of Modeling Quantum Mechanics Using Semi-Classical Models” (120)

11.00 – 11.30 Maria Luisa Dalla Chiara (University of Florence), Roberto Giuntini (University of Cagliari), Roberto Leporini (University of Bergamo) and Giuliano Toraldo di Francia (University of Florence), “Holistic Semantics: From Quantum Theory to Music” (121)

IV: Philosophy of Social Sciences (Realism, Relativism, Normativity)

Room: Sala de Juntas

Chair: Alfred Nordmann (Darmstaadt University)

9.00 – 9.30 Daniel Andler (University of Paris IV, Sorbonne/ENS) “Naturalism and the Scientific Status of the Social Sciences” (122)

9.30 – 10.00 Hans Bernhard Schmid (University of Basel) “Intentional Autonomy and Methodological Individualism” (123)

10.00 – 10.30 Caroline Baumann (Cambridge University) “Reconsidering Gilbert’s Account of Norm-Guided Behaviour” (124)

10.30 – 11.00 Erik Weber (Ghent University)“Social Mechanisms, Causal Inference and the Policy Relevance of Social Science” (125)

11.00 – 11.30 Martin Kusch (University of Cambridge) “Boghossian on Relativism and Constructivism – A Critique” (126)

V: Historical, Social and Cultural Studies of Philosophy of Science (Vienna Circle / Feyerabend)

Room: A217

Chair: Thomas Uebel (University of Manchester)

9.00 – 9.30 Sirkku Ikonen (University of Helsinki) “The Vienna Circle, Lebensphilosophie and the Analytic-Continental Divide in Philosophy” (127)

9.30 – 10.00 Edwin Glassner (Institute Vienna Circle) “Between Pure Intuition and Popular Impercipience: Schlick and the Early Reception of Relativity Theory” (128)

10.00 – 10.30 Flavia Padovani (University of Geneva) “Topologies of Time in the 1920’s: Reichenbach, Carnap, Lewin” (129)

10.30 – 11.00 Matteo Collodel (University of Venice "Ca' Foscari") “The Last ‘Viennese’: Feyerabend, Logical Empiricism and the Vienna Circle” (130)

11.00 – 11.30 Paul Hoyningen-Huene (University of Hannover) and Eric Oberheim (Humboldt University of Berlin) “Reassessing Feyerabend’s Philosophy” (131)

11:30 – 12:00 Coffee Break

12:00 – 13.15 **Plenary Lecture** ***Room: Paraninfo***

Chair: Mauricio Suárez (Complutense University)

Michael Friedman (Stanford University): “Einstein, Kant and the A Priori”

13:15 – 15:00 Lunch Break

15:00 – 17:00 Parallel Sessions VIII

I: General Philosophy of Science (Theory and Phenomena)

Room: Paraninfo

Chair: Robin Hendry (University of Durham)

15.00 – 15.30 Martin Carrier (Bielefeld University) “Theories for Use: On the Bearing of Basic Science on Practical Problems” (132)

15.30 – 16.00 Christian Sachse (University of Lausanne) “Relation of Theories and Concepts” (133)

16.00 – 16.30 Mieke Boon (University of Twente) “Phenomena: A Transcendental Stance” (134)

16.30 – 17.00 Jan-Willem Romeijn (University of Groningen) “Formal Models of Explorative Experiments” (135)

II: Formal Methods in Philosophy of Science (Formal Epistemology and Semantics)

Room: A217

Chair: Sorin Bangu (University of Toronto)

15.00 – 15.30 Henri Galinon (IHPST – CNRS) “Deflationism, Inferential Semantics and the Logicality of ‘True’” (136)

15.30 – 16.00 Yves Bouchard (University of Sherbrooke) “Epistemic Closure in Context” (137)

16.00 – 16.30 Gabriella Pigozzi (University of Luxembourg) “Evaluating Social Decision Rules” (138)

16.30 – 17.00 Neil Kennedy (University of Quebec at Montreal / University of Paris I) and Carlo Proietti (University of Paris I / IHPST) “Yet Another Paper on Fitch’s Paradox” (139)

III: Philosophy of Natural Sciences (Models and Data)

Room: Salón de Grados

Chair: Tarja Knuuttila (University of Helsinki)

15.00 – 15.30 John Worrall (London School of Economics) “Do we Need some Large, Simple Randomized Trials?” (140)

15.30 – 16.00. Lawrence Shapiro (University of Wisconsin, Madison) and Thomas Polger (University of Cincinnati) “The Dimensions of Realisation” (141)

16.00 – 16.30 Sabina Leonelli (London School of Economics) “Can We Have Knowledge Integration without Theoretical Unification? The Travel of Data in Model Organism Biology” (142)

16.30 – 17.00 Xavier de Donato (UNAM, Mexico) “Interactive Representations in Science: From Modelization to Interaction” (143)

IV: Philosophy of Social Sciences (Semantics and Games)

Room: Sala de Juntas

Chair: Martin Kusch (University of Cambridge)

15.00 – 15.30 Jesús Zamora (Spanish National Open University, UNED) “What Game Do Scientists Play?” (144)

15.30 – 16.00 Aspassia Kanellou “On the Distinction between Content Realism and Realism about Intentional States” (145)

16.00 – 16.30 Alexandra Arapinis (IHPST) “How to Maintain Literalism Without Change of Semantic Paradigm” (146)

16.30 – 17.00 Claudia Bianchi (University San Raffaele, Milan) and Nicla Vassallo (University of Genova) “Semantic Contextualism: An Epistemic Account” (147)

17:00 **Closing of the Conference**

Plenary lectures:

Anne Fagot-Largeault (Collège de France)

Styles in Philosophy of Science

Ilkka Niiniluoto (University of Helsinki)

Theory-Change, Truthlikeness and Belief-Revision

Michael Friedman (Stanford University)

Einstein, Kant and the A Priori

Abstracts

Marshall Abrams (University of Alabama at Birmingham)

Radical Pluralisms about Units of Selection

I argue for a pluralism about units of selection which is in the tradition of Sterelny and Kitcher (1988) but which is more flexible and more strongly realist, and for a pluralism about causes of evolution which is in the tradition of Brandon's (1988, 1990) levels of selection, but which is more flexible (and at least as strongly realist). Among other things, my view provides a way of resolving the "book-keeping problem" of understanding natural selection in cases of organisms which spread without reproduction.

First, I characterize evolution as a change in the distribution of a set of properties, and a distribution of a set of properties as the sort of thing which can be an evolutionary effect. The same set of objects in the world can instantiate different sorts of properties simultaneously, allowing one population to be involved different effects. These different effects, in turn, may have distinct causes. Thus, for example, a distribution of alleles at certain loci in a population of organisms is one evolutionary effect; a distribution of phenotypic characters--perhaps continuously varying--in the same population at the same time is another evolutionary effect. The same population might also have a distribution of properties of groups within the population--a third evolutionary effect. A set of properties thus defines a unit of selection and a kind of evolutionary effect. This way of defining a plurality of units of selection means that their existence does not depend on our choices, descriptions, theories, etc.

Second, I argue that although we can distinguish different causes of a given evolutionary effect using probabilistic relationships, a cause at the level of groups (of given units) represents just one of many kinds of probabilistic dependence on other units. As a result, we must recognize a multi-dimensional continuum of "levels" of selection. Pluralism about units of selection comes into play because different units of selection may require different levels of selection.

In addition to providing a general account relationships between traditional units and levels of selection, my way of justifying pluralism about units of selection allows a resolution of the book-keeping problem (e.g. Ariew & Lewontin 2004). This problem arises because there is no one obvious way to understand natural selection in cases, for example, when plants "reproduce" by sending out underground runners. On my view, proposed solutions such as taking selection to operate on amount of biomass, amount of resources reserved, etc. are all potentially legitimate, since each may define a different evolutionary effect.

Daniel Andler (University of Paris IV, Sorbonne/ENS)

Naturalism and the Scientific Status of the Social Sciences

Naturalism as a philosophical thesis is usually taken to be a group of related claims, the most central of which is that the sciences of man differ in no essential way from the natural sciences. This non-bifurcation thesis was proposed, under the heading “unity of science”, by the Vienna Circle, particularly by Neurath, as a critical weapon aimed at destroying the protective shield behind which the sciences of man, with the assistance from some sectors of philosophy, could freely conduct work which was thought by Neurath and his friends to be worthless or outright toxic. It was a methodological principle with no substantive import, and unconnected to other aspects of naturalism (to which many Circle members were in fact opposed). In today's naturalistic context, non-bifurcation has become a constructive program which aims to develop a comprehensive natural science of man, by pursuing the agenda of psychology, linguistics, anthropology, sociology, etc. by novel means, drawn from cognitive science and evolutionary theory. These naturalistic sciences of man are identified by their proponents as the one vector of true scientificity for the field, so that if they succeed, monism will be vindicated, and if they fail, bifurcation will be confirmed. On the other hand, they encounter conceptual problems, as well as continued resistance from the mainstream of social science. The upshot is a Manichean vision of the situation, which this paper attempts to undermine.

On the one hand, it is simply false that naturalistic programs are the sole agents of scientificity at work in the field; in fact, their impact is still far from equaling that of the formal and quantitative approaches. And it cannot be assumed, as is often the case today, that these two currents are fated to merge into one. Second, there are strong arguments, both empirical and conceptual, against the prediction that the sciences of man will eventually consist entirely of theories developed in the language and style of natural or formal-quantitative theories.

One is thus led back to a Neurathian view of the sciences of man, where unity means no more “but no less” than “orchestration” or “encyclopaedism”, a situation where critical dialogue across disciplinary and methodological boundaries is possible and even required, but which does not call for reduction, direct or indirect via a hierarchy of levels. Physicalism (the notional outcome of the naturalization program) as well as hermeneutism (understood as the ultimate method in the study of man) will eventually be seen as philosophical excesses driven by an overestimation of conceptual analysis, a conclusion which should give solace to the naturalist.

Theodore Arabatzis (University of Athens)

Rethinking the Theory-Ladeness of Observation: Implications for the New Experimentalism

Although experimentation has been a central feature of modern science since the seventeenth century, it was only recently, during the 1980s, that experimental practice attracted the attention of philosophers of science. In this paper I argue that the philosophical analysis of experimentation compels us to reconsider a central tenet of post-positivist philosophy of science, namely the theory-ladenness of observation and its implications for theory choice. The paper is structured in four parts. First, I sketch the history of the theory-ladenness thesis from Duhem and Reichenbach to Hanson and Kuhn. Second, I give a brief account of recent philosophical work on experimentation, focusing on its novel insights concerning the theoretical knowledge employed in the design, implementation, and description of experiments. Third, I argue that in philosophical analyses of theory testing, the theory that informs the design and understanding of the instruments employed in an experiment is often confused with the theory under test. In practice there is usually no overlap between the background knowledge that makes an experiment possible and the theory that the experiment is supposed to test. The theory-ladenness of experimentation does not have to compromise the comparative evaluation of theories, because the crucial experiments that are designed and carried out for that purpose do not usually involve any of the competing theories. I illustrate this point by drawing on my historical analysis of the Zeeman effect and its role in the development of the theory of atomic structure. In the fourth part I suggest that even when the theory employed in the design of an experiment is the same as the theory under test, its confirmation is not a priori guaranteed. On the contrary, in those cases the refuting import of disconfirming results is more clear-cut than in situations where the hypothesis under test and the auxiliary hypotheses informing the experiment are different. In the latter, but not in the former, one could retain the hypothesis under test by modifying some of the auxiliary hypotheses.

Aristidis Arageorgis (National Technical University of Athens)

Holism and Nonseparability by Analogy

In nonrelativistic quantum mechanics, the issues of holism and of nonseparability are usually discussed by reference to entangled states of pairs of spin 1/2 particles. It is usually claimed that such systems exhibit state nonseparability (the state assigned to the compound system does not supervene on the states assigned to its component subsystems) as well as physical property holism (some physical property of the compound system does not supervene on intrinsic physical properties and relations of its subsystems). Setting aside qualms as to philosophical explication of such concepts, I explore an analogue in algebraic relativistic quantum field theory. The main model is a double quantum dynamical system like the ones studied by B. S. Kay [Commun. Math. Phys. 100: 57-81 (1985)] in connection with the Unruh effect on Schwarzschild and Minkowski spacetimes. I consider a double quantum dynamical system whose commuting subalgebras are the algebras associated with the left and right, respectively, Rindler wedges in Minkowski spacetime. It is well known, by means of rigorous results such as the Reeh-Schlieder theorem, the Bisognano-Wichmann theorem and the Tomita-Takesaki modular theory, that ambient quantum states of bounded energy (notably, the Minkowski vacuum state) (1) appear as pure states on the double-wedge algebra with definite values for observables of the double-wedge system but (2) appear as states of thermal equilibrium when restricted to the algebra of either wedge - specifically, they are KMS states at nonzero temperature with respect to the automorphism group which, in this case, corresponds geometrically to the group of Lorentz boosts that leaves invariant the Rindler wedges. This is analogous to the fact that entangled states for pairs of spin 1/2 particles (1) are pure states on the tensor product Hilbert spaces with definite values for observables of the compound system but (2) yield partial states describing “completely unpolarized spins” when restricted to either subsystem. I argue that by exploiting the analogy one can deduce in relativistic quantum field theory conclusions concerning nonseparability and holism similar to those established in nonrelativistic quantum mechanics (modulo the assumption that in the field-theoretic context subsystems correspond to spacetime regions and the subsystem relation corresponds to containment). As a by-product of this approach, I show that quantum field theory exhibits the following type of spatiotemporal nonseparability: there are physical processes occupying regions of Minkowski spacetime which do not supervene upon assignments of intrinsic physical properties to neighborhoods of the points of those regions.

Alexandra Arapinis (IHPST)

How to Maintain Literalism Without Change of Semantic Paradigm

In the last decade, philosophy of language has been marked by the increasing influence of contextualism. In a nutshell, contextualism corresponds to the view according to which the notion of “literal meaning” of sentences – or what is equivalent, the idea that it is possible to distinguish what sentences say from what speakers mean in uttering those sentences – is misleading and mis-founded. Contextualists, such as Travis [1997] and Recanati [1995], [1997], [2004] among others, thus argue that context-sensitivity is an essential feature of natural language which isn’t restricted to a small class of terms, but affects even purely descriptive sentences.

In this talk, my aim will be to show that the standard criterion for context-sensitivity, namely that context-sensitive expressions carry different values in different contexts, does not lead to the generalized context-sensitivity postulated by contextualists. I will argue that, at least in considering simple descriptive sentences containing no quantification nor tense, it is possible to maintain a literalist view without any reformulation of the context-sensitivity criterion (Cappelen & Lepore [2005]), nor multiplication of indexical variables (Stanley [2002], Stanley & Szabo [2000]).

I will defend the idea that analysing purely descriptive sentences in terms of incomplete semantic content – semantic incompleteness being the other side of context-sensitivity – is inconsistent with the linguistic data. More precisely, I will defend that the possibility of anaphoric constructions and VP-ellipsis (i.e. anaphoric predication) contradict the hypothesis of a contextually enriched content.

After going through a number of linguistic data supporting this argument, I will advocate that what contextualists take to be evidence of contextual enrichment is better understood in terms of integrated metonymy (Kleiber[1994], [1999]) or active zones (Langacker [1984], [1991]). The keystone of these theories is the idea that an object can sometimes be characterized by features of its parts. As Kleiber and Langacker argue, this is made possible in contexts were the parts are salient enough with respect to the whole or, in other words, when some zone of an object is made active by the context. This principle then amounts to the idea following which, while we generally grasp objects under a given perspective, this perspective isn't part of what we refer to in using names of objects.

I will thus conclude my talk by pointing to a misunderstanding I believe to be at the origin of much of the ongoing debates between contextualists and literalists. This misunderstanding, which was pointed out by Kleiber in [1999] but unfortunately hasn't received much echo, consists in confusing the level of the semantic structure and the level of the discursive interpretation of a sentence. Indeed, it is important to make a clear distinction between the way speakers actually compute the truth-value of an utterance on the one hand, and the semantic structure and truth conditions of the sentence uttered on the other. At the end of my talk, I thus hope I will have convincingly shown that at least part of the literalist/contextualist debate is based on a misunderstanding, which itself originates in the omission of important linguistic data.

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Eric Audureau (CEPERC/CNRS/University of Provence) and Gabriella Crocco (CEPERC/CNRS/University of Provence)

Relativity Theory and Poincare's Conception of Space

Poincaré's theory of relative motion anticipated several important features of Special Relativity (SR) although Poincaré never wanted to adhere to it. A lot of studies have been dedicated to the comparison of Poincaré's theory of motion with SR but very few to its comparison with General Relativity (GR) despite the fact that Poincaré advocated, long before Einstein, a mathematical principle of relativity generalized to accelerated motion .

This lack of interest for this aspect of Poincaré's viewpoint is probably due to the fact that the few researchers who considered it (e.g. J.Vuillemin and M.Friedman) ruled it out from the outset on the basis of Poincaré's rejection of Riemannian geometries of variable curvatures. This rejection, it is claimed, is a consequence of Poincaré's conventionalism and since GR requires that space has a variable curvature, GR dismisses Poincaré's conventionalist conception of space.

This diagnosis seems to be incomplete, mainly because it overlooks the importance of

Poincaré’s distinctions between geometrical, physical and representative spaces.

1) According to Poincaré, things and properties of things are definitively out of the domain of scientific knowledge. To suppose the existence of a physical space, as the medium of the motions of physical bodies is a mere illusion. Hence, it would be delusive too, to believe that geometry could describe this medium, even ”up to a convention”. Instead of this materialisation of geometrical space, Poincaré emphasizes the role of representative space as the frame within which we represent the relations between our body and the objects surrounding it. Representative space is a well-ground concept because its construction obeys group laws.
2) It equates unduly Poincaré’s geometrical conventionalism with his plea for space of constant curvature. Conventionalism does not mean that one can choose between any kind of geometry because it is a prejudicial question to understand what could be considered as a geometry. But if they were no rigid bodies, says Poincaré, there would be no geometry at all. Namely it would be impossible for the mind to construct representative space (this frame within which we imagine our own motions) on the grounds of visual, tactile and kinaesthetic impressions.
3) It overlooks the difference between approximate and exact solutions of Einstein’s field equations. Observational confirmations of GR obtained within the framework of approximate solutions proposed by Einstein in his famous 1916 paper. According to Einstein himself this “mixed” framework (GR motions within SR space-time) was unsatisfying from both a physical and a conceptual standpoint. But there are indefinitely many exact solutions of the field equations. The choice of any one of them rests on a conventional choice which a) deletes the discrepancies between astronomical data and the idealized shape of the Universe and b) go beyond the possibility of being dismissed by experiments. Ironically, the most part of exact solutions of RG equations, including Einstein’s one, share all the properties of Poincaré’s conventional geometrical space, with the sole exception of infinity that relativists, sometimes, claim to deny: they are isotropic, homogeneous and R3xR differentiable manifolds.

Sorin Bangu (University of Western Ontario)

The Principle of Indifference and Statistical Tests: A Critique of Gillies’ Eliminative Strategy

A central and controversial component of the ‘classical’ conception of probability, the Principle of Indifference (hereafter PI) claims that equipossibility entails equiprobability. A more complete version can be formulated as follows: Given a null state of background information, equal regions of the space of possible outcomes should be assigned equal probabilities. The principle plays an important role not only in physics (in the foundations of statistical mechanics), but also in our everyday probabilistic inferences (e.g., in predicting the outcomes of coin-tossing). Yet many philosophers and scientists also hold that PI is subject to two serious objections. The first objection stresses the inconsistencies associated with the application of the principle: PI must be rejected since it leads to the Bertrand-type paradox(es). The second criticism was advanced by Hans Reichenbach (1949), and it does not focus on the paradox. He notes that the principle seems to license the inference of the frequency of occurrence of some physical phenomena from our epistemic state (of ignorance); hence it follows that the acceptance of the principle would amount to the acceptance of the idea that the occurrence of events in the physical world somehow “follow the directives of human reason” (Reichenbach 1949, p. 354). Reichenbach finds this disconcerting, and deals with this problem by attempting to show that PI is in fact dispensable in probabilistic reasoning. (See especially sections 68-71 of his (1949)). This approach is an illustration of what I’ll call here an eliminative strategy. The guiding idea of such a strategy is to show that the inference of the observed frequencies can be made on less problematic grounds (i.e., without appealing to PI), thus rendering our reliance on the seemingly a priori principle unnecessary.

My goal in this paper is to criticize a recent attempt to implement the eliminative strategy , Donald Gillies' heuristic approach (2000). Despite the fact that Gillies allows PI a certain role in our probabilistic inferences (namely, to help us conjure probabilistic hypotheses), I construe his view as an attempt to dispense with the role of PI in chance reasoning. My reason for construing it this way is motivated by Gillies' emphasis on the incapacity of the principle to justify those hypotheses, and thus to yield a substantial epistemic benefit. After I present Gillies' position, I point out that the alternative method of justification/rejection of probabilistic hypotheses endorsed by Gillies - in essence, the method of statistical relevance tests - is subject to the same kinds of difficulties as the method of a priori justification involving the use of PI.

Caroline Baumann (Cambridge University)

Reconsidering Gilbert's Account of Norm-Guided Behaviour

Gilbert's understanding of social norms is considered by some as a promising alternative proposal to standard rational choice accounts of social norms. In this paper, I evaluate her position on social norms and norm-guided behaviour.

Gilbert's main objective is to provide an account of social norms which makes sense of what she takes to be the key feature of social norms, namely the fact that members of a group have the obligation to act on the norms governing this group irrespective of their individual preferences. According to Gilbert, the normativity of social norms is grounded in joint commitments. She argues that two or more people are subject to a social norm if and only if they are jointly committed to accept the norm. To be jointly committed to accept a norm implies that one has the obligation to act accordingly unless one is released from being bound by the norm by the other parties to the joint commitment. Based on this understanding of the obligatory nature of social norms, Gilbert and other philosophers argue that Gilbert's account helps overcoming collective action problems, such as the problem of explaining why people follow social norms, which remain unsolved by standard rational choice theorists. According to her account, the obligations underlying joint acceptances of social norms create reasons to conform to social norms which go beyond standard rational choice accounts.

In this paper I criticise Gilbert's position on social norms on two accounts.

(1) I defend the claim that the normativity of Gilbertian joint commitments does not capture the normativity underlying social norms. First, I argue that a joint commitment is not a necessary condition for being bound by social norms. Second, I defend the view that the obligation of social norms is not essentially an obligation to act according to the norms until or unless one is granted the permission to be released from this obligation by the other parties to these social norms. Third, I argue that Gilbert's account of social norms and rules is circular: it presupposes the existence of the social rules governing the social practice of joint commitments. I think my point goes beyond existing arguments on the circularity of Gilbert's account.

(2) I argue that Gilbertian joint commitments do not allow us to overcome the problems of standard rational choice theory in accounting for collective action problems and particularly in explaining norm-based behaviour. Gilbert holds that it is rational for parties to a joint commitment to act accordingly irrespective of the desires of the individual parties. I argue that her position is unconvincing. She does not provide reason enough for thinking that it is rational to act according to joint commitments independently of the desires one has. First, the reasons grounded in joint commitments are not motivating but normative reasons. Second, it is far from clear why the obligation underlying joint commitments is a rational obligation and not merely an obligation grounded in a social rule.

Hanoch Ben-Yami (Central European University)

Backward Light-cone Simultaneity, with Special Application to the Twin Paradox

In an earlier publication (Ben-Yami 2006) I argued against Malament's claim (Malament 1977) that, given the causal theory of time and some additional minimal constraints, Einstein's standard definition of simultaneity is the only acceptable simultaneity definition in the Special Theory of Relativity. Among other things I have shown there that if we adopt Malament's approach, the definition of all events on a given event's backward light cone (BLC) as those simultaneous with that event is at least as acceptable as Einstein simultaneity. In this paper I show how BLC simultaneity has some conceptual advantages over Einstein simultaneity, I present kinematics with BLC simultaneity, and show how it resolves the Twin Paradox.

First, according to Einstein simultaneity, one and the same event can occur more than once relative to the same observer, if that observer is not inertial. This result shows that nothing much is left of our concept of temporal order if we adopt Einstein simultaneity. Secondly, if we understand by a system 'an organized or connected group of objects' (OED), and if we should aspire that all parts of a system would ascribe roughly the same coordinates to the same events, then, I argue, BLC simultaneity does that better than Einstein's. Thirdly, I show that BLC simultaneity reduces the gap between the scientific image of the world and its manifest image. Fourthly, I show that while coordinates that rely on BLC simultaneity express a fact about the world—how things really appear to observers—coordinates using Einstein simultaneity do not represent any observable fact but serve only as middle terms between observables. In this sense any exposition of Special Relativity should include the results obtained with BLC simultaneity, but not vice versa.

I then present the results for length and time change in a moving body according to BLC simultaneity. I limit myself here to the one-dimensional case: a body moving towards or away from an observer. If a body's velocity is positive when it moves away from the observer, then the length of a body moving with velocity v is:

$$L_v = L\,(1 - 2v/c)^{1/2}$$

The time interval that a clock moving with velocity v would show, compared with that shown by the observer's clock is:

$$\Delta t_v = \Delta t\,(1 - 2v/c)^{1/2}$$

We get not only length contraction and time dilation, but, if the body and clock are moving towards the observer, length expansion and time acceleration. These are the changes of length and time that would actually be seen by the observer, and not their Einstein equivalents.

I next proceed to apply these results to the Twin Paradox. I first argue that according to Einstein simultaneity a paradox, or at least a puzzle, still remains. This is because the situation, according to Einstein simultaneity, is by and large symmetric. In contrast, if we describe the situation by means of BLC simultaneity, the symmetry is canceled and the paradox doesn't arise. The paradox was a result of an artificial symmetry imposed on the description of the situation by Einstein simultaneity.

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Claudia Bianchi (University San Raffaele, Milan) and Nicla Vassallo (University of Genova)

Semantic Contextualism: An Epistemic Account

Epistemological contextualism and semantic contextualism are two distinct but closely entangled projects in contemporary philosophy. According to epistemological contextualism, our knowledge attributions are context-sensitive. That is, the truth-conditions of knowledge ascribing sentences –sentences of the form of (1) S knows that p- vary depending on the context in which they are uttered. According to the classic view in epistemology, knowledge is justified true belief.

Invariantism claims that there is one and only one epistemic standard for knowledge. On the contrary, contextualism admits the legitimacy of several epistemic standards that vary with the context of use of (1); it is right to claim – for the same cognitive subject S and the same proposition p – that (1) is true in one context, and false in another.

The epistemological contextualist thesis is grounded in a semantic claim about the context sensitivity of the predicate "know": we can gain insight into epistemological problems by investigating our linguistic intuitions concerning knowledge attribution sentences. Broadly speaking, the semantic thesis grounding epistemological contextualism is that a sentence of the form (1) does not express a complete proposition. Different utterances of (1) can, in different contexts of utterance, express different propositions. The proposition expressed by a knowledge attribution is determined in part by the context of use: we must add in information about the context in order to determine the proposition expressed by (1). If we fill in the gaps by appealing to low epistemic standards, (1) will be evaluated as expressing a true proposition; if, in a different context, we fill in the gaps by appealing to high epistemic standards, (1) will be evaluated as expressing a false proposition.

Many scholars have tried to spell out the semantic contextualist thesis on which epistemological contextualism is grounded. Our aim in this paper is to evaluate the plausibility of a project that takes the opposite starting point, i.e. that of establishing the semantic contextualist thesis on the epistemological one. Our paper is structured as follows. We present a standard version of semantic contextualism: according to it, the truth conditions of any sentence are not fixed by the semantics of the sentence - different utterances of the same sentence can, in different contexts, express different propositions. We sketch a theory of meaning as justification: our account is based on Wittgenstein and Dummett. Following Annis, we show how the notion of justification can be contextualized. S may be justified in uttering p in context C1, but not justified in uttering p in context C2: justification depends on a specific issue-context, which determines the appropriate objector-group. We then argue that if Annis' attempt is sound, it could provide an interesting and quite straightforward way of contextualizing meaning. In the conclusion we point out advantages and drawbacks of this thesis.

Justin Biddle (Bielefeld University)

The Ambiguously Social Character of Longino's Theory of Science

In her Science as Social Knowledge (1990) and The Fate of Knowledge (2002), Helen Longino argues that scientific knowledge is social, and she attempts to spell out which particular forms of social arrangement are necessary for the objectivity of scientific research. More specifically, she argues for the following two claims: (1) communities are required, in principle, for the development of scientific knowledge, and (2) these communities, in order to produce objective research, must be structured so as to approximate a Millian marketplace of ideas.

Her argument for (1) begins as follows. There is a logical gap between hypotheses, on the one hand, and data, on the other, and this gap is inevitably filled by background assumptions that are context-dependent, i.e., dependent upon the aims and interests of the communities undertaking the research. Furthermore, experience teaches us that in certain areas of research, particularly areas that are significant for human values and behavior, these interests will be morally and politically-laden. In the face of this, how is the objectivity of science to be ensured? She argues that the only way to ensure that the subjective values of individuals do not infect the evaluation of research – and hence, the only way to ensure the objectivity of science – is to demand that putative knowledge-claims be scrutinized within certain sorts of communities. Which sorts of communities? It is here that Longino argues for (2). More specifically, she argues that objective research must be produced within communities that possess the following four characteristics: public venues, uptake of criticism, shared standards for the evaluation of research, and a tempered equality of intellectual authority.

In this paper, I argue that there is a fundamental tension, if not outright contradiction, in Longino's characterization of the sociality of science. While (1) states that scientific knowledge is necessarily social – i.e., that a community is required, in principle, for objectivity in science – her explication of an ideal scientific community in (2) strongly suggests that it is not. According to this explication, part of what makes a community ideal is that it is composed of open-minded individuals interacting with one another on a level playing-field. The requirement of the uptake of criticism, for example, requires of individuals that they evaluate research in a thoroughly open-minded fashion. But this suggests that individuals qua individuals have the capability of being objective. This implies that communities are not required for objectivity in science. They might be helpful means of promoting open-mindedness and objectivity, but they are not, strictly speaking, necessary for it.

Given this fundamental tension within Longino's theory of science, (1) and (2) cannot both be true; one of them must be rejected. I suggest that the primary problem lies in her characterization of ideal scientific communities as Millian marketplaces of ideas.

Floriane Blanc (LEPS)

Analyzing an Aspect of the Inaugural Lectures of the Paris Museum of Natural History: An Appropriate Concept of Representation

This paper presents part of a scientific study focused on the social aspects of research and its impact on the process of constructing knowledge. For this, we use a corpus of hitherto unexploited texts, the inaugural lectures from the Paris Museum of Natural History. This central French teaching institution demanded that each newly appointed professor gave a formal opening lecture, resulting in the source texts.

Given to an audience composed by institutional representatives, colleagues and friends, this lecture was a symbolic way to usher the professor into his new function. One might dismiss such a formal and codified exercise as being uninteresting for the epistemologist. Instead, reading these inaugural lectures (1869 - 1979) brings to light “science as it was done”. Indeed, these texts serve to reveal unknown aspects of scientific activity in contrast to the naive image of science one might expect them to present.

Certain assertions in these lectures particularly caught our attention. Many words or expressions like “truth”, “coincidence”, “luck”, or the phrase the “first beings made by the hands of the Creator” reveal the personal position of the orator. These elements of the speech generally introduce more epistemologically interesting aspects.

In these elements of the lecture, the orator referred to components of a more or less conscious system of thought, which constituted his “representation of the world”. The difficulty was to understand the object - i.e. the “representation of the world” – in all of its dimensions. Moreover, it was necessary to take into account the systemic relationship established between the different components of the study's object. Thus, we began to research a conceptual construction that would enable us to do this. Either we could have tried to develop a new conceptual tool, or we could have borrowed one from a related science. The concept of “representation” is already used in many disciplines including science studies, sociology, cognitive psychology, history, and social psychology. The concept has been developed according to many different perspectives, which can differ to the point of being opposed. What we propose is neither to add a new definition to the concept, nor to build a rigid model. The aim of this research is firstly to find a suitable model drawn from the approaches developed by some related disciplines, which can be adapted to the present case study.

In the first part of the paper, we will present our methodology, explaining which definition was chosen for the concept of “representation” in this study case and why.

The second part will be devoted to the presentation of the results obtained by the application of this methodology, i.e. what systems of representation have been revealed. We will present their characteristics and discuss whether or not the conceptualization is valid.

Giovanni Boniolo (IFOM) and Marcello D'Agostino (University of Ferrara)

Biomedical Networks and their Logics

In the presentation we show how the structure of an arbitrary biomedical network can be given a logical interpretation in terms of a family of non-standard logics.

Under this interpretation, any scale free biomedical network can be considered as a sort of axiomatic theory. In this way, on the one hand we propose a promising new approach to the investigation of empirical networks (especially scale free networks), and on the other hand we suggest an answer to the long-standing question of whether logic is empirical.

Mieke Boon (University of Twente)

Phenomena: A Transcendental Stance

Philosophy of science traditionally has focused on scientific theories and on how theories can be true about the world. More recent approaches have shifted their focus to scientific practices. This has loosened the traditional distinction between the context of discovery and the context of justification. The context of discovery has become conceptually accessible by an important new notion, introduced by Ian Hacking: Scientists not only acquire knowledge by observing the world, but also by intervening with it.

This shift of focus raises new philosophical issues. Traditionally the issue has been how we know that our representations of the world are true. A new issue is how to conceive of knowledge acquired by interventions with the world. I will propose that understanding scientific practices needs a transcendental stance. A philosophical stance encompasses presuppositions about the rock bottom of our knowledge. An empirical stance is dominant in most philosophy of science: perception and sense experience have an epistemologically privileged status regarding the justification of beliefs about the natural world.

From a transcendental stance one asks what needs to be presupposed about scientist's ways of acquiring knowledge about the world in order to explain the possibility of representing the world and the possibility of reasoning upon it in scientific practice. A transcendental stance does not take perceptions as the rock bottom on which true knowledge is to be built. Instead, the basic ways in which scientists structure perceptions and knowledge of the world is taken as rock bottom.
As a consequence, the notion of phenomena moves to the centre of our philosophical interest. In scientific practices, phenomena result from interventions, and scientific reasoning aims to be empirically adequate with respect to interventions that produce, control or prevent phenomena. Intuitively, the notion of phenomena seems to be unproblematic, but when we look closer it is not. Bogen & Woodward (1988) have proposed a distinction between data and phenomena which they think is crucial for understanding scientific practice. Loosely speaking, data are the observations reported by experimental scientists, while phenomena are objective, stable features of the world to which scientists infer based on reliable data.

This view generated only a moderate amount of direct discussion in the philosophy of science literature. For example, James R. Brown (1994) endorses it with minor modifications, James McAllister (1997) agrees that it is an important distinction, but argues that phenomena must be relative to the investigator rather than objective features of the world as Bogen & Woodward claim, while Bruce Glymour (2000) argues that the distinction is at best superfluous and at worst misleading.

It will be argued that Bogen & Woodward's distinction between data and phenomena is important but cannot be maintained within an empirical stance. McAllister's anti-realism about phenomena, on the other hand, runs into the dangers of subjectivism. This paper explores whether these difficulties can be avoided by developing a conception of phenomena from a transcendental stance.

Yves Bouchard (University of Sherbrooke)

Epistemic Closure in Context

The general principle of epistemic closure stipulates that epistemic properties are transmissible through logical means. According to this principle, an epistemic operator, say E, should satisfy any valid scheme of inference, such as: if E(p entails q), then E(p) entails E(q). The principle of epistemic closure under known entailment (ECKE), a particular instance of epistemic closure, has received a good deal of attention since the last thirty years or so. ECKE states that: if one knows that p entails q, and she knows that p, then she knows that q. It is widely accepted that ECKE constitutes an important piece of the skeptical argument, but the acceptance of an unrestricted version of ECKE is still a matter of debate. On the side of the defenders of ECKE, one finds Stine (1976), Brueckner (1985), Vogel (1990), and Feldman (1995). Others proposed a refutation or a limitation of the principle, like Dretske (1970), Nozick (1981), Hales (1995), Williams (1996), and Sosa (1999). As it turns out, the relevant alternative view (RAV) elaborated by Dretske, which restricts the scope of ECKE, has been discussed extensively and acknowledged as one of the most important contributions. There is nonetheless a major unsolved difficulty pertaining to Dretske-RAV: the notion of relevant alternatives is defined in such a way that it is bounded by counterfactual possibilities. This ontological import leaves open the door to the skeptic. Some have tried to give more precision to this notion, like Stine (1976), who appealed to a Gricean approach to define relevant alternatives in conversational contexts. My proposal is in accordance with the gist of Dretske's strategy, i.e. to restrict the validity of ECKE, and I claim that in order to escape the difficulties inherent to RAV one has to introduce a more robust notion, the notion of epistemic context. Epistemic contexts are a subclass of propositional contexts. In that perspective, the closure property is expressed in terms of a property of a relation between epistemic contexts. ECKE holds when and only when either the epistemic context of the premisses is the same as the epistemic context of the conclusion, or the epistemic context change between the premisses and the conclusion is permissible. Permissibility of epistemic context change is a function of consistency. By means of this epistemic context approach, I will show that: (1) epistemic contexts are defined by basic propositions (unchallenged justified beliefs), (2) ECKE holds only under very specific constraints, and (3) the skeptical argument involves a non-permissible change of epistemic context and, by the same token, cannot rely upon ECKE.

Simon Bowes (University of Sussex)

Natural Kinds and Reduction in the Cognitive Sciences

If natural kind terms pick out parts of the world that share causal properties, and human kinds pick out aspects of the human mind that are uniquely human, then the causal nature of those parts of the human mind must be irreducible to the causal nature of the parts that they are made up of. On the face of it, this is at odds with the belief that the only stuff in the world that has causal efficacy is physical stuff. If, as Jaegwon Kim argues, 'downward' causation is impossible, and all causation happens at the level of physics, then 'higher-level' kinds are left with no causal nature to call their own. This highly metaphysical argument (based on the notion of supervenience) is in fact often marshalled to the cause of more mundane reductionisms, like those of Evolutionary Psychology, neuroscience, or genetics. I will claim that the metaphysical argument relies on an unwarranted assumption resulting from 'physics envy' - the principle of the causal closure of the physical. I will refer to new theories of mind in cognitive science (externalist, embodied, enactive) that extend the supervenience base of human/mental kinds out into the world, and back in time.

My position is that although the world is entirely physical, not all causation is captured by physical theory, which applies to synchronic properties of matter. Evolved entities, however, are part of a process that is irreducibly diachronic. In evolution a feedback happens between random variation, reproduction, and the environment. Such processes cause the emergence of entities with causal natures that are not simply the additive result of the parts those entities happen to be instantiated by at any time. This argument generalises beyond biological evolution, to the other evolution-like processes that make us what we are. These result from our having evolved a plasticity of mind that allows us to ‘grow into’ a social niche, and thus gain an adaptive advantage on creatures that rely on genetic selection alone. This powerful dynamic, where beliefs, behaviours and meanings evolve among communities of minds through ‘horizontal reproduction’, outruns biological reproduction, and the result is minds like ours, full of shared states of mind, not because we share our physical make up, but because we were born into a human society.

The kinds of states that fill our minds are unique for another reason; another feedback dynamic is at work. We are not just automata, here for the benefit of the genes and memes that we carry; we have also evolved self-awareness. This allows us to ask of ourselves what kind of person we are. We are not mindless; we have the ability to be creative (and this is essential for the variation necessary for an evolutionary dynamic), but we are generally limited to working with the conceptual tools we find around us. Our very application of knowledge about people to ourselves, through internalisation, causes reproduction of kinds of minds. What we believe about ourselves has a tendency to become what is true about ourselves.

Alex Broadbent (University of Cambridge)

The Difference between Cause and Condition

Science explains things, and one way it does so is by citing causes. Causal theories of explanation are, therefore, widely seen as central to an account of scientific explanation more generally. However, most current theories of causation (notably all of Lewis's) are unselective. They do not discriminate between the striking of the match and the presence of the oxygen as causes of a particular flame. This view of causation might be thought to gain some support from the fact that scientific explanation of the flame mentions both the oxygen and the match-strike.

In this paper, however, I argue that causal selection needs to be accounted for, to underpin any causal theory of the scientific explanation of particular events. One distinctive advantage of a causal theory, over deductive-nomological predecessors, is that to cite the causes of an event is seems to be a much less strenuous task than to cite the entire condition that is nomically sufficient for it. However, on most current theories of causation, the causes – strictly speaking – of a match lighting when struck include a much wider range of factors than just the striking of the match, the presence of oxygen, and similar. For instance, on Lewis's view, they will also include the birth of the match-striker and the absence of a tsunami. Yet an explanation would normally be thought adequate without including such things. To say why, I argue, we need an account of the principles governing causal selection.

Lipton's strategy of assimilating causal selection to the contrastive mechanism claimed for causal explanation is evaluated. Although this approach enjoys some success, I argue that some of our selective practices demand greater objectivity than it provides. I also argue that many of our selective practices are highly predictable, and that a good account should therefore be able to make concrete predictions about what we will select in given situations. Schaffer has recently hoped to answer these objections with a semantic account of contrast choice, but it seems doubtful whether this helps. I introduce a simple counterfactual conditional which, I argue from the commitments of commonly acceptable inferences, is true of causes but not of mere conditions, yielding an account of selection which overcomes the objections raised against the contrastive strategy.

The relation between the contrastive strategy and the proposed account of causal selection is discussed. It is claimed that the role of causation in explanation is accounted for by the fact that causation is selective – a reversal of the usual strategy of seeking to account for causal selection via a theory of causal explanation. A very cursory overview is conducted of the significant challenges remaining before the proposed account of causal selection can be considered a full account of causation.

Francesco Calandra (University of Trieste) and Gustavo Cevolani (University of Bologna)

Belief Revision and Truth-Approximation

Belief revision (or belief change, or belief dynamics) studies how the epistemic state of a rational agent should change in response to new information, which can be inconsistent with the old beliefs of the agent. In the dominant belief revision theory -the AGM model, so called after its three originators Carlos Alchourrón, Peter Gärdenfors, and David Makinson- the epistemic state of the agent is assumed to be a logically closed set of sentences (Gärdenfors, 1988). Such a set is called a belief set and can be seen, from a logical point of view, as the formal reconstruction of a scientific theory. Thus, the AGM model of epistemic change can be also considered, more generally, as an account of theory dynamics or theory change.

Theories of verisimilitude (or truthlikeness, or approximation to the truth) investigate the relationship between scientific theories and “the truth”, i.e., the notion of being closer -or more similar- to the truth. The topic of verisimilitude was first introduced by Karl Popper, who also offered the first formal explication of this concept in his Conjectures and Refutations (1963). However, this first attempt was later shown to be completely inadequate, since, according to Popper's definition, two false theories can never be compared with respect to their closeness to the truth. The refutation of Popper's definition led to a new approach based on the idea that relations of similarity or resemblance between states of affairs (or their linguistic representations) can be successfully used to give an account of the distance between a theory and the truth. The most important contribution in this direction is due to Ilkka Niiniluoto's 'Truthlikeness' (1987).

Verisimilitude and belief revision can both be considered as formal accounts of theory change. After highlighting the relations between these two approaches, we consider the possibility of “truth-approaching belief revision”, i.e., the possibility that a rational agent approaches the truth by belief revision. As a preliminary step along this path of research, we explore the relations between the AGM model and two of the best developed theories of verisimilitude presently available, namely Niiniluoto's similarity approach (Niiniluoto, 1987) and Kuipers' structuralist approach (Kuipers, 2000) to truth-approximation. Firstly, we present the basic ideas of the current theories of verisimilitude and belief revision. Secondly, we point out the formal relationship between Grove's version of the AGM model (Grove, 1988) and the similarity approach to verisimilitude. Finally, we present some results concerning the possibility of approaching the truth via belief revision in both Niiniluoto's and Kuipers' frameworks.

Our main conclusion is that the standard AGM model is not well-suited to describe the informal intuitions about truth-approaching belief revision. For this task, some other principles, besides the standard ones, seem needed in order to pursue truth-approximation within belief change. Such principles might work as adequacy conditions both for the theories of verisimilitude and for the theory of belief revision.

Werner Callebaut (Konrad Lorenz Institute for Evolution and Cognition Research)

Contigency and Inherency in (Eco)EvoDevo

In evolution, inherency means that the morphological motifs of modern-day organisms have their origins in generic forms assumed by cell masses interacting with one another and their microenvironments, and were only later integrated into developmental repertoires by stabilizing and canalizing genetic evolution. Thus, the causal basis of phenotypic evolution is not reduced to gene regulatory evolution and population genetic events, but includes the formative factors inherent to the evolving organisms themselves, such as their physical material properties, their self-organizing capacities, and their reactive potential to external influence.

Regarding development, inherency locates the causal basis of morphogenesis in the dynamics of interaction between genes, cells, and tissues – each endowed with their own "autonomous" physical and functional properties, thus defying "blueprint" or "program" notions.

The paper explicates the notions of contingency and inherency and investigates how inherency calls for a reinterpretation of the role of contingency in evolution (Monod, Williams, Gould, Beatty…) and development (Oyama).

Martin Carrier (Bielefeld University)

Theories for Use: On the Bearing of Basic Science on Practical Problems

Funding policies for science are usually directed at supporting technological innovations. The impact and success of such policies depends crucially on how science and technology are connected to each other. I propose an "interactive view" of the relationship between basic science and technology development which comprises the following four claims: First, technological change derives from science but only in part. The local models used in accounting for technologically relevant phenomena contain theoretical and non-theoretical elements alike. Second, existing technologies and rules of experience constitute another major repository of technological inventions. Third, technology dynamics is only weakly coupled to progress in basic science but it is closely related to science. There is a dependence of technological change on a more fundamental understanding, to be sure, but it is of an indirect and long-term character. Fourth, progress in basic research is sometimes the effect (rather than the cause) of technological change. Technological change sometimes brings about increased theoretical understanding (application innovation).

Elena Castellani (University of Florence)

Dualities and Intertheoretic Relations

Duality symmetries offer a particularly interesting case-study to the philosophical discussion on intertheoretic relations: first, for their very nature, being by definition symmetries relating different physical theories; second, for the peculiar way that theories are in fact related by these symmetries. Physical dualities are of various types -- starting with Dirac's electric-magnetic duality, the prototype of the various forms of physical dualities, to arrive at the dualities interconnecting the different supersymmetric string theories --, but some general characteristic features may be individuated. Dualities, for example, relate a quantum theory that describes a strong force to another quantum theory that describes a weak force (while leaving the 'physics' invariant). That is, dualities 'exchange' physical regimes that are very different, with the remarkable consequence that calculations involving strong forces in one theory can be obtained from calculations involving weak forces in the dual theory. Dualities also typically exchange fundamental quanta with solitons (thus exchanging Noether charges with topological charges), with the consequence that what was viewed as fundamental in one theory becomes composite in the dual theory.

This paper is devoted to exploring the implications of these and other peculiar features of duality symmetries for the current philosophical debate on intertheoretic relations. A specific attention is also paid to the significance of dualities from the viewpoint of today's revival of interest in a structural approach to the philosophy of science. In particular, the paper examines what duality symmetries may imply for such questions as 'structural continuity' (i.e., retention of structure through theory change) and, more in general, the use of intertheoretic relations in the debate on scientific realism.

Angelo Cei (University of Leeds)

A Form of Ramseyan Humility? David Lewis's version of the Ramsey Sentence and the debate on Structural Realism

In the debate over Structural Realism, Ramsey sentences (RS) have been indicated by various contributors as the tool to define the Structure and rigorously characterize the position. In particular the adoption of (RS) has been seen as a way to formulate the Epistemic version of Structural Realism (ESR) in such a way that allows to understand both what is meant by hidden natures and what is the structure of the theory.

My aim in this paper is to explore the consequences for ESR of the adoption of RS along the lines proposed by David Lewis in his later work "Ramseyan Humility". In that paper Lewis was reconsidering the framework formulated in "How to define theoretical terms": RS was based on a logic equipped to express intensional predication and thus notions like "_is a cause of_" or "_is a law of nature" etc. Certainly, the adoption of a similar framework seems to place the position on a safe ground with respect to the familiar problems of triviality bothering the use of RS for realist purposes. Such result, I argue, comes to a cost. The advocate of ESR is committed to very specific metaphysical views on the nature of properties and on the nature of the fundamental laws. Indeed it turns out that for the position to work we have to postulate quidditism for the properties and assume that it holds in a universe in which laws are not necessary.

Demetra Christopoulou

How to Deal with Ianus' Face of Natural Numbers

This paper addresses a dilemma arising from the linguistic behaviour of arithmetical expressions in two basic ways: they occur, either as singular terms in identity statements or as predicates of concepts in adjectival statements. However, those forms of syntactical behaviour give rise to opposite accounts of the ontological status of natural numbers. The substantival use of arithmetical expressions is associated with the interpretation of natural numbers as abstract particulars while the adjectival use of arithmetical expressions ordinarily supports the interpretation of natural numbers either as properties of physical collections or as properties of sortal concepts, i.e. as second-order properties.

Both types of interpretation are taken under consideration and the special difficulties of each position are sketched, in view of recently discussed aspects of the 'arithmetical platonism' issue.

The interpretation of numbers as abstract particulars by the neo-Fregean program presupposes the discrimination of singular terms from other categories of expressions. However, to achieve the alleged syntactical distinction by means of an appropriate set of criteria is considered as a very ambitious task which has not yet been met in an adequate way. On the other hand, interpretations of natural numbers as (1st or 2nd-order) properties face difficulties which, to a large extent, are taken to arise from the fact that those interpretations do not offer a satisfactory account of arithmetical identities. The paper investigates also the reasons why interpretations of numbers as properties often result in an extensional treatment of those properties.

Then the paper tries out a reduction strategy to investigate the relationship between the substantival form and the adjectival form and determine, if possible, the most fundamental of the two accounts. Hence, it presents three options. The first option is to examine whether the substantival form is reducible to the adjectival form, by an attempt to undermine the semantic role of arithmetical terms as genuine singular terms. The second option is to apply the converse reduction strategy. The third option is to take the substantival form to be equivalent to the adjectival form.

In particular, the paper focuses its attention on the third option. To articulate the proposal about the alleged equivalence between the two forms of syntactical behaviour of numbers, it embarks on a discussion of Ramsey's argument that no essential difference between particulars and universals can be asserted. Then the paper moves on to present the reasons why the third option appears to be the most prevalent of the three. To highlight this claim, it presents an account of how an equivalence principle can actually be settled so that the substantival and the adjectival form can be taken as two sides of the same coin. The paper concludes with suggestions about possible ways by which we can construe the alleged equivalence, considering that if language has anything to say about ontology then numbers are entities with two different modes of linguistic presentation.

Matteo Collodel (University of Venice "Ca' Foscari")

The Last 'Viennese': Feyerabend, Logical Empiricism and the Vienna Circle

Concluding a trend which dates back to the late 70s, during the last decade the relationship between Kuhn's work and Logical Empiricism (LE) has came under a closer scrutiny, resulting in a much more reconciling image of 20th century history of the philosophy of science than the traditional one. The main thesis of this paper is that this point can be made even more forcefully and significantly with Feyerabend, if due attention is paid to both archival resources and his own early writings.

Not only Feyerabend's philosophical education in the late 40s Vienna was deeply influenced by senior and junior former members of the Vienna Circle (Kraft, Juhos and Hollitscher) and by its most close neighbourhood (notably, Popper), but his own philosophical position had been developing in the early 50s in close, polemical contact with LE (through Pap and Feigl). In particular, Feyerabend devoted his doctoral dissertation to the protocol sentence issue and Carnap's work became a constant target of his early, published and unpublished, critical reflections, which climaxed in 1957 in a noteworthy, but little known episode: the so-called "Feyerabend-Carnap controversy". In the light of the latter incidents, Feyerabend's early attempts to find fault with LE could be simply dismissed as ultimately deriving from a blatant misunderstanding of Carnap's programme of Wissenschaftslogik (as an ambiguously descriptive-normative foundationalist epistemology), severely biased by Popper's outlook and personality. Although to some extent illuminating, this conclusion seems however much too hasty. Indeed, the further developments of Feyerabend's critical attitude towards LE shed a different, definitely more irenic light on the entire issue.

Feyerabend's final and most pointed attack against LE came with his famous thesis of incommensurability, whose elaboration received crucial impulses during his visits to the LE headquarters in Minneapolis in 1957, 1958 and 1959-60. Yet, this thesis, rather than undermining LE, just emphasized a tangle of problems, which, if not discussed, were still not at all unknown to its advocates. (It should not be surprising in this connection that the metaphor of incommensurability itself had already appeared in Kraft's and Nagel's writings in the late 40s). This suggests a more fruitful interpretation of Feyerabend's whole critical work as reviving a trend already present within the Vienna Circle, but progressively marginalized with the emergence of LE. More specifically, both Feyerabend's earlier reflections on protocols and meaning, which supported an anti-foundationalist stance and semantic holism, and his later historically- and sociologically-oriented, pluralistic and naturalistic position can be seen as taking sides with and actually joining Neurath's and Frank's efforts in exposing the drawbacks of Wissenschaftslogik with respect to the irreducibility of linguistic practices to formal calculi and the pragmatic and conventional aspects of the scientific enterprise, and in proposing a more adequate and productive image of science.

Thus understood, on the background of the division of philosophical labour at work within the "Vienna Circle" tradition, on Neurath's side, Feyerabend may then well deserve the title of "last Viennese".

Marcello D'Agostino (University of Ferrara) and Corrado Sinigaglia (University of Milan)

Forecasting Accuracy and Subjective Probability

De Finetti's favourite justification of the probability laws, within his "subjectivist" account of probability theory, was in terms of "scoring rules". These are rules that are often employed in evaluating the accuracy of probabilistic forecasters and measuring their predictive success. De Finetti showed that if a specific scoring rule is adopted, the so-called Brier's rule, consisting in taking the mean squared error over a time series of predictions, then the score of a forecaster whose predictions are in accordance with the probabilistic laws dominates that of any forecaster whose predictions violate them. If this has to be read as a subjectivist justification of the probability rules, a natural question to ask is: why Brier's rule? Why couldn't we measure forecasting accuracy by means of any other reasonable rule, such as the one based on the mean absolute error?

De Finetti's typical answer is that Brier's rule is a "proper" scoring rule, i.e. one which forces the forecaster to be honest about her true probability estimates. This answer relates the justification of the probability laws to another widely discussed problem raised by the subjectivist approach, namely that of eliciting probability judgements: how can a subject be persuaded to reveal his true estimates of the values that the random variables will take? How can we make sure that he will not lie about his own judgements, maybe adopting some fraudolent strategy to raise his score? However, Brier's rule is not the only proper scoring rules, and so its choice requires some further justification. For this purpose, De Finetti often resorts to considerations of simplicity or to empirical arguments. Several attempts have been made in the literature to characterize Brier's rule as the only proper scoring rule satisfying some general, more or less compelling, properties.

In this paper we take a different approach. We construe the scoring problem as a special case of the general problem of measuring the distance between two times series of predictions, arising when one of the two forecasters is "infallible", i.e. one (i) whose predictions are always 0 or 1, and (ii) who is always one hundred per cent accurate. So, comparing the predictions of a real forecaster with the observed outcomes is tantamount to comparing them with the predictions of the ideal "infallible" forecaster. After making this heuristic shift, we address the general problem: "how far" from each other are two time series of predictions concerning the same sequence of events?

We present a set of natural properties that a distance function between two forecasters' time series of predictions should satisfy and show that these properties uniquely determine a metric which, in the special case in which one of the forecasters is the infallible one, coincides with Brier's scoring rule. We then argue that, in this way, De Finetti's subjectivist approach to the justification of the probabilistic laws can be accomplished without appealing to the, somewhat misleading and inconclusive, arguments based on the elicitation problems and on the notion of "proper scoring rule" which have so far been the main, if not exclusive, concern of the relevant literature.

Maria Luisa Dalla Chiara (University of Florence), Roberto Giuntini (University of Cagliari), Roberto Leporini (University of Bergamo) and Giuliano Toraldo di Francia (University of Florence)

Holistic Semantics: From Quantum Theory to Music

Quantum theory gives rise to some characteristic holistic semantic situations, where the meaning of a whole determines the meanings of its parts (and not the other way around, as happens in classical semantics). These situations are connected with the mysterious quantum entanglement phenomena, which admit of an interesting informational interpretation. Quantum computational logics are new forms of quantum logic that have been suggested by the theory of quantum logical gates in quantum computation. In the standard semantics of these logics, formulas denote quantum information quantities (systems of qubits, or, more generally, mixtures of systems of qubits), while the logical connectives are interpreted as logical operations defined in terms of special quantum logical gates (which have a characteristic reversible and dynamic behavior). The concrete quantum computational semantics has been generalized to an abstract version that is not necessarily Hilbert-space dependent. This semantics can be naturally applied to investigate different kinds of semantic phenomena where holistic, contextual and gestaltic patterns play an essential role (from natural languages to musical compositions).

George Darby (University of Leeds)

Is Quantum Vagueness Vagueness?

Some philosophers think that some features of the world itself, such as objects, properties or states of affairs, as opposed to the language used to describe it, might be vague. Others regard the idea as unintelligible – Gareth Evans for example famously argued against the apparent corollary that there might be indeterminate identities as features of the world. In any case, most agree that worldly vagueness would at least be fairly weird. Since quantum mechanics is weird, it is a natural place to look for concrete examples. Accordingly, E. J. Lowe suggests that a counterexample to the Evans argument can be found in a particle absorbed by an atom and a particle later emitted, such that it is indeterminate whether those particles are identical. Steven French and Decio Krause, while rejecting Lowe's semi-classical account of the situation, characterise their own examples of quantum vagueness using (the lack of well-defined) identities between 'nonindividual' particles. Perhaps then a metaphysician searching for worldly vagueness can find support from quantum mechanics.

But that metaphysician might also worry that although this kind of example may present genuine indeterminacy in the world (and an indeterminate identity, which at least undermines the Evans argument), it is not really what they were looking for. The vagueness that they are used to is all about sorites series, borderline cases, small changes and elusive sharp cut-offs, none of which are immediately obvious in this case. Vagueness may deliver indeterminate identities, but so may other, distinct, sorts of indeterminacy. Perhaps what we have here is worldly indeterminacy, but not vagueness.

The aim of this paper is to allay that kind of worry, by producing from the kinds of indeterminacy found in quantum mechanics something that more closely approximates the cases found in typical discussions of vagueness. The match will only be approximate, since I think the suspicion is justified that quantum mechanical examples don't really quite fit all the traditional characteristics of vagueness – sorites susceptibility and the like. That however is less of a problem than it would appear, since if those characteristics are combined into a necessary condition for worldly vagueness then they end up ruling it out entirely; thus quantum vagueness cannot be dismissed on those grounds without giving up the search altogether. In any case, an unduly narrow characterisation rules out some of the examples of worldly vagueness given in the mainstream vagueness literature. Instead the approach should be to formulate conditions that are plausibly sufficient for worldly vagueness, which can then be used to construct a quantum mechanical case that, more than the previous examples, looks like the genuine vagueness we were looking for.

Claude Debru (ENS)

Neurophilosophy of Sleep and Dreaming

Since 1953, the cerebral correlates of dreaming have been thoroughly studied, with help of psychophysiological and neurobiological techniques. However, the biological function of dreaming as a cerebral process remains unknown. In this presentation we will deal with some unsolved questions in a more conceptual way.

1) How to define dreaming compared with other mental activities during sleep? Does dreaming occur only in humans, or does some form of dreaming occur also in animals?

2) What is the relationship between «paradoxical sleep» or «rapid eye movement sleep» and dreaming? Is it legitimate to constitute dreaming as a biological phenomenon occurring in a large part of the animal kingdom, as well as during late ontogeny, as widely claimed ? And if so,

which would be the theoretical consequences regarding a «neurobiological» functional theory of dreaming? This question of a «neurobiological» function is the most crucial question we will deal with in this presentation. In its deep motive, it helps to integrate in the most intimate way the physiology of neurones with their properties and the psychological. Due to the polyfunctional character of most biological processes, the idea of a neurobiological function of dreaming does not exclude the idea of psychodynamical functions of dreaming. It will be claimed that both orientations can benefit from each other. The example of the so-called «reprogramming hypothesis» for paradoxical sleep will be presented and discussed.

3) How to define dreaming consciousness, how to describe the difference between ordinary dreaming consciousness and lucid dreaming (in which the subject is aware of the fact that he/she is dreaming)? Could this difference help us to understand cerebral mechanisms of self-consciousness?

New data on dreaming in humans based on modern brain imagery techniques will be presented. Contemporary research being performed at a different levels of organisation, from single neurons to neuronal assemblies, brain nuclei and larger structures, the question of the appropriate level of relevance for psychic experience (which is a new version of the old question of cerebral localisations) should be considered in a more critical way, with the increasing influence of more global views of brain function.

Lieven Decock (Free University of Amsterdam)

Carnap and Quine on the Analytic-Synthetic Distinctions

I will focus on one aspect of the Quine-Carnap debate, namely analyticity as truth by virtue of meaning, or rather truth by virtue of the rules of a chosen linguistic framework. This characterisation best conforms to the original Kantian use of the word analytic. I will argue that it is a perfectly respectable philosophical concept, which both Carnap and Quine could accept. I will distinguish this notion of analyticity (true by virtue of meaning) from other proposals such as analyticity as unrevisability, analyticity as a prioricity, analyticity as conceptual truth, or analyticity as arbitrary postulation.

I will demonstrate that for Quine and Carnap, this concept of analyticity need not be problematic, but that they would certainly disagree over the range of concept ‘analytic’. An analytic/synthetic distinction depends on the conventional choice of a logical or linguistic framework, and therefore it is worthwhile to study the various linguistic frameworks Carnap and Quine have introduced, and to scrutinise the arguments both authors have used in favour of these frameworks. These pragmatic arguments for choosing particular linguistic frameworks have immediate repercussions for the analyticity of the non-factual statements in these frameworks. It will transpire that the class of statements Quine would accept as analytic is much more restricted than what Carnap would allow. Nevertheless, Quine’s most convincing argument against Carnap is that Carnap’s notion of analyticity may be too narrow. I will conclude, pace Quine and Carnap, that a broad notion of analyticity may be philosophically useful.

I will argue that this notion of analyticity may be useful in contemporary philosophy of science. From the analysis of the Carnap-Quine discussion, it will transpire that both Quine and Carnap could agree that logic is analytic. Furthermore, it is possible to regard the theorems of mathematics, and more particularly set theory, in this way. Subsequently, I will extend the scope of analyticity to other sciences. The best application of the notion of true by virtue of meaning can probably be found in biology and the life sciences. This notion of analyticity is closely related to linguistic standardisation. At present, in the life sciences, all kinds of ‘ontologies’ are constructed in the form of large databases. It would be better to regard the structure of these databases as a form of linguistic regimentation defining strict relations between terms in medicine or biology. In Carnap’s terminology, they are meaning postulates, and thus analytic relations.

Xavier de Donato (UNAM)

Interactive Representations in Science: From Modelization to Interaction

The production of scientific knowledge is associated to the production of different types of representations. But, what should we understand under “production of representations”? Traditionally, the importance of the representations oriented to model reality has been highlighted. If taken in such manner, the debate concerning scientific representations has to do with well known debates that include analogies, iconic models, idealization, and science abstraction. However, in recent times the importance of representations of interactive order has been emphasized inside the representational production to develop and increase the social cohesion of a group of individuals – humans, institutions – and ease some functions of the interaction of nature and society (Ibarra and Mormann, 2006). In this way the representations relate to the practical dimension of scientific investigation, particularly with the models of transdisciplinary investigations which nowadays constitute the methodological basis of many advanced investigation centers. The representational practices, by themselves constitute one of the basis of social cohesion of the groups of investigation, due to the fact that those practices are learned during the interactions with the natural and social media and that they are oriented to the resolution of common problems that constitute the sense of the group investigation. Science is basically a representational activity of cognitive order, but the question that arises is if the requested cognitive type needed in the interactive practices is different from the one needed in the theoretical modelization activity and what is the relationship between them. This last question has to deal with the debate between the subjects and the scientific rationalization. I maintain that interactive representations assume the existence of a cognitive and intentional capacity common in all the individuals whose purpose as a group is to integrate the individual efforts and orient them to general aims and a plausible theory (partially based in Philip Pettit work, 2004) in which the collective and individual rationalization are integrated, without the need of postulating collective minds, and presupposes the attribution of intentionality to collective scientific groups and organisms.

Sun Demirli (Bogazici University)

Does Lewis’ Account of Chance Bear on Scientific Ontology?

Lewis’s Principal Principle can be expressed within the framework of a deontic logic as follows:

If E is admissible with respect to <Ch(A) = x>, then OB Cr(A| E<Ch(A) = x>) = x.

Here Cr denotes the credence function (of a person s at time t), and Ch is the chance function (at time t), assigning point-values in the interval [0,1] to an appropriate sigma-algebra of propositions. Braces are used to flank propositions. Formulation within a system of a deontic logic takes care of the rationality desiderata on the credence function Cr.

Lewis argues that one can recover chances from this formula by imagining the credence function of an agent who is i) certain about the world history H up to time t, and ii) certain about all the laws T derivable from history H. Let’s call such an agent an historically omniscient agent. For this possible agent, Lewis establishes that:

OB Ch(A|H)=Cr(A|HT). (2)

Lewis uses this formula to argue that systematic features of objective chances supervene on the systematic features of rational credences. If rational credences satisfy a finitely additive probability function, so does chance. The two formulae above seem to imply the following elimination result in systems of deontic logic:

OB Cr(A| E<Cr(A|HT)=x>)=x, where E is admissible with regard to <Cr(A|HT)=x>.

We pay special attention to the well-known puzzles regarding conditional obligation statements in order to spell out the conditions when this formula holds.

This elimination result can be regarded as the basis for Lewis's attempt to show that chances are dispensable for an historically omniscient agent. That is the gist of his claim that "chance is an objectified subjective probability". Yet, because the objectification in question involves cognitively impossible agents, there are two conflicting implications of this result: The first implication is that since all the occurrent facts should be assumed to be chance-free (the Humean supervenience principle), chance is an illegitimate concept in any scientific ontology. The 'language-exit rule' for any talk of chances as provided in (2) is at the same time a 'language extinction rule' for chances. The second implication is that since scientists will never approximate the informative affluence of an historically omniscient agent, chances are not dispensable after all. They have a function within any scientific discipline where the regulation of partial beliefs plays an important role. This function, in its turn, is used in representing chances, for they provide the 'language-entry rules' for chances. However, such language-entry rules being based on conditions short of being objectifying in Lewis's lights, Lewis does not succeed in showing that scientific chances could be objective chances. The last point will be argued by assuming an inferentialist theory of scientific representation, according to which ontological categories should have an essential bearing on the inferences made about the system represented.

Emmanuel d'Hombres (University of Paris VII)

Differentiation as a Modality of Evolution: From Biology to Sociology and Back

Morphogenesis and differentiation are the words which are used today to indicate the two basic modalities of ontogenesis in biology. Morphogenesis refers to the process of progressive complexification during embryonic development, whereas differentiation refers to that of the functional specialization of the egg's cells. Paradoxically, for more than a century, the term of differentiation has been employed to express the first of these variables, mainly if not exclusively. How did it come in developmental biology to name the physiological phenomenon which is parallel to structural complexification and which 19th century naturalists readily called the division of physiological labour? In fact, it appears that this inversion is only the latest episode in a series of semantic adventures affecting the history of the concept over nearly two centuries. We intend to explain them in this communication.

We will show that: 1° the term "differentiation" appears in the field of animal anatomy at the beginning of the 19th century, because of its resemblance to "complication", which is currently used by morphologists at this period (term which is itself practically synonymous with what 18th century naturalists called the "composition of the organization"); 2° from comparative anatomy the term will migrate to embryology when the principle of epigenesis triumphs, and will know a considerable rise in epistemological status, reaching its climax when ranked by von Baer as a fundamental concept of new scientific embryology ; 3° its pairing with the concept of the division of physiological labour will confer on differentiation the role of criterion with which anatomists on the one hand, embryologists on the other hand, will judge the degree of improvement reached by embryonic formations and adult forms, respectively; 4° the morphological significance of the term is enriched with a new evolutionary meaning, through the diffusion of the darwinian theory and the adoption of the biogenetic law; 5° at this degree of conceptual elaboration, we witness an extension of differentiation's field to the phenomena concerned with anthropology (comparative analysis of different societies) and history (comparative analysis of different formations of the same society) thanks to sociologists such as Spencer who adopted the principle of cultural evolutionism; 6° evolutionary meanings of differentiation regress correlatively, in life sciences and social sciences, during the inter-war period, to such an extent that its legitimate field of extension is reduced only to developmental biology ; 7° consequently with the invalidation of the problem of the organic basis of living beings, differentiation loses its quasi etiological function (degree of differentiation as criterion of organic improvement) and comes back to its modal and descriptive primitive status.

Thus, after having been, among others, a concept of evolutionary morphology and a concept of evolutionary sociology, differentiation regains its initial rank of modal concept. But this return movement takes place in a different setting (differentiation is now a concept of embryology rather than a concept of comparative anatomy). Moreover, the term “differentiation” is henceforth freed from of any metaphysical aspect (idea of a hierarchy of species) and takes on a significance which is no longer anatomical but physiological. We will seek, in our communication, to enravel this story and get to the bottom of the apparent paradox made up of a curious mix of old and new in the contemporary significance of differentiation in biology.

Dennis Dieks (Utrecht University)

Structuralism, Symmetry and Identical Particles

Structuralism in the philosophy of science has been hailed as providing completely new perspectives on a number of traditional questions. Although structuralism and structural realism, are certainly valuable new options that improve on older approaches, I think a number of these claims are exaggerated. Here I examine a recent argument purporting to show that a structuralist approach demonstrates that identical quantum particles with an anti-symmetric state (fermions) are "weakly discernible" objects, just like irreflexively related ordinary objects in situations with perfect symmetry (Black's spheres, for example). That is, they are ordinary entities that differ from each other by virtue of Leibniz's principle. I argue that the argument uses a silent premise that is not justified in the quantum case; and that the structuralist approach, although certainly applicable here, does not lead to radically new insights.

Jacques Dubucs (IHPST – CNRS)

Intended Models

The notion of intended model, which is in everyday use is in contemporary logic, raises philosophical problems that deserve examination: the very notion of intendedness belongs to the theory of speaker's meaning, while standard semantics of mathematical languages is not supposed to make any room for the intentions of the users of these languages. Given that context, the paper has three objectives:

1. To indicate the philosophical background ot the notion of ``intended model'' from the famous Locke-Berkeley problem to the contemporary discussions about standard models of number theory
2. To distinguish between three grades of intentedness:
   a. Subjective intendedness, which refers to the particular realization the user has in mind in using mathematical concepts (that first grade corresponds to the kind of reference Locke and Berkeley had in view)
   b. Transcendantal intendedness, which refers to the the class of realizations that admittedly fits with the cognitive equipment of the user (that second grade corresponds to Kant's Anschauungsmöglichkeiten, contrasted with mere ``logical`` or ``conceptual`` possibilities); I will argue that the so-called Beth-Hintikka interpretation is faulty, by lacking to distinguish between these two first grades).
   c. Logical intendedness, which refers to the class of realizations that are compatible with the presuppositions of using formal mathematical systems; I will argue with Gödel that the lack of a distinction between the two last grades is the origin of the formalist misconception of mathematics, which considers *all* mathematical systems as mere

``hypothetico-deductive systems'' in which the meaning of the primitive terms is only fixed by the axioms; I will also argue, against Gödel, that one doesn't need any platonistic interpretation of intended models to make sense to this third grade of intendedness (as far as number theory is concerned, it can be obtained by considering Tennenbaum's theorem, which establishes that the standard model of number theory is the only one in which the interpretation of addition and multiplication is recursive).

3. To discuss the contemporary situation in set theory in the light of the last grade of intendedness

Sophia Efstathiou (UCSD, visiting LSE)

Articulating 'Race' in Genetic Terms

Two claims are made in recent genetics literature. (1) First, human genetic structure corresponds to major geographical regions (Rosenberg et al. 2002, 2005). (2) Second, human genetic structure in the U.S. is well approximated by self-identified race/ethnicity categories (Tang et al. 2005). These claims are important. They challenge the view that 'race' is an inappropriate proxy for 'genetic variation' (cf. Root, 2003) and they fuel a booming interest in race-specific medicine and pharmacogenomics (cf. 2004 Nature Genetics Supplement on race and genetics and the literature it spawned).

My goal in this paper is to examine how these two claims 1. are inferred and 2. how they can be legitimately inferred, in genetics practice. I explain that a notion of 'race' legitimately enters into the realm of genetics. I argue that an "articulation tool" is used to render this common population notion articulate in genetic terms. But this fact does not warrant its relevance of 'race' for genetics. I demonstrate that a population classification can be articulated in genetic terms but not speak to questions of genetic interest.

In both Rosenberg et al. (2002) and Tang et al (2005) the structure of human genetic variation is established using software STRUCTURE (Pritchard et al. 2000). STRUCTURE implements a model-based clustering algorithm which clusters genetic data into a predefined number of clusters. Multiple clusterings are obtainable using this program and the selection of which one of these is relevant for genetic practice occurs outside the setting of the algorithm. Though STRUCTURE can structure genetic data, into "genetic" clusters, it does not structure populations into "genetic" populations. What criteria justify the selection of (1) continental population classes, (2) self-identified race ethnicity groups as relevant?

I offer an analysis of this situation. I argue that in each case STRUCTURE is used as an "articulation tool". It is used to a. articulate human population structure in terms appropriate to genetics, and b. to derive a standard for judging the articulateness, in genetic terms, of common population classes. I offer a formalism for the second function. It becomes apparent that in its second function as a way for generating standards, STRUCTURE, is limited. STRUCTURE is only used to set entry rules for common classes to enter the context of genetically articulate populations. Whether common classes that are found to be genetically articulate get to speak to genetic interests is decided by rules external to the sorting tool which contain biological theory as well as pragmatic interests.

Epistemic value is placed on the techniques of modern population genomics. But using these techniques as instruments for public policy relies on their calibration against population categories already defined via biological, social and political theory as the ones we care to track.

Markus Eronen (University of Osnabrück)

Reductionism and Problems of Explanatory Pluralism

Explanatory pluralism is a position in philosophy of science that has been proposed as an alternative to both reductionism and the kind of antireductionism that has dominated philosophy of mind for some decades now. It offers a view of cross-scientific relations, particularly between neuroscience and psychology, that highlights the benefits of simultaneous inquiries at different levels of analysis and across different sciences, and does not leave room for the drastic ontological conclusions that reductionists argue for. However, it also eschews classic nonreductive arguments, like the argument from multiple realizability, as they are based on an unrealistic account of what reduction is.

The explanatory pluralists, first and foremost William Bechtel and Robert McCauley, have convincingly shown that traditional and new wave reductionists, like the Churchlands and John Bickle, have failed to appreciate the plurality of cross-scientific relations in their search for general theories of reduction. Particularly, they have focused on relations between theories, while in neuroscience and psychology theories are more the exception than the rule.

However, this does not mean that reductionism is dead. I will show that explanatory pluralists have not given enough attention to certain reductionistic considerations that undermine the credibility of explanatory pluralism.

First of all, explanatory pluralists welcome explanations of all levels without giving lower-level explanations any priority, but there are several reasons to consider lower levels explanations more fundamental than higher-level explanations. Lower level explanations tend to have a wider scope and fewer exceptions, and there is a corrective asymmetry between levels: resources from the lower level are necessary to correct explanations at the higher level, but not vice versa. In addition, lower level explanations often render higher-level explanations otiose or merely heuristic. For example, when the cellular and molecular explanations of memory consolidation are complete, the general psychological explanations of memory consolidation become just practical but inaccurate tools to facilitate understanding.

Secondly, development in neuroscience in recent decades indicates that the focus of research is shifting more and more to lower levels, to cellular and molecular neuroscience. It is conceivable that in future the point might be reached when practically no new discoveries are made at the psychological level, and the search for explanations of human behavior has moved to lower levels. Even though this would not mean the wholesale elimination of psychology, it would certainly undermine its status among the sciences.

In the end, I will argue that one conclusion that can be drawn from the recent debates between reductionists and explanatory pluralists is that the concept of reduction has become extremely diffuse. For example, the models of reduction of Bickle, McCauley and Kim have very little to do with each other or the classic accounts of reduction. The danger of conceptual confusion is very real, and it is questionable whether reduction is a useful concept at all.

Brigitte Falkenburg (University of Dortmund)

Wave-particle Duality in Physical Practice

The wave-particle duality of quantum objects is a neglected topic in the philosophy of physics. In my talk I will sketch the historical roots, starting with the Einstein and de Broglie relations, Born's probabilistic interpretation of the quantum mechanical wave function, and Bohr's complemementarity view of quantum phenomena. I will show which role they play in physical practice up to the present day, in particular in the recent "which way"-experiments of quantum optics. The philosophical interpretation of wave-particle duality will be discussed with a simple polarization experiment.

Jan Faye (University of Copenhagen)

Interpretation in the Natural Sciences

The distinction between the sciences and the humanities is very often regarded as quite significant. Not only do they deal with ontologically distinct objects, but the ways they come to terms with these objects are very different. In philosophy of science there has been a focus on philosophy of explanation because it was thought that providing explanation is one of the key issues in the natural science. Since Carl Hempel's seminal works on explanation the world of philosophy has seen a huge amount of literature devoted to explanation. The results have been prolific, but I think they can be divided into basically three different approaches: 1) the formal-logical ones, 2) the ontological ones, and 3) the pragmatic ones, all of which have important proponents.

Although philosophers of science refer to both scientists' understanding and the interpretation of theories in their accounts of the natural sciences, they make little attempt to develop philosophical theories of understanding and interpretation to grasp this side of the formation of scientific knowledge. This is undoubtedly due to the old, but long standing, positivistic distinction between the context of discovery and the context of justification. The context of discovery is then regarded as part of psychology, whereas the context of justification (including explanation) is seen as an object to which logical and philosophical methods apply. After the impact of Thomas S. Kuhn, modern philosophers of science are in general more sceptical about the possibility of drawing such a sharp distinction, but nobody seems to have explored the full consequences of this scepticism, realizing that explanation and interpretation are interdependent notions and therefore should be included in a systematic study of how we reach understanding in science. In the present talk it will be argued that the natural sciences involve interpretation as much as the human sciences. I distinguish between two notions of interpretation which are rarely set apart. One is concerned with what X represents; the other deals with how to represent Y. In the first sense interpretation may be regarded as an interpretive explanation by which one explains a representational problem. Such a problem arises in contexts where a phenomenon X is considered to represent something else but where there are doubts about what the phenomenon really stands for; it may be in connection with the consideration of physical phenomena, data, evidence, signs, formalisms or symbols, and texts or actions. The second sense of interpretation sees it as presenting a tentative explanation of how to represent a phenomenon Y. In support of such an analysis, I shall make use of a pragmatic-rhetorical theory of explanation, which I presented in a couple of recent papers, to gain a better grasp of interpretation inside as well as outside science.

Peter Fazekas (Budapest University of Technology and Economics)

Different Models of Reduction and the Inevitability of Bridge-Laws

Though mainstream physicalism--admittedly or not--professes non-reductive views, in Philosophy of Science, it is still a fundamental question how a sound reduction should be executed. This paper surveys the three most prominent accounts in contemporary debates. The Nagelian model of reduction pledges itself to the Hempelian deductive-nomological pattern of explanation, in conformity with which reduction becomes a process of deduction, deriving the laws and phenomena of the target-theory from the laws of the base-theory plus some auxiliary premises (so-called bridge-laws) connecting the entities of the target- and the base-theory. In contrast with this, the functional model of reduction--proposed by Jaegwon Kim--which adopts the functional pattern of explanation, emphasizes the causal definitions of the target-entities referring to their causal relations to base-entities, by which the problems raised by bridge-laws become avoidable, since the so-defined process of functionalization does not require bridge-laws at all. And finally, the third main account, Hooker's model of reduction, tries to generalize the Nagelian approach by deducing not the original target-theory but an analogous image of it. The image-structure remains inside the vocabulary of the base-theory, thus Hooker's model claims that within this framework bridge-laws--connecting the terms of different vocabularies--can be evaded.

The present paper tries to show that bridge-laws are inevitable, i.e. that none of these models can evade them. On the one hand, the functional model of reduction needs bridge-laws, since its fundamental concept, functionalization, is an inter-theoretical process dealing with entities of two different theories. Theoretical entities of different theories, by definition, do not have common causal relations, so the functionalization of an entity can be executed only within the framework of its own theory. Thus the functional model of reduction--without bridge-laws--is unsuitable for performing inter-theoretical reductions. On the other hand, the images of Hooker's account cannot be constructed without the use of bridge-laws. These connecting principles are needed to guide the process of deduction within the base-theory; without them one would not be able to recognize if the deduced structure was an image of the target-theory.

Laura Felline (University of Rome III)

Structural Explanation: From Relativity to Quantum Mechanics

In this talk I intend to discuss the character of structural explanation in physics. I will define two criteria for the explanatory power of a structural account: ontological clarity and explanatory relevance. The first criterion asserts that the formal structure to which a genuine explanation appeals always refers to some aspect of physical reality. The second criterion impose that the features of physical reality to which a structural explanation makes appeal are explanatorily relevant to the occurrence of the explanandum. The relation of explicatory relevance is to be defined by the theory itself, and I will propose an argument which aims to show that in physical science this relation corresponds to the relevance in determining the occurrence of the explanandum event.

As an illustration of the importance of the two criteria I pose, I will consider Jeffrey Bub's interpretation of quantum mechanics as a principle theory and the parallel Bub draws between his structural explanation of quantum phenomena and the one provided by special relativity of relativistic phenomena.

I will analyse this parallel and show that it is not compelling because: i) the explanatory power of relativity rests in the fact that the structural explanation it provides makes appeal to the objective aspects of reality (i.e. the structure of space-time) which produce the relativistic phenomena and, hence, in the fulfilment of the two criteria proposed; ii) the same does not hold for Bub's structural account of quantum phenomena, which lacks explanatory power. The formal structure to which Bub's theory appeals for the explanation of phenomena (the C-algebra) represents the structure of information, considered as a new physical primitive. However, on the one hand, Bub's characterization of information does not present any new definition of relevance relation between information and quantum phenomena—on the other hand in classical information theory it is the results of the experiments (=quantum phenomena) which is relevant for the determination of the structure of information, and not the other way around. Without an appropriate relevance relation provided by the theory the structure of information cannot have explanatory relevance in respect to the results of the experiments.

Finally, I will argue that, given my characterization and contrarily to most part of the literature about the subject, structural explanation should not be seen as so distant to causal explanation. Instead, both causal and structural explanations are encompassed in what Wesley Salmon calls the ontic conception of scientific explanation.

José Ferreirós (University of Seville)

Mathematical Knowledge and the Interplay of Practices

The aim of this paper is to describe a new approach to the analysis of mathematical knowledge, currently being developed by the author in a book provisionally titled "The Interplay of

Mathematical Practices: From numbers to sets".

The emphasis is on mathematical practices in the plural, for it is a key thesis of this approach that several different levels of knowledge and practice are coexistent and that their links and interplay are crucial to mathematics. Crucial, that is, for the constitution of meaningful concepts, the determination of admissible principles, and the development of mathematical knowledge through the rise of new practices. Being an approach that emphasizes the links between diverse practices, it is naturally centred upon the mathematician as an epistemic agent that establishes those links.

The paper will explore basic features of that perspective, based on some of the simplest traits of such an account. Even if we disregard subtler aspects of a mathematical practice such as the images of mathematics it incorporates, and the values that are being promoted by participants in the practice, we are still left with sufficient material for an interesting analysis of the constitution of mathematical knowledge. This will be shown by focusing on two exemplary cases which in fact are interwoven: the constitution of the concept of a natural number from the interplay of non-scientific practices and new symbolic practices; and the way in which previous mathematical practices (in particular arithmetical ones) have conditioned the admissible principles of set theory.

As this already suggests, the proposed approach puts an emphasis on the interconnections between mathematical practices and other kinds of practice (e.g., technical practices such as measuring, and scientific practices such as modelling), in such a way that the problem of the "applicability" of mathematics ceases to be posed as external to mathematics itself, and becomes internal to this analysis of mathematical knowledge.

Roberto Festa (University of Trieste), Vincenzo Crupi (University of Trieste) and Carlo Buttasi (University of Trieste)

The Grammar of Confirmation

Some general desiderata for an adequate confirmation measure C are the following:

(a) C should be grounded on some simple and intuitively appealing "core intuition";

(b) C should be ruled by a plausible "grammar of confirmation", i.e., C should satisfy a set of adequacy requirements which formally express sound intuitions;

(c) w.r.t. (a) and (b), the methodological role of C in science should be specified.

Here, we will focus on (a) and (b), w.r.t some new Bayesian confirmation measures. In particular, the following issues will be addressed.

(1) The grammar of P-incremental confirmation. The concept of P-incrementality for Bayesian confirmation measures will be defined by a set of basic requirements.

(2) Core intuitions and grammar of some new confirmation measures. Along with traditional P-incremental measures, such as the difference measure $Cd(h,e) = P(h|e) - P(h)$ and the ratio measure $Cr(h,e) = P(h|e)/P(h)$, a new continuum of confirmation measures Cp and a new confirmation measure Cz and will be introduced:

$$Cp = [P(h|e) - P(h)]/[P(h|e) + P(h) + pP(h|e)P(h)] \quad \text{with } -1 \leq p \leq +1$$

$$Cz = \begin{cases} [P(h|e) - P(h)]/[1 - P(h)] & \text{if } P(h|e) \geq P(h) \\ [P(h|e) - P(h)]/P(h) & \text{if } P(h|e) < P(h) \end{cases}$$

(2a) It will be shown that the grammar of Cp is ruled by the parameter p w.r.t. the following alternative requirements:

(PP=) Prior-probability independence

If $P(e|h1) = P(e|h2)$, then $c(h1,e) = c(h2,e)$, no matter what the values of $P(h1)$ and $P(h2)$.

(PP>) Prior-probability bonus

If P(e|h1) = P(e|h2), then c(h1,e) >/=/< c(h2,e) iff P(h1) >/=/< P(h2).

(Cont>) Content bonus

If P(e|h1) = P(e|h2), then c(h1,e) >/=/< c(h2,e) iff P(h1) </=/> P(h2).
Since the content of a hypothesis h is commonly defined as cont(h) = 1 - P(h), one can restate (Cont>) as follows:

If P(e|h1) = P(e|h2), then c(h1,e) >/=/< c(h2,e) iff cont(h1) >/=/< cont(h2).

Indeed, either (PP=), (PP>) or (Cont>) is satisfied in case p is either positive, null or negative, respectively.

One can show that the ratio measure Cr(h,e) is essentially identical to Cp=0 and that the measure of corroboration advocated by Popper's is essentially identical to the measure Cp=1 which satisfies (PP>). This is somewhat surprising since, given the highly Popperian flavour of requirement (Cont>), one would expect that Popper advocated (a measure similar to) our Cp=-1 which satisfies (Cont>). Notably, no other confirmation measure known from the literature is consistent with (Cont>).

(2b) It will be shown that the grammar of Cz includes the fulfilment of the following set of symmetries and asymmetries, representing a generalisation of the grammar of logical implication (conclusive confirmation) and logical refutation (conclusive disconfirmation) ["=/" denotes "not equal"]:

IF e CONFIRMS h

for some pair e, h, C(h,e) =/ C(e,h)
for some pair e, h, C(h,e) =/ C(¬h,¬e)
for any pair e, h, C(h,e) = C(¬e,¬h)
for some pair e, h, C(h,e) =/ –C(h,¬e)
for any pair e, h, C(h,e) = –C(¬h,e)
for some pair e, h, C(h,e) =/ –C(¬e,h)
for any pair e, h, C(h,e) = –C(e,¬h)

IF e DISCONFIRMS h

for any pair e, h, C(h,e) = C(e,h)
for some pair e, h, C(h,e) =/ C(h,¬e)
for some pair e, h, C(h,e) =/ C(¬e,¬h)
for some pair e, h, C(h,e) =/ –C(h,¬e)
for any pair e, h, C(h,e) = –C(¬h,e)
for any pair e, h, C(h,e) = –C(¬e,h)
for some pair e, h, C(h,e) =/ –C(e,¬h)

Finally, the distance-based core intuition undelying Cz will be specified and discussed.

Paul Franco (University of Pennsylvania)

The Constitutive A Priori and the Quine/Carnap Debate

Quine maintains that in a properly empiricist epistemology that accepts testing holism the statements in our web of belief are not different in kind from one another, but all sit on the same spectrum of being more or less resistant to revision in the light of a recalcitrant experience.

Carnap, on the other hand, who also accepted testing holism, maintained that one could make a principled distinction between analytic and synthetic statements, and furthermore, against Quine, that it was important to an empiricist epistemology to do so.

Whereas Quine takes Carnap's insistence on maintaining the analytic/synthetic distinction to be primarily motivated by a commitment to a verification theory of meaning and a desire to preserve the necessity of the statements of logic and mathematics, this was not Carnap's main motivation. On the contrary, Carnap is not seeking to find a way in which he can justify and ground the analytic/synthetic distinction within a properly empiricist epistemology. Carnap's commitment to the analytic/synthetic distinction is a consequence of his views concerning linguistic frameworks and what can be characterized as the constitutive role said frameworks and their principles play in our body of knowledge.

If we construe the a priori as that which is constitutive of the possibility of empirical knowledge, then the debate between Quine and Carnap concerning the analytic/synthetic distinction can be placed in a larger debate concerning the importance of a conception of the constitutive a priori to epistemology. Quine has no such conception of the a priori. For him, those statements traditionally thought to be a priori, those statements that can be held come what may in experience, are simply well-entrenched parts of our web of belief. Carnap, in rejecting Quine's characterization of analytic statements as those that can be held come what may in experience, also rejects the traditional conception of the a priori. Though Carnap never explicitly offers a conception of the constitutive a priori, it is implicit in his views about the importance of linguistic frameworks to meaningful empirical knowledge: without certain important rules of a linguistic framework in place, no empirical knowledge is possible; hence, such rules are constitutive a priori.

Thus, the debate between Quine and Carnap concerning the analytic/synthetic distinction is, at bottom, really a debate concerning whether or not certain principles play a constitutive role in our web of belief, i.e., whether or not there are a priori elements in our web of belief. My paper, then, will be an examination of Carnap's actual motivation (and not Quine's caricature) in maintaining the analytic/synthetic distinction in an empiricist epistemology in the face of Quine's arguments against the meaningfulness of said distinction. In the process, it will be shown that Carnap is committed to a conception of the constitutive a priori, and that the real debate between Quine and Carnap is concerning whether or not such a conception is possible or meaningful in an empiricist epistemology.

Mathias Frisch (University of Maryland)

Causation and Physics

It appears to be both natural and intuitive to think of the world as causally evolving. We conceive of events in the present as being caused by events in the past and, in turn, as being responsible for what happens in the future. But it is also a widespread view—at least among philosophers of physics—that this conception is not part of how the mature sciences represent the world. According to this view, the notion of cause survives—if at all—as part of a 'folk' scientific conception of the world but has no place in our mature theories of physics. In this paper I want to examine critically some of the arguments in favor of this causal skepticism and will try to defend the view that the notion of cause plays an important role even in the mature sciences. Rich causal notions, I want to maintain, are an integral part of how physicists represent the world within the context of at least some mature theories and causal reasoning plays an important role even in contemporary physics.

As positive evidence in support of my claim I will briefly discuss as case study the derivation of classical dispersion relations, which invokes an explicitly causal time-asymmetric assumption. The majority of this paper, however, will be devoted to fending of arguments advanced by the causal skeptics. The negative arguments I examine in a longer version of the paper, from which this talk is taken, aim to show the following:

1) A physics with time-symmetric laws is incompatible with an asymmetric notion of cause. (Versions of this argument can be traced to Russell's famous attack on the notion of cause.)

2) There is acausality even in those physical theories that prima facie appear most hospitable to causal notions. (A version of this argument has been advanced by John Norton in several widely discussed recent papers.)
3) The distinction between causation and determination can only be meaningfully drawn for coarse-grained macro-variables, since on the micro-level an event's causes would include the entire past, or at least the entire past-lightcone of that event.
4) The most plausible current theory of causation appeals to the relation between causation and intervention. But the notion of intervention can only be applied to finite systems and, hence, not in the context of putatively universal theories.
5) To the extent that we do find causal notions in science they are imprecise and serve a preliminary role in the development of a field.

I plan to discuss a subset of these arguments and argue that none of them are cogent.

Henri Galinon (IHPST – CNRS)

Deflationism, Inferential Semantics and the Logicality of 'True'

Deflationism about truth is the thesis that truth is a «thin» or «non-substantial» property the meaning of which is entirely given by the tarskian T-equivalences (or T rules) . «True», it is said, is a kind of logico-linguistic device: it is useful for expressing some generalizations but it is devoid of any «real» explanatory power. A problem with this view is that it has proved difficult to render this «non-substantiality» claim precise. The aim of this paper is to show that the deflationist could take it to mean that truth is a kind of logical property, and that this answer is in harmony with the deflationist's methodological commitments and some classical insights on logicality coming from philosophical proof-theory.

One of the main methodological consequences of deflationary views of truth is that truth and truth-conditions can play no real explanatory role and, in particular, that truth-conditional semantics are misguided. Deflationists thus have to propose an alternative account of meaning; a popular option is to rely on philosophical investigations which think of «meaning as use». To be sure, there is no universal consensus on what such an alternative theory of meaning should look like; indeed there is not even a consensus on what is to count as «use» in the expression «meaning as use». Nonetheless, it is widely agreed that in the special case of the allegedly «logical» expressions a purely inferential account seems a plausible and attractive picture. A deflationist thus should naturally turn to them when he comes to the question «what is a logical expression?».

Consequently, the rich array of work in philosophical proof-theory related to investigations on the meaning of logical constants seems highly relevant to the deflationist. As is well-known, philosophers in this tradition (Belnap 1962, Dummett 1991, Dosen 1989, Kremer 1988...) have offered various related tentative answers to the following basic questions : under which conditions can a set of introduction and /or elimination rules for an expression be said to give the meaning of this expression? What are the conditions under which an expression can be said to be a logical expression? Various principled criteria have been provided against which classical, intuitionist or modal connectives have been tested.

A tempting further move for the deflationist is then to build on this work and ask whether «true» itself is a logical expression. This is the question we treat in this paper (on behalf of the deflationist). We consider on the one hand different possible versions of the T-rules as providing the meaning of «true» (rules being restricted or unrestricted to the truth-free language, background language being arithmetical or having quotation devices, underlying logic being classical or non-classical, truth-rules being understood as rules of inference or as rules of proof). And, on the other hand, we compare two criteria of logicality, one which refer to the harmony of the rules (stemming from the theme of logical rules as self-justifying), the other based on the schematicity of the rules (logical sequents thought of as reflecting metalinguistic purely structural sequents), and show that a small relaxing of these criteria allows for the introduction of a notion of “logico-syntactic” or “quasi-logical” expression. We are led to the conclusion, which gains further support from its stability, that ” true” can be analysed as a ”quasi-logical” concept.

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Axel Gelfert (National University of Singapore)

Coherence and Indirect Confirmation between Scientific Models: A Case Study and its Epistemological Implications

The topic of coherence has recently received new attention, primarily within epistemology but also in the philosophy of science, where it has been applied to questions in relation to indirect confirmation of hypotheses by evidence. While a number of authors have argued that it is doubtful that coherence is truth-conducive in general (in the sense that, given certain non-arbitrary background assumptions, a set of hypotheses that is more coherent is always more probable), it has also been shown that, under certain conditions, coherence is confirmation-conducive (in the sense that confirmation may be transmitted across sets of coherent statements and hypotheses). These conditions can be made mathematically precise on any coherence measure that satisfies certain non-trivial confirmation transmission properties. The significance of these findings lies in their providing a clear sense in which coherence is epistemically advantageous. Unlike (some forms of) traditional coherentism, there is no suggestion that coherence alone can somehow generate justification ‘from scratch’, as it were; rather, what is shown is how empirical confirmation may be transmitted across different (sets of) statements. The present paper generalises this point by arguing that an analogous relation between coherence and indirect confirmation obtains in the case of models. This shift in emphasis, from relations between hypotheses and theoretical statements towards relations between models, requires several modifications of standard concepts of coherence and indirect confirmation. On standard accounts, indirect justification is a matter of a piece of evidence E confirming one hypothesis H' by way of confirming another hypothesis H, which is more directly related to E than is H' (for example because E is logically entailed by H, but not by H'). Typically, then, the existence of an overarching theory, which not only comprises the hypotheses to be confirmed but also places constraints on which empirical findings constitute confirming evidence, is taken as a given. By contrast, models often rest on simplifying assumptions and idealizations, which may or may not be limiting cases of an underlying fundamental theory. Also, importantly, the relationship between models and empirical evidence also often is more tentative, insofar as a model is typically only intended to explain certain empirical aspects of a phenomenon. The present paper argues that, notwithstanding these differences, coherence and indirect confirmation are of equal importance, and can be given a clear meaning, in the case of scientific models. Furthermore, it is argued, the distinctive character of scientific models as compared with scientific theories (and theoretical hypotheses) also affords new ways of indirect confirmation. As the paper demonstrates by way of a historical example from actual scientific practice, this is most striking in the case of mathematical models, which, by virtue of their status as mathematical structures, may be formally related in such a way as to allow the transfer of empirical confirmation from one to another. This suggests that a proper appreciation of the role of scientific models, and of mathematicals models in particular, may be able to shed light on general questions concerning confirmation and coherence as applied to science.

Edwin Glassner (Institute Vienna Circle)

Between Pure Intuition and Popular Impercipience: Schlick and the Early Reception of Relativity Theory

Moritz Schlick undoubtedly played a prominent role in popularizing Einstein's theory of relativity. This required writing about relativity theory without the mathematical apparatus, thereby appealing to intuition. At the same time, though, as has been noted on several occasions (Turner, Pulte), Schlick's epistemology at that time describes an abyss separating thought from intuition. This feature of his epistemology was intertwined with the critique of pure intuition used in Kantianism for the foundation of geometry. It is no coincidence that Schlick (1921+1922) interpreted Helmholtz's epistemological writings as an ancestor of Einsteinian ideas, since with Helmholtz (recall Helmholtz's idiosyncratic reading of Kant) he could emphasize the fruitfulness of contrasting intuition and thought. Yet, although pure intuition accordingly doesn't do the trick, Schlick and others must have thought it possible that there is some way to present the philosophical contents of relativity theory without going through the whole conceptual framework. This is what many "gemeinverständliche" (commonly comprehensible) presentations of the theory seemed to hope for.

It is a rather underestimated fact that in this vein discussions were lead in newspaper articles by philosophers of science. From a certain point on, though, the discussion was steered against popular intuition. The problem was that the great number of non-technical introductions to the theory of relativity, which were published from 1917 on, made people believe they actually knew everything they needed in order to understand what was going on without studying the formal apparatus. Reichenbach, Carnap and especially Schlick, as the designated "prophet of the community" (Born) of physicists believing in the new theory, made strong efforts to make philosophically comprehensible the outstanding importance of relativity theory for any world conception. But their attempt at explaining relativity theory to laymen(-philosophers) failed. The intuitive approach just wasn't sufficient to convince intellectuals who misunderstood or outright denied the conceptual basis of relativity theory. Philosophical arguments stood against philosophical arguments, and the fact that more and more explicitly non-technical introductions to relativity theory were published made the force of additional physical arguments subject to inflationary pressure. Eventually, even the existing empirical confirmation (eclipse etc.) was questioned in its epistemological status.

The main adressees in this discussion were the Brentano-school with its emphasis on immediate evidence (Kraus, Husserl), interpretations of relativity theory in terms of philosophical subjectivism à la Protagoras (Vaihinger), and a krypto-conventionalist position (Dingler). Schlick (1921) and Carnap (1921) had arguments with Dingler, Schlick (1923) –this article has only just been rediscovered– and Reichenbach (1923) disputed with Kraus, and Schlick (1923) attacked Vaihinger.

The problems with the intuitive approach in turn lead to controversies within the "community" as to how relativity theory and its corollaries were to be presented. (Berliner, Born) One way to put it is to say that there is no way to make the (or any) new theory understandable intuitively. This in turn implies a specific recalibration of the epistemological framework along the lines implied by Schlick's abyss (and somehow anticipating the emergence of full-fledged logical empiricism). As Einstein put it, "Physics is mainly conceptual. Intuition is a function of time."

Nathalie Gontier (Free University of Brussels, VUB)

Philosophy of Anthropology and the Gradualism versus Punctuated Equilibrium Debate

Within paleo-anthropology, skeletal remains are the main sources to speculate upon the physical origin and further evolution of hominins. This field is currently divided into splitters and lumpers. The former understand hominin evolution to be nonlinear, since many genera are understood to be paraphyletic and different sister taxa emerged that did not all evolve into the Homo lineage that would evolve our species. Hence, many different species are distinguishable in the fossil record (Wood & Richmond, 2000; Wood & Constantino, 2004). Lumpers (e.g. Tobias, 2005) on the other hand emphasize evolutionary continuity between ancient and modern species and therefore argue in favour of the recognition of only a few species that show great variation through time.

Stone stools are often used to draw inferences on the cognitive evolution of early Hominini. More recently, personal ornaments too (Foley & MirazÃ³n 2003, Henshilwood et al. 2001, Vanhaeren & d'Errico, 2006) are used for this purpose. The lithic tools and personal ornaments are the earliest windows on early symbolic behaviour we have. Variation in these cultural artefacts not only provides insight into the origin and evolution of language, but possibly also into the origin and evolution of ethno-linguistic and cultural variation in general.

The punctuated equilibrium debate versus the gradualist debate plays a major role in both the study of the physical origin of hominins and the origin of their cognitive abilities, inferred from their artefacts. Within the former discussion, it is gradual versus punctuated models that lie at the basis of distinguishing between a few or many species. Within the latter, more archaeological inspired disciplines, the debate upon the existence of an Upper Palaeolithic cultural revolution or a more gradual out-of-Africa origin of symbolic behaviour (McBreathy and Brooks, 2000) is also driven by punctuated versus gradual debates.

The discussions on the role of these cultural and anatomical remnants in hominin evolution displays an inter- and transdisciplinary character: archaeologists, anthropologists, cognitive scientists, evolutionary linguists and biologists, participate in the debate. What is lacking however in all these fields is a serious consideration of when a certain series of events is to be called a gradualist or punctuated equilibrium event. It is here that philosophy of science can help. After the introduction of philosophy of science and its subdivisions into philosophy of physics and philosophy of biology, etc. the time has come to launch philosophy of anthropology (physical, social and cultural) as well.

Simone Gozzano (University of L'Aquila)

Multiple Realizability and Identity

It is generally held that type-identity theories of mind have been definitively discarded by Putnam's multiple realizability argument and by Kripke's thesis concerning necessary identities. In this paper, I would like to challenge this opinion, even if under some conditions.

The multiple realizability argument is generally taken to show that identity statements between mental properties (say, have pain) and their realizers (C-fibers firing) are not necessarily true. These are contrasted, by Putnam as well as by Kripke, with statements such as "heat = molecular motion", which are taken as necessarily true. However, after briefly introducing the issue in its generality, I point out that the latter identity statement is subject to the same kind of multiple realizability.

On the one side, as already noticed by many authors, the concept of heat can be applied to different states of the matter (gases, solids, plasma, …). In such cases, the supposed identity with molecular motion is no longer necessarily valid; on the other hand, others have noticed that inter-level identities allow for minimal variability: two objects having the same temperature may have different physical arrangements of moving molecules. However, while correct, these observations seem to allow that if applied to a very specific state of the matter (say, gases), the identity holds. I want to argue that in this case too it is nevertheless multiply realized, and in a more serious way then individual variability. In fact, and this is crucial to the argument, heat can be realized through different natural kinds, that is different kinds of molecules. This shows that the supposed identity "heat = molecular motion" is nothing more than a schema of identification. In order to obtain an identity statement it is necessary to constrain the logical form of the schema of identity by introducing co-referential rigid designators on both sides of the identity sign. Once this is done, we can have necessarily true identity statements again, but these have a quite narrow scope of validity. The same kind of reasoning can be applied in the case of the supposed identity between pain and C-fiber activation. In this latter case, we have to narrow the scope of the physical realization condition of the mental state or property in the same way in which this is done in the case of purely physicalistic statements.

So, in the second part to the paper, I argue that the way in which the multiple realizability argument can be tackled with respect to purely physicalistic identity statements (heat = molecular motion) can be applied to mentalistic identities too. I argue that this strategy not only provides an answer to Putnam's argument but that, if supplemented, blocks the inference that allows to Kripke to maintain that phenomenology is all there is in mental life. In order to supplement it, I argue that feeling pain and detecting pain are two different conditions, and that recent medical literature provides strong support to this view.

The general upshot of the paper is that the identity thesis, as originally proposed by Smart, Place and others, is no longer defendable. In its place we should introduce more narrow tailored identities, but these are not different from those that we should accept in case of purely physicalistic terms, such as heat and molecular motion, being the result of theoretical identifications. Having set all this, I conclude that the identity theory of mind can be vindicated.

Gerd Grasshoff (University of Bern), Samuel Protmann (University of Bern) and Adrian Wüthrich (University of Bern)

Minimal Assumption Derivation of a Bell- type Inequality

In the light of recent criticism and discussions we review the main results of our project aiming at deriving a Bell-type inequality from the weakest possible assumptions. The main results include that a Bell-type inequality can be derived from the assumption of separate common causes (Grasshoff, Portmann & Wüthrich 2005). This is even possible without the assumption of perfectly anticorrelating event types (Portmann & Wüthrich 2006). In particular we will address the critique to the effect that in Grasshoff et al. (2005) we implicitly assume a common common cause (Hofer-Szabo 2006).

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Alexei Grinbaum (CEA-Saclay)

Reconstruction of Quantum Theory

What belongs to quantum theory is no more than what is needed for its derivation. Keeping to this maxim, we record a paradigmatic shift in the foundations of quantum mechanics, where the focus has recently shifted from interpreting to reconstructing quantum theory. Several historic and contemporary reconstructions are analyzed, including the work of Hardy, Rovelli, and Clifton, Bub and Halvorson. We conclude by discussing the importance of a novel concept of intentionally incomplete reconstruction.

Fabrice Gzil (IHPST/Collége de France)

Animal Models of Alzheimer's Disease and Cognitive Ageing

My aim is to compare animal models of Alzheimer's disease (AD) in medicine and biology. I have used as a conceptual starting point Evelyn Fox Keller's paper about 'models of' and 'models for' (1). I chose for the focus of my analysis APP transgenic mice and the development of a mouse model for spatial learning impairments out of the experimental work of Laure Rondi-Reig and her colleagues (2).

As Keller argues, animal models are simultaneously 'models of' and 'models for'. But in the study of human ageing and dementia, it is not sure that this dichotomy should be interpreted exactly as Keller does. In this case, mice are at the same time equivalents ('models for') and paradigms ('models of'). In the first sense, as equivalents, they are equated with aged and demented people, they put in concrete form the knowledge on ageing and dementia, they recapitulate and materialize the current knowledge. In the second sense, as paradigms, they allow a simplified representation of the complex processes which are under study, the thing that matters is not their materiality but their ideality, they do not support current but potential knowledge.

Producing an animal model of cognitive ageing or dementia does not mean the same thing in both cases. Producing an equivalent of AD or cognitive ageing consists of trying to reproduce what is known about the pathology. The criterion is the similarity with (at least one aspect of) the process. As there is no natural equivalent of AD in animals, transgenic mouse models have been developed. As none of the current transgenic models exactly mimics the pathophysiology of the disease in the human, the strategy consists in 'humanizing' the mouse (implanting human genes into the mouse genome or implanting human neurons into the mouse brain). On the contrary, producing a paradigm of AD or cognitive ageing consists in developing a model which allows us to gain new knowledge about the process which is under study, i.e. a model which allows us to extrapolate to the human the knowledge gained from the animal. In this case, the criterion is not similarity but fruitfulness. Because it is difficult to assess behavioural changes in mice that are reflective of cognitive deficits observed in AD patients, researchers now consider ways of 'murinizing' the human for behavioural studies.

While biological models are first intended to be good paradigms, medical models are first intended to be good equivalents. This does not mean that medicine is more 'instrumental' than biology, or that biology is more 'theoretical' than medicine. As equivalents of AD, transgenic mice are intended to have an instrumental value (pharmacological screening), but they first have an epistemic value (they test out current hypothesis). As paradigms of cognitive ageing, experimental work on mice is intended to have an epistemic value (gaining new knowledges), but they first have an instrumental value (biologists develop new ways of measuring cognitive performances and cognitive deficits).

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Jens Harbecke (University of Lausanne/University of Bern)

Conservative and Eliminative Reduction: Exploring the Spectrum

1. The question concerning the precise border between a conservative and an eliminative reduction of special science kinds is a notorious source of confusion in the philosophy of reduction and reductive explanation. In my talk, I am concerned with a criterion that sufficiently specifies the distinction. In a second step, I aim to show that, within a physicalistic metaphysics, only conservative reduction is an acceptable solution with respect to a great number of special science kinds and I explore some consequences of this finding.
2. When a special science theory A is reduced onto a more basic theory B, it is assumed that the kind terms and laws of A are "mapped" onto certain kind terms and laws of B. What distinguishes cases of conservative and eliminative reduction is the WAY theory A is mapped onto B.
3. Typically, different cases of theory mappings involve different degrees of "approximation", where the criterion for approximation is defined over an agreement of the extensions of the kind terms and laws contained in A and B. Ideally, all cases of theory mappings are orderable on a continuum of lesser and greater approximative degree (cf. Bickle 1998). At one end of the continuum lies a perfect approximation based on a co-extension of the kind terms and laws of A with certain (possibly conjunctive) kind terms and laws of B. At the other end lie cases in which the mapping is blurred. A blur results from at least one out of the following two sources: i) The extensions of some or all terms of A are not fully covered by theory B as in the classical case of the reduction of the "Phlogiston Theory". Or ii) B fully covers all extensions of the terms of A, but the B-terms by which it does are disjunctive. This is the case if the types postulated by A are "multiply realized".
4. My aim is to argue that co-extensions are necessary and sufficient for a conservative reduction given physicalism is presupposed. In contrast, the failure to establish a co-extension of a particular term of a special science theory A with a term of a more basic theory B implies that the A-term fails to pick out an ontological type. Hence, the type that the A-term postulated must be eliminated from the ontology. Mappings involving some approximation but failing to establish co-extensions are therefore eliminative reductions. I briefly discuss Nagel (1961), Lewis (1970), Churchland (1985), Kim (1998), and Marras (2002) (some of which infer different conclusions than mine) to support and justify my view in this respect.
5. Finally, I argue that a metaphysics implying the elimination of certain special science kinds departs too far from commonsense to be acceptable. However, at least prima facie many of these special science kinds seem to allow for blurred and, hence, eliminative reductions only. If physicalism is to be retained, a strategy must be found that proves the conservative reducibility for these special science kinds, contrary to the prima facie appearance. I shall indicate what this strategy may consist in and what problems it may face.

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Richard Healey (University of Arizona)

Gauge Symmetry and the Theta- Vacuum

According to conventional wisdom, local gauge symmetry is not a symmetry of nature, but an artifact of how our theories represent nature. But a study of the so-called theta-vacuum appears to refute this view. The ground state of a quantized non-Abelian Yang-Mills gauge theory is characterized by a real-valued parameter Θ (theta)—a fundamental new constant of nature. The structure of this vacuum state is often said to arise from a degeneracy of the vacuum of the corresponding classical theory: this degeneracy allegedly arises from the fact that “large” (but not “small”) local gauge transformations connect physically distinct states of zero field energy. If that is right, then some local gauge transformations do generate empirical symmetries. In defending conventional wisdom against this challenge I hope to clarify the meaning of empirical symmetry while deepening our understanding of gauge transformations.

I distinguish empirical from theoretical symmetries. Using Galileo’s ship and Faraday’s cube as illustrations, I say when an empirical symmetry is implied by a theoretical symmetry. I explain how the theta-vacuum arises, and how “large” gauge transformations differ from “small” ones. I then present two analogies from elementary quantum mechanics. By applying my analysis of the relation between empirical and theoretical symmetries, I show which analogy faithfully portrays the character of the vacuum state of a classical non-Abelian Yang-Mills gauge theory. The upshot is that “large” as well as “small” gauge transformations are purely formal symmetries of non-Abelian Yang-Mills gauge theories, whether classical or quantized.

It is still worth distinguishing between these kinds of symmetries. An analysis of gauge within the constrained-Hamiltonian formalism yields the result that “large” gauge transformations should not be classified as gauge transformations. Moreover, in a theory in which boundary conditions are modeled dynamically, “global” gauge transformations, as well as “large” gauge transformations, may be associated with empirical symmetries, corresponding to transformations among these extra dynamical variables. But if a Noether charge in the theory is conserved, then no such empirical symmetry is forthcoming. The upshot is that if a “global” gauge transformation is a symmetry of the Lagrangian, then there is no associated empirical symmetry. Rather than thinking of conservation of charge as an indirect empirical consequence of “global” gauge symmetry of the Lagrangian, perhaps one should think of charge conservation as the empirical precondition for the “global” gauge symmetry of the Lagrangian not to be associated with any empirical symmetry.

Robin Hendry (Durham University)

The Chemical Bond: Structure, Energy and Explanation

Chemical bonds are central to understanding the behaviour of matter. The starting point of many chemical explanations is a molecular structure: a group of atoms linked together in a certain way, by bonds. Many aspects of chemical reactions, including their products and the heat generated or absorbed in the process, are understood in terms of the breaking and making of bonds. Molecular spectra arise from the vibrations and rotations of bonded groups of atoms.

From the middle of the nineteenth century, organic chemists began to explain the behaviour of substances, and the different ways in which elements combined to form them, in terms of structural formulae. These structural formulae were not, at first, understood as hypotheses about how the atoms are arranged in space, but instead as encoding a substance’s patterns of chemical reaction, or, less cautiously, topological connections between atoms. With the rise of stereochemistry in the late nineteenth century came a fleshing out of structural formulae and a change in their status. Stereochemical theories like Jacobus van ’t Hoff’s explanation of optical isomerism and Adolf von Baeyer’s strain theory were intrinsically spatial, in that they depended for their explanatory power on their describing the spatial structure of molecules. Yet the physical nature of the bonds holding these structures together raised a number of puzzles. (i) Electrolysis established a link between electricity and chemical combination, suggesting that the basis of combination is the electrostatic attraction between opposite charges. However, this made the existence of homonuclear diatomic molecules like H2 and N2 seem incomprehensible. (ii) Although the evidence seemed to support valence formulae as representing the real structures of many organic compounds, the significance of valence formulae for inorganic substances was less clear.

In the twentieth century, chemists G.N. Lewis and Linus Pauling addressed these puzzles with physical theories of the bond. Lewis presented his shared-electron chemical bond as unifying the earlier dualistic and structural theories, which in the nineteenth century had become associated with inorganic and organic chemistry respectively. Pauling saw his quantum-mechanical theory of the bond as a synthesis of Lewis' insights with physical theory. But some quantum chemists were sceptical of the theoretical basis of Pauling's explanations, and questioned whether classical bonds, though a useful explanatory tool for classical chemistry, were physically real.

In an earlier paper I have articulated two opposed conceptions of the chemical bond to express the views of Pauling and his critics respectively. The structural view emphasises the continuity of the modern chemical bond with nineteenth-century structure theory. The energetic view seeks to replace the classical conception, identifying bonds with well-defined energy changes, rather than structural features of molecules. In this paper I assess how far modern quantum-mechanical studies of electron density within molecules support the structural view, or a more radical revision of the concept of bond.

Paul Hoyningen-Huene (University of Hannover) and Eric Oberheim (Humboldt University of Berlin)

Reassessing Feyerabend's Philosophy

The talk will report new results on Paul Feyerabend's philosophy, in both historical and in systematic respects. Historically, we will argue that Feyerabend used as yet unrecognized sources for the introduction of the central concept of his philosophy: the notion of incommensurability. For example, although Feyerabend did not credit these individuals, Feyerabend built his notion of incommensurability directly from insights he found in the works of the physicist-philosopher Pierre Duhem and the gestalt psychologist Wolfgang Köhler. The former described the phenomenon of incommensurability very similarly to Feyerabend, and the latter even used the term 'incommensurability' to do so. Second, with respect to Feyerabend's relationship to Popper, we challenge the common opinion that the early Feyerabend was a Popperian. Various contradictory facts relevant to this question will be discussed. Third, there is quite some confusion in the literature concerning the question of whether Feyerabend was a realist and if yes, of what brand. Distinguishing and considering two relevant forms of realism will clarify this question.

Systematically, three elements of Feyerabend's philosophy that have not yet received due attention will be examined. They resolve certain tensions that seem to make many of Feyerabend's philosophical moves fairly erratic. Three underlying themes of his philosophy that unify it to an unexpectedly high degree will be exposed. The first theme is methodological. It concerns Feyerabend's consistent use of immanent criticism. This form of criticism, when misread, appears to generate many inconsistencies in Feyerabend's philosophy. Properly understood, however, it shows a consistent critical engagement with the many philosophical positions of his most important contemporaries. The second theme concerns a philosophical enemy that Feyerabend constantly fought from very early on: conceptual conservatism. Many apparently very disparate critical moves by Feyerabend can be subsumed and thereby understood under this philosophical theme. The third theme is meta-philosophical and it concerns Feyerabend's support of a pluralist epistemology of which an object-level and a meta-level variant will be distinguished. We will discuss some aspects of this position and make Feyerabend's most important philosophical motifs for his support of it plausible. By way of conclusion, we will argue that the philosophical reception of Feyerabend's early philosophy has not been very satisfactory.

Ghislaine Idabouk (University of Paris VII)

Randomness, Financial Markets and the Brownian Motion: A Reflection on the Role of Mathematics, its Interactions with Economics and the Ideological Implications in the Financial Theory of the late 20th Century

In May of 1973, the Journal of Political Economy publishes an article entitled "The pricing of Options and Corporate Liabilities" by Fisher Black, from the University of Chicago and Myron Scholes from the Massachusetts Institute of Technology. The authors address a question that had been a topic of interest among economists since the 1960's: the pricing of financial securities used for speculation and hedging purposes, options. They derive an option pricing formula which depends on perfectly known parameters and explicitly uses the standard normal distribution.

Their article will have a major influence. First, it is to become the keystone of a theoretical stream, later known as continuous-time finance or mathematical finance, which substantially uses probability theory and stochastic calculus. It will also serve, from a practical standpoint, as the pricing reference on several exchanges. In 1990, Fisher Black, one of the co-authors of the founding article, stated: "because the formula is so popular, because so many traders and investors use it, option prices tend to fit the model even when they shouldn't". This sentence, which might seem odd, is, from an epistemological standpoint, an invitation to rethink the relationship between model and reality, to reflect on the role assigned to a model and the consensus value of a model.

In a broader perspective, the construction of continuous-time finance as a theoretical field, since the Black and Scholes article of 1973, raises many questions for a philosopher of science. The first has to do with the mathematization of randomness. Randomness here is randomness of the observed price processes of the risky primitive assets (stocks) in a financial market. In these articles, it is modelled through Brownian motion and stochastic calculus. In traditional neoclassical economic theory, a price is determined through equality of supply and demand that emanate from rational agents. If one clings to these fundamentals, what need is there to give a probabilistic representation of the price of a stock?

Another issue that arises from the use of the Brownian motion to model randomness, and from the martingale property which will, soon after the Black Scholes paper, be claimed for the discounted prices of financial securities, is the question of the ideological implications of such models. Indeed, underneath the Brownian motion, there are the ideas of independent identically distributed increments and scale invariance, and the use of the normal distribution to model the random part of the rates of return on financial securities. At this point, let us recall that financial theory is a social science. It is therefore eventually human behaviours and interactions that are modelled by a normal distribution. The relevance of the normal distribution in many fields of natural sciences is unquestionable. It is however not the case when it comes to social sciences. Besides, the mathematical martingale property relates to an economic assumption, the "Efficient Market Hypothesis", a cornerstone of modern financial economics, first developed by Eugene Fama in his PhD dissertation of 1964. This cannot either be regarded as ideologically neutral.

A last interesting feature in the development of mathematical finance, suggested in the previously mentioned statement made by Black in 1990 and also by the fact that the Black and Scholes formula is often referred to as a "self-fulfilling prophecy", is the particular role assigned to mathematics. What role do they play here? Predictive? Normative?

This paper aims at shedding some light on the points mentioned above.

Sirkku Ikonen (University of Helsinki)

The Vienna Circle, Lebensphilosophie and the Analytic-Continental Divide in Philosophy

The encounter of the Vienna Circle and Lebensphilosophie, philosophy of life, is one of the most neglected episodes in the recent history of philosophy. In this paper my aim is to explore that encounter and shed light on the far-reaching implications it has had for analytic philosophy, and in particular for the formation of the so-called analytic-Continental divide in philosophy.

It is customary to think that the rift between the two traditions stems from the conflicting views on logic and science. Analytic tradition is characterized by centrality of science and logic. In the Continental tradition on the other hand, science plays a minor, if not a nonexistent role.

I will argue, that there is, however, also another theme that separates the two traditions; it has to do with the human life, existence and culture as a philosophical problem. The questions concerning the human existence, life and death, freedom and fate, love, hate are central particularly to the Continental philosophy. In the analytic philosophy the cultural and existential themes, on the other hand, are virtually nonexistent. In this paper I will ask why this is the case.

I will argue that the real contrast between the analytic and Continental camps does not derive from different views on science and logic but from the notions of Leben, life, and Erleben, immediate "lived experience".

I will show that that the analytic/Continental divide can be traced back to the early 20th century German philosophy, to the clash of the Vienna Circle and Lebensphilosophie, and in particular to the clash of Moritz Schlick and Henri Bergson.

Although almost forgotten today Lebensphilosophie, i.e. philosophy of life (represented by e.g. Bergson, Dilthey, Klages, Spengler), was without a doubt the most influential philosophical movement in the early 20th century German philosophy. The Vienna Circle members were among those who reacted strongly against Lebensphilosophie. I will argue that the Vienna Circle's criticism of metaphysics was aimed explicitly against philosophers of life, and particularly against their notions of Erleben and intuition.

I will discuss especially the distinction introduced by Moritz Schlick, that of Erleben and Erkennen. I will show that Schlick's distinction was directed specifically against the life-philosohical notion of Erleben, According to Schlick the qualities (e.g. colors, tones and feelings) and expressive meanings pertaining to immediate experience (Erleben) are not expressible in symbols or words, and therefore do not belong to the field on knowledge (Erkennen). Metaphysics was rejected as an effort to "utter the unutterable." To "utter the unutterable" with non-symbolic means was, on the other hand, the aim of Lebensphilosophie and Bergson in particular.

Shutting out qualities and expressive meanings from the realm of rational discourse shaped analytic philosophy decisively. One of the central consequences is that cultural questions are still a rare topic within analytic philosophy.

Valeriano Iranzo (University of Valencia)

Severe Tests and Use-Novelty

The Popperian tradition (Popper, Lakatos, Musgrave, Zahar, Worrall) has insisted that, when assessing the respective merits of rival hypotheses, predictions of new phenomena should be conferred more weight than explanation of known facts.

The "predictivist" thesis may be understood as: (a) a necessary criterion for counting as evidence: "only prediction of novel facts can give some support (not necessarily spelled out in confirmational terms, pace Popper) to a hypothesis"; (b) a comparative criterion for assessing the relative weight of different bits of evidence: "other things equal, being e a novel fact predicted by h, and e' a known fact explained by h, e counts in favour of h more than e'". Besides, several senses of novelty have been distinguished: temporal-novelty, theoretical-novelty, heuristic-novelty (also called use-novelty). The underlying idea is, however, that the evidential import of phenomena depends on certain historical constraints.

But, why the empirical merits of a theory should depend either on the temporal order of theory and evidence, or on the way it was built? Although not only (a) but also (b) have to cope with historical counterexamples (see Achinstein 2001), it can hardly be denied that predictivism has some intuitive basis. After a brief discussion of the different meanings of "novelty" I will compare two different justifications of use-novelty. The first one has been proposed by D. Mayo. She thinks that demands for novelty could be explained away in favour of demands for "error-severity" in testing procedures (in principle, this fits quite well with Popper's initial suggestions, although Mayo's notion of severity is stricter than that of Popper). On the other side, J. Worrall defends a refined version of heuristic-novelty, and has insisted that heuristic-novelty considerations explain our intuitions about severe tests rather than the other way round. I agree with Worrall that Mayo's examples are not representative of reasoning in science, but I shall try to argue also that Worrall's distinction between conditional and unconditional kind of support is too much artificial as to account for the intuitions underlying heuristic-novelty.

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Gurol Irzik (Bogazici University)

Is Science Being Commercialised? A Manifesto for Philosophers of Science

There is a growing concern that certain segments of science, most notably biomedicine, is rapidly being commercialized in the US and to a lesser degree elsewhere, and that while such commercialization does make economies more competitive and productive, it also has a number of negative effects on various aspects of science. Surprisingly, almost the entire community of philosophers of science, and, more surprisingly, most practitioners of social studies of science met this concern with near total silence.

In this paper I argue on the basis of recent work by Sheldon Krimsky, Martin Kenny, David Greenberg and Sheila Jasanoff that the claim regarding commercialization of science should be taken seriously. This is a new and complex phenomenon, and I first briefly outline the technoscientific, economic, and legal developments that gave rise to it in the US. I then show that, as a result of commercialization, the venerable culture of science and its social function as we know it is being radically transformed: scientific research is skewed toward what is patentable and commercially profitable; what Robert Merton called "the ethos of science" is being undermined; unprecedented conflicts of interest arise; the autonomy of the scientist and her control over the results of her own research is being weakened.

Such consequences should cause alarm for all of us who value science. I urge philosophers of science to pay attention to this phenomenon, mobilize their expertise to examine it in more detail and suggest solutions to minimize its detrimental effects both for science and society at large. As some philosophers of science have argued, after WW II philosophy of science became less and less interested in larger socio-political issues regarding science. I call for a more political philosophy of science.

Andrés L. Jaume (University of Salamanca)

Are all Biological Functions Adaptations?

The aim of this paper is to demonstrate that the reduction from functions to adaptations is not possible. Adaptations don't capture all the richness of functions. There is a tradition in the philosophy of biology which following backward-looking or etiological accounts of biological functions puts them on the same level as adaptations. This is a theoretical reduction from functions to adaptations. They supporters assume that each function is the result of a selection process and, consequently, it counts as an adaptation. But it is arguable that this view is not comprehensive: many biological traits are not explained in terms of an adaptation process despite they are currently functional. Furthermore, the mentioned equivalence is excesively strong and can be charged of dogmatic adaptationism. Adaptationism can be viewed in a constructive way as an interesting program of research without panglossian assumptions as the caricatured view of Gould and Lewontin. Despite the threat of atomizing organisms, it is valuable like heuristic. But, in spite of the mentioned virtues, we have to face up the following problems when we persist in identify functions with adaptations, this is the core of my argument: (1) We can distinguish many levels, all of them allegedly objective, in a biological system, an organism, and each level presents functional traits. It is obvious that different kinds of purposes emerge on each level of organization. It generates the following problem: How to explain functional jerarchies? (2) We can remain in a heuristic adaptationism but it is possible that there is no adaptation process in the origin of the functional trait considered. It is possible that it becomes functional because of design constraints. In such case, we identificate a trait which is not product of a selection for process but a selection of one. (3) We can be faced with an exadaptation: a change in the functionality of a trait maintaining equal its structure. Nature show us many examples. The reasons referred before show us how the concept of adaptation is not sufficient in order to capture functionality. But it is not enought. It may be thought that there is possible to posit the equivalence between proper functions and adaptations maintaining a pluralist theory like Preston (1998) does. She traces the following equivalence: Proper Functions, in the sense coined by R. Millikan are, in fact, adaptations and Systemic ones, as Cummins intends, are exadaptations. This equivalence has been refused by Millikan. She claims that Preston misunderstands the concept of proper function and conflates exadaptations with systemic functions. But I consider they both are wrong. A pluralist theory of functions in the biological realm is neither desirable nor possible. I consider with P.S. Davies (2001) that the systemic theory, with the necessary amendment, is more basic, hence maintaining the two theories, the systemic and the aetiologist one, is redundant.

To summarize, I refuse the equivalence between functions and adaptations, the equivalence between proper functions and adaptations and the pluralist theory. Instead of the regarded proposals I hold a non selectionist account capable of explaining functionality in the way of P.S. Davies. This one is based in systemic functions as they was presented by R. Cummins; functions depicted in terms of causal contributions in a biological given system. I think the advantadges of this kind of accounts are powerful than selectionist ones and, against the common idea, systemic functions are capable of maintaining normativity. This is my positive contribution to the discussion.

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Aspassia Kanellou

On the Distinction between Content Realism and Realism about Intentional States

In this paper I raise the question whether there is a viable distinction between realism about intentional states and content realism. I argue that the inability of functionalism to individuate the propositional content of propositional attitudes is linked with its inability to individuate the phenomenal aspects of sensational states. Content realism is understood not merely as the claim that a) there exist states which are defined by their causal relations to one another and to behavioural outputs and sensory inputs but that in addition b) they owe their causal powers to their content. Whereas condition a) seems to suffice for providing a realist view of intentional state —in so far as they are states that have a characteristic function, or play a causal role— a content realist would probably have to add b). However, what would be a basis for adding such a clause as b)? One such basis could be the denial that intentional content can be individuated solely on the basis of functional role. The argument offered (e.g., by Bermudez and Fodor) is that functional role is not fine-grained enough to individuate even propositional/intentional content and inevitably leads to holism. The usual way to individuate content is either through a) the sentence reporting it or the proposition that corresponds to it, which is again expressed by a corresponding sentence in languages of different sorts or b) its phenomenology, the way it appears to a subject, in case sensational states have content, which again can be reported by a sentence of some form (though there might be some loss in the translation). Thus it seems unlikely that the content of, say, a propositional attitude can be individuated by its causal role or solely its function. If it is granted that functional role cannot individuate content, it could then be argued that it is not an accident that functionalism faces problems with the individuation of the phenomenological aspects or content of sensational states, as with the individuation of the content of propositional attitudes. In both cases, it is failure to individuate the content of certain mental states. One can draw the further conclusion that intentionality and sensory or phenomenal consciousness are inexorably tied. So both failures of functionalism have the same source. If a state's content cannot be individuated simply in terms of the state's causal role or its function what does this imply about the individuation of intentional states? How should the thesis of content realism be formulated? It is suggested that the individuation of intentional states must proceed along two parameters analogously to propositional attitudes, i.e. as certain relations of subjects to propositions or contents. Perhaps, we can understand the thesis of content realism as follows: the intentional state in question must have some vehicle (which realizes its content), which presumably is structurally isomorphic and mirrors the structure of the corresponding content in some way.

Andreas Karitzis (University of Athens)

Defending Realism: Can Ontology Do the Trick?

It is widely held that realism is taken to be basically a metaphysical issue. I criticize one specific kind of this general approach: realism construed as a metaphysical issue is heavily based on ontology. Hence, its defense must be deployed along similar lines: the basic arguments against antirealism come from the specific ontology of what there is in the world. I call this view of defending realism Ontological Defense of Realism (ODR).

I begin by outlining Devitt's account of realism as a paradigmatic case of ODR. In section 3 I argue that ODR cannot block 'verificationist antirealism'. The latter does not centre on what types of entity exist but rather on what is involved in claiming that they exist. What there is in the world is exhausted by what can be known to exist independently of the specific ontology of the relevant entities. My first attack blocks only the strong claim that ontology is all that is needed for the defense of realism. A weaker thesis, that ontological claims are necessary parts of a realist defense, is still at play.

In section 4 I argue that ODR has a problematic consequence. ODR usually implies the commitment to the fundamentality of a specific ontology. But the sheer fact of commitment to a specific ontology has the problematic consequence of putting realism in danger in other domains. For the truthmakers of these domains may not belong, at least in the first instance, to the set of the fundamental objects. So, we suspend our realist commitment to these domains and we render realism dependent on the success of a reductive philosophical project.

In section 5 I argue further that ODR is in tension with a very powerful realist insight: the absolute priority of the world vis-Ã -vis our theorizing of it. From this insight, a realist attitude towards the problem of realism is derived: our commitments, in various domains, should be driven by the fact that the world resists to our theorizing and, by doing it, reveals its existence. It is against the realist intuition a stance of suspending the realist commitment, in a domain where the world presents itself. Moreover, the deep realist insight (absolute priority of the world) is violated when we appeal to our philosophical theorizing (reductive project) in order to support realism in these domains.

In Section 6 I address two possible objections and in section 7 I conclude by presenting my view of the relation between ontology and realism: The motive was to find the best way to defend realism: the commitment to the existence of the external world. My thesis that ontological claims cannot do the trick of defending realism doesn't imply the denial of adopting specific and robust ontological views. In fact, it is perfectly compatible with any ontology whatsoever. Actually, this is a very natural consequence of my point of separating realism from ontology.

Neil Kennedy (University of Quebec at Montreal / University of Paris I) and Carlo Proietti (University of Paris I / IHPST)

Yet Another Paper on Fitch's Paradox

In the language of philosophical inquiry, no one would suspect that a contradiction could arise from the simultaneous adoption of verificationism, i.e. "All that is true can be known", and epistemic modesty, i.e. "There exists a proposition that is true but unknown" (M). However, when translated in the formalism of epistemic logic, these two principles lead straightforwardly to a contradiction, and this unfortunate consequence has been dubbed Fitch's paradox. Two main strategies have been adopted in the face of this surprising result: 1) the recasting of the principles V and M in a more appropriate framework (reformulation strategy); and 2) the restriction of substitution in the context of principle V (restriction strategy). Edgington, Segerberg and Rueckert have opted for the first; Tennant, van Benthem and Dummett have opted for the latter. In this talk, we will consider the possible arguments from both standpoints. On the firsthand, following a restriction strategy, we will consider two important issues surrounding the derivation of the paradox: substitution and (existential) quantification of propositions in (propositional) modal (epistemic) logic. Unrestricted substitution is crucial to the derivation of the contradiction, and existential quantification is necessary in order to formulate the principle of epistemic modesty. In this context, we ask the following: is the contradiction the result of sloppy pragmatics (impredicativity) combined with a Cantorian-styled diagonal argument? On the other hand, this time considering a reformulation strategy, we will examine the philosophical underpinnings of (standard) epistemic logic in order to determine if the symbol "K" is really up to the task of capturing verificationism and modesty. The idea being that the modality "can be known" is, in our philosophical opinion, much more than the concatenation of two independent modal operators, namely, the alethic diamond and the epistemic square. We will favour a solution of the latter type, and hope that dynamic approaches to epistemic logic can capture more appropriately the evolution of knowledge intrinsic to the validity of principle of verificationism.

Jeremy Kessler (University of Cambridge)

Analogy by Exemplar: A Kuhnian Alternative to Hesse's Account of Analogy in Science

After forty years, Mary Hesse's account of analogy is still the strongest available treatment of the use of analogy in science. Hesse argues convincingly that analogy is central to scientific investigation, both descriptively and normatively. While her story of analogical reasoning is flexible and elucidating, I present two objections which demonstrate that it is too open-ended to explain the particular prevalence and efficacy of analogy that Hesse locates in scientific practice, and which I assume. I argue that she can account neither for a scientist's ability to locate specific properties shared by two analogues in a principled manner, nor for a scientist's warrant in assuming that two analogues are similar enough in causal structure to license hypotheses based on analogy. I then present a different account of analogy as it operates in normal scientific practice, an account modeled on Kuhn's theory of exemplars. On the one hand, the learning of exemplars conditions the student to see new problems as being like old problems and thus to recognize problems amenable to already-learned problem-solutions. On the other hand, exemplar-learning restricts the range of problems that a student will be disposed to tackle, thus ensuring that the student's problem-solving vision will not be unduly distracted by unsolvable problems. I argue that these features of problem-identification and problem-selection provide the cognitive tools necessary for successful analogical reasoning in scientific investigation. My Kuhnian story does not overturn Hesse's, but rather provides a supplement which better explains the frequency and efficacy of scientific analogy. To do so, however, the Kuhnian account must treat scientific analogy as a relatively conservative and constrained operation. I thus conclude by considering some objections to this narrowing of the focus of analogical justification.

Max Kistler (Université Pierre Mendès-France, Grenoble and Institut Jean Nicod, Paris)

Mechanistic Explanation and Causation

This paper explores the link between the concepts of explanation and causation. To explain phenomenon R by showing how mechanism M yields output R each time it is triggered by circumstances C, is to give a causal explanation of R. However, some have put exaggerated expectations in the concept of mechanism. In the first place I will show, against Glennan (1996), that a reductive analysis of causation in terms of mechanism is not possible, for two reasons.

1. The concept of mechanism presupposes that of causation: it is the causal interactions between the parts of a mechanism that make it possible for the system to yield result R when triggered by circumstances C.

2. There are elementary physical causal interactions, such as the Compton effect, that cannot be analyzed in terms of a mechanism because the objects involved do not have any parts. Therefore causation is a more general concept than mechanism.

In the second place, I will show that Craver and Bechtel (2006) do not succeed to establish that the framework of mechanistic explanation dissolves the appearance of causal process that "cut across levels": 1) When a virus (molecular level) kills a person (level of macroscopic organism), this seems to be a case of "bottom-up" causation. Craver and Bechtel claim that such causal relations can be analyzed into a causal process within a given level (in this case, molecular), and a relation of constitution. I show that such an analysis is not possible: the reason is that the relation of determination of high-level phenomena by lower-level facts is not logical, as the concept of constitution requires, but depends on laws of nature.

Tarja Knuuttila (University of Helsinki)

Some Consequences of Pragmatism: Whatever Happened to the Notion of Representation in the Philosophy of Science

Philosophers of science have recently become engaged in offering different analyses of representation. More often than not these conceptions have been presented in the context of modelling in an attempt to answer the question of how do models give us knowledge. The standard answer has been that models give us knowledge because they represent but then this has opened the further question of how to understand representation. The different analyses given to representation can roughly be grouped into the structuralist (or semantic) and pragmatist ones. Whereas the structuralist approaches conceive of representation as a dyadic relation between the representative vehicle and its target object, the pragmatists argue that representation is irreducibly a triadic relation: to understand representation we have to take into account also the users of representation, their intentions and interpretations. I will argue that the crux of the issue does not lie in whether the users of representation are taken into account or not. What is at stake is whether or not the possibility of representation is based on some privileged parts that the actual representative vehicles are supposed to contain. With privileged parts I mean such parts that are assumed to be more or less accurate depictions of some aspects of the target phenomena.

That the structuralist accounts of representation are based on privileging some parts of the representative vehicles should seem quite clear. For them "only the underlying structures matter": the privileged parts (i.e. the underlying structures) are necessary for representation and thus in their analysis of representation the structuralists need not to take into account the activity of human representers. The pragmatists have challenged the structuralist dyadic accounts of representation in various ways criticising usually their way of casting the representative relation in terms of isomorphism or similarity. Typically, they have argued that we should also take into account the intentional activity of human representation-users. I suggest, however, that the issue is not closed by this move alone, something that seems to have escaped the attention of most of the pragmatists. The point is not that representation is also an accomplishment of representation-users but whether there is anything in the nature of the representative vehicle and its target system that would guarantee the representative relationship between the two. For pragmatists no such privileged parts can be found from the actual representative vehicles that could guarantee representation. This being the case, only the activity of representation-users can establish the representative relationship, which leaves us with a weak notion of representation. Consequently, I will argue that in regard to representation we face a following dilemma: Either we choose the strong structuralist notion of representation with all its shortcomings or then we opt for the pragmatist alternative, which is too weak to do any significant philosophical work. In my presentation I will examine this situation by studying the recent discussions concerning models in the philosophy of science and internal representation in cognitive science.

Robert Kowalenko (University of Hertfordshire)

A Curve-Fitting Approach to Ceteris Paribus Laws

Law-like generalisations hedged with a ceteris paribus-clause such as widely in use in psychology, the social and biological sciences, are best construed as incomplete strict laws. These incomplete laws can be “fleshed out” by adding a set of enabling, or completing, conditions to their antecedent. In other words, the logical form of a cp-law, ceteris paribus (A ? B), is (A & CB ? B). The nature of CB must be subject to non-ad hoc constraints, however, failing which all putative ceteris paribus-generalisations will be trivially true. Two simple and plausible constraints are that: (i) A and CB be jointly sufficient for the consequent of the law, and (ii) the relevant completer also occur in the antecedents of other laws-in other words, that there be many other law-like generalisations of the form (D -> CB ? E), (F -> CB ? E), etc. Apparent counterexamples to this proposal can be disarmed by interpreting the epistemology of cp-laws as a curve-fitting problem, which consists in determining the relevant nomic regularity and plotting the correct curve over a very noisy data-set that contains large numbers of outliers and anomalies. The process of specifiying the content of the ceteris paribus-clause that is hedging a law-candidate is in fact isomorphic with the process of determining which parts of one's data are outlying and anomalous, and which are part of the regularity. I submit that statistical theorems such as the Akaike Information Criterion (AIC) are instrumental in the latter process, and therefore also in the former. AIC states that a law-hypothesis which minimizes both the number of adjustable parameters and error variance (i.e. a hypothesis that achieves an optimal balance between simplicity and adequacy to the data), displays the highest estimated accuracy of prediction of future data from the same distribution. I go on to discuss how AIC in combination with conditions (i) and (ii) illustrates the fundamental difference between a ceteris paribus-law and a statistical law, and how it yields the distinction between spurious and genuine hedged regularities that is necessary to make cp-laws “respectable”. Thus, I show how popular putative problem cases, such as “aspirin cures headaches”, “ravens are black”, and “turtles live long lives”, can be dealt with by the theory. Finally, I utilise work by Lange (2000, 2002) to deflect the criticism that cp-laws are, by their very nature incompletable, and hence indeterminate. I close by briefly comparing my theory with other accounts currently on offer, and conclude that it provides a very simple, powerful, and yet metaphysically conservative account of ceteris paribus-laws.

Ulrich Krohs (University of Hambrug)

Epistemic Consequences of two Different Strategies for Decomposing Biological Networks

It is the mission of systems biology to explain the structure and dynamics of large biological networks, paradigmatically on the cellular scale. Explanations are given in terms of models of the network. I will inquire the epistemic implications of alternative modeling strategies which are regularly applied, concentrating on two different kinds of so-called top-down modeling.

Top-down modeling starts from data on the dynamics of the system as a whole and aims at giving a simplified theoretical account of this dynamics. Though there are also plans for a 1:1 mapping, I concentrate on the standard approach and defend that only simplified models can count as being directly explanatory (in a sense to be specified). As a basic step of modeling, biologists decompose networks into subsystems or modules, which are described as partly independent parts of the network. There are two modularization strategies, and these are the subject of my inquiry.

The first strategy uses criteria of functionality to identify modules, the second is based on a mathematical analysis of the network structure. Functional decomposition starts from capacities of the network and analyzes the functional contributions to these capacities. A module, then, is any part of the network which performs closely interrelated functions. This is the classical physiological way to decompose large systems and to model biochemical pathways. It finds its limitation where contributions to network capacities turn out to be distributed: realized by the interaction of a larger number of components, without any single type of component being crucial. Functional decomposition, then, seems to distort the picture and in some cases may even become unfeasible. Therefore, as an alternative, structure-based decomposition of networks receives increasing attention. Here, scientists look for differences in the number of interactions between components of the network. Modules are conceived as substructures of the system with high internal and low external interaction. Speaking with Plato, structural decomposition carves nature at its joints. This method is unbiased with respect to capacities and functions and counts as the more faithful way to decompose a network. The modules identified according to the different strategies need not coincide.

I will discuss the different explanatory goals that can be reached by each of the strategies. One result is particularly disturbing: If biologists aim at a functional understanding of living entities, they seem to be committed to the biased and somehow artificial functional decomposition of large networks, which seems to result in inferior explanations of what is going on physically as compared with structural decomposition. If they go for a better mechanistic understanding and base modeling on structural decomposition, the results seem to loose physiological relevance.

Theo Kuipers (University of Groningen)

Bridging the Gap Between Belief Revision and Truth Approximation

Belief revision, AGM-style (Hanson, 1999), is typically conceived of as aiming at coherence optimisation between a given set of beliefs and new information. From the truth approximation perspective in philosophy of science, the plausible question is how to revise a given theory in the light of increasing evidence such that it serves the purpose of truth approximation. In the qualitative, structuralist approach to truth approximation (Kuipers, 2000), the goal is more specifically the nomic truth. That is, given the set Mp of conceptual possibilities, determined by a vocabulary, the nomic truth amounts to the (unknown) subset T of physical or, more generally, nomic possibilities. A theory X amounts to a specified subset with the weak claim that it is a superset of T and the strong claim that it is equal to it. Evidence typically comes in by experimentally realising conceptual possibilities, say R(t) up to time t. They are, of course, nomic possibilities, hence, if we do not make mistakes, whatever T is, R(t) is a subset of T. Neglecting corrections, R(t) is an increasing set. It will grow in particular due to testing general hypotheses, each of them claiming that all nomic possibilities satisfy it. They may have been derived from the weak claim of theory X or of some other theory, or may have been put to test for other reasons. However this may be, at each point of time we may assume that one or more of them are considered to have been sufficiently established as empirical laws by inductive generalisation. Let subset S(t) of Mp represent at time t the resulting strongest, induced empirical law, which amounts to the claim that S(t) is the smallest induced superset of T, whatever T is. Assuming the basic definition of 'more truthlikeness' it is possible to prove a success theorem, which gives good reasons to abduce, under certain conditions and for the time being, that when theory Y is more successful than theory X, relative to R(t)/S(t), Y is more truthlike than X.

The belief revision approach now suggests the question how to revise a theory X in the light of R(t)/S(t) such that the resulting theory Y is more successful relative to R(t)/S(t) than X, and hence, by default, that it is more truthlike. In this paper, an attempt will be made to define appropriate operations of expansion, contraction and revision. It will be guided by Grove's (1988) semantic approach to belief revision, Niiniluoto's (1999) 'quantitative bridge', and some plausible qualitative conditions of adequacy, notably retaining the core idea of the theory.

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Martin Kusch (University of Cambridge)

Boghossian on Relativism and Constructivism – A Critique

I this paper I revisit the problem of relativism in the social sciences, focusing on some of the arguments in Paul Boghossian's recent work (in particular his book "Fear of Knowledge: Against Relativism and Constructivism" [Oxford University Press, 2006]). My main aim will be to determine whether Boghossian's arguments threaten the kind of constructivism and relativism advanced by various authors in the sociology of scientific knowledge (especially by Bloor and myself). In the first part of my paper, I seek to show that Boghossian misrepresents Bloor's views. In the second, main, part of the paper, I shall discuss Boghossian's main arguments and suggest that they fail to threaten the sociology of scientific knowledge.

Wang-Yen Lee (University of Cambridge)

The Probative Force and Dialectical Value of Structure-Oriented Second-Order Abductive Arguments for Scientific Realism

In his celebrated Inference to the Best Explanation (London: Routledge, 2004), Peter Lipton puts forward three brief arguments for scientific realism, which he claims to have probative force for the constructive empiricist. In contrast to the no-miracle argument for scientific realism which focuses on the result of scientific inference to the best explanation (or scientific abduction), these three arguments centre on the structure of scientific abduction. For this reason I call them structure-orientated arguments. In this paper I first challenge the claim that these arguments have probative force for constructive empiricists. I then argue that the arguments are nonetheless valuable for the purpose of increasing the confirmation of scientific realism for those who have already accepted scientific realism, particularly for former constructive empiricists who have recently 'converted' to scientific realism.

Below is a brief outline of my arguments for these two claims. To show that structure-orientated arguments have no probative force for constructive empiricists, I argue that each of these three arguments (i.e. the same-path-no-divide argument, the transfer-of-support argument, and the synergistic argument) is an implicit abductive argument which appeals to the truth-conducive simplicity principle (hence the name 'second-order abductive argument' – they are abductive arguments about first-order scientific abduction). Specifically, I shall show that Lipton's same-path-no-divide argument can quite easily be expressed as an abductive argument from analogy which appeals to the truth-conducive simplicity principle. Though the abductive structure of the transfer-of-support argument and the synergistic argument is much less clear, I contend that each of them implicitly appeals to a crucial premise in the same-path-no-divide argument, namely, the claim that there is no principled epistemic distinction between abduction about observables and abduction about unobservables. This is the premise that makes all three of them abductive arguments from analogy. The fact that these arguments for realism employ a rule of inference unacceptable to constructivist empiricists (i.e. they are rule-circular arguments) shows that they have no probative force for constructive empiricists. However, they may still be worthwhile for the purpose of increasing the confirmation of scientific realism for those who already accept scientific realism in virtue of acceptance of the simplicity principle. But even this suggestion is challenged by the fact, which I shall show in the paper, that these arguments are premise-circular arguments: each of these arguments has at least a premise that can only be justifiably/warrantably believed when one has already known the conclusion. If a premise-circular argument has no dialectical value for the increase of confirmation of the conclusion of that argument for those who already accept it, then the structure-orientated arguments will not have this dialectical value. I shall contend that whilst no premise-circular argument has probative force for those who reject the conclusion of that argument, only a certain type of premise-circular arguments have no force for the increase of confirmation for those who have already accepted the conclusion. I then argue that the structure-orientated arguments belong to a kind of premise-circular arguments which have force for the increase of confirmation for these people. Therefore, these arguments for scientific realism still have dialectical value.

Dennis Lehmkuhl (University of Oxford)

Geometrization(s) of Matter

The basic categories postulated by physics are space and time (or rather spacetime) on the one hand, and matter on the other hand. However, the relationship between the two categories is still controversial. There are essentially three families of positions one can take towards this relationship:

(i) substantivalism (roughly: both spacetime and matter are fundamental in their own right);

(ii) relationalism (roughly: just matter is fundamental and spacetime is in some way reducible to matter);

(iii) super-substantivalism (roughly: just spacetime is fundamental and matter is in some way reducible to spacetime).

The first two families of positions have been discussed in great detail in the modern philosophical literature, whereas the third has received almost no attention. However, there are research programmes in physics that directly correspond to this philosophical idea, and my aim is to (i) elaborate the philosophical idea of super-substantivalism; (ii) analyze and categorize various theories within physics that correspond to this idea; (iii) discuss possible advantages of such positions.

I will start out by investigating in how far super-substantival ideas are already compatible with standard general relativity theory (GR), i.e. in how far matter can be seen as an aspect of spacetime geometry even if no other fields than the gravitational one are associated with the geometry of spacetime.

I will then review John Wheeler's `geometrodynamics' programme, which in its original version incorporates super-substantival ideas in a very direct manner. I will compare this programme (and the attempts to find a quantum version of it) to some very recent developments in quantum gravity research and on this basis develop a first categorization of super-substantival research programmes.

Aki Lehtinen (University of Helsinki)

Farewell to Arrow’s Theorem

The normative and descriptive relevance of preference intensities, and the normative validity of Arrow's Independence of Irrelevant Alternatives (IIA) have been debated for decades in social choice theory. It has been argued since the very beginning that IIA does not take preference intensities into account.

Donald Saari has recently argued that Arrow’s IIA is not normatively acceptable because voting rules that satisfy this condition fail to respect the rationality of the voters. Saari's proposal is to replace IIA with a condition called binary intensity IIA. It requires that the relative ranking of each pair of alternatives is to be determined by each voter's relative ranking of that pair, and the intensity of this ranking, as determined by how many other candidates are ranked between them. Those who have not been willing to abandon IIA tend to emphasise the close link between strategic voting and IIA.

Aki Lehtinen's computer simulations show that utilitarian efficiency (the frequency with which the alternative with the highest sum of utility is selected) is higher if the voters engage in strategic behaviour than if they always vote sincerely. Strategic voting is thus unambiguously beneficial under a utilitarian evaluation of outcomes. What has been considered the main argument for IIA thus turns out to be one against it. These results show that the intensity argument against IIA does not need to rest on mere intuition that intensity information is not taken into account. They show that if and when IIA is violated through strategic voting, the voting rule takes intensity information into account, and this has beneficial aggregate-level consequences.

In this paper, I will discuss two interrelated topics concerning IIA and preference intensities. First, Lehtinen's results, and the intensity arguments against IIA that were presented before Saari's contributions were based on a cardinal notion of preference intensity, whereas Saari's notion of the intensity level upon which the binary intensity IIA is based, is best characterised as an ordinal notion. I will show that IIA is also violated in amendment agendas. The importance of this example is to show that Saari's arguments concerning the transitivity of preferences and the intensity level should not be understood as providing support for the Borda rule and against the majority rule, even though they are convincing qua arguments against IIA.

Second, I will draw the methodological and philosophical implications of Lehtinen's results on strategic voting for the interpretation of Arrow's theorem and the Gibbard-Satterthwaite theorem. This is done by discussing the methodological and philosophical arguments concerning preference intensities and IIA. These include the idea that it is possible to observe preference orderings, but not preference intensities or interpersonal comparisons of utilities, and the idea that von Neumann-Morgenstern (vNM) utilities should not be used in social welfare judgements because they reflect individual's attitudes towards risk. I conclude by arguing that the importance of Arrow’s theorem has been heavily overemphasised because the crucial IIA condition is not normatively acceptable.

Johannes Lenhard (Bielefeld University)

The Platform Concept of Simulation Modelling

Computer simulation has recently attracted attention in philosophy of science. One of the core epistemological problems related to the validity of simulations. Why do they give reliable results (if they do) and, directing the question more toward scientific practice, why are they thought to give reliable results? In general, the philosophical discussion of these questions rests upon the assumption that different simulation techniques provide cases different in kind and that philosophical analysis has to deal with them separately.

In opposition to this view, I want to defend the claim that an important point in the epistemology of simulation applies to a broad range of simulation techniques: Typically two levels can be distinguished in simulation modelling; I like to call them (i) platform and (ii) specification. The platform embraces the structure of the model, but doesn’t fix model output – think of a finite difference model with unassigned parameters to compensate discretization effects, or consider a cellular automaton where the interaction strength between cells is variable. Depending on further adjustments of variables (parameters), a platform can be adapted to a broad range of output behaviours, i.e. model dynamics can be said to be structurally underdetermined on this level.

The modelling process involves an experiment-like activity on this platform to calibrate overall simulation output. Only by specification of variables (parameters) can the output patterns of the simulation be determined. Hence the reliability of simulations hinges – to an important degree – on the plasticity of model behaviour. That means a given platform can be calibrated and adapted to certain patterns during the specification part of the modelling process.

The paper will analyze some cases to support my argument. First, the scope of the argument will be restricted to neural networks. There a very generic architecture of nodes and synapses (platform) can show a very general behaviour, i.e. generate a large class of output patterns. Which patterns are actually produced depends nearly entirely on the weights assigned to the connections in the network (specification).

In the sequel I shall enlarge the scope of the claim by investigating other types of simulations. Examples from meteorology, based on computational fluid dynamics and finite difference methods, and from astronomy, based on cellular automata, will be discussed. These cases both allow us to distinguish platform and specification. In these cases, the platform rest on a

strong theoretical basis (e.g. fluid dynamics) while the specification features not so prominently as in the neural network case. I maintain, however, that this is a difference in degree, not in kind.
At the end I will discuss whether platforms conserve features commonly related to mathematical modelling. The specification, on the other side, uses much quasi-empirical fitting, exploration, and experiment which thus seemingly add features of epistemic opacity to simulation modelling.

Sabina Leonelli (London School of Economics)

Can We Have Knowledge Integration without Theoretical Unification? The Travel of Data in Model Organism Biology

Understanding organisms as complex wholes has long been a cherished aim for both biologists and philosophers of biology. Current 'system biology' is only the latest attempt to integrate the massive, yet highly disunified, body of available knowledge about organisms. The achievement of such integration seems, however, to remain elusive. An analysis of the strategies hitherto employed by biologists to obtain integration provides rich terrain for an epistemological assessment of how scientific knowledge travels across different fields and is integrated for specific research purposes. This paper spells out some of the philosophical implications of the successes and failures experienced by scientists in their quest to integrate knowledge about organisms.

In particular, I consider the efforts to build databases incorporating data pertaining to various aspects of organism biology. In that context, theoretical unification has proved neither the only, nor the most fruitful, means towards obtaining integration in biology. The unification of existing theories remains an important component of any integration project: this is well demonstrated by the current popularity of networks of concepts (called 'bio-ontologies') as ways to organise and share existing biological knowledge. However, theoretical unification has turned out to be very problematic in biology, where knowledge is mainly expressed through data, models, experimental know-how and standardised specimens of specific model organisms, rather than formal theories. The subsumption of the existing forms of knowledge under a common theoretical framework is likely to result in massive loss of information, as convincingly argued by, among others, John Dupre and John Beatty.

I propose an alternative to the focus on theoretical unification: a different, but equally powerful form of integration is obtained through the use of data that have been gathered by biologists with different theoretical commitments as evidence towards general claims about organisms. As pointed out by Bogen and Woodward (1988), the circulation of data across scientific communities can be relatively theory-free: in other words, the theoretical contexts in which data are used need not be the same as the context in which they are produced. Indeed, biologists have long realised that finding ways to share and distribute data is more relevant to the goal of integration than finding common theoretical frameworks under which those data can be subsumed and interpreted. Depending on the research context in which they are adopted, data can serve as evidence for a variety of claims: underdetermination can thus be exploited by biologists wishing to expand their understanding of organisms.

Bert Leuridan (Ghent University)

The Need for Causal Modellling in Philosophy of Science

During the past decades, the formal treatment of causation and causal reasoning has changed drastically. By combining probability and graph theory, Judea Pearl (2000), Spirtes et al. (2000), developed interesting algorithms for causal discovery. Likewise, James Woodward (2003), Daniel Hausman (1998), and others reconsidered the concept of causation, bringing it in line with and describing its meaning by means of causal graphs.

In my paper, I will use causal graphs as a formal framework to abstractly describe the causal content of an entire scientific theory, viz. classical genetics, and its evolution through several stages. From 1865, the year Gregor Mendel firstly presented his theory, till the formulation of the theory of the gene by Thomas H. Morgan (1926), the theory of heredity underwent dramatic changes (cf. Darden, 1991).

Using the framework of causal modelling (note 1) will allow me to describe many different aspects classical genetics in an enlightening and unifying way. In line with the achievements of the semantic account of scientific theories (cf. Balzer and Lorenzano, 2000, for a semantic description of classical genetics), I will pay much attention to the structure of scientific theories. By explicitly focussing on their causal structure, however, my approach will improve on existing desciptions of the history of classical genetics. More specifically, causal models provide an excellent tool to deal with the following concepts from philosophy of science: theory dynamics, the logic of experimentation and the logic of explanation.

First I will show that the causal structure of the subsequent stages of classical genetics was essentially identical. All genuine differences can be described as differences regarding the random variables (more specifically their possible values) represented by the nodes and regarding the relations between these variables (i.e. the probability distribution over these random variables, conditional on the values of their causal parents). These differences were brought about by novel insights from other fields (e.g. cell biology) as well as by new ‘experimental’ explananda.

Then I will show that Gregor Mendel’s explanation of the phenotypic regularities he had experimentally obtained, was both probabilistic and causal-mechanical and I will elucidate the relation between these phenotypic explananda and the explanatory mechanism.

Finally I will show how the unobservable character of large part of this causal mechanism constrained the possibility of performing direct experiments in genetics. In fact, the experiments of Mendel and his successors should be considered as intermediate between experiments and prospective designs, rather than as genuine experiments (cf. Woodward 2003b for a discussion of experiments in terms of causal models).

Note 1: In my paper I will use ‘causal graph’ and ‘causal model’ interchangeably. A causal model consists of a causal structure (consisting of nodes and directed edges) together with a probability distribution over the random variables represented by these nodes. Examples of such variables comprise parental phenotype, maternal phenotypes, filial genotype, etc. Note that I’m using these expressions anachronistically, here. This shortcoming will be remedied in my paper.

Holger Lyre (University of Bonn)

Structural Realism: Intermediate View and Laws of Nature

This talk has two parts: In the first part I will argue for a more refined intermediate position between epistemic and ontic Structural Realism (SR). While many proponents of SR construe their position as the idea that relata are all and only constituted on the basis of the relations in which they stand, my argument will be that this view is not in accordance with the role of symmetry structures in modern physics and the corresponding properties. Instead, I will make the point that, on the fundamental level, entities are to be constituted both by relational and structurally derived intrinsic properties. This is all the more so true for symmetry structures where the corresponding symmetry transformations do not possess any real instantiations (most prominently: the case of gauge symmetries). The question will be addressed whether the distinction between epistemic and ontic SR can usefully be applied regarding such an intermediate view.

In the second part of the talk I will deal with the seldom considered question whether SR is committed to a realism about laws of nature. While at first glance this seems to be the case I will defend the view that SR is still in accordance with a regularist conception, although of a special kind. My arguments will be that, first, the structurally derived intrinsic properties shouldn’t be considered as essential properties (while, nevertheless, they are close to universals) and that, second, SR doesn’t lead to any necessity relations. The structuralist view of laws I will present in a sense supplements the Mill-Ramsey-Lewis systems approach, since the models that fit best in fundamental science correspond to really existing structures. It might also support a deeper understanding in which sense the fundamental laws provide exceptionless regularities. Here again, SR seems to lead to an intermediate position between two traditional extremes.

Geerdt Magiels (Free University of Brussels) and Gustaaf Cornelis (Free University of Brussels)

Dr. Jan Ingen Housz, the Forgotten Discoverer of Photosynthesis

Who discovered photosynthesis? Not many people know. Jan Ingen Housz's name has been forgotten, his life and works have disappeared in the mists of time. The tale of his scientific endeavour shows science in action. Not only does it open up an undisclosed chapter of the history of science, it also shines some light on the processes, phenomena and relationships in the development of science.

Dr. Jan Ingen Housz

Jan Ingen Housz was born on 8 December 1730 in Breda (NL) and died in 1799 in Bowood House, Wiltshire (UK) after a life of travelling between Vienna, London, Paris and Milan. He was a medical doctor with a broad scientific interest. He was a close friend of John Pringle, prominent scientist, Royal Physician and president of the Royal Society. Ingen Housz was appointed as Emperial Physician by Maria Theresia in Vienna after successfully inoculating her family against smallpox. He befriended people such as Joseph Priestley and Benjamin Franklin. He kept close contacts with important scientists of his time, such as Lavoisier, Van Swieten and Senebier.

The discovery of photosynthesis

Priestley discovered oxygen in 1774, although he didn't call it as such and probably did not really understand what he discovered. It would be Lavoisier who would later give this gas its name and a place in modern chemistry. At that pivotal point in chemical history, where oxygen was coming to replace phlogiston, Ingen Housz performed in the summer of 1770 some 500 experiments on plants and wrote down his conclusions in Experiments upon vegetables, discovering their great power of purifying the common air in the sun-shine, and of injuring it in the shade and at night. From this publication and the subsequent articles and correspondence, it is clear that he was the first to describe and understand the process of photosynthesis. It is the most important chemical process on earth, as the central reaction that makes animal life possible, something which Ingen Housz made abundantly clear.

Old story, new perspective.

This underresearched case of scientific enquiry is representative for what science as a method for acquiring trustworthy knowledge can do. Ingen Housz was a typical exponent of the Enlightenment, trying to contribute to a better society. His works offer a privileged and unknown starting point for a philosophical enquiry into the history of biology as well as the dynamics of science in general, based on as yet unstudied letters and documents and a reconstruction of his experimental method.

It is also a attempt to an 'ecological' approach to the philosophy of science. Science studied as an ecosystem: individual organisms (scientist are people of flesh and blood), groups of animals and plants (scientist seem always to operate in groups), their environment (society as the culture on which scientific knowledge grows and by which it is limited at the same time) and the flows of energy and information that link all components together and define their interactions and dynamic equilibrium (the interactions between all these factors in the multidimensional 'game' of science).

Uskali Maki (University of Helsinki)

Models and the Locus of their Truth

If models can be true, where is their truth located? Ronald Giere (eg 1988) has suggested accounts of theoretical models on which models themselves are not truth-valued. Truth-values can at most be ascribed to what he calls theoretical hypotheses. My paper seeks to improve upon this account by building on what I see as its correct intuitions and rejecting what I take to be its mistaken presumptions. It offers an elaboration on earlier papers (1992, 1994, 2001, 2005, 2006) that suggest relocating truth in models. The strategy of the paper is to ask, if I want to get truth inside models, how do I get it, what else do I need to accept and reject? The case used as an illustration is the world's first economic model, that of J.H. von Thünen (1826/1842) concerned with agricultural land use in the highly idealized "isolated state".

On Giere's account, models, such as the linear oscillator, are 'abstract entities' that are non-linguistic and in his view for this very reason not truth-valued. Descriptions of models are sets of linguistic statements, or assumptions, that characterize or define those models, and they are therefore necessarily – but trivially - true about the models. Factual truth is located in theoretical hypotheses that are statements about (respects and degrees of) similarity between abstract entities and real systems. Hence the relevant truth bearers are theoretical hypotheses, while the truth makers are matters of similarity. I take Giere's account as my prime target for revision and elaboration.

Here is an intuition that I will accept as right: A model itself is unable to identify the intended truth bearers when using the model to talk about the world. A further 'commentary' of the model is needed. And here is a presumption I will question: Nothing in a model is a serious truth bearer. I consider the possibility that some limited parts of models, identified by the commentary, can serve as truth bearers for which there may be truth makers in the world.

Unlike Giere, I am prepared to consider the possibility of truth bearers that are not linguistic, perhaps not even propositional. Thus, negatively, it is not necessarily an obstacle to models being true that they be viewed as non-linguistic abstract entities or imagined systems. Positively, I consider the idea that the relation of similarity in this connection could be viewed truth as correspondence: to say that the imagined model system and the real system are similar is to say they correspond to one another. These two revisionistic claims are needed in order to be able to relocate truth inside models (rather than locating it just in statements about them). But they are not sufficient. We also need the idea of a commentary that is used to identify the precise truth bearers in model systems (and the respective truth makers in real systems). Due to the involvement of idealizations and simplifications, large parts of those model systems are not among the intended truth bearers (if those idealizations were treated as truth bearers, they would be false). Just as models isolate parts of real systems for closer inspection, model commentaries isolate those parts of models that are intended as bearing their truth as (respects and degrees of) similarity with real systems. The account that emerges with these revisions and amendments seems to fit with von Thünen's model and the way he himself conceived of it. But how important philosophical dogmas would one have to drop in order to accept these revisions and amendments?

Caterina Marchionni (Erasmus University Rotterdam) and Jack Vromen (Erasmus University Rotterdam)

Ultimate and Proximate Explanations of Cooperative Behaviour: Plurality or Integration?

The behavioral sciences are characterized by a multiplicity of forms of explanation. Mayr's famous distinction between ultimate and proximate explanations has been generally invoked to make sense of at least part of this plurality. Whereas evolutionary theorizing explains human behavior by appeal to evolutionary forces (such as, notably, natural selection) working in the past, proximate explanations explain by appeal to current cognitive and psychological mechanisms. Each is held to be a legitimate form of explanation, and to be indispensable for a full understanding of behavior. At the same time the belief in a single unified theory of human behavior has revived in recent years (e.g. Glimcher and Rustichini 2004; Gintis 2006), suggesting that evolutionary and proximate explanations can, and perhaps should, be integrated. In this paper we enter the debate by scrutinizing the relation between proximate explanations and ultimate explanations of human cooperative behavior. We show that there are different ways in which ultimate and proximate explanations complement each other, not all of which equally support plurality of explanations. First, in some cases ultimate explanations are thought to directly account for behavior (for example, Ken Binmore's game-theoretical account of reciprocity). Second, ultimate explanations are held to explain behavior only indirectly, that is, by explaining proximate mechanisms (for example, Robert Frank's account of emotions as commitment devices and evolutionary psychology). Finally, in still other cases, there is only one explanation that appeals to both proximate mechanisms and evolutionary forces to explain current proximate mechanisms, and thereby behavior (for example, dual inheritance co-evolution theory). These three kinds of complementarity correspond to distinct ways in which different approaches (both within and across disciplines) can relate to each other in providing understanding of cooperative behavior: in particular, the approaches tend be increasingly integrated as the kind of complementarity gets stronger. If ultimate and proximate explanations both account for behavior but do so differently, then evolutionary and 'proximate' approaches can proceed without taking much notice of each other as long as their explanations are compatible. If ultimate explanations explain proximate causes, then ultimate explanation helps in identifying proximate causes ("from function to form") and, conversely, the detection of proximate causes provide an empirical test for ultimate causes ("from form to function"). In the third case, different approaches and fields are integrated to produce more complete explanations. The pursuit of more complete explanations is here driven by the recognition that ultimate explanations that completely disregard proximate causes, for example, might seriously distort the actual causal history. The current trend in the behavioral sciences is to opt for the second or third position, and hence seems to be a trend towards greater integration rather than plurality. The general lesson to be drawn is that finer-grain analyses of complementarities between actual scientific explanations are needed to illuminate kinds of inter-theoretical and inter-fields relationships, and the degree to which they support plurality vis-a-vis unity.

Jean-Pierre Marquis (University of Montréal)

Mathematical Forms and Forms of Mathematics: Homotopy Types

My goal in this talk is simple and straightforward: to explore some of the philosophical implications of the nature of homotopy types, what we know about them and how we know their properties. More specifically, knowing that homotopy types are fundamental mathematical objects, not only in topology, but in general for they can be used to define all the "classical" mathematical objects, e.g. numbers and, of course, spaces, I will argue that since they cannot be accommodated in a set-theoretical framework and for reasons that have nothing to do with size, they open the way to an intensional understanding of mathematical objects. Furthermore, it is not even clear that they can be understood as structures in the sense developed by contemporary philosophers of mathematics (which does not mean that they cannot be so understood). Thus, if I am correct, philosophers of mathematics ought to look carefully at these objects, since they might force us to revise some of our fundamental beliefs concerning the nature and our knowledge of mathematical objects.

Homotopy types are easily defined. Given two topological spaces X, Y, and two continuous functions f, g: $X \to Y$, a homotopy from f to g is a continuous map H: $X \times I \to Y$, where I denotes the unit interval [0, 1] with the usual topology, such that $H(x, 0) = f(x)$, $H(x, 1) = g(x)$. Informally, the map H is a continuous deformation of f into g. Two maps are said to be homotopic if there is a homotopy between them. Two spaces are said to be homotopy equivalent if there are maps f: $X \to Y$ and g: $Y \to X$ such that $g \bullet f$ is homotopic to 1_X and $f \bullet g$ is homotopic to 1_Y. For instance, in the standard Euclidean plane, a disk is homotopically equivalent to a point. A homotopy type is an equivalence class of homotopically equivalent spaces.

Thus, we have a basic equivalence relation in which the equivalence of mathematical objects does not rely on any set-theoretic bijection. Two spaces of radically different cardinalities can be homotopy equivalent, a situation rarely encountered with other types of structures. We are thus, right from the start, leaving a purely extensional framework and entering a realm in which intesional considerations are intrinsic. But what are we left with? This is what we intend to explore more carefully in this talk.

Erika Mattila (London School of Economics)

Explanatory and Predictive Functions of Simulations

Simulations may hide the initial model assumptions upon which they are built. This, hence, reduces their capability to produce reliable explanations and predictions of the phenomena of interest. To overcome this problem, we need to analyse the initial explanatory and predictive questions that a simulation was built to address. In other words, by studying a “what if”-type of question enables us to see how the capability of a simulation to project possible future conditions of a phenomenon also requires explanatory answers to why, what and how questions. This means tracing back the set of sub-models that were built to address these questions and that simultaneously resulted in accumulation of knowledge beneficial for the efforts of answering the “what if”-question.

My argument is that “what if”-questions, when studied in simulations, are not only explaining the phenomena, but also integrating the aspiration of predicting future behaviour of the phenomena. A simulation in this case is an agent-based model used to examine the transmission of an infectious disease in a population via individual contacts (considering also the age-structure of contact sites and including the information of the administered vaccination programmes). The model incorporates mechanisms of disease transmission (e.g. SIS-pattern) or herd immunity. Following Bechtel and Abrahamsen (2005), explaining a phenomenon involves describing the mechanism responsible for it, and furthermore, resulting in building a model to specify the key parts and operations of the phenomenon. Mechanisms, hence, are sought in order to explain how a phenomenon comes about or how a significant process works (Machamer, Darden and Craver 2000). In the model under scrutiny, the mechanisms are detected to explain, for example, why certain timing of booster vaccinations increases herd immunity to protective level. Furthermore, we are able to detect a set of questions addressing the how, what and why of the phenomena and resulting as a set of explanations of the specificities of its behavioural patterns. These questions can also be classified as manipulative questions, which is in line with Woodward’s (2003) argument that explanatory relations in principle support interventions. These interventions or idealised experiments are used in order to understand possible developments in the behavioural patterns of the phenomena. These explanations are incorporated in the simulation by answering a broader, predictive “what if”-question. Hence, the simulation is not only explaining the phenomena, but predicting how it would behave when the initial conditions are changed (e.g. herd immunity levels decrease, the population is exposed to external, cross-reactive bacteria, or vaccination schedules are changed). Therefore, we may conclude that analysis of explanatory and predictive functions of simulations by defining the initial questions facilitate uncovering the hidden model assumptions.

Cornelis Menke (Bielefeld University)

On the Explanation of Predictive Success due to Chance

I consider the question whether or not predictive success --the successful prediction of novel phenomena-- can be attributed to chance.

Predictive success plays an important role in several discussions within the philosophy of science, especially the debate on scientific realism and the miracle argument and the debate about whether there is a specific epistemic value of predictions as opposed to accommodations.

In the case of successful predictions of novel phenomena, the agreement between theoretical prediction and experimental findings cannot be explained by an accommodation between the theory and the facts. But there is still the possibility of an accidental agreement: the fulfilment of the prediction may be a fluke. Insofar as predictive success actually is due to chance, it cannot have any epistemic weight and so it cannot play the role it is meant to play in any of the three contexts mentioned above.

An especially forceful variant of the chance explanation of predictive success refers to the vast number of predictively unsuccessful scientific theories. Most of the theories either does not make any successful predictions of novel phenomena or make no predictions at all. These unsuccessful theories were abandoned by the scientists and so (wrongly) neglected by methodologists; taking them into consideration, it seems less astonishing that some theories are successful: this is just what one should expect due to chance.

While the proponents of the chance explanation are right in treating the problem as an empirical question, it is hardly possible to decide the question whether or not predictive successes are in general explainable by chance in this way: for neither do we know the number of unsuccessful theories, nor do we know which fraction of successful to unsuccessful theories is explainable by chance.

I shall argue that it is possible to overcome these shortcomings by considering the distribution of predictive successes among the theories. If predictive successes were due to chance, one should expect that there are more theories with only a few successes than theories with lots of successful predictions. But the contrary is the case: predictive successes are statistically correlated: if a theory makes one successful prediction, this makes it more probable that it makes a second one. This feature is hardly reconcilable with chance.

To consider the distribution has some advantages over the usual way of testing forms of predictivism, namely by considering whether successful theories were afterwards abandoned. Firstly, it is independent of the theory choice of scientists. If scientists use predictive success as a criterion of theory choice, then on should expect a dependency of predictive successes on theory abandonment. Secondly, the distribution is independent of the question how strict the notion of `novelty' is defined, i.e., whether it is meant to include only the most remarkable or a wider class of predictions.

John Michael (University of Vienna)

Simulation as an Epistemic Tool between Theory and Practice

In this paper I investigate the concept of simulation that is employed by so-called simulation theory within the debate about the nature and scientific status of folk psychology. According to simulation theory, folk psychology is not a sort of theory that postulates theoretical entities (mental states and processes) and general laws, but a practice in which we put ourselves into others' shoes and simulate their situation from our own perspective. On the basis of this sort of simulation, we supposedly know how we would act or think or feel, and then expect the same of others. A closer look at the concept of simulation reveals two problems with this view, but also helps to clarify the insight motivating simulation theory.

The first problem is that we need some sort of theoretical knowledge in order to set the parameters of a simulation. In folk psychology, we need to know what aspects of a situation are relevant with respect to the interests of the person being simulated, and we need to know in what respects the person is similar and/or different from us in order to evaluate our simulation. It is not clear how simulation theory can integrate this sort of theoretical knowledge while still maintaining its position as an alternative to the theoretical construal of folk psychology. Secondly, it is unclear whether the claim that we perform simulations of others' situations is to be interpreted as a realistic or a metaphorical description of the psychological processes involved in folk psychology. If is taken realistically, it is important to formulate criteria according to which a simulation can be said to take place. If not, then it is important to ascertain in what respect the psychology processes involved in folk psychology are similar to simulations.

Addressing these problems will require us to clarify the central idea of simulation theory, namely that our own first-person experiences provide us with an epistemic tool for interpreting other people. I suggest that we can do so by assessing the conditions under which simulations not only employ knowledge in order demonstrate models but can be productive of new knowledge. This could indeed be the case insofar as techniques developed by means of theoretical knowledge can produce unexpected results that, in turn, suggest new theoretical knowledge. If this distinction between theoretical prerequisites and unpredictable practical operation could be mapped onto the distinction between knowing that and knowing how, then simulations in folk psychology could be identified a source of knowledge that is based upon abilities to act and involves but is not limited to propositional knowledge. It is an open question whether folk psychology and scientific psychology differ in the way in which they involved an integration of simulation and theory.

Alessio Moneta (Max Planck Institute of Economics)

Can Graphical Causal Inference Be Extended to Nonlinear Models? An Assessment of Nonparametric Independence Tests

Graphical methods for causal inference are usually based on conditional independence tests among random variables. Such tests are indeed used to constrain the types of causal relationships that exist among the model variables, via some general assumptions on the relation between stochastic and causal independence, such as the Causal Markov Conditon and the Faithfulness Condition (Spirtes et al. 2000; Pearl 2000). When the data are continuous, to make sure that conditional independence tests are available and to allow causal inference directly from vanishing partial correlations, the researcher often assumes that the underlying model is linear. This may constitute a serious limitation, especially in several contexts of social science in which background knowledge does not permit the specify the functional form of the model, or if background knowledge is in fact at odds with linearity.

In this paper we investigate the possibility of inferring causal structures for models of continuous variables in which the functional form of the equations and the probability distributions of the disturbances remain unspecified. A first step in this direction, we claim, consists in finding what is the appropriate test for the type of continuous data set (e.g. time series or cross section, with high or low number of variables, small or large sample size) available to the researcher. We focus on tests that exploit the fact that if X and Y are conditional independent given a set of variables Z, then the distance between the two conditional densities f(Y|X, Z) and f(Y|X) must be equal to zero.

The idea is to estimate via nonparametric techniques (kernel methods) the conditional densities f(Y|X, Z) and f(Y|X) and to test if some metric expressing the distance between them is significantly close to zero. There are several metrics available in the literature (e.g. Euclidian distance, Hellinger distance, L(q) norm, entropy, etc.) to express such distance. We investigate through Monte Carlo studies how these metric-based tests perform as input of graphical algorithms for causal inference. We also study how they perform when certain conditions (i.e. autocorrelations in the data, number of variables, and sample size) vary. Moreover, we compare how nonparametric conditional independence test perform vis-a-vis parametric tests that assume the linear functional form.

We will also draw some implications for the philosophical debate on causality in complex systems (see e.g. Wagner 1999). Exploring under which condition nonparametric tools are able to delineate causal interactions can indeed shed new light on some crucial philosophical questions, like: can a meaningful notion of causality defined for systems with nonlinear interactions among the variables? Is, in nonlinear frameworks, still statistical regularity the notion on which causality has to be based?

Matteo Morganti (London School of Economics/IHPST)

Individual Particles, Properties and Quantum Statistics

Those who aim to defend the idea that quantum particles are individuals must cope with the difficulty represented by the 'non-classicality' of quantum statistics. For quantum many-particle systems, only (anti-)symmetric states are allowed, while non-symmetric ones (analogous to 'coin 1 heads and coin 2 tails'-like classical states) are not. This appears to be readily explained if one assumes that the statistics describes systems of non-individuals, as in the latter case there could not be any reference to specific particles in the definition of the state, as particles simply do not have well-defined identities.

It is commonly claimed that all one can say from an individual-based perspective is that non-symmetric states are inaccessible to particles but the latter are, nevertheless, individuals. However, this appears ad hoc, and gives rise to a problem with so-called 'surplus structure': non-symmetric states are meaningful given the theory, and yet they are never realized. This paper suggests an ontological interpretation of quantum systems able to provide a justification for the state-accessibility restrictions, so avoiding ad hocness and the problem of surplus structure.

The suggestion is that, for all many-particle systems and state-dependent properties, particle exchanges do not 'make a difference' because the systems exhibit emergent relations. As a consequence, the specific identities of the particles are not relevant for the determination of the available arrangements not because – since particles are not individuals – there are no such identities; but, rather, because the latter exist but do not enter in the determination of the properties being described.

This requires one to i) understand entangled systems along the lines of Teller's relational holism (entanglement amounts to the existence of emergent relations holding between individual particles that do not possess the property in question as a monadic intrinsic property); and then ii) generalize this to all many-particle systems. Non-symmetric states are then automatically excluded.

This entails that we can attribute separate properties to each one of the particles in (anti)symmetric states such as, for instance, 'particle 1 spin up & particle 2 spin up' only because we know that upon measurement both particle 1 and particle 2 will have spin up; but in fact, such an ontic understanding of the Eigenstate-Eigenvalue Link is wrong. More specifically, it follows from the ontological suggestion being made that the Eigenstate-Eigenvalue Link only applies to the total system, and inferences such as [Prob(particle 1 has spin up)=1] entails [(particle 1 actually has spin up)] are in fact not warranted, as each separate particle possesses the property under consideration only after measurement. This modification to the link - which is at any rate not an integral part of quantum theory – appears acceptable, especially once one notices that, although essential from the perspective of ontological interpretation, such a modification does not make any difference in practice.

F.A. Muller (Erasmus University Rotterdam/Utrecht University)

The Concept of Structure

Structural Realism is a variety of realism that aims to stay out of reach of the pessimistic meta-induction yet to mobilise some version of the no-miracles argument.
John Worrall is usually credited for having put this position on the map in the early 1990ies. A small but passionate group of philosophers have developed this view further, notably S. French and J. Ladyman.

In order for Structural Realism (perhaps better: Structure-Realism, because it is realism about structures) to be a variety of realism distinct from its realist siblings, it needs a clear concept of structure. If set-theory is chosen (the default option), some substantial representation theory is needed in order to say which sets of the set-structure are supposed to be up for a realist interpretation. Mutatis mutandis when category-theory is chosen. We explore the construction of a third possibility: to create a new theory of structure. Our approach will be formal. Reason: if we can't get the formal details right, we shall not get anything right.

An elementary formal language and some axioms are proposed.

Samir Okasha (Bristol University)

On the Significance of R. A. Fisher's Fundamental Theorem of Natural Selection

This paper discusses the biological and philosophical significance of Fisher's famous 'fundamental theorem' of natural selection. The ongoing controversy over the correct interpretation of the theorem is also briefly examined. I argue that the so-called 'modern interpretation' of the theorem, originally advocated by G.R. Price and recently defended by a number of evolutionary theorists, provides the correct account of what Fisher meant by his theorem, and resolves the purely mathematical aspects of the controversy. However, the modern interpretation does not resolve the question of the theorem's significance. Fisher himself believed that the theorem expresses a deep truth about the Darwinian process, but many other biologists have dispute this. I argue that in the light of the modern interpretation, there is a sense in which Fisher is right, but it depends on our being prepared to accept his unusual notion of 'environmental change'. (On Fisher's view, any change in the average effects of the alleles in the population constitutes an environmental change.) I argue that this notion of environment ultimately stems from Fisher's adherence to what is today called the 'gene's eye' view of evolution.

Flavia Padovani (University of Geneva)

Topologies of Time in the 1920's: Reichenbach, Carnap, Lewin

In the early 1920s Hans Reichenbach, Rudolf Carnap and Kurt Lewin offered three different accounts of the topology of time. These accounts have never been compared, but are interestingly interrelated.

Both in his article "Bericht über eine Axiomatik der Einsteinschen Raum-Zeit-Lehre" (1921) and in his more famous work "Axiomatik der relativistischen Raum-Zeit-Lehre" (1924), Reichenbach presented a remarkable distinction between light axioms and material axioms, a distinction that was intended to found and clarify the structural relations of the causal series of events of which reality is composed. Here, the topology of space follows as a result of the topology of time, and the concept of "spatially nearer than" is reduced to the concept of "temporally earlier than". The purpose of this work was to determine the properties of the type of order characterizing the causal series, namely time, and to consider the spatial order only after giving a definition of simultaneity for distant events.

In his "Über die Abhängigkeit der Eigenschaften des Raumes von denen der Zeit" (1925), Carnap proposed a space-time topology by means of the theory of relations, ultimately based on the two relations K and Z (where K stands for the "coincidence relation" and Z for the relation of "temporal antecedency on the same world line"). This article meant to show that the topological properties of space can be derived from those of time, when considering in particular spatial neighbourhood as a temporally short effectual connection (Wirkungsverknüpfung).

In 1923, in a paper entitled "Die zeitliche Geneseordnung", Lewin also presented an original description of time order, but in mereological terms, where the key concept is the concept of genetic series (Genesereihe) and its related notion of genidentity, or identity through time. This topology is restricted to the time order that is expressed between any two real events series in existential relationship with each other.

Common to all these points of view is the idea that time order can be shown to be founded on certain structural properties of the world, but this very same aim is pursued in diverse fashions. Moreover, the notion of genidentity appears to play a fundamental role in all three cases. This paper examines the analogies and the differences in these three versions of the topology of time and shows how, assigning genidentity a specific meaning, the three constructions reveal a different level of fundamentality one with respect to the other.

Daniel Parker (Virginia Tech)

Was There an Ice Cube There, or Am I Just Remembering It?: Reposing the Question of the Veracity of Memory

It is commonly thought that the statistical mechanical reversibility objection implies that our putative records of the past are far more likely to have arisen as spontaneous fluctuations from equilibrium states than through causal processes that correctly indicate past states of affairs. Hence, without some further assumption that solves the reversibility objection, we are led to the sceptical disaster that all our beliefs about the past are almost surely false. There are many such accounts available in the literature, including Albert's Past Hypothesis, but also suggestions due to Reichenbach and Horwich, for instance.

This paper disputes this claim and it is formally argued that, by and large, our records of the past can and should be thought to be veridical because the intentional contents of records are not included as part of their statistical mechanical description. Taking as a model a computer memory cell that occupies either the 1 or 0 bit position, I show that the representational contents of a memory cannot be included in a statistical mechanical description of that cell, since from a thermodynamic perspective there is no entropic difference between the physical description of 1 and 0 bits (though the representational content of the cell can vary depending on which bit position the cell occupies). As a result, one can formulate a probability distribution over the possible contents of such memory cells that is independent of any statistical mechanical considerations.

Conspicuously, the contents of our memories and records are well correlated with the present state of the world, insofar as the present macrostate of the world appears to be very much like how we would expect it to look like if our memories were veridical. The traditional formulations of the sceptical conundrum posed by the reversibility objection treat this as a problem that can only be solved by introducing some novel physical postulate, but it is not obvious that this is necessary. Rather, one can ask, given the apparent correlation between the contents of one's memories and the present observable state of the universe, whether such a correlation is more or less probable than both the memories and the present state of the world arising (with the spurious appearance of correlations) through spontaneous fluctuations.

Reposing the sceptical question in this way allows one to assess the reliability of one's records by methods developed by Bovens and Hartmann. In their book, 'Bayesian Epistemology', they develop methods that evaluate the reliability of an individual witness report when the event reported has a low prior probability but many independent witnesses corroborate the report. These results are applied to the sceptical problem posed by the reversibility objection, and it is argued that, given the present state of the actual world, it is far from obvious that the Past Hypothesis, or anything like it, is required to ground the belief in the veracity of our records.

Fabrice Pataut (IHPST)

Verifiability, Scientific Realism and Constructive Empiricism

The notion of verifiability, as it appears in twentieth-century analytic philosophy, stands at a crossroads. It pertains to formal semantics, the philosophy of language, philosophical logic, and the philosophy of science. The notion held the stage with the positivists' defence of the principle of verifiability. A powerful counterargument to the use of the principle has been proposed by Church, according to which, under the positivists' notion of verifiability, any statement is verifiable, either directly, or indirectly.

Van Fraasen has proposed a refined version of empiricism,called "constructive empiricsm", according to which theories must not be "verified" in a strict or narrow sense, but must "save (or fit) the phenomena".

The problem, then, is to determine to which extent empirical adequacy plays, for van Fraassen's constructive empiricist, the role which verifiability played for the positivists, and whether belief in empirical adequacy plays a role akin to that of belief in warrants obtained by humanly feasible verification, where implemented verification constitutes the privileged rational ground for belief in the statements of scientific theories.

It will be suggested that van Fraassen's theory runs into difficulties which are structurally similar to those detected by Church's counter-argument.

Johannes Persson (Lund University)

Mechanism-as-activity and the Threat of Polygenic Effects

Polygenic effects have more than one cause. They are examples of how several causal contributors are sometimes simultaneously involved in causation. The importance of polygenic causation was noticed early on by Mill, and has been demonstrated as a problem for causal law-approaches to causation and for accounts of causation in terms of capacities since then. It needs further discussion in the emerging literature on causal mechanisms. In this talk I examine whether an influential theory of mechanisms, proposed by Peter Machamer, Lindley Darden, and Carl Craver, can accommodate polygenic effects and other forms of causal interaction. They have a problem, I will argue, because of the central position attributed to activities in their theory. Not only is an activity needed to constitute a mechanism, but the activity also plays the causal role of that mechanism. Any such mechanism-as-activity is incompatible with causal situations where either no or only another kind of activity occurs. Both kinds of situation may be frequent.

Gabriella Pigozzi (University of Luxembourg)

Evaluating Social Decision Rules

The problem of the aggregation of consistent individual judgments on logically interconnected propositions into a collective judgment on the same propositions has recently drawn much attention in the decision theoretical literature. The difficulty lies in the fact that a seemingly reasonable aggregation procedure, such as propositionwise majority voting, cannot ensure an equally consistent collective outcome. The literature on judgment aggregation refers to such dilemmas as the discursive paradox. So far, three procedures have been proposed to overcome the paradox: the premise-based and conclusion-based procedures on the one hand, and the merging approach on the other hand. This raises the question of how these aggregation procedures can be evaluated. Which procedure is the best? The answer, we argue, depends on the purpose in question. We may, for example, want a procedure that avoids paradoxical outcomes, a procedure that tracks the truth, a procedure that maximizes the utility of the whole group, or we may also need an aggregation method that combines some of these goals.

In an earlier paper, Pigozzi and Hartmann (2006) assume that the decision that the group is trying to reach is factually right or wrong. They then analyze how good the merging approach is in tracking the truth, and how it compares with the premise-based and conclusion-based procedures.

In this paper, we assume that the acceptance or rejection of a proposition has an impact on the welfare distribution within the group. Extending Beisbart, Bovens and Hartmann's (2005) model for single propositions to several logically interconnected propositions, we evaluate the utility distributions that result from the application of the above-mentioned aggregation rules according to the following two criteria: (i) Utilitarianism: which rule leads to a maximization of the total utility of the group? (ii) Egalitarianism: which rule minimizes the variance between the individual utilities? Our results have implications for social epistemology and the study of the decision-making in a scientific community. (This abstract is based on joint work with Stephan Hartmann.)

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Tomasz Placek and Leszek Wronski (Jagiellonian University)

On the Infinite EPR-like Correlations

Muller, Belnap and Kishida (2006) have recently raised the question whether infinite EPR-like correlations which do not involve finite EPR-like correlations are possible. As for EPR-like correlations, we take its essence to consist in (1) an n-tuple of particles subjected to measurements at distant locations, with (2) the measurement events being space-like related and with (3) measurement results being perfectly correlated. Given that perfect correlation is interpreted pre-probabilistically, branching space-times (BST) theory of Belnap (1992) is an adequate tool to tackle the issue.

Muller et al's BST model has infinite EPR-like correlations without there being finite EPR-like correlations, yet the model has no affinity to any physically motivated space-time. Thus the question remains whether their model is a mere set-theoretical gizmo, or whether it can be translated into some other, physically motivated, model.

We first ask under what conditions in general BST there can be infinite EPR-like correlations that do not involve finite EPR-like correlations. We then find two postulates, each of which is sufficient to generate infinite EPR-like correlations, and which both must be false in a BST model for infinite EPR-like correlations not to occur in that model.

As physically motivated models we take Minkowskian branching structures (MBS), recently introduced in Wronski and Placek (2006). We investigate under what conditions each of the postulates can be true in Minkowskian branching structures, and under what conditions infinite EPR-like correlation might occur in MBS models. Somewhat surprisingly, we find MBS models in which each postulate is true, and which have infinite EPR-like correlations; the models have strange features, however.

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Sabine Plaud (University of Paris I)

On Photographs and Phonographs: The Influence of Some Technical Innovations on Ernst Mach's and Ludwig Wittgenstein's Conceptions of Pictures

Ernst Mach and Ludwig Wittgenstein had both received a scientific rather than a philosophical training and, in both cases, such a scientific training was of direct influence on their philosophical tenets. More precisely, their common interest in mechanics has led both of them to elaborate a theory of pictures intended as “models” of reality. In Mach's case, this “picture-theory” is rooted in his account of science itself since, in his opinion, scientific theories are nothing but theoretical pictures of the world. In Wittgenstein's case, the concept of picture as a model has its applications in the realm of the philosophy of language, where it is the key to the theory of propositions supported by the Tractatus logico-philosophicus:

> "The proposition is a picture of reality. The proposition is a model of reality as we think it is". (TLP, 4.01)

My claim will be that this common concern for pictures or models might have been elicited by two contemporary events in the history of sciences and techniques which, as stressed by Susan Sterrett in her article named “Pictures of Sounds: Wittgenstein on Gramophone Records and the Logic of Depiction” , offered an opportunity to reconsider the very idea of a “picture”. The first of these events is the publication, in 1887, of Mach’s photographs of shock-waves displaying the invisible waves propagated in the air when a projectile is shot . Such photographs were crucial to the elaboration of a schematic conception of pictures, for what turned them into genuine pictures of waves was not their similarity to their object, but rather their structural correspondence with it.

The second of these events is the invention of the gramophone by Emile Berliner in 1888, about ten years after Thomas Edison had introduced his 1877 phonograph. Such an invention highlighted the possibility not only of a visual but also of a sound picturing. In the Tractatus, Wittgenstein takes this as a clue in his own investigations of the nature of pictures. His strategy to determine what is a picture in general is consequently to examine what these “sound pictures” have in common with traditional or logical pictures:

> "A gramophone record, the musical idea, the written notes, and the sound waves, all stand to one another in the same internal relation of depicting that holds between language and the world. They are all constructed according to a same logical pattern" . (TLP, 4.014)

I will thus draw a parallel between these two references to technical devices in Ernst Mach and in Ludwig Wittgenstein, and I will compare their influence on the respective philosophy of pictures developed by these two authors. This will give me an opportunity to reflect upon the direct influence technical developments may have on philosophical insights.

Oliver Pooley (Oxford University)

Background Independence

This talk will explore the links between a number of interrelated concepts: general covariance, dynamical fields, absolute objects and background independence. I will discuss recent work by Belot, Earman, Giulini and Pitts.

Two views form part of textbook wisdom about general relativity (GR): (1) general covariance (or diffeomorphism invariance) is a requirement only on the formulation of a theory and not on its physical content; (2) GR differs from previous theories chiefly in its treatment of spacetime structure: in previous theories this structure is fixed, in GR it is dynamical. These days some people, especially quantum gravity specialists who work on alternatives to string theory, identify GR's background independence as its essentially novel element. This view is compatible with (1) and (2) if background independence is simply a matter of lacking non-dynamical spacetime structure. Unfortunately things seem not to be so simple. Background independence is routinely linked to GR's ('active') diffeomorphism invariance. A closely related view is often defended: that the nature of the observable content of GR differs fundamentally from that of previous theories (observables in the former, but not in the latter, are supposed to be diffeomorphism invariant).

I argue that (1) and (2) are correct. The novelty of GR is not to be made out in terms of any natural sense of general covariance or in terms of a fundamental difference between the observables in generally relativistic and non-generally relativistic theories. I claim that Anderson's programme of defining absolute objects also fails to pin down the novel element of GR. All of this is to make all the more pressing the questions: what is special about GR, and why is it so hard to quantize?

Demetris Portides (University of Cyprus)

Idealization and Abstraction in Scientific Modelling

It is widely accepted that scientific models represent their target systems albeit in idealized and abstract ways. The immediate question is how can their relation to phenomena be explicated? That is to say, what epistemological and methodological implications stem from the fact that our models offer descriptions of target physical systems which comprise of highly idealized concepts and from which a number of influencing factors are abstracted. Let me refer to this as the problem of idealization. The problem of idealization could be divided into two components. The first component concerns the clarification of what the relata are in the theory/experiment relation. In other words, where and how do we set the dividing line between a description that belongs to the side of theory and another that belongs to the side of experimental reports (if it can be clearly set)? In the literature on models we can discern two conceptions of the relata: the structuralist and the non-structuralist views. In the structuralist conception the theoretical description is clearly distinct from other conceptual ingredients that are necessary for relating model predictions to experiment, whereas in the non-structuralist conception the model predictions are the result of a complex amalgamation of theory together with conceptual ingredients deriving from auxiliaries, which within the representational device, i.e. the model, cannot be clearly distinguished. The second component of the problem of idealization concerns the way by which a theoretical description is brought closer to the features observed to be present in its target system. Improvements in the models' representational accuracy are generally understood as involving de-idealizations or concretizations. This is obviously related to the first component in the sense that how one addresses the issue of de-idealization depends upon her understanding of the relata in the theory/experiment relation. In the structuralist conception of the relata de-idealization can only be understood as a process that is used in order to reconstruct the data. If one, however, conceives the relata as in the non-structuralist view, then de-idealization must be understood as a process strongly tied to the quest for improving the representational capacity and accuracy of the model. I argue for a non-structuralist conception of the relata, and attempt to formulate a theory of de-idealization that explicates both theory-driven and phenomenological modelling.

Hernán Pringe (University of Pittsburgh)

Cassirer and Bohr on Intuitive and Symbolic Knowledge in Quantum Theory

In this paper I aim at comparing the epistemological function which Cassirer assigns to sensible intuition in quantum theory with that assigned by Bohr. I shall show that both agree on the impossibility of considering sensible representations as direct exhibitions in intuition of quantum objects and processes. In this sense, both consider our knowledge of the quantum realm to be symbolic. However, while for Cassirer this entails that spatio-temporal images play no substantial role in quantum theory, Bohr maintains that these images provide the mathematical formalism of the theory with reference to the physical world. Instead of celebrating an alleged abandonment of sensible representations, as Cassirer does, Bohr claims that spatio-temporal pictures should be retained in order to exhibit quantum objects and processes indirectly in intuition. In this way, while Cassirer states that it is only by completely renouncing sensible representations in quantum theory that may we gain systematicity in our physical knowledge, Bohr is able to account for the sensible content of quantum theory as well as for its systematic relation to classical physics.

In the following, I first reconstruct Cassirer's view on the role of sensible intuition in modern science. Then, I turn to Bohr's account of the epistemological function of classical pictures in quantum theory. Finally, I consider the problem of the systematic relationship between our knowledge of the classical and the quantum realm.

Hans Puehretmayer (University of Vienna)

Beyond Judgemental Relativism: Combining Feminist Standpoint Theories and Critical Realism

Several contemporary approaches in the philosophy of science (e.g. feminist standpoint epistemologies, critical realism, Foucault, Bourdieu, cultural studies, …) argue that there exists an internal connection between the production of scientific knowledge and the social and political positioning of scientists. It is, however, controversial, how this connection should be exactly conceptualized: whether content and form of scientific knowledge can be wholly explained by its social and cultural context; whether one can establish a 'pure', 'socially un-contaminated' core of scientific knowledge; or whether a relative autonomy of the scientific can be argued despite the social embeddedness of every scientific activity.

My paper will consist in a dialogical confrontation of feminist standpoint theories and critical realism. Feminist standpoint theories have done intensive theoretical reflection and empirical research on epistemological questions, while critical realists have reflected upon the implications of the ontological assumptions each theory necessarily makes.

Feminist standpoint accounts of scientific knowledge production are certainly richer and more sensitive to the scope and details of historical cases than those of critical realism. Critical realists, on the other hand, have provided a comprehensive theoretical critique of prevailing epistemological premises: they have developed a non-Humean conception of causality, elaborated a non-essentialist ontology, they differentiate between historical, sociological, epistemological (which they all embrace) and judgemental relativism (which they reject). In contrast to feminist standpoint theories the claim for historical and epistemological relativism (or better: relationism) is rather programmatical than elaborated. A further difference between critical realism and standpoint theories consists in the fact that the former refers rather to Bachelardian historical epistemology than to a (post-)Kuhnian history of science. Both -heterogenous- approaches have critizised empiricism in different ways.

Furthermore I will raise the question whether feminist standpoint theories tend to priviledge ethical and political issues as criteria for the justification of scientific knowledge over epistemological-scientific lines of argumentation, thereby marginalizing the critical-reflexive potential of scientific reasoning I will argue that a specific combination of feminist standpoint and critical realist theories can justify a non-relativist explanation of the outcomes of scientific knowledge production.

Panu Raatikainen (University of Helsinki)

Theories of Reference and the Philosophy of Science

The theses of meaning-variance and incommensurability of Kuhn and Feyerabend have consequences which are not easy to digest: that it does not make sense to say that a later scientific theory is more adequate, or is closer to the truth, than an earlier one, and that there is therefore no genuine progress in science. It is widely agreed that there must be something wrong with this view, but there is no general agreement on the best cure.

The ideas of Kuhn and Feyerabend derive from their assumption of the contextual theory of meaning. It has been sometimes suggested that the causal theory of reference, developed especially by Kripke, provides an alternative picture of meaning and reference which would avoid the unwelcome consequences of the meaning-variance thesis. This proposal has been welcomed by some (e.g. Boyd 1973, Kitcher 1978, Hacking 1983, Devitt 1979, 1984/1999), but many philosophers of science have been quite critical towards the causal theory of reference (e.g. Fine 1975, Enc 1976, Mellor 1977, Papineau 1979, Nola 1980, Dupre 1981, Kroon 1987, Psillos 1999, Niiniluoto 1999, Bird 1998, 2000). Some favour instead “causal descriptivism” (e.g. Bird, Psillos).

I shall argue that these philosophers of science have an over-simplified and in part mistaken understanding of what the causal theory of reference amounts to. I shall briefly review the principal ideas of the causal theory of reference, and explain how the causal theory can account reference failure and reference change. I also discuss whether it can be applied to unobservable theoretical entities. I argue that causal descriptivism is in any case a non-starter. I submit that the causal theory of reference, when correctly understood, can be an important ingredient in the realist toolkit for defending the rationality of science.

Hans Radder (Free University of Amsterdam)

Mertonian Values, Scientific Norms and the Commercialisation of Academic Research

In the course of the past decade, the problematic consequences of commercialised (university) science have been widely documented and increasingly acknowledged (see, e.g., D. Bok, S. Krimsky, S. Shulman). In response to these problems, universities, research institutes and science policy organisations have composed and adopted a variety of normative codes of good scientific behaviour (e.g., the VSNU, the Dutch Association of Universities). Almost invariably, these codes are based on, or derived from, the social ethos of science formulated by Robert Merton in 1942. In the 1970s, however, this Mertonian ethos and, more generally, the entire notion of a normative structure of science, has been strongly criticised by the then rising sociology of scientific knowledge (e.g., by S.B. Barnes and R.G.A. Dolby and by M. Mulkay).

In my contribution, I will set out Merton's view on the ethos of science, demonstrate that the criticism by sociologists of scientific knowledge is only partly right, and explain in which sense and to what extent a Mertonian approach is still valuable, and even badly needed, in the current context of strongly commercialised academic research. My claim is that Merton's notions of communism, universality, disinterestedness, and organised scepticism are best interpreted as overarching values that need to be realised, or brought closer, by following more specific methodological and ethical norms. By way of example, I will focus the discussion on the issue of the patenting of the results of university research and examine the significance of Mertonian values and scientific norms for this issue.

Athanasios Raftopoulos (University of Cyprus)

Ambiguous Figures and Representationalism

Ambiguous figures present a challenge to Philosophy of Psychology. Macpherson (2006) recently argued that the square/regular diamond figure threatens representationalism, one of the main theories in the Philosophy of Psychology, construed as the theory which holds that the phenomenal content or character of experience is either identical, or supervenes on, the nonconceptual content of experience. The brunt of her argument is the claim that representationalism is committed to the thesis that differences in the phenomenal experience of ambiguous figures, the gestalt switch, should be explained by differences in the NCC of perception of these figures. However, in the square/regular diamond figure such differences allegedly do not exist, and thus, representationalism fails. This is so because when subjects perceive a square or a diamond there is only state of affairs that is being represented and therefore differences in representational content cannot account for the perception of two different figures, namely a diamond and a square. Furthermore, a recent attempt of representationalists to explain the difference on the basis of the different axes of symmetry of the two figures allegedly fails because axes of symmetry, if they were the factors that were responsible for the different percepts, should also make subjects perceive, upon viewing an A and a tilted A, two different figures, which subjects, Macpherson claims, do not.

Here, I examine Macpherson's challenge and argue that representationalism can account for ambiguous figures. My thesis is that the diamond/square ambiguous figure can be met by representationalism in two ways, depending on what happens when subjects view that figure. On one account there are two nonconceptual contents involved. On another, there is one NCC but two different conceptualizations of that content. In either case, representationalism faces no problems. In both cases, the decisive factor is the kind of axes of symmetry or reference that the subjects impose to the ambiguous figure. I argue that the nonconceptual content (NCC) of perception that determines the phenomenal content of experience is cast in a relational Cartesian frame of reference the axes of which are determined with respect to the body of the perceiver. According to the orientation of these axes the subjects organize the ambiguous figure into two different ways by perceiving two different NCC and as a result they phenomenally see two different figures. This means that differences in the phenomenal character of experience are due to differences in NCC.

To neutralize Macpherson's objections against the use of the axes of symmetry to explain the gestalt switch in ambiguous figures, I examine her interpretation of the experimental evidence regarding the perception of an A and a tilted A that Macpherson uses to claim that axes of symmetry cannot account for the gestalt switch. I argue that Macpherson seriously misinterprets that evidence because she does not distinguish between two kinds of awareness, to wit phenomenal and access or report awareness. The lack of this distinction drives her to mistake the reports of the subjects in the above experiment as evidence for what the subjects phenomenally see.

Julian Reiss (Erasmus University Rotterdam)

Is There a Role for Clinical Expertise in Evidence-Based Medicine?

It is now part and parcel of the evidence-based medicine (EBM) movement that in diagnostic and therapeutic decision making evidence from systematic research should be integrated with clinical expertise (CE), the doctor's specific prowess at diagnosis and treatment in the unique circumstances of the patient considered. Nevertheless, the methodological discussion has hitherto almost completely focused on the systematic part of the movement. The major aims of this paper are to introduce this other highly important element of EBM to the methodological discussion, to raise a number of worries about the use of CE in EBM and to suggest some responses to these worries.

Suppose a clinician needs to diagnose and treat a patient P with symptoms S. Any evidence from systematic research, that is, from standardised methods such as randomised clinical trials, about the disease or condition that is likely to be responsible for S, and which is the best treatment, is necessarily established on experimental subjects who differ from P in innumerable respects. As a consequence, any recommendation based on systematic evidence can only be accurate for an 'average patient' who belongs to a certain population, which may or may not be relevant for the patient considered.

CE is supposed to fill in this gap. In the ideal case, the doctor decides knowing all the relevant evidence from systematic research and all pertinent facts about the patient and his circumstances and is thus able to derive a more accurate conclusion than could be reached on the basis of mechanical procedures alone.

There is a long standing tradition in science and methodology that seeks to minimise or eradicate the influence of subjective elements in the production of scientific knowledge. And this desire to keep scientists' idiosyncrasies at bay appears to be well warranted: scientists, qua human beings, have limited cognitive capacities, as agents they will not always act on the same motives as their principals, and they may be subject to internal biases and outside influences that affect the result of their inquiries. Medical doctors are no different in this respect. To mention but a few problems, we know that doctors display overconfidence in estimating their diagnostic accuracy; that doctors' and patients' perspectives differ and that recommendations are not always made on the basis of the patients' goals; and that 94% of all US doctors receive benefits from the pharmaceutical industry.

Even if we suppose the ideal case in which a doctor is free of biases, acts on the patient's goals and desires and is competent and alert, we might still ask why one should trust the subjective expert judgement more than the evidence from systematic research. The main part of this paper discusses ideas that have been put forward in this context (mostly by philosophically sophisticated historians such as Ted Porter, Peter Galison and Stephen Shapin) and argues that

CE or expert judgement in general is a necessary ingredient of all clinical/scientific decision making but that its functioning is very poorly understood philosophically and in response suggests some lines for future research.

Maria Rentetzi (National Technical University of Athens)

Rose Rand: Between two Different Gendered Cultures of Physics and Philosophy in Interwar Vienna

"The only advice I can give you is to do work in which you can make use of your manual skill and not to think that it is shameful to do decent work with your hands. As this is all that I can say, please stop asking me for my opinion and advice. May a ray of real intelligence enlighten you!" This is how Ludwig Wittgenstein responded to Rose Rand's kind request for an advice and a recommendation letter on October 5, 1946. In the same letter he wrote to her "quite bluntly" that she is not qualified for an academic position. Although known for his misogynist attitude, Wittgenstein was not the only one to discourage Rand from taking philosophy seriously. In a recent biography of Karl Popper, Malachi Haim Hacohen makes it clear that the presence of women in philosophical circles was insignificant in comparison with liberal circles around the physiologists Karl and Charlotte Bühler or—drawing on my own work on the physics community of Vienna—around Franz Exner and Stefan Meyer.

Born in Poland, Rose Rand moved to Vienna with her family in the late 1910s. She graduated from the University of Vienna both in Philosophy and Physics in 1938. Already as a student she was accepted as a member at the Vienna Circle while at the same time she worked at Women's Department of the Viennese Psychiatric Clinic. The paper explores Rand's trajectory in Vienna as a student of Rudolf Carnap and Moritz Schlick but also of Exner and Meyer. It also focuses in her struggle to survive first in England and then in the USA after the Anschluss. Based on Rand's case the paper attempts to compare the two Viennese communities that of philosophers and physicists, and contrast their different gendered cultures during the interwar period.

Thomas Reydon (Leibniz University of Hannover)

Natural Kinds as Tools for Philosophers of Science

How important is the notion of natural kinds for philosophy of science? Several philosophers (e.g., Russell and Quine) have argued that science does not at all need a notion of natural kinds. More recent authors, among others Paul Churchland and Brian Ellis, have expressed views of science in which natural kinds feature only in the most fundamental fields of natural science, such as particle physics, but not in fields that focus on higher levels of organization. This would make the notion of natural kinds useful for understanding a handful of selected scientific fields, but inapplicable to most parts of science. In a similar manner, developments in the philosophy of biology in the past three decades, among others regarding the ontology of species, have led philosophers of biology to adopt a rather pessimistic view of the importance of natural kinds in biological investigation and biological theorizing. It thus would seem that natural kinds at most play only a minor role in science and, hence, that the notion of natural kinds is not a very important part of the philosopher of science's toolbox.

In the present paper I argue against this dim view of the usefulness of the notion of natural kinds for philosophy of science. I argue that this dim view results from a tradition of treating the topic of natural kinds as foremost a question of ontology – a line of work that philosophers have followed for too long. I suggest an alternative approach to the topic of natural kinds, that starts by examining which epistemic roles kinds play in actual science. That is, elaborating a philosophical account of natural kinds begins by looking at how classifications of the subject matter of various scientific disciplines into kinds are actually being used in these disciplines' practices of investigation and knowledge production, in their ways of reasoning and in the explanations that they provide. Once we know what these epistemic roles are and in which ways they can be performed, we can move on to investigate what the ontology of natural kinds must be to enable them to perform these roles. The principal challenge for this approach to the topic of natural kinds is to navigate between a too strict view of natural kinds, on which natural kinds feature only in a few selected fields of physics, and a too liberal view of natural kinds, on which just any kind that is considered useful by some group of scientists is counted as a natural kind.

I shall address this challenge by considering a couple of cases from various fields of investigation in the life sciences, including kinds in behavioral biology and ecology. These cases show how an intermediately strict notion of natural kinds can function to make sense of how kinds perform core epistemic roles in science and thereby support the view that a notion of natural kinds is an indispensable tool for achieving philosophy of science's central goal of understanding how actual science works.

Menno Rol (University of Groningen)

Explanatory Progress and Tendencies in Economics

The natural sciences seem to increasingly stand out as sciences due to their inten-sive use of observation theories without which one cannot understand the data generated. These data are needed to empirically test the theories.

Before the nineteen sixties, economists hardly engaged in the inference of test hypotheses. Econometric techniques to collect and interpret data did not exist. Earlier, in the nineteenth century, the empirical basis of economics was even pretty much the same as that of the layman observer: phenomena like trends in market prices, unemployment and inflation were observed by the economist as well as by the non trained eye. Without scientific empirics, how can economics be qualified as scientific? A case about Austrian economics shows how.

Eugen von Böhm-Bawerk (1851-1914) defended the Subjective Value Theory (SVT) developed by Carl Menger against the British Classica Economists. The latter had proposed the Objective Value Theory (OVT). SVT says that the determining causal factor of objective market price is the ordering of preferences and utility of the last (marginal) demander, who is to acquire the desired good on the market. The cause of price allegedly lies at the acting subject. Under conditions concerning a relative stability, one can see a convergence of market price and costs of produc-tion. SVT explains this tendency. Classical OVT, in turn, give a competing explana-tion for the convergence. This theory proposes that the price of a good is, ulti-mately, caused by the costs of its production. Hence, OVT fixes the cause of market prices at the objective costs. Convergence of cost and price can be inferred immedi-ately.

Curiously, the tendency toward convergence precisely is no more than a mere tendency: the phenomenon of convergence fails to show up when initial con-ditions keep on changing, for instance, when preferences are instable or inconsis-tent or when productivity rises rapidly. In fact, OVT is falsified in these cases. The objective approach, so to say, only explains the tendency but not the exceptions to the tendency. However, SVT can explain both tendency and the breaking down of the tendency equally well. This, then, is a clear advantage of the Austrian approach over the British approach.

So how can two schools of thought seek and find the origin of market prices both at the cost side of the market mechanism and at the demand side? Clearly, OVT is confirmed only in case initial conditions remain stable, while SVT can deal with an explanandum that comprises deviations of the tendency. In other words, the latter offers explanatory progress over the former. Interestingly, for both ex-planations mere layman's empirics are used. Regardless the absence of test hy-potheses this type of scientific progress is empirical. So how are we to appraise this type of progress as scientific?

The answer is that, apparently, an empirics inaccessible for lay people does not in itself make an explanation scientific. What matters in the case treated is how the explanation runs. In case explanations are based on a conceptual apparatus overstretching lay interpretations, rather than lay empirical methods, there can be scientific progress in an explanatory sense. For the social sciences at least, this is no different today.

Kristina Rolin (Helsinki School of Economics)

Science as Collective Knowledge

In contemporary philosophy of science it has become a truism to claim that scientific knowledge is social knowledge. Yet there is a diversity of views about what is 'social' in scientific inquiry and why it is of epistemic interest. Some philosophers understand the 'social' in science to refer to scientists' social values, that is, value judgments concerning a desirable social order. Others understand the 'social' in science to refer to relations among scientists (e.g., collaboration, distribution of research effort, relation of trust). Margaret Gilbert introduces yet another dimension to this debate. She claims that scientific knowledge is social knowledge in the sense that it includes collective beliefs held by scientific communities. By collective beliefs she means beliefs which cannot be accounted for in a summative way. According to a summative account, all or most of the members of the community must believe that p in order for that community to believe that p. According to Gilbert, collective beliefs are held by communities as plural subjects. To say that a community as a plural subject believes that p means that the members of the community are jointly committed to believe as a body that p. A plural subject account of collective belief differs from a summative account in an important respect. In a plural subject account of collective belief it is neither a sufficient nor a necessary condition of a community believing that p that all or most of its members believe that p. A community having a collective belief that p involves a consensus but it is, as John Beatty explains, a consensus at a different level: not agreement concerning p but rather agreement to let p stand as the position of the group.

My aim is to explore to what extent scientific knowledge is properly understood as collective knowledge. By knowledge I mean justified true belief or acceptance. Thus, collective knowledge is justified true belief or acceptance held or arrived at by groups as plural subjects. In the first section I discuss Margaret Gilbert's argument for the claim that scientific knowledge includes collective knowledge held by scientific communities. In the second section I discuss K. Brad Wray's argument for the claim that neither the scientific community as a whole nor the various communities that constitute particular sub-fields are capable of having collective knowledge. Wray argues contra Gilbert that merely research teams are capable of having collective knowledge. In the third and the fourth section I introduce a third position into this debate. I argue contra Wray that collective knowledge is not limited to research teams. As Gilbert claims scientific communities are also capable of having collective knowledge. However, I argue contra Gilbert that collective knowledge is a pervasive phenomenon in science not because of the nature of scientific change as she thinks but because of the contextual nature of epistemic justification in science.

Jan-Willem Romeijn (University of Groningen)

Formal Models of Explorative Experiments

In the wake of logical empiricism, philosophy of science focused primarily on a rationalisation of science. Moreover, it largely ignored two major scientific activities: experimentation and theory generation. This changed with the advent of historians and sociologists of science, especially new experimentalists (Hacking, Franklin, Galison). They did study theory generation and experimentation, and argued that the two are intimately linked. But mostly they aimed at a social and historical understanding, and not at a formal representation of them. With the exception of certain developments in artificial intelligence (Simon, Thagard), formal treatments of experimentation and theory generation have not been forthcoming.

This talk aims to be a first step in developing a formal philosophy of science that covers these two activities. It does so by employing some new mathematical tools and specific findings from cognitive and developmental psychology. The aim is to provide a framework that can give a convincing rational reconstruction of at least some experimental interventions and theory generations in the history of science.

In the first part of the talk I briefly show that causal Bayesian networks provide a convenient mathematical framework for capturing experimental interventions (Pearl, Spirtes et al, Korb). A Bayesian network captures the probabilistic relations between a set of variables that describe an experimental setting. If we interpret the network causally, interventions can be represented by operations on the network structure. After that I discuss some results from cognitive psychology, in which Bayesian networks are employed to show that subjects employ interventions effectively when building up a causal picture of an experimental task (Gopnik, Tenenbaum, Steyvers). Arguably, this goes some way towards a formal treatment of experimentation and theory generation.

As I will argue in the second part of the talk, certain aspects of theory generation cannot be captured adequately in this framework. Historical studies of experiments (Franklin, Gooding, Steinle, Damerov et al) suggest that experiments are often geared at fixing the reference of theoretical terms, and not only at testing a model couched in terms that are already fixed. But the theory of Bayesian networks does not provide the tool for changes and tranformations of the nodes themselves. Thus, insofar as theory generation involves the construction of variables and not just the testing of relations between them, Bayesian networks cannot adequately describe theory generation. The nodes in the Bayesian network that are directly connected to observations may perhaps be fixed in advance, by operational definitions. But the unobservable nodes must somehow allow for being changed and reorganised.

In the third part of the talk I sketch how the machinery of Bayesian networks can be extended with a statistical method from psychology, called exploratory factor analysis. I argue that this extension allows us to model the construction of unobservable variables, and thus capture the aspect of theory generation that is not covered by Bayesian networks themselves.

Juha Saatsi (University of Leeds)

Whence Ontological Structural Realism?

Structural realism has become a household name in the scientific realism debate. It proves difficult to say, however, what structural realism exactly is. There are various contemporary positions that fall under the label 'structural realism', but which have rather diverse motives and defining features, being genuinely unified perhaps only by the words 'structure' and 'realism'. Ontological structural realism is one prominent variant of the structuralist revival in the context of the scientific realism debate. 'Ontic' structural realism is motivated by considerations from the foundations of physics, and its advocates characterise it as metaphysics. Hence it is rather different from a mere epistemological structural realism. Although ontological structural realism has also implications regarding our knowledge of the unobservable world, these ultimately derive from the metaphysical lessons.

This paper critically analyses the basic tenets of this position, its motives and the advocacy of the semantic view of theories as the preferred meta-scientific framework in which to spell out what ‘structure’ in this context amounts to. The considerations advanced in favour of ontological structural realism have an air of schizophrenia. On one hand structuralism is motivated as a solution to well-known epistemological problems faced by the realist. Thus understood we have a valid motivation for epistemological structural realism. On the other hand, considerations from philosophy of physics are used to suggest that structuralism should not be limited to a characterisation of epistemic humility forced upon us by the historical track-record of false yet successful theories. Rather, structuralism becomes a metaphysical doctrine, driven by the requirement that the realist should be able to spell out what she exactly believes in.

My critique of ontological structural realism (in this limited context of the scientific realism debate) focuses on this mixing of metaphysical and epistemological issues. I will argue that the kinds of considerations that may call for structuralism in metaphysics can be fully separated from the debate that leads to a well-defined motivation for epistemic structural realism. In particular, the thesis of 'metaphysical underdetermination' that has been used to motivate the move from standard realism to ontic structural realism is problematic. There is a need to withdraw from some of the implicit metaphysical commitments of the entity-oriented standard realism, but the move to realism about ontological structures is unmotivated. It is not within the traditional realism debate that ontological structuralism is called for, and hence the dichotomy between epistemic and ontic structural realism is a false one.

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Christian Sachse (University of Lausanne)

Relation of Theories and Concepts

In contemporary philosophy of science, ontological reductionism, or the claim that everything that exists in the world is something physical, is the mainstream position. Contrary to a widespread belief, the aim of this paper is to establish that ontological reductionism and theory reduction stand or fall together. I shall set out an argument for non-eliminative theory reduction. My strategy takes the supervenience argument for ontological reductionism (Kim) and the multiple realization argument against theory reductionism (Fodor) as starting point: causally efficacious property tokens of the special sciences are identical with configurations of physical property tokens (Kim). Nevertheless, a functionally defined property type of the special sciences (F) is multiply realized by physical realizer types (P1, P2, or P3). Thus, the special science theory seems to be irreducible to physics (Fodor). Against this background, there are, among others, two questions to raise: what are property types and how to spell out the relationship between property types of different sciences? Since we take ontological reductionism for granted and argue that property types of the special sciences are not identical with property types of physics (because of multiple realization), property types cannot be something ontological. Property types are concepts. On this basis, I would like to vindicate the indispensable scientific character of the concepts of the special sciences within a reductionist approach I call conservative reductionism. This approach is based on the three following steps:

I. One implication of multiple realization is that, from a physical point of view, there is a causal distinction between the possible physical realizers described by the concepts P1, P2, and P3. If there is a difference in composition between the realizer configurations, there is also a difference in their causal dispositions. P1 refers to physical configurations that have causal dispositions that are different from the ones described by P2 or P3.

II. For any causal difference between these realizers, there is an environment physically possible in which that difference becomes manifest also on the functional level, from a biological point of view for instance. There are functional side effects that distinguish between the property tokens that are tokens coming under P1 in comparison to the property tokens coming under P2, or P3.

III. It is possible to take those functional side effects into account by the special science theory by introducing functionally defined sub-concepts of F. Each sub-concept of F (F1, F2, and F3) is co-extensional with one physical concept. As result of this, a reduction of these functional sub-concepts, and thus of the abstract concept F, to physics is possible:

Abstract concept: F

Sub-concepts: F1 F2 F3

Physical concepts: P1 P2 P3

To conclude, multiple realization does not prevent a reductionist approach to the special sciences. To the contrary, multiple realization is necessary to vindicate the indispensable scientific character of the special sciences: there are no physical concepts that are co-extensional with the abstract concepts of the special sciences (indispensable), but their scientific quality is not put into question (like by eliminativist approaches) since they can be reduced to physics by means of their sub-concepts.

Mario Santos-Sousa (Autonomous University of Madrid)

Natural Mathematics: A Pluralistic Approach to Mathematical Cognition

Accounts of mathematical cognition commonly divide into separate research fields, pivoting on a distinction that has been a matter of great methodological dispute: the distinction between personal and subpersonal levels of explanation. I read it as one between accounts of mathematical performance and accounts of mathematical competence. This is a necessary distinction for a complete characterization of mathematical cognition, both of mathematical knowledge and its acquisition. Thus, while personal level or 'philosophical' accounts of mathematical performance turn on the normative requirements for mathematical knowledge, subpersonal level or 'psychologistic' accounts of mathematical competence focus on the implementational and/or computational requirements for its acquisition. How do both levels of explanation relate to each other? Are we facing two independent explanatory projects? If not, how do they depend on each other? It is the aim of my talk to address these questions.

I examine two views that fall short of yielding a comprehensive account of mathematical cognition: autonomy theory, which fails to explain our acquired facility with mathematical concepts, and eliminativism, which misses the mark and fails to see mathematics as a normative inquiry.

Finally, I assess a more substantive view, Martin Davies's interaction-without-reduction conception of the relation between the personal and subpersonal levels of explanation, and show how my version differs from his. According to his position, we have to allow for downward inferences from personal level phenomena to subpersonal level requirements for such phenomena. These inferences take the form of an inference to the best explanation: a kind of "transcendental deduction" with a specific theoretical structure on "the only game in town" grounds. It is on these grounds that Davies has found himself committed to the truth of the language of thought hypothesis, which requires that, in order to account for the conceptual structure of thought, subpersonal processes mirror that structure syntactically.

However, although the pluralistic approach I envisage leaves enough room for the language of thought hypothesis, whose truth ultimately must be empirically established, it also questions the need to invoke such hypothesis. Why should we conclude from the fact that personal level descriptions have a certain structure that there is a matching structure in the brain? Alternatively, all the relevant structure could be distributed into the (linguistic) environment, which is a more parsimonious view and explains mathematical performance in light of the inferential practices of the mathematical community, which are describable on a personal level of explanation. Hence, as I will argue, mathematical competence need not require the existence of a syntactically structured language of thought.

Steven Savitt (University of British Columbia)

The Transient Nows

It is often claimed that features of the spacetime of special relativity are inimical to the passage of time. In opposition to this view, I show how the passage of time is to be understood in Minkowski spacetime. A (local, specious) present is construed as an open set in the Alexandroff topology and the passage of time is a succession of such presents along a timelike curve. Temporal becoming is a local, rather than a global, phenomenon.

I offer some motivations for the view I propose, and I consider five objections that might be raised against it.

Georg Schiemer (University Vienna)

Frege and Peano on Quantification and Logical Scope

Frege's Begriffsschrift of 1879 is generally considered as the historical initial point of modern logic. It is supposed to comprise an adequate logical language for the formalization of mathematics. Its main innovations are the systematic development of polyadic predication and a theory of modern quantification. Despite this standardized picture there is still room left for interpretation concerning Frege's specific conception of quantifiers. Specifically the question of the logical expressibility of complex quantifier-dependencies has led to diverging commentaries. Dummett (1973) is convinced of the possibility of modelling “multiple generality” in Frege's system, whereas, following Hintikka & Sandu (1998), the functional dependencies between nested quantifiers are not representable due to his theory of quantifiers as isolated “higher-order” concepts.

In my talk, I will take up this discussion on the features and limitations of Frege's theory of quantification and develop it further by adding a new aspect: it can be argued that a precise analytical treatment of these issues presupposes a clarification of his understanding of the central concept of logical scope (Bereich) of a quantifier in a proposition. I will start from an analysis of Frege's specific notation in Begriffsschrift, his introduction of the graphic sign of “concavity” (Höhlung) as a symbolic tool for binding variables. Concerning the question of the syntactic indicability of limited scopes it will become evident that his model of quantification remains ambivalent in the text and allows conflicting interpretations.

This ambivalence can be solved, however, once we widen our perspective and take a glance at the relatively unknown correspondence between Frege and his contemporary colleague, Guiseppe Peano between 1894 and 1906. In the critical engagement with the latter's logical symbolism, especially his use of subscripts as indicators of the universal validity of formulas, Frege's intuitions behind his concept of scope will become transparent. His specific arguments against Peano's notation and syntactic modelling of scope will be reconstructed and their relevance for a specified understanding of his theory of quantification evaluated.

The Frege-Peano correspondence prima facie seems to supply additional textual evidence to Dummett's thesis that the logical system expressed in the Begriffsschrift is fully capable of modelling complex quantifier-dependencies. Against this I will finally argue that in the debate with Peano (as well as in Grundgesetze der Arithmetik from 1893) certain implicit “semantic stipulations” concerning quantifiers and the compositional structure of quantified formula can be identified, that raise serious doubts whether heterogeneous nested quantifiers are meaningfully conceivable in his logical theory at all.

Hans Bernhard Schmid (University of Basel)

Intentional Autonomy and Methodological Individualism

In this paper, I propose to distinguish the following three principles, which are lumped together in traditional accounts of methodological individualism:

1) The principle of individual intentional autonomy states that under normal circumstances, each individual's behavior instantiates his or her own action (no 'intentional heteronomy', i.e. 'remote control' behavior).

2) The principle of individual intentional autarky states that on the basic level of interpretation, each individual should be interpreted as acting exclusively on his or her own desires / on his or her own

intentions (people cannot act on other people's desires or intentions without either making them their own, or having a desire / forming an intention to conform to the other's desires / intentions, or having a desire / intention to conform to the respective set of rules, or some such desire / intention (compliant desires / intentions assumption); this excludes intentional heterarky, i.e. people's acting on other people's desires or intentions without compliant desires / intentions.)

3) The principle of intentional individualism states that people act exclusively on individual desires / intentions (either their own or other individuals') (this excludes intentional commonality, i.e. people sharing one (token) desire / intention).

I argue that 1 does not entail 2 and/or 3. In other words, I defend the following two claims:

a) Intentional heterarky does not compromise individual intentional autonomy: in principle, it is possible to understand an individual as acting on other people's desires/intentions without compliant desires and still interpret his or her behavior as his or her own action.

b) Intentional non-individualism (intentional commonality) does not compromise individual intentional autonomy: in principle, it is possible to understand people as acting on a shared desire/intention and still interpret each individual's behavior as his or her own action.

Gerhard Schurz (University of Düsseldorf)

Universal vs. Local Prediction Strategies: A Game Theoretical Approach to the Problem of Induction

In this paper I develop a general game-theoretical approach to the problem of induc-tion. Different prediction methods are compared with respect to their long-run and short-run predictive success, in varying environments (or possible worlds). Of par-ticular importance is a general prediction strategy called of meta-induction. Meta-induction applies the principle of induction to all competing prediction methods (or predictive clues) which are epistemically accessible. It is demonstrated that a certain variant of meta-induction, namely weighted-average meta-induction, is indeed an op-timal prediction method in the long-run. Moreover, in almost all environments, its short-run loss of predictive success as well as its additional complexity, as compared to simple inductive prediction methods such as "Take the Best" (Gigerenzer), is low. In this sense, weighted-average meta-induction is a universal prediction method, and it provides a possible solution to Hume's problem of induction. On the other hand, it is demonstrated that in certain classes of environments which satisfy special condi-tion (such as converging event frequencies, or dominance relation among predictive clues), there exists simpler prediction methods which are superior to weighted-average meta-induction in terms of short-run success and low complexity. These pre-diction methods are local in the sense that their predictive success is restricted to these special environments. From an evolutionary viewpoint one would conjecture that humans have developed both universal and local prediction methods. This con-jecture is confirmed by results in philosophy of science and cognitive psychology.

Astrid Schwarz (Technical University Darmstadt)

Commuting Concepts and Objects in Scientific Ecology

Most studies on the production of scientific knowledge have focused on the lab as a place, where instruments and epistemological things, experimental and innovation systems are localised and originate from. In contrast, field sciences such as geology, anthropology or scientific ecology have attracted much less attention regarding the constellations of theory building and empirical practices. These sciences have in common that place – geographically or socially understood – plays an important role. Research objects cannot be separated from these places without

epistemological loss and, inversely, objects influence places with which they are associated. From this constellation results a permanent movement of practices, concepts, theories and things between science and society, fundamental, applied and application oriented research. In this sense concepts such as „race“, „landscape“ or „biodiversity“ might be called commuting concepts.

“Biodiversity” was introduced during a conference of the American National Academy of Science in 1986. The term spread very quickly in the scientific as well as the social context. Originally, the word was still recognizable as a compound that derived from scientific terminology. It was soon entirely unmoored from this background and spread without referring to any biological theories or to a socially defined applied context. Indeed, the concept became so successful that it served as an umbrella for big scientific and research policy programmes, while at the same time focussing new scientific institutions or/and reforming already existing ones.

In my presentation I will be discussing the power of biodiversity as an “agent of social change” and how the resulting “dance of agency” affects both the social and scientific shaping of the concept (and also the objects involved). I will explore these movements of the biodiversity concept also against the background of “Pasteurs Quadrant”, the model proposed by Donald Stokes to classify scientific research. Biodiversity would then be located in the fourth so far empty quadrant referring to “pure local understanding” - meaning research in the style of natural history and relying on the presumption that diversity is a value in itself . The attractiveness and stability of the concept might thus be described as follows: it allows for aiming at different modes – such as “pure applied research” or “application oriented fundamental research” - and for commuting between them without raising definitional problems.

Michael Seevinck (Utrecht University)

On the Merits of Modeling Quantum Mechanics Using Semi-Classical Models

Modeling quantum mechanics using (semi-)classical models has a long tradition, going back to at least David Bohm's work of the 1950's. In the last few years there has been a renewed interest in this kind of modeling. In this contribution I will provide a critical evaluation of the modeling practises in this field in order to show that they provide novel and unexpected understanding of the theory in question.

I will consider models that are used for reproducing - in a classical way - crucial aspects of quantum mechanics. One such aspect is reproducing the correlations produced by so-called entangled states, notably the singlet state. In this paper I will focus on this aspect of the modeling.

Since it is a classical world that is longed for, this modeling is performed using mathematical models that do not use the traditional Hilbert-space structure of quantum mechanics, but instead use the formalism of so-called hidden variables, whose function it is to encode the classical world.

We know from the work by John Bell that hidden variable models that are local do not suffice. However, in the last few years more general semi-classical hidden variable models have been constructed that can indeed reproduce the desired quantum correlations. Among them are classical protocols supplemented with some communication by classical bits, or using so-called non-local machines. Furthermore, some of them can even give stronger correlations than quantum mechanics.

Here I will not deal with how these models precisely work, since I want to focus on more foundational and philosophical questions: Firstly, what does this modeling practise add to our understanding of quantum mechanics? Secondly, how precisely does this take place?

To answer these questions a shift of focus is important. Instead of asking whether or not the models succeed in giving a classical world behind quantum theory, it is more illuminating to ask how much of the structure of quantum mechanics can indeed be retrieved in the specific model used and what part cannot. And, since no classical model has retrieved all of quantum theory, an even more important question arises: why can't we retrieve more? Research focusing on these two questions has presented us with a great deal of novel understanding of quantum mechanics.

So although the models were designed to uphold a (semi-) classical world behind quantum mechanics I will argue that the real contribution they make is elsewhere: they teach us what is essentially quantum about quantum mechanics, i.e., they contribute to the understanding of what kind of a theory quantum theory in fact is; and I will argue that it is an understanding one would not have easily gained if there had not been such modeling practises.

Lawrence Shapiro (University of Wisconsin, Madison) and Thomas Polger (University of Cincinnati)

The Dimensions of Realisation

Philosophers of science have struggled to explain how kinds or properties in the "higher-level" sciences (e.g. economics, psychology, biology) are related to kinds or properties in the "lower-level" sciences (e.g. chemistry, physics). A popular view is that higher-level properties are realized by lower-level properties. Until recently, however, little attention has been paid to the nature of this realization relation. There now exists a controversy between those who take realization to be a "dimensioned" relation and those who see realization as "flat." On the former view, a realized property, e.g. hardness, is realized by kinds that are not themselves hard (molecules and their organization). On the latter view, hardness is realized only by hard things (diamonds, steel, and so on). Proponents of the dimensioned view claim that it better represents the aims and needs of scientists than does the flat view. In this paper we argue that the dimensioned conception of realization fails to do the work for which the idea of realization was originally introduced -- work that is better left to the flat view. Moreover, there are other relations, e.g. composition, that seem to capture all the phenomena that dimensioned realization is intended to explain. Thus, we conclude, efforts to replace the flat view of realization with the dimensioned view should be resisted.

Norman Sieroka (ETH Zurich)

Dynamic Agents and Geometrisation: A Weylian Approach towards Theories of Matter

Despite contrary claims, Hermann Weyl’s agens theory formulated in the early 1920s and John Wheeler’s geometrodynamics conceived around 1960 are conceptually strikingly different accounts of matter. Geometrodynamics was particularly motivated by general relativity and aimed at reducing matter to geometrical features of space-time. Starting from rather different speculative and unificational motivations, the agens theory on the other hand attributes physical and even metaphysical importance to matter. According to the agens theory, matter is a transcendent agent causing effects in space-time “from beyond”.

The conceptual difference between Wheeler and Weyl parallels the famous one between Descartes’ attempt to reduce matter to pure extension and, as Weyl makes explicit, Leibniz’ ascription of activity to matter. Although this difference is not always as clear cut as in the aforementioned cases, tempered versions of it can be found throughout the history of modern physics (cf., e.g., Mie) and arguably is somehow still with us. Loop quantum gravity today is primarily motivated by general relativity (as is Wheeler’s approach) and starts from the allegedly firmly based concept of background independence of fourdimensional space-time. In contrast to this, and rather similar to Weyl’s agens theory, string theory starts from a more general speculation about what matter might be (other than curved space-time or pointlike particles) and about how all the known interactions could be unified.

This conceptual difference in approach towards matter is not only of historical but also of philosophical relevance; especially if one adopts Weyl’s own view (of that period when he defended his agens theory). Most of Weyl’s philosophical writings arise from (i) combining systematic thought with explicating historical developments and (ii) showing a wavering between differently motivated approaches. For example, in his 1925 paper “The Current Epistemological Situation in Mathematics“ Weyl presents the history of mathematics as a to and fro process alternately putting emphasis on passive/confined being and on active/infinite process. For Weyl the wavering between these different emphases is an integral part also of the current situation during the debate between intuitionism and formalism. Because of this wavering, as Weyl puts it, “there arises a third realm”; namely that of “symbolic constructivism”. And the first philosopher who, according to Weyl, entered this realm (although, according to Weyl, his constructivism had still to be improved) was the German Idealist Johann Gottlieb Fichte.

In my paper I intend to: (i) demonstrate parallels between Weyl’s account of the history of mathematics and the history of some theories of matter in physics and (ii) then explain Weyl’s idea of a “rising third realm” and of how it relates to Fichte’s concept of “the wavering of the power of imagination” (“das Schweben der Einbildungskraft”).

Corrado Sinigaglia (University of Milan)

The Shared Space of Actions: Mirror Neurons and Motor Intentionality

We live in a world of inanimate and animate objects, each of which not only has its own shape, color, texture, etc., but also type of movement. Of these latter, biological movements, especially those performed by our conspecifics, are very crucial stimuli for us. How do we recognize them? How do we grasp their possible meaning? How do we understand them not only as bodily movements but also as intentional actions?

According to a philosophically and psychologically influential view, in order to understand the intentional behavior of others we have to attribute them with the mental states such as beliefs, desires, intentions etc. that are likely to have driven that behavior, make it explicable and eventually predictable. Our intentional understanding of the actions of others would therefore be rooted in our ability to mentalize or to read their minds, in other words to represent them as having mental states. Without this meta-representational ability, the actions of others could not have any intentional meaning for us and this would prevent us from interacting with our peers and performing adequately even in the most simple social situations.

Over the last few years, however, this view has been radically challenged by the neurophysiological analysis of the cortical mechanisms involved both in producing our own actions and in understanding those performed by others. The study of the functional properties of the cortical motor system and the discovery of a specific class of visuomotor neurons (mirror neurons) has suggested the hypothesis that our understanding of the actions of others is primarily based on a mechanism that directly matches the visual representation of observed actions with our own motor representation of the same actions. According to this hypothesis, we understand the actions of others by means of our own “motor knowledge”: this knowledge enables us to attribute an intentional meaning to the movements of others and also to predict their consequences. This does not exclude, of course, that other processes, such as those that characterize our meta-representational abilities, may be at work and play a role in these functions. It simply underlines that this kind of mentalizing is neither the sole nor the primary way to intentionally understand the actions of others.

In my talk I will briefly review the evidence for the existence of a mirror system in monkeys and humans, and will focus on the implications of its action observation/execution matching mechanism for intentional understanding. This will lead me to demonstrate the extent to which intentional and motor components of action are intertwined and how they can be fully appreciated only on the basis of a motor approach to intentionality, thus going beyond any hyper-mentalistic view of action understanding, which reduces the intentional content of an action to that of the pure (i.e. no-motor) mental states that are thought to cause such action, as well as any hyper-simplified view of motor behavior, that relegates the motor content of an action to the mere (i.e. non intentional) bodily movements needed to execute it.

Daniel Sirtes (University of Basel) and Marcel Weber (University of Basel)

Scientific Significance Scrutinized

In 'Science, Truth,and Democracy' Philip Kitcher claims that scientists do not aim at any old truths but at significant truths. After rejecting four proposals for the (epistemic) aim of science and therefore for the notion of epistemic significance (explanation, laws, unification, and causal processes) he proceeds by giving a ‘commonplace and disappointing’ account of scientific significance, basically describing it as a historically contingent ‘healthy curiosity’.

Although, we agree with Kitcher that what seems significant is context and subject sensitive, we believe that there is much more to be said on the topic. Scientific significance is an important epistemic category that informs various choices made by scientists as well as by funding agencies. By focusing on research in biology we propose a set of (positive and negative) factors for significant research questions (biological functions related, disease-related, causal vs. descriptive, directed towards manipulation, etc.).

We support our claims by an empirical analysis of reviews of grant applications to the Swiss National Science Foundation.

Wolfgang Spohn (University of Konstanz)

Measuring Ranks by the Complete Laws of Iterated Contraction

The talk will present two important results.

First, ranking theory is well known to deliver an account of iterated contrac-tion; each ranking function induces a specific iterated contraction behavior. The talk will specify a complete axiomatization of that behavior, i.e., a complete set of laws of iterated contraction. In view of how central the issue of iterated contraction has become and how little agreement has been reached in the last fifteen years this result considerably advances the present state of discussion.

Second, ranking theory is well known to be a strengthening and generalization of AGM belief revision theory. It is so, however, by making free use of cardinal degrees of (dis)belief, a feature that always seemed objectionable. The paper meets the objection by presenting a rigorous measurement theory for ranking functions in terms of iterated contraction. That is, each iterated contraction con-forming to the specified axioms determines a ranking function uniquely up to multiplicative constant, i.e. measures it on a ratio scale. This result parallels the corresponding and long known results about the measurement of subjective probabilities.

Jan Sprenger (University of Bonn)

Statistics do not Require Frequentist Justifications

Plenty of classical statistical methods, e.g. hypothesis tests and interval estimation, are justified by their frequentist properties, i.e. high relative success frequencies in the long run. For instance, we justify „95%-confidence intervals” by the fact that in 95% of all cases, the confidence interval will comprise the true parameter. In particular, there is a longstanding tradition to conjoin those frequentist justifications with frequentist interpretations of probability. Consequently, crucial probabilities in hypothesis tests or interval estimation are interpreted as relative frequencies in a hypothetical long run of experiments.

I believe that this line of reasoning is flawed and that frequentist justifications are not required. First, frequentist justifications of statistical methods do not depend on a particular interpretation of probability: due to the Laws of Large Numbers, any objective interpretation of probability guarantees the desired long run properties. So there is no need to stick to frequentist interpretations of probability -- the long run justification arises from any objective interpretation. Second, long run justifications of statistical methods are futile: The limiting relative frequency of an event in an experimental setup (e.g. the true parameter being inside our confidence interval) does not affect the rational degree of belief in a particular occurrence of this event, i.e. it does not specify the fair betting odds on the single occurrence of the event. Thus, limiting frequencies cannot justify single applications of a test or interval estimate. Third and last, the probabilities of classical statistics (e.g. the probabilities that determine confidence intervals and hypothesis tests) are analytic probabilities. Inferences in classical statistics are based on probabilities of events under a certain distribution, e.g. If I toss a fair coin three times (independently), the probability of observing only "heads" is 1/8.

Statements as (1) apply to any interpretation of probability since it is the meaning of a fair coin that the probability of "heads" coming up in any single toss is 1/2, implying the truth of (1). Whatever interpretation of probability we choose, statements as (1) remain true. Therefore they are also valid as statements about rational degrees of belief and success expectations, and they are able to justify the use of confidence intervals and hypothesis tests.

Frequentist justification of statistical methods would be necessary if there were any reason to determine the meaning of probabilities in a frequentist sense. But such a reason does not exist. Then, the vices of frequentist justifications become clear: the failure to forge a link between limiting frequencies and single case decisions and success expectations. The analyticity of the relevant probabilities entitles us to trust in the estimation or decision procedure in any case. This virtue of classical statistics decouples statistical practice from the metaphysics and epistemology of probability and explains that so many practicing statisticians do not care for interpretations of probability.

Chrysovalantis Stergiou (National Technical University of Athens)

Some Remarks on Causal Processes in Classical and Local Quantum Physics

Salmon's theory of causation is generally regarded as one of the best available theories of physical causation. This work is an attempt to study examples of causal processes in actual physical theories. I first examine the case of a relativistic theory of classical particles in Minkowski spacetime. On the basis of the Wigner – van Dam 'no-interaction' theorem (1966), I identify a causal process with the world line of a collision-free particle. Second, I examine the case of a classical Klein-Gordon field in Minkowski spacetime where I discuss the problem of whether the characterization of a causal process has to be made in terms of a globally conserved quantity or in terms of a locally conserved current. In the first case, causal processes are identified geometrically with the integral curves of a constant vector field which represents the globally conserved quantity, while in the second case, a causal process is the world-line (defined by a unit time-like vector) in the direction of which the component of a locally conserved current remains constant. The characterization of a process as causal by means of a globally conserved quantity depends on global considerations about the field, regarding the existence of this quantity. Instead, the characterization of a process as causal by means of a locally conserved current can be obtained in cases where the corresponding globally conserved quantity cannot be defined. Last, I examine the case of a standard Haag – Araki formulation of algebraic quantum field theory in Minkowski spacetime and I argue that we cannot characterize a process as causal in terms of locally measurable conserved quantities. In the context of this theory, the values of locally measurable quantities are represented by projection operators that belong to the von Neumann algebra of a finite region of Minkowski spacetime. Schlieder (1969) has shown that such a local projection operator cannot be time – translation invariant; hence, it cannot represent the value of a conserved quantity. By a simple argument I show that one cannot find any physically admissible state that yields a time invariant probability for the value of a local quantity which is represented by a local projection operator.

Michael Stöltzner (University of Wuppertal)

Can the Principle of Least Action be Considered a Relativised a Priori?

Hardly another principle of classical physics has to a larger extent nourished hopes into a universal theory, has simultaneously been plagued by mathematical counterexamples, and has ignited metaphysical controversies than did the Principle of Least Action (PLA). I investigate whether the PLA can be interpreted as a historically relativized constitutive a priori principle of mathematical physics along the lines Michael Friedman has drawn in Dynamics of Reason, using mainly the example of the emergence of relativity theory.

Such an interpretation suggests itself, historically, because two main advocates of the PLA during the 1910s, Max Planck and David Hilbert, considered relativity theory as a case in point for the almost universal applicability of the PLA. But they were both aware of the principle's mathematical pitfalls and that without physical specification it only represented an empty form.

There are also important systematic parallels. Friedman intends to overcome the joint challenges of epistemological holism and a relativist reading of Kuhnian incommensurability by a stratification of mathematical physics. The strata are: (i) empirical laws properly so-called; (ii) a set of constitutive a priori principles that (a) render these laws meaningful as mathematical entities and (b) relate these mathematical entities to possible empirical circumstances by coordinative definitions; (iii) philosophical meta-principles that motivate and rationally justify the transition from one paradigm to another as a 'natural extension'.

Hilbert's 1915 axiomatization of general relativity exhibited a three-layered structure that not only agrees with the different steps in specifying the PLA by a Lagrangian function, the boundary conditions, and the space of possible solutions, but that is also typical for his axiomatic method as a whole. (Hilbert did not treat an axiom system as a homogeneous conceptual framework in which only logical deductions operate.)

Accordingly, Hilbert's axiomatic method may be understood as a mathematical reorganization and stratification of a physical theory aimed at casting as much as possible in terms of mathematical constitutive a priori principles that provide a space of possible physical realizations. Mathematics has the advantage that the relationship across different mathematical frameworks is rigorous and that one can precisely spot coordinating principles, such as general covariance. Hilbert's axiomatic treatment of some phenomenological theories, moreover, shows that the axiomatic method did not necessarily involve realist or ontologically reductionist commitments. In the same vein, Friedman views his approach superior to a structural realist resolution of the Kuhnian problem.

Although in many respects the PLA is water on Friedman's mills, two major problems remain. First, the mathematical and physical levels of the PLA are more intertwined than Friedman assumes, and what counts as 'natural' or 'deep' according to the respective standards does not always coincide. Second, although the PLA has survived quite a few scientific revolutions, so has the formulation of physical theories in terms of differential equations. Hence, there have always been two different lines of constitutive principles that show little sign of convergence despite the fact that in many cases both formulations yield physically equivalent results.

Patrick Suppes (Stanford University)

Upper Probabilities, Entanglement and Decoherence

Bell states and other entangled states exhibit correlations that cannot be accounted for by a non-contextual local hidden-variable model. Various authors have shown that the non-existence of a non-contextual local hidden variables model entails that there is no joint probability distribution over random variables that represent the observables in question. The converse is also true. If there is no joint probability distribution, then there is no non-contextual local hidden variables model.

Starting from the observation that entangled quantum states, in the absence of any stabilizing fields, will decay under the influence of decoherence, Hartmann and Suppes (2006) have investigated the decay of a GHZ state under the influence of decoherence in a Markovian Master equation model. Using necessary and sufficient conditions for the existence of a joint probability distribution derived by de Barros and Suppes (2000) they showed that a joint probability distribution emerges after about 20% of the half time of the decay. Interestingly, at this time the system is still highly entangled, although a classical model can account for the correlations in it.

The work just described leaves open the question how the physics before the emergence of a joint distribution can be described. In this talk, we show that the correlations of Bell states and GHZ states can be accounted for by an upper probability distribution. Upper probability distributions are well known in the theory of uncertain reasoning. These distributions are explicitly constructed for the cases at hand. We then move on to discuss the implications of our results. Most importantly we ask what kind of hidden-variable model is needed to reproduce correlations that can be accounted for by an upper distribution. (This abstract is based on joint work with Stephan Hartmann.)

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Adán Sus (Autonomous University of Barcelona)

Absolute Objects and General Relativity: Dynamical Considerations

It has been argued recently (Pitts, 2006) that the Theory of General Relativity (TGR), according to the Anderson-Friedman definition, has an Absolute Objects; namely the scalar density defined by the square root of the determinant of the metric. It is not clear what work, if any, this object might be doing in the theory. It is also an open question whether, to the light of this result, we should regard this program as of no use in detecting the peculiarities of the notion of General Covariance involved in TGR (it rightly detects Absolute Objects but contradicts our intuitions of TGR lacking them, therefore General Covariance cannot be the absence of Absolute Objects) or it fails in defining what to be an Absolute Object means.

Here I analyse the dynamical relevance of the scalar density in TGR by contrasting this theory with Unimodular Relativity (where undoubtedly a fixed element of volume exists) and establish a parallelism with the different ways in which the flat spacetime metric can appear in different formulations of special relativistic theories. I pay special attention to the role of the symmetries of the geometrical objects present in the theory through their connection to conservation laws given by Noether’s theorems. In the case of the Absolute Object detected in TGR by the Anderson-Friedman definition, I discuss whether physically meaningful conservation laws can be derived and their relationship to the constancy of the cosmological constant.

The objective of the previous analysis is to discuss to what extent the Anderson-Friedman program is suitable to characterise a notion of substantive general covariance (or background independence). I argue that this strategy, in some cases, might be detecting Absolute Objects that do not have any dynamical relevance, or not the right one, to understand the notion of background independence. As a conclusion, I defend the necessity of going beyond the formal definitions of absoluteness.

Predrag Sustar (University of Rijeka)

Functions in the Morphospace

I will examine (i) the phylogenetical model of morphospace, and (ii) its utility in the philosophical debate on functions in biology. (i) In phylogenetical analyses, the model of morphospace is usually considered as a suitable theoretical device for studying the interface of functional and evolutionary morphology. Thus, according to this model, the basic distribution is concerned with the relationship between observed organic forms and all possible forms, which has a characteristic value for a given morphotype (Alberch [1982]). Besides clarifying certain specific features of this kind of model building in biology, I will pay special attention to apparently conflicting strategies by which distributions of organic forms in the morphospace are explained (Amundson [1994]; Griffiths [2002]). With regard to the so-called “adaptationist” and “developmentalist” explanatory strategies, I will focus on different roles that the mechanism of natural selection plays in these explanations. In that respect, it will be particularly relevant to our purposes in (ii) to determine a certain stabilizing role of natural selection in the “developmentalist” approach to the characteristic distributions within the model of morphospace. (ii) In ascribing functional properties to biological items, we appeal directly or indirectly to evolutionary considerations in their ‘backward-looking’ sense. Thus, according to the prevailing direct appeal, the function of biological item is the effect produced by the item, which was selected by certain evolutionary pressures in the past. However, the ‘selected-effect’ account of functions, and in general those accounts that refer to this type of restrictions, deploy incorrectly evolutionary considerations. For instance, the evolutionary pressures of natural selection can only track the biological item that functions better in a given range of environmental circumstances than the item-competitors, but not its function (for this, and other similar objections to the ‘selected-effect’ accounts, see Cummins [2002]). Now, I will argue here that the proposed assessment of the phylogenetical model of morphospace provides a more adequate scientific resource to the ways in which we can relate philosophical accounts of functional claims to evolutionary analyses.

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Laszlo E. Szabó (Eötvös University)

Empirical Foundation of Space and Time

First I demonstrate how sloppy and circular is the way we talk about the empirical meanings of such fundamental physical quantities as time and distance. Then, I sketch the empirical/operational definition of space and time tags of physical events, without circularities and with a minimal number of conventional elements. As it turns out, the task is not trivial; and the analysis of the problem leads to the following surprising conclusions:

1. Although the space and time tags so obtained are, of course, "relative" to the trivial semantic convention by which we define the meaning of the terms - this kind of "relativism" is common to all physical quantities having empirical meaning - they are absolute in the sense that they are not relative to a reference frame but prior to any reference frame.

2. No objective meaning can be assigned to the concept of "proper" time. "Time" is what the etalon clock reads, by definition.

3. It is meaningless to talk about "non-inertial reference frame", "space-time coordinates (tags) defined/measured by an accelerating or rotating observer", and the likes.

4. Whether the standard clock used in the contemporary physical laboratories is appropriate for the definition of space and time tags is still an open empirical question. (A realistic experiment will be suggested by which the question could be decided within the Solar System.)

5. There is no additional conventionality attached to simultaneity over and above the original choice of the value of Reichenbach's epsilon in the trivial semantic convention. Moreover, this freedom merely consists in the choice of a single real number between 0 and 1; it cannot depend on "space coordinates" and/or "direction"; simply because there are no such things as "space coordinates" and "direction" prior to fixing the value of epsilon.

6. Assuming that the future experiments mentioned in point 4 will confirm what our present physical theories suggest, there seems no way to build up the spatial concepts operationally, if epsilon is not 8equal to 1/2. And, given that our aim is to define not only the temporal but also the spatial concepts, this is a strong experimentally testable argument against the non-standard synchronization.

Tuomo Tiisala (University of Chicago)

Hacking's Verificationism

Little attention has been devoted to the specific version of verificationism that Ian Hacking advocates as a core element of his philosophy of science. The goal of this paper is to provide an analysis of the distinctive features of this version of verificationism. This will be done by means of first relating Hacking's verificationist ideas to the tradition of French positivism (from Comte to Foucault) in systematic terms and then specifying the distinctive contribution Hacking makes to this tradition of philosophy of science with his work on styles of reasoning understood as scientific practices that bring about new types of true-or-false statements together with new methods of verification.

Despite Hacking's ample tribute to Foucault's work in the history of sciences as the single most important influence on his own work on styles of reasoning, this connection remains poorly understood – essentially due to an ignorance of Foucault's self-declared "positivisme hereux" that essentially informs Hacking's work. Foucault's entire archaeological project in the history of science rests on the assumption that, in addition to the general cognitive capacities of the human brain, the possibility of formulating true-or-false statements about a certain field of objects requires specific conceptual resources that are not found as such in the brain but come into being only in historically specific and changing discursive practices. It is this Foucault's understanding of fields of verification/falsification (positivités) as being dependent on the conceptual structures of historically specific discursive practices that provides the background for Hacking's historically sensitive yet verificationist philosophy of science. Hacking's work on styles of reasoning has not, however, focused exclusively on the conditions, both conceptual and technological, that make objectivity possible in specific scientific practices, but he has treated the coming into being of new methods of verification as an integral part of these practices.

The relevance of Hacking's advocacy of verificationism in connection with his case studies in the history of sciences goes beyond the French tradition of positivism, for the questions at issue have intrinsic philosophical importance, especially in the current epistemological context marked by a new, and not merely historical, interest in logical positivism, as promoted by Michael Friedman. In fact the sharp distinction between conditions of possibility and conditions of validity that runs through Hacking's understanding of styles of reasoning and go back to Foucault's work is essentially the same distinction by means of which Carnap separated practical questions concerning the formation of linguistic frameworks from questions of validity that can be meaningfully formulated only within such a framework once it has been put in place. In these terms the French tradition of positivism will be aligned with its logically oriented Viennesse counterpart and contrasted with the epistemological naturalism Quine argued for, interesting enough, on the grounds of his own commitment to verificationism.

Thomas Uebel (University of Manchester)

Carnap, Explication and Ramseyfication

This presentation will consider whether Carnap's philosophical programme of deflationist explicationism is threatened by what many theorists consider to be a misadventure late in his career, his forays into the ramseyfication of scientific theories. In doing so it seeks (i) to highlight (one) underdiscussed aspect of the debate about the propriety of employing the distinction between analytic and synthetic statements, (ii) to distinguish Carnap's use of Ramsey sentences for expressing the content of the non-observational parts of scientific theories from that of structural realism, and (iii) to present a qualified defense of Carnap's explicationist programme.

The problem at issue is the following. The impossibility of formulating the analytic/synthetic distinction for theoretical statements prompted Carnap to look beyond the arguably defensible criterion of empirical significance he published in 1956. Using Ramsey's method of replacing descriptive theoretical terms by variables bound by higher-order quantifiers, Carnap, in publications from 1958 to 1966, claimed to be able to give a characterisation of the cognitive content of theoretical terms so as to distinguish synthetic and analytic statements concerning them. Now according to newman's well-known objection, a ramseyfied theory is trivially satisfied once the empirical constraints set down by its observational part are met. This speaks not only against structural realists but also against Carnap's plan to use ramseyfication to exhibit the cognitive content of theoretical terms and thereby re-establish the analytic/synthetic distinction for theoretical statements. The question arises how much damage ensues for Carnap's explicationist programme.

Subsequent to some observations under (ii) and bringing points (i) and (iii) together, the possible strategy of simply abandoning ranseyfications will be considered. Two ways of doing so appear to remain open to Carnap. The first way envisages dropping the wide analytic/synthetic distinction for theporetical languages but retaining the narrow distinction between logical and descriptive terms (which he was ptrepared to endorse before he hit upon ramseyfications). This would amount only to a partial diminuition of his explicationist programme, for Carnap could still distinguish between what is, in a given logico-analytic framework, a logical truth from an empirical truth and so retain analyticity in its very narrowest sense. Here the question arises whether the basic distinction between logical and descriptive (in theoretical languages) can be sustained. The second way would be that of building on Carnap's claim that theoretical terms can (after all) be given a direct semantic interpretation and regarding as analytical statements of the theoretical language those that follow from the logical and semantical rules of the language in question. Here the question arises whether the reduction chains and correspondence rules that are still needed to enable the indorect testing of theoretical statements do not interfere unduly. The assessement of these and other objections to these two possible Carnapian responses to the problem with ramseyfications and their bearing on the fate of Carnap's programme for philosophy will conclude this presentation

Giovanni Valente (University of Maryland)

Is There a Stability Problem for Bayesian Noncommutative Probabilities?

Advocates of the Bayesian interpretation of quantum mechanics maintain that quantum states reflect degrees of belief of an observer, who is in turn idealized as a rational agent. A quantum measurement is then assumed to be tantamount to performing a Bayesian statistical inference. Accordingly, the so-called wave-function collapse would just reduce to a noncommutative analogue of the Bayes rule, whereby the agent-observer revises her degrees of belief on the basis of the information gathered from the measurement. A necessary condition for noncommutative probabilities to be interpreted as Bayesian probabilities is that they obey constraints of rationality. Hence, the failure of a rationality constraint in quantum mechanics would prove the Bayesian interpretation of the theory inconsistent.

Rédei (1992, 1998) formulated a stability condition as a rationality constraint characterizing Bayesian statistical inference. Suppose an agent has revised her degrees of belief about an event A in light of certain evidence; then, if she is presented with the same evidence again, in order to be rational, she must assign the same conditional probability to A. The argument is cast in the framework of von Neumann algebras theory, generalizing classical probability theory, that neatly distinguishes between the cases in which quantum observables have discrete or continuous spectra. The satisfaction of the stability condition rests on the existence of a proper noncommutative conditional expectation. As Rédei argues, the evidence which an agent-observer is presented with by a quantum measurement would amount to the set of probabilities of all events in the Boolean algebra generated by the measured observable. However, if the latter does not have a discrete spectrum, then the required conditional expectation does not exist.

One can distinguish two levels at which Rédei's argument challenges Bayesianism. From the point of view of philosophy of physics, the violation of a rationality constraint undermines the interpretation of quantum states as degrees of belief. From the more abstract point of view of philosophy of probability, it blocks the extension of Bayesian probability theory to general (noncommutative) spaces of events. I will argue, however, that both these problems can be solved.

First, I will show that Rédei's noncommutative conditionalization rule is not the proper quantum generalization of the Bayes rule. Moreover, since it encompasses only non-selective measurements and cannot account for selective measurements, the evidence his objection relies on does not correspond to the usual information-theoretical notion, as it is far from clear what information should constitute evidence for the agent-observer if no outcome is actually selected by the measurement. On the other hand, conditionalizing on non-selective measurements does change a quantum state, hence it enacts a genuine revision of degrees of belief. So, the Bayesian interpretation would not be out of the woods yet. I will demonstrate that, even in this broad sense of evidence, one cannot obtain the same evidence twice in quantum mechanics. This means, in the last analysis, that the stability condition cannot be applied at all, and therefore one cannot claim its failure.

Finally, I will address the second problem stemming from Rédei's argument by pointing out that a noncommutative conditional expectation, although different from the one he considered, can be always constructed. Interestingly, and equally importantly, such map can be also proven to satisfy the stability condition whenever the latter is applicable, thus completing the extension of Bayesian statistical inference to general probability theory.

Mark van Atten (IHPST(CNRS/Paris I/ENS))

Phenomenology and Transcendental Argument in Mathematics: The Case of Brouwer's 'Bar Theorem'

On the intended interpretation of intuitionistic logic, a proof of a proposition of the form p ---> q consists in a construction method thattransforms any possible proof of p into a proof of q. This involves the notion of the totality of all proofs in an essential way, and this interpretation has therefore been objected to on grounds of impredicativity, notably by Gödel. In fact this hardly ever leads to problems as in proofs of implications usually nothing more is assumed about a proof of the antecedent than that it indeed is one, and this assumption does not require a further grasp of the totality of proofs.

The prime example of an intuitionistic theorem that goes beyond that assumption is Brouwer's proof of the `bar theorem': For every tree x, if x contains a decidable subset of nodes such that every path through the tree meets it (a `bar'), then there is a well-ordered subtree of x that contains a bar for the whole of x. Instantiated with an arbitrary tree t, this proposition takes the form P(t) ---> Q(t). Brouwer's proof of the bar theorem mainly consists in an analysis of the inner structure that any proof of P(t) must have, where proofs are taken to be primarily mental objects. So here Brouwer engages in phenomenological reflection by considering the acts in which we think about bars. From that analysis he obtains the information from which to construct a proof of Q(t).

In this talk I will argue that in his proof, Brouwer circumvents the problem of impredicativity by resorting to a transcendental argument based on phenomenological description. A transcendental argument is here understood as an argument of the form: “We have mental experience E, it is a necessary condition for having this experience that P, therefore P”. I will explain why this type of argument was natural on Brouwer's notion of constructivity, but never on Gödel's (as expounded in his publications). Finally, I will consider how such a transcendental argument fits into Husserl's phenomenology (which philosophy Gödel came to adopt).

Maarten Van Dyck (Ghent University)

The Historical A Priori: The Case of Inertia

Michael Friedman’s work has been very instrumental in reviving the idea that the Kantian synthetic apriori might still hold promises as an interesting concept for analyzing scientific theories. One of the main post-Kantian evolutions has of course been the necessity of relativizing the notion, as we can no longer hold on to the idea of fixed universal apriori structures. Yet, historicizing the apriori is not without its puzzles. Most importantly, it becomes hard to see wherein to ground the normative force of the constitutive principles. I will try to tackle this issue, which is not unrelated to some of the conundrums surrounding the Kuhnian notion of incommensurability, by analyzing the evolution of the principle of inertia in the seventeenth century. This principle is of course one of the prime examples of a constitutive principle, both for Kant himself and for Friedman, so a detailed historical analysis might help us in understanding how to think the notion of a historical apriori.

I will show how the forerunner of our principle of inertia arises in Galileo Galilei, where it finds its ground in the intersection of thinking on cosmological issues and the mechanical theory of machines. Especially the second – mechanical – component is of great importance, and has been almost completely neglected in all studies in the origin of inertia. I will argue that an inertia-like principle first acquires a truly constitutive role in Galileo’s successful treatment of the inclined plane, where it serves to delineate the relevant physical system. Moreover, it can only acquire this role because of a new regulative ideal of nature, which crucially links natural behaviour with conservation principles in Galileo’s mechanics. I will then sketch the evolution from Galilean inertia to its full-blown enunciation in Newton’s first law. I will again stress that we cannot neglect the properly speaking mechanical nature of the principle, which, although detached from all thinking about machines, is still the heir of a new way of understanding what constitutes a physical system that had its origin in the theories about machines. We must thus understand the extrapolation from this limited mechanical context to a grand cosmological principle as made possible by the new regulative ideal that found its first expression in the work of Galileo. The important post-Galilean evolution is that Newton found out how to characterize dynamical equilibrium systems (by formulating his three laws of nature which taken together allowed him to define such systems), thus significantly broadening the scope for conservation in nature.

This stress on the role of a regulative ideal of nature provides part of the answer for the question about the grounds for the normative power of a historical apriori. We can only properly analyze the functional role of constitutive principles in delineating physical systems if we take into account the guidelines provided by such a regulative ideal. This implies, among other things, that we must replace the grounding of constitutive principles in the constitution of the human mind with a renewed attention for their origin in particular practices of explanation, such as could be found e.g. in the mechanical science of machines.

Peter Vickers (University of Leeds)

Bohr's Theory of the Atom: Content, Closure and Consistency

Philosophers and historians have made a habit of referring to a collection of theories in science and mathematics as inconsistent. Bohr's theory has been widely cited as the example par excellence of an internally inconsistent theory (Lakatos, 1970; Brown, 1992; Priest, 2002; da Costa and French, 2003). But when we ask how exactly the inconsistency manifests itself, the literature to date provides no rigorous answer. From several papers which give an informal reconstruction, three different foci of alleged inconsistency emerge: (i) The fact that electrons follow periodic, continuous trajectories, but jump mysteriously between orbits; (ii) The fact that electrons in stationary states obey Coulomb's law but do not radiate; (iii) The fact that the orbits are strictly non-classical, but the radiation interacting with the atom is treated classically. However, clearly there is quite a distance between these three aspects of the theory and the definition of 'inconsistent' provided by logicians. From an inconsistent set of assumptions we would expect a contradiction, an 'A&~A', to follow – if not logically then by some other type of truth-preserving consequence (e.g. analytical or mathematical). But no such contradiction is demonstrated by the authors noted.

Ultimately the only questions we need ask to establish the consistency of a set of assumptions are as follows: (a) What are the assumptions (what is the content)? (b) What types of consequence are legitimate (how may we 'close' the theory)? The problem lies in the widespread disagreement amongst philosophers on both of these issues. In particular I focus on (a), since the three inconsistency claims noted primarily depend on what is made a material part of the theory. My claims, in short, are as follows: (i) is not an inconsistency, because the theory specifies mutually exclusive contexts of application for the conflicting principles; (ii) is not an inconsistency, because the theory can take on board electrostatics whilst contradicting electrodynamics (Bartelborth, 1989); (iii) is not an inconsistency, because the classical treatment of radiation is best thought of as an approximate, non-fundamental treatment. Any decisions of theoretical content are based in the commitments of the community, as is usual. Other possible units of analysis may count as inconsistent, but then they are not really theories (Belot, 2006).

If I am right there are some important consequences. For example, claims made by paraconsistent logicians that Bohr's theory presents itself as an application for their logics can be questioned. But most seriously, in the absence of inconsistency, it is left to the philosopher of science to provide a characterisation of the theory's conceptual problems which does justice to the protestations and misgivings which have accompanied it from the very beginning. In conclusion I indicate a way forward in this regard, presenting some options from the existing literature on conceptual problems (Laudan, 1977; Newton-Smith, 1981; Darden, 1991).

Marion Vorms (IHPST)

Understanding Theories: Formats Matter

In this paper, I construe scientific understanding not only as understanding the phenomena by means of some theoretical material (theory, law or model), but more fundamentally as understanding the theoretical material itself that is supposed to explain the phenomena. De Regt and Dieks (2005) emphasise the contextual aspects of the intelligibility of theories, showing that it depends on their "virtues", on the historical standards of intelligibility, and on the particular "skills" of their users. My paper aims at continuing this proposal, first by giving a more precise definition of one's understanding of a theory and then by emphasising the importance, for this issue, of the particular formats in which a theory is expressed and hence grasped by its users. To defend this, I take the example of the versions of classical mechanics (variational versus vectorial) and the various formats of representation of its main principles and models.

What does "understanding a theory" mean? At first sight, we could say that it amounts to having a clear view of the logical relations between its core principles and theorems. This kind of understanding, though global, is quite abstract: one can understand the logical structure of a theory without being able to connect it to the phenomena. Moreover, this definition depends on how one construes the structure of theories: it will vary according to whether one defines theories as logical sets of statements with interpretative rules (following the “syntactic conception” of theories) or as families of models (“semantic conception”). I thus suggest that there is another sense of “understanding a theory” that itself has two aspects. To understand a theory, one has to understand both what the theory says or means and how it works; in other words, one has to grasp the phenomena by means of its conceptual apparatus (representational aspect) and to be able to manipulate it and make it fit the phenomena (computational aspect).

I claim that these are essentially contextual and practical matters, and that the particular format in which the theoretical content is displayed is crucial to them. Following Humphreys' proposal (2004), I claim that one never accesses to a theory as a whole. Be it a set of statements or a class of models, in practice, it is always displayed in some particular equations, statements, images, graphs, diagrams. Humphreys' proposal of the notion of “template” to complement the classical “units of analysis” of science, like theories and models, may be a good candidate to study the relationship between the representational and computational aspects of understanding: a template is a “concrete piece of syntax” (most of the time an equation, but I suggest that Humphreys' claim could be extended to other formats) that has both a representational and computational function. With the example of classical mechanics, I show how these two functions are interrelated and, as Humphreys suggests, sometimes in tension with each other. Adressing these issues by focusing on the particular formats that are dealt with in practice may enlight this problematic relationship.

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Ioannis Votsis (University of Düsseldorf)

Making Contact with Observations

A stalwart view in the philosophy of science holds that, even when broadly construed so as to include theoretical auxiliaries, theories cannot make direct contact with observations. This view owes much to Bogen and Woodward's (1988) influential distinction between data and phenomena. According to them, data are observable whereas (physical) phenomena are unobservable. Theories only talk about the latter. As they stress, “…data typically cannot be predicted or systematically explained by theory” (pp. 305-306). Following Bogen and Woodward, various philosophers (e.g. Prajit K. Basu (2003), Stathis Psillos (2004) and Mauricio Suárez (2005)) claim that for observations or data to be of use in theory testing, they first need to be transformed into evidence via the introduction of theoretical vocabulary. This prevents any direct observational assessment of theories. In this paper I argue contrary to this view that at least in some cases we can derive observation statements straight from the theory. In so doing I utilise a rather well-known scientific controversy between Antoine Lavoisier and Joseph Priestley.

The Lavoisier-Priestley controversy concerns two conflicting results emanating from what appears to be the same experiment. Both scientists agreed that observationally the experiment resulted in the production of a given quantity of a particular kind of black powder. Yet neither of their respective theories of oxygen and of phlogiston spoke of the presence of such a black powder. In both cases, the raw observational data first had to be theoretically treated. For Priestley, when iron was heated in dephlogisticated air it led to the production of iron calx. For Lavoisier, the heating of iron in oxygen led to the production of iron oxide. Yet, the presence of iron calx is only entailed by the phlogiston theory and the presence of iron oxide is only entailed by the oxygen theory. In other words, the same observation (i.e. the presence of the black powder) is theoretically transformed as two different evidential statements, each only confirming its respective theory.

Prima facie this case seems to support the Bogen and Woodward inspired view that theories do not make direct contact with observations. A more sustained examination however reveals that all one needs is a theoretical auxiliary of the form ‘observation x implies evidence y’ to secure a sufficiently direct link between observation and theory. In the historical case at hand theoretical auxiliaries of this form are already available. This much is admitted by Basu (ibid., p. 361), though he claims that even when we include such auxiliaries in the respective theories, i.e. when we take a broad construal of the theories, the two scientists cannot derive the relevant observation statements. This is so, Basu argues, because the converse auxiliaries are needed, i.e. something of the form ‘evidence y implies observation x’. Contra Basu, I show that the original auxiliary is sufficient to establish an auxiliary of the form ‘evidence y implies a disjunction one of whose disjuncts is an observation x’ and that this auxiliary allows the theory to make direct contact with observations.

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Carl Wagner (University of Tennessee)

Old Evidence and New Explanation

A basic principle of scientific inference asserts that if hypothesis H is known to imply the less-than-certain proposition E, the subsequent discovery that E is true confirms (i.e., raises the probability of) H. There is a straightforward Bayesian account of such confirmation, for from $p(E|H)=1 > p(E)$ it follows immediately that $p(H|E)>p(H)$. This probabilistic account of the hypothetico-deductive principle is perhaps the simplest of what John Earman calls the "success stories" of Bayesian philosophy of science.

Suppose,however, that we first attain certainty regarding E and subsequently discover, quite apart from this certainty, that H implies E. There are numerous examples of this in the history of science, perhaps the best known of which is Einstein's explanation of the previously observed "anomalous" advance in the perihelion of Mercury in terms of the general theory of relativity. Just as it does when explanation precedes observation, this explanation of the previously known fact E by the hypothesis H ought to confirm H, but how? This problem was first posed by Clark Glymour, who called it the "old evidence problem." As Glymour noted, conditioning the prior p here on E is otiose since $p(E)=1$, and so $p(H|E)=p(H)$. In any case, such conditioning is simply not to the point, since what is required is a revision of p based on the discovery that H implies E(explanation), not the discovery that E is true(observation).

Accordingly, one proposed solution, due to Daniel Garber, extends the algebra on which probabilities are defined to include the proposition $H \Rightarrow E$ that H implies E. Under certain conditions, which have been criticized by Earman in his book, Bayes or Bust, as unrealistic, it can be shown that $p(H|\ H \Rightarrow E\ ) > p(H)$. On the other hand, Richard Jeffrey proposed a different solution that retains the original algebra, but revises probabilities by an entirely new method called "reparation." Central to Jeffrey's approach is the imaginative reconstruction of a probability distribution u that predates both our certainty about E and our discovery that H implies E. The explanation-based revision of this "ur-distribution" u then serves as a paradigm for the explanation-based revision q of p, for which it is easily seen that $q(H)>p(H)$.

In this talk I will show how Jeffrey's solution can be generalized in a natural way to cases in which observation raises our confidence in E without rendering it certain, and the subsequent explanation afforded E by H is probabilistic rather than implicational, and I delineate the intuitively reasonable conditions under which H is confirmed by the proffered generalization of reparation. I also show that no Garber-type approach is capable of reproducing the results of generalized reparation.

Erik Weber (Ghent University)

Social Mechanisms, Causal Inference and the Policy Relevance of Social Science

My starting point is Daniel Steel's article "Social Mechanisms and Causal Inference" (in Philosophy of the Social Sciences 34 (2004), pp. 55-78). To put it roughly, I think that what Steel says about the main topic of his paper (the role of mechanisms in solving the problem of confounders) is correct. But he also claims that he can avoid the conclusion that social mechanisms are necessary for causal inference in the social sciences. Steel distinguishes between the following claims (2004, pp. 60-61):

(M) X is a cause of Y if and only if there is a mechanism from X to Y.

(M*) One knows that X is a cause of Y only if one can identify at least one mechanism from X to Y.

Steel accepts the ontological claim (M) but rejects the epistemological claim (M*). Again, I do not have any quarrel with that. What then are the aims of my paper?

My first aim is to show that there is a difference between reliable causal inference (i.e. providing a good argument for a causal claim) and showing that a causal claim has policy relevance. I will clarify this difference and show that we need social mechanisms in order to establish the policy relevance of causal claims.

The second aim is to show that, though (M*) is false, that is not much of a consolation for researchers in the social sciences: in most research contexts, they will find out that they do need mechanisms in order to make reliable causal inferences. In other words, my second aim is to show that there is a tension between the falsity of the philosophical principle and actual scientific practice, because practice seems to support it.

Charlotte Werndl (University of Cambridge)

Mathematical Definitions that Capture Real-World Phenomena or Features: On the Formation and Justification of Definitions

In mathematics definitions are not only required to be eliminable and non-creative. Mathematicians typically have good reasons for studying a definition, and these reasons often make clear how this definition has been formed. Furthermore, finding a proper definition is often regarded as a considerable advance in mathematical knowledge.

These considerations motivate the following general questions: how are definitions in mathematics formed and justified? And is this formation and justification of definitions reasonable, i.e. rational?

Lakatos (1976, 1997) coined the term 'proof-generated definition', which is a definition that is formed in order to be able to prove a specific conjecture. His main example of a proof-generated definition is the definition of polyhedron, which he argued was formed in order to be able to prove the conjecture that for a polyhedron the sum of the vertices minus the edges plus the faces equals 2. However, apart from this idea, there is rather little on the above questions in the philosophy literature.

I will treat those questions for the case study of definitions of deterministic chaos, which are part of ergodic theory and topological dynamics (cf. Berkovitz at al. 2006; Frigg 2004; Lichtenberg and Liebermann 1992, Robinson 1995). Here the picture seems different to the one Lakatos draws. Moreover, one might plausibly hope that this analysis leads to a deeper understanding of chaos (cf. Smith 1998, chapter 10), a notion of crucial importance in the sciences.

For the case of chaos I will discuss the main kinds of formation and justification of definitions next to proof-generation. They are: first, to form or justify a definition because it captures a preformal notion constituting an important property in the world or an important property found in mathematical and physical models; second, as has not been identified before, to form or justify a definition because it corresponds to a natural mathematical condition. I will argue that these kinds of concept formation and concept justification are reasonable when properly applied. The way definitions are formed and justified is often not explicitly stated but implicit 'in the mathematics'.

These results show that Lakatos's (1976, 1997) strong emphasis on proof-generated definitions is not warranted. My research illustrates that in nearly all cases various types of formation and justification of definitions play a role.

Yet for chaos we also find a few misguided arguments for definitions and an undesirable lack of justification of definitions. I will conclude that while the majority of the definitions of chaos have been formed and justified in a reasonable way, it still happened too often that definitions were not properly motivated.

Finally, I will argue that, to some extent, the abstract reflection on formation and justification of definitions helps understanding chaos.

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Torsten Wilholt (Bielefeld University)

Values in Science and the Problem of Bias

Bias is more and more recognized as a serious problem in many areas of scientific research, especially in private research (e.g. drug testing) and in policy-related areas (e.g. climatology). But how exactly should one describe and define the phenomenon of bias, and characterize it as a shortcoming of the research in question? I will first propose a prima facie plausible characterization of bias in terms of inductive risk. In testing a hypothesis, a lower risk of committing a false positive error can often be traded off against a higher risk of committing a false negative (or vice versa), by altering problem selection, experimental design, data analysis or even one’s practices of disseminating and publishing results. Bias can then be regarded as a researcher’s failing to be impartial between the two kinds of risk, and allowing her different attitudes with regard to the desirability of a positive or negative result to influence the set-up of the test or even the whole research project.

However, this analysis of bias faces a serious problem. From the times of C. West Churchman on, philosophers of science have again and again argued that there is no non-arbitrary and convincing way to mark out any particular balance between the two types of inductive risk as the correct or “impartial” one. Researchers who test hypotheses will always have to evaluate the consequences of errors (as well as the consequences of getting it right) in order to make their methodological choices. A researcher testing the toxicity of a food additive will and should typically strike a different balance between the two kinds of inductive risk than a scientist contributing another experimental probe to the ongoing discussion of some academic hypothesis.

Against this background, it is prima facie not possible to distinguish cases of diverging judgements concerning the evaluation of consequences from cases of different “biases” as defined above. It might therefore seem that often (viz., in cases that don’t involve outright deception), to speak of bias is merely to express one’s disagreement with the particular kind of value-judgement concerning the consequences of error that must have been applied in the respective case. In a way, bias-talk would thus be revealed as involving the charge of a moral shortcoming rather than an epistemic one. However, one need not rest with this counter-intuitive conclusion.

The solution, I will argue, is to consider scientifc practices (as governed by methodological conventions) as some kind of social institutions. Standards of experimental design, data analysis, and the like, are often highly conventional. Such conventions often imply a certain balance between types of inductive risk and thereby an implicit evaluation of the consequences of error. They can differ from discipline to discipline and even from one type of research institution to another. They are nevertheless not arbitrary, because they serve the social purposes of organized science. Conventional standards in scientific research permit other actors to develop differentiated attitudes of trust towards different kinds of institutionally sanctioned scientific “results”. Bias often involves deviation from conventional standards and thereby disrupts this trust. Bias thus comes out as an epistemic inadequacy under the wider perspective of social epistemology.

John Worrall (London School of Economics)

Do we Need some Large, Simple Randomized Trials?

A number of arguments have convinced nearly all of the medical community that randomized controlled trials (RCTs) provide (at least) the most telling, most scientifically weighty evidence for the efficacy of any treatment and that other forms of evidence -for example, from “historically controlled trials” in which the controls are provided by “equivalent” patients treated under the earlier regime- are inevitably less weighty. I have surveyed these arguments (e.g., Worrall (2002) and Worrall (2007)) and found all but one to be highly problematic.

This argument -the only one which Bayesians directly endorse- is the one from “selection bias”. Randomization (when performed in the normal way) sees to it that the clinician has no influence over the arm of the trial to which a particular patient is assigned. Where the clinician does, on the contrary, have such influence the outcome may be affected by his or her choices and give a false reflection of the true effectiveness of the therapy. Although it is another argument (the one that claims that randomization controls for all possible “confounders”, known and unknown) that has carried the greatest weight sociologically speaking, it is to this argument from selection bias that the most acute defenders of randomization tend ultimately to give priority (Doll and Peto (1980), Peto et al (1988)).

However Doll, Peto and others admit that it is implausible that selection bias should produce anything like a large (apparent) effect in a non-randomized study. But they also insist that most new treatments in medicine nowadays are themselves likely to have only small (one hopes positive) effects. Such small improvements are not to be scoffed at since a small improvement for a very common disease may end up saving many more lives than a treatment with major effect on a disease that is extremely rare. They make this the basis for their plea for at least some (very) large, simple and randomized trials -they need to be large because the effect concerned is likely to be small, simple so that only one question is asked and so all the sample count as evidence for the answer to that one question , and randomized because the trial must be sensitive enough to pick up a small effect, which the selection bias involved in non-randomized trials might obscure. Although this sounds like a very plausible position it in fact brings into focus a number of both epistemological and practical issues. In particular it focuses attention on the relatively under-emphasised (epistemic) issue of “external validity”; (generalisability to the “target population”) and on the (practical) issue of whether such small effects are really worth having once a more balanced and encompassing view of “the” outcome of treatment is adopted.

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Stuart Yasgur (London School of Economics)

The Money Pump and the Justification of the Transitivity Condition

Rationality is said to require that agents have transitive preferences. This so-called transitivity condition is widely thought to be justified by the money pump argument. In this paper I argue that there are serious reasons to doubt this widely held view.

More specifically, I argue that the money pump argument does not justify the transitivity condition based on any of the various forms of arguments associated with it in the literature, including those based on: rational choice, matters of definition, reductio ad absurdum arguments, and/or consequentialist considerations.

Further, I suggest answers to two questions raised by my arguments: If the money pump does not justify the transitivity condition, why has it remained so prominent in the literature? Is the transitivity condition a requirement of rationality?

Jesús Zamora (Spanish National Open University, UNED)

What Game Do Scientists Play?

Scientific research is reconstructed as a language game using some insights from argumentation theory and Robert Brandom's inferentialism. Researchers' main goal is assumed to be that of persuading their colleagues of the validity of some claims, and the assertions that each scientist is allowed or committed to make depend on her previous claims and on the inferential norms adopted in her research community. The most relevant types of inferential rules governing such a game are classified, as well as some of the ways in which this approach can be used for the (both epistemic and social) assessment of scientific knowledge and scientific practices. It is argued that this language-game approach offers an optimal combination of the insights from 'social constructivist' theories of science and those of more traditional, even 'positivist' accounts of the rationality of science, in the following sense: social competition according to those rules of the game that 'merit-seeking' scientists would prefer most will tend to make scientists act in a way consistent with the maximal satisfaction of sound epistemic goals (at least under some reasonable institutional constraints). The most important conclusion of the paper is that the language-game approach offers a way of rationally discussing the institutional design of science in which epistemic and social goals can be taken into account.

Antonio Zilhao (University of Lisbon)

Incontinence, Honouring Sunk Costs and Rationality

Davidson's account of the possibility conditions of incontinent action renders continent action materially unfeasible. The very idea of incontinent action becomes thus a hollow one. In the first part of my paper, I'll present an alternative description of these possibility conditions that renders continent action materially feasible, and both, continent and incontinent action, cognitively plausible. In a nutshell, my proposal rests upon the drawing of an essential cognitive contrast between explicit processes of deliberative reasoning and lower level heuristic procedures.

In the second part of my paper, I'll argue that my proposal is explanatorily useful. I'll do this by showing how it can be applied to account for an intriguing but well documented cognitive phenomenon. This is the following.

According to a basic principle of rationality, the decision to engage in a course of action should be determined solely by the analysis of its consequences. Thus, considerations associated with previous use of resources should have no bearing on an agent's decision-making process. Frequently, however, agents persist carrying on an activity they themselves judge to be nonoptimal under the circumstances because they have already allocated resources to that activity. When this is the case, agents are said to be honouring sunk costs. Honouring sunk costs is thus typically viewed as irrational economic behaviour.

The considerations above notwithstanding, it is not impossible to devise rational justifications for behaviours of honouring sunk costs. One such a justification is that this behaviour might result from the triggering of a lower level cognitive mechanism aimed at teaching agents not to waste scarce resources. Given the fact that such a teaching would use the experience of the unpleasant consequences of past careless decisions in order to make agents improve their own future decision-making, the mechanism generating it would actually be driven by a rational care not to waste precious resources in the future rather than by an irrational concern with resources irretrievably lost in the past. Therefore, this pattern of behaviour would make good evolutionary sense.

I'll review some evidence supporting this interpretation for at least some cases of behaviours typically regarded as instantiations of honouring sunk costs. Particularly striking in the psychological literature is, however, the finding that the sunk cost effect is, in general, not lessened by having taken previously courses in economics. This fact notwithstanding, agents who did take such courses seem to be frequently aware of a cognitive dissonance in their behaviour.

I'll conclude the paper arguing that my redescription of incontinent action accounts for this cognitive dissonance effect better than the alternatives and that viewing at least an important subset of behaviours of honouring sunk costs this way will enable us to consider them as peculiar manifestations of a more general and meaningful pattern in human behaviour.

Henrik Zinkernagel (University of Granada)

Causal Fundamentalism in Physics

Norton (2003) has recently argued that causation is merely a useful folk concept and that it fails to hold for some simple systems even in the supposed paradigm case of a causal physical theory – namely Newtonian mechanics. The purpose of the present contribution is to argue against this devaluation of causality in physics. I shall try to defend not only that Norton's charges against causality in Newtonian mechanics are flawed but also that the central causal message of Newtonian mechanics may proliferate into its supposed successor theories, namely special (and to some extent general) relativity and quantum mechanics. My main argument is that Norton's (2003) alleged counterexample to causality (all events have causes) within standard Newtonian physics fails to obey what I shall call the causal core of Newtonian mechanics (essential parts of the first and second law). More specifically, I argue, Norton's example is not in conformity with Newton's first law – and his attempt to reformulate this first law (in order to make it conform to his example) results in an impoverished theory which lacks central physical features present in Newtonian mechanics. In particular, in Norton's version of mechanics, the close connection between the first law and the notion of time is lost and, moreover, the physical justification of the crucial notion of inertial frames is lacking. A second and more contentious argument which I shall only sketch is that, on a plausible relationist account of time, the causal core of Newtonian mechanics may play a fundamental role also in the theories of relativity, and perhaps also in quantum mechanics.

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