EPSA11

Third Conference of the
European Philosophy of Science Association

Dept of Philosophy and History of Science
University of Athens
Greece

5–8 October 2011

EPSA11 is financially supported by

the University of Athens
the Dept of Philosophy and History of Science of the University of Athens
the Welfare Foundation for Social and Cultural Affairs
the Foundation for Education and European Culture-Nikos and Lydia Tricha
the Springer Publishers
the European Philosophy of Science Association (EPSA).

Editor: Maria Panagiotatou

Contents

Committees

Programme Committee

Dennis Dieks, Utrecht (Chair)
Kristina Rolin, Helsinki (Chair)

Hanne Andersen, Aarhus
Guido Bacciagaluppi, Aberdeen
Mieke Boon, Enschede
Marcel Boumans, Amsterdam
Jeroen Van Bouwel, Ghent
Luc Bovens, London
Anastasios Brenner, Montpellier
José Díez, Barcelona
John Dupré, Exeter
Uljana Feest, Berlin
Carla Fehr, Ames
Maarten Franssen, Delft
Heidi Grasswick, Middlebury
Ladislav Kvasz, Bratislava
Holger Lyre, Magdeburg
Inmaculada de Melo-Martín, New York
Thomas Müller, Utrecht
Ilkka Niiniluoto, Helsinki
Johannes Persson, Lund
Demetris Portides, Nicosia
Maria Rentetzi, Athens
Thomas Reydon, Hannover
Stéphanie Ruphy, Aix-en-Provence
Sonja Smets, Groningen
Miriam Solomon, Philadelphia
Friedrich Steinle, Berlin
Karin Verelst, Brussels
Erik Weber, Chent
Greg Wheeler, Lisbon
K. Brad Wray, Oswego
Henrik Zinkernagel, Granada

Local Organising Committee

Stathis Psillos (Chair)
Theodore Arabatzis (Vice-Chair)
Dionysios Anapolitanos
Costas Dimitracopoulos
Vassilis Karakostas
Vasso Kindi
Vassilis Sakellariou
Andreas Vourtsis

EPSA Steering Committee

Friedrich Stadler, Vienna (President)
Mauricio Suárez, Madrid (Vice-President)
Theodore Arabatzis, Athens
Henk W. de Regt, Amsterdam
Steven French, Leeds
Maria Carla Galavotti, Bologna
Gürol Irzik, Istanbul
Miklos Redei, London
Kristina Rolin, Helsinki
Matti Sintonen, Helsinki

Preface by the EPSA President

The European Philosophy of Science Association (EPSA), founded officially in 2007 with the aim of “bringing together professional philosophers of science and students from all over Europe (and the rest of the world) and fostering collaboration and exchange of ideas amongst them”, is now organising its 3rd international conference, which will be taking place in Athens (Greece) this time. After two successful conferences –one in Madrid (2007) and the other in Amsterdam (2009)– we are pleased to welcome the three invited lecturers, along with 211 excellent speakers, who were selected from 400 submissions by the International Program Committee. On behalf of the Steering Committee I would like to express my gratitude to its 33 members and especially to both Chairs, *Kristina Rolin* (Helsinki) and *Dennis Dieks* (Utrecht), who took on the difficult responsibility of conducting this refereeing process, in which they unfortunately also had to reject a number of excellent submitted papers given the high quality level and infrastructure of our conferences. The work of the Local Organising Committee (LOC) and its staff, headed by *Stathis Psillos* together with *Theodore Arabatzis,* proved specially challenging in view of the ongoing political and economic crisis in Greece. The representatives of EPSA never had any doubts about the realization of this event, even if the circumstances were –and still are– not always supportive. In this connection I would like once more to express my conviction that the EU is not solely an economic, but also a political and cultural union. As such it should be committed to the advancement and enhancement of the scientific community and its younger generation –and this is an obligation for the present and future philosophers all over Europe.

Since its inception EPSA has shown a promising development. Apart from the biennial conferences followed by the publication of the proceedings in a special series –hopefully to be continued 2013 in Helsinki– our publisher Springer also launched the first volume of its *European Journal for Philosophy of Science*. This is another step towards improving the research and the international visibility in the philosophy of science. The reliable and professional work of the editorial team with seven colleagues chaired by *Carl Hoefer* (Barcelona) and *Mauro Dorato* (Rome) and the 31 members of the Editorial Board deserve special mention here. Additionally, in the spring of this year EPSA issued the first *EPSA Newsletter* as an electronic forum for our members. It features reports on the past conferences in Madrid and Amsterdam, the (Pre)History of EPSA, the *EJPS*, and on the cooperation with the ESF Research Networking Program “Philosophy of Science in a European Perspective” (PSE). In this context it should also be mentioned that EPSA has decided to sponsor a special plenary lecture by *Philippe Mongin* (Paris) at the 14th Congress of Logic, Methodology and Philosophy of Science in Nancy (F) July 2011 in order to be also globally present. In this regard we are pleased to have achieved an agreement on a joint membership with our sister society in North America, the *Philosophy of Science Association* (PSA), which testifies to the transatlantic cooperation and interaction. (By the way, I am happy to see that a women’s caucus meeting will be taking place for the first time as a reflection of a policy sensitive to gender issues.) The most recent *Newsletter* includes a note from our publisher Springer, who –according to our contract– is for the first time sponsoring a plenary Springer Lecture in the Philosophy of Science which *Nancy Cartwright* (LSE) will be delivering here in Athens.

Taking all of this into account, I am truly confident that EPSA is on the right track to promoting philosophy of science in Europe as well as European philosophy of science by connecting individuals and institutions and by backing new trends and perspectives in the philosophy of science in the broader sense from the natural, formal to the social and cultural sciences in its current and historical contexts. The dynamic scene in this field alone seems to be an encouraging backdrop for all these joint efforts and yet another manifestation of the rich European tradition and innovative spirit.

Let me conclude by thanking everyone who contributed to this exciting scholarly enterprise and to those who enabled this conference to take place just like here in Athens where the cradle of European philosophy emerged some 2,500 years ago and is now to be continued into the 21st century.

Friedrich Stadler
President of EPSA

Preface by the LOC Chairs

Dear Conference Participant,

On behalf of the Local Organising Committee, we welcome you to the 3rd Conference of the European Philosophy of Science Association here in Athens, Greece. The EPSA conferences have now become established; they constitute a forum where philosophers of science from around the world meet and engage in a lively exchange of ideas and arguments. We have had to compete with two excellent previous conferences in Madrid in 2007 and in Amsterdam in 2009. We have done our best to put together a first-rate event and we trust that you will enjoy this conference, both intellectually and socially.

The very idea of a philosophy conference in Athens is quite daunting. 2500 years ago, not far from the location of the conference, the very intellectual enterprise we love and pursue had its defining moment. The first schools of philosophy, and the first predecessors of philosophy conferences (known as symposia), took place in this very city. There is a sense in which philosophy has come home. Naturally, modern Athens is very different from the town of Socrates, Plato and Aristotle. Yet, their intellectual heritage—the idea that philosophy is conducive to intellectual flourishing—is pretty much alive among the members of the Greek philosophical community.

Greece is currently going through a dramatic economic crisis which has substantially affected the Greek Universities. For us, the success of this conference would be a proof that the Greek philosophical community can stand up to serious challenges and bring to the fore the positive and creative forces in the Greek Academia.

Sadly, a few weeks before the conference, the young talented philosopher Joshua Haddock, who had his paper titled *The Principal Principle, and Theories of Chance: An Account of Primitive Conditional Chance* accepted for the conference, died in a climbing accident. We offer our deepest sympathy to his family.

This conference has been made possible thanks to the hard work of the members of the LOC and a number of volunteers. We thankfully acknowledge the financial assistance of the University of Athens, the Dept of Philosophy and History of Science of the University of Athens, the Welfare Foundation for Social and Cultural Affairs, the Foundation for Education and European Culture-Nikos and Lydia Tricha and the Springer Publishers.

Welcome to Athens EPSA11

Stathis Psillos (Chair of LOC)
Theodore Arabatzis (Vice-Chair of LOC)

Practical Information

Public transport to conference location

The **Hotel Titania** is located at the heart of Athens at 52 Panepistimiou Avenue.

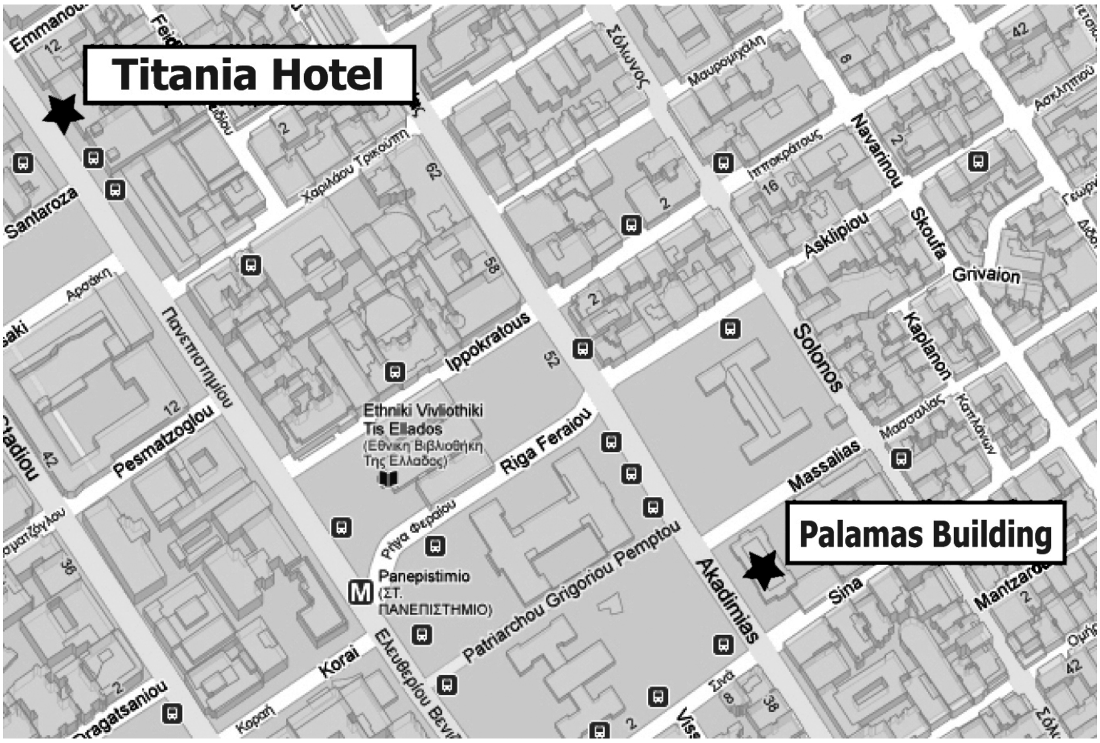
From the Athens International Airport (Eleftherios Venizelos)

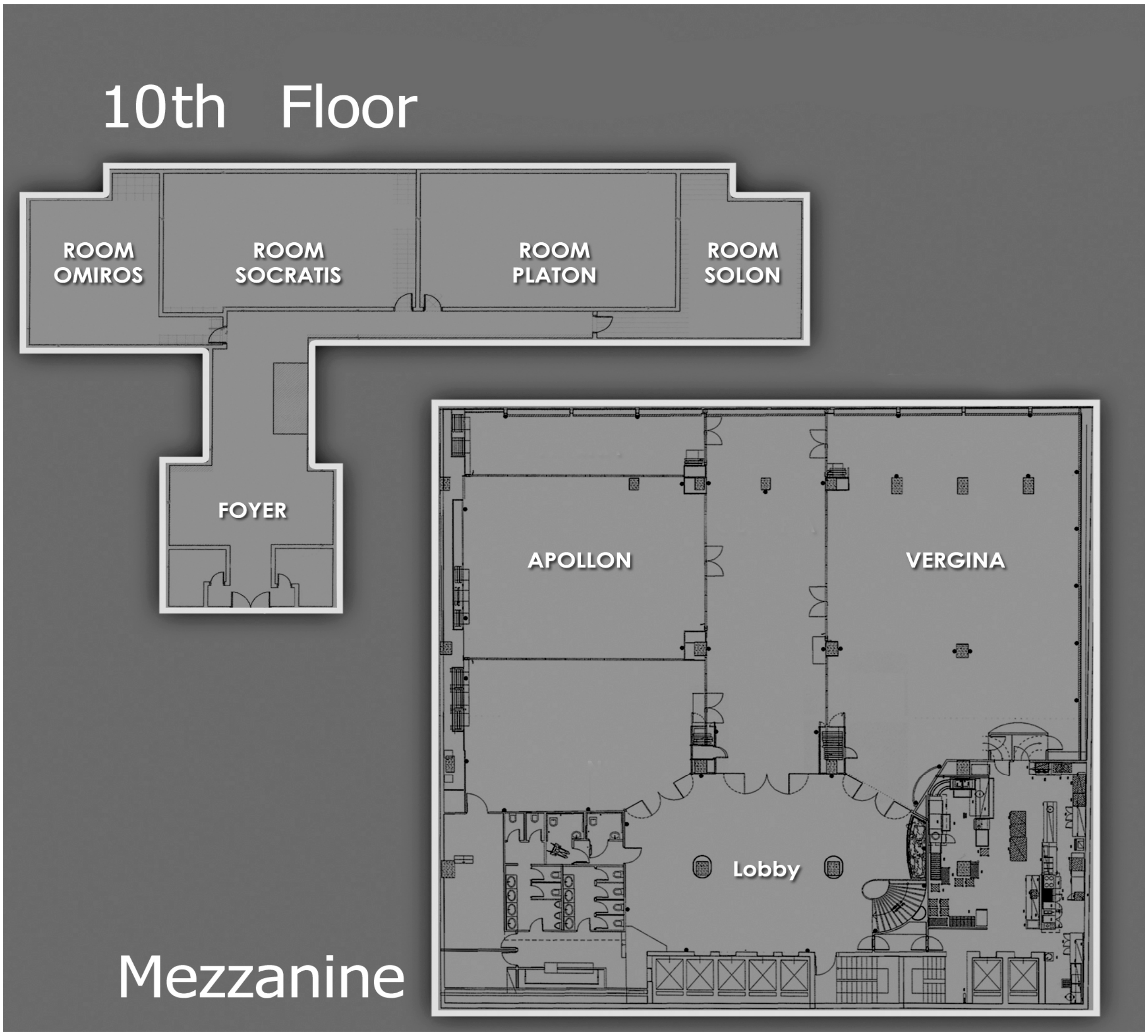
– **Taxi:** There is a taxi rank available outside the arrivals hall. A ride to the centre of Athens costs 35 Euros (this is a fixed price including the tolls). Taxis are easy to get, but unless you carry very heavy luggage or have someone else pay for your taxi ride, it might well be preferable to use the metro or the bus. Note that the tariff should show '1', except after midnight and until 5am when the tariff is '2' (the price then is 50 Euros). Always get a taxi from the official taxi rank at the airport and ask for a receipt. Tipping is optional but very welcome.
– **Metro:** Follow the signs to the Metro station at the airport. There is a metro to the city centre (blue line/line 3) every half hour (on the hour and half past the hour). It takes roughly 45 minutes and costs 8 Euros (14 Euros for a return ticket.) Get off at Syntagma station. Then change to the red line/line 2 towards Aghios Antonios and alight at Panepistimio station (just one stop after Syntagma). Note that you should validate your ticket before entering the metro cars. Alternatively you can get off at Syntagma Station and walk down Panepistimiou Avenue for about 800 meters.
– **Suburban Railway:** Next to the Metro Station at the Airport. You can get off at Plakentias station and get on the metro (blue line/line 3) to Syntagma (direction Egaleo). Not really recommended, given the availability of a direct metro connection from the airport to Syntagma Square.
– **Bus:** Take bus X95 just outside the arrivals hall. Get off at the end of the journey at Syntagma Square. The ride costs 5 Euros and takes about an hour (depending on the traffic). At Syntagma Square you can walk down Panepistimiou Avenue for about 800 meters. Note that you should buy your ticket either from the booth outside the bus-stop or directly from the driver and you should validate your ticket upon entering the bus.

Venue
The conference will take place in the Hotel Titania**,** 52 Panepistimiou Avenue. The full address is:

Hotel Titania
Panepistimiou 52
10678 Athens
http://www.titania.gr/2008/default_EN.asp

The location of Hotel Titania is shown on the map.


Titania Hotel
Palamas Building
Santaroza
Πανεπιστημίου
Χαριλάου Τρικούπη
Ippokratous
Ethniki Vivliothiki
Tis Ellados
(Εθνική Βιβλιοθήκη
Της Ελλάδος)
Riga Feraiou
Ρήγα Φεραίου
Pesmatzoglou
Panepistimio
(ΣΤ.
ΠΑΝΕΠΙΣΤΗΜΙΟ)
Patriarchou Grigoriou Pemptou
Korai
Κοραή
Akadimias
Σόλωνος
Μαυρομιχάλη
Ιπποκράτους
Navarinou
Asklipiou
Skoufa
Grivaion
Solonos
Kaplanon
Μασσαλίας
Massalias
Sina
Σίνα


10th Floor
ROOM
OMIROS
ROOM
SOCRATIS
ROOM
PLATON
ROOM
SOLON
FOYER
APOLLON
VERGINA
Lobby
Mezzanine

Rooms ***Vergina*** and ***Apollon*** are in the **Mezzanine**
Rooms ***Hypatia*** and ***Aristotle*** are on the **1st floor**
Rooms ***Thales*** and ***Hippocrates*** are on the **8th floor**
Rooms ***Omiros***, ***Socrates***, ***Platon*** and ***Solon*** are on the **10th floor**

Registration
The Registration Desk will be in the **Mezzanine** of the Hotel. Registration opens on Wednesday 5 October at 11.00. The Desk will be open as follows:

– Wednesday 5 October: 11.00-18.00
– Thursday 6 October: 09.00-19.00
– Friday 7 October: 09.00-19.00
– Saturday 8 October: 09.00-13.00

Lunches, coffee/tea breaks
Light lunches are included in the registration fee. Lunches will be served at the **Mezzanine** of the Hotel. Tea and coffee will be served all day long near the conference rooms.

Opening Reception
The Opening Reception will be on Wednesday 5 October at 19.30 in the Kostis Palamas Building, the Cultural Centre of the University of Athens, at 48 Academias Street (see map above for its location).

Book Exhibit
Publishers will display their books in the **Mezzanine** of the Hotel.

Internet facilities
There is free WiFi service on the **1st and the 10th floors** of the Hotel.

Restaurants
Athens has many restaurants, from cheap to expensive, including a variety of local tavernas – some of which are really unmissable. You can find information about dining in Athens at the conference website:
http://epsa11.phs.uoa.gr/index.files/Restaurants.pdf.

Conference dinner
The Conference dinner will take place on Friday the 7th of October at 8.30 pm at the Olive Garden Mediterranean Cuisine Bar & Restaurant, which is located at the Roof Garden of Hotel Titania. Only pre-booked places will be available.

Banking hours
From 08.00 to 14.00 Monday - Thursday. Fridays until 13.30. Closed Saturday and Sunday. There are plenty of ATM cash machines on Korai pedestrianised street, two blocks from the conference venue.

Getting around Athens
Walking around the centre of Athens is pleasant and safe. Most hotels will be within walking distance from the conference venue. Do try to explore Athens on foot, especially the area from Acropolis down to Thission and then up to Monastiraki via

Plaka to Syntagma Square and then down to Klathmonos Square (next to the Conference Venue). Ippocratus street (next to the Conference Venue) separates two of the most well-known and very different neighbourhoods of Athens: Exarchia (on the left) and Kolonaki (on the right). Exarchia is the lovely quarter of students, intellectuals and bohemians. The Exarchia Square (one of the most famous of the city) has lovely cafes and tavernas. It's been known as the place where anarchists and ultra-leftists hang out (you won't fail to notice the presence of the riot police on various street corners) but it is a safe place to visit and explore. Kolonaki is the neighbourhood of the old aristocrats and a lot of nouveau riche. Around Kolonaki Square there are plenty of trendy cafes and restaurants-mostly on the expensive side. There are also some very interesting galleries and museums. From Kolonaki you can easily reach mount Lycabettus and have a spectacular view of Athens from above.

Taxi (yellow cab) is relatively inexpensive in Athens. There are not many taxi ranks, but you can always hail a taxi on the street. Make sure the meter is running and ask for a receipt. Tipping is optional but very welcome.

There is a reliable **metro** network with three lines: the blue/line3, the red/line 2 and the green/line 1. The green line is the oldest (mostly overground; we call it the 'electric') and runs from the port of Pireas to the northern affluent suburbs of Athens (Kiffisia). The blue line runs from the Airport to the western working class suburbs of Athens (Egaleo); the red line runs from the western working class suburb of Peristeri (Aghios Antonios) to the mixed eastern suburbs of Athens (Aghios Dimitrios). For a map of metro lines go the conference website.

Places you can visit: go to conference website,
http://epsa11.phs.uoa.gr/index.files/Getting%20around.pdf

Programme: Overview

Wednesday, October 5th

11.00-14.00	Registration	
14.00-14.30	**Opening of the conference (Vergina)**	
14.30-16.00	**Plenary lecture (Vergina)** Chair: Stathis Psillos Helen Longino (Stanford University) *The Sociality of Scientific Knowledge: Not Just an Academic Question*	
16.00-16.30	Tea Break	
16.30-19.00	Hypatia *Models in Explanation*	Socrates *Science and Democracy*
19.30-20.30	Opening Reception (Palamas Cultural Centre of UoA)	

Thursday, October 6th

09.00-11.00	Hypatia Symposium: *Is there a Nature of the Chemical Bond?*	Socrates Symposium: *From Evolution to Development, and Back: Towards a Developmental Theory*
11.00-11.30	Coffee Break	
11.30-13.30	Hypatia Symposium: *Symmetries, Superselection and Statistics*	Socrates Symposium: *The Reliability of Climate Model Predictions*
13.30-15.00	Lunch (Women's Caucus meeting 14.00-15.00 **—**Socrates)	
15.00-16.30	**Plenary lecture (Apollon)** Chair: Friedrich Stadler Dan Sperber (Institut Jean Nicod, ENS) *The Argumentative Theory of Reasoning and its Relevance to the Study of Science*	
16.30-17.00	Tea Break	
17.00-19.00	Hypatia Symposium: *Can We Really Lewis the Laws of Nature?*	Socrates Symposium: *Cognitive and Evolutionary Foundations of Human Sociality*

Platon *Philosophy of the Life Sciences I*	Omiros *Philosophy of Experimental Practice*	Solon *Philosophy of Psychology and Psychiatry*

Aristotle *Philosophy of Quantum Mechanics I*	Omiros *Mechanisms in Explanation*	Solon *Philosophy of the Cognitive Sciences I*
Aristotle *Realism and Anti-realism I*	Omiros *Science as Collective Knowledge*	Solon *Philosophy of the Cognitive Sciences II*
Aristotle *Philosophy of Quantum Mechanics II*	Omiros *Local Epistemologies*	Solon *Philosophy of Mathematics*

Friday, October 7th

09.00-11.00	Hypatia Symposium: *New Challenges for Philosophy of Science*	Aristotle Symposium: *The Social Organization of Research and the Flow of Scientific Information*
11.00-11.30	Coffee Break	
11.30-13.30	Hypatia Symposium: *Lawish Generalizations in the Special Sciences*	Aristotle *Philosophy of the Life Sciences II*
13.30-15.00	Lunch	
15.00-16.30	General Assembly Meeting (EPSA) (Platon)	
16.30-17.00	Tea Break	
17.00-19.00	Hypatia Symposium: *Perspectives on Spontaneous Symmetry Breaking in and Beyond the Standard Model*	Aristotle Symposium: *Modelling Social Aspects of Science*
20.30-22.30	Conference Dinner	

Saturday, October 8th

09.00-11.00	Hypatia Symposium: *Where to Draw the Line Between What's Real and Unreal in Biological Knowledge*	Aristotle Symposium: *Technical Functions and Artefacts in Philosophy*
11.00-11.30	Coffee Break	
11.30-13.30	Hypatia Symposium: *Emotion in Scientific Reasoning*	Aristotle *Ontology and Structural Realism*
13.30-14.30	Lunch	
14.30-16.00	**Springer Lecture in the Philosophy of Science (Vergina)** Chair: Martin Carrier Nancy Cartwright (LSE) *Evidence, Argument and Mixed Methods*	
16.00-16.30	Closing	

Thales *Formal Philosophy of Science I*	Hippocrates *Models and Simulations in the Life Sciences*	Solon *Philosophy of Quantum Mechanics III*
Thales *Philosophy of Space and Time I*	Hippocrates *Trust and Peer Review in Science*	Solon *Theories of Theories*
Thales *Theories of Natural Kinds*	Platon *Realism and Anti-realism II*	Solon *Formal Philosophy of Science II*

Thales *Epistemic Virtues and Theory Assessment*	Hippocrates *Pluralism and Reductionism*	Solon *Philosophy of the Social Sciences*
Thales *Theories of Natural Selection*	Hippocrates *Reduction and Idealization in the Physical Sciences*	Solon *Philosophy of Space and Time II*

Programme: Sessions

WEDNESDAY, 5 OCTOBER 2011

11.00-14.00 Registration

14.00-14.30 Opening of the conference (Vergina)

Thomas Sfikopoulos, Vice-Rector of the University of Athens
Costas Dimitracopoulos, Chair of the Dept of PHS, University of Athens
Friedrich Stadler, President of EPSA
Stathis Psillos, Chair of LOC of EPSA11

14.30-16.00 Plenary lecture (Vergina)

Chair: Stathis Psillos
Helen Longino (Stanford University)
The Sociality of Scientific Knowledge: Not Just an Academic Question

16.00-16.30 Tea Break

16.30-19.00 Parallel sessions
Models in Explanation (Hypatia)
Chair: Dionysios Anapolitanos
Anna-Mari Rusanen: *Information Semantics and the Problem of Imaginary Models*
Alisa Bokulich: *Explanatory Models vs. Predictive Models: Some Lessons from Geomorphology*
Joel Katzav: *Climate models and Inference to the Best Explanation*
Demetris Portides: *Idealization and Scientific Models: Reducing the Information Content*

Science and Democracy (Socrates)
Chair: Kristina Rolin
Kristen Intemann and Inmaculada De Melo-Martín: *Scientific Dissent, Objectivity, and Public Policy*
José Luis Luján and Oliver Todt: *Epistemic and Non-Epistemic Values in Regulatory Science: The Case of Risk Assessment*
Matthew J. Brown: *The Democratic Control of the Scientific Control of Politics*
Rose-Mary Sargent: *Early Twentieth Century Debates over Science in the Public Interest*
Elisabeth Nemeth: *What is the Role Science Can (And Ought to) Play in Democratic Decision-Making? Harry Collins' "Normative Theory of Expertise" in Historical Perspective*

Philosophy of the Life Sciences I (Platon)
Chair: Kenneth Waters
Kirsten Schmidt: *What Genes are not—The Postgenomic Gene as a Process Gene*

Christopher Pearson: *Description versus Explanation in Developmental Biology*
Maria Kronfeldner: *The Full Slate: Human Nature and Causation*
Emily Carter Parke: *Lessons from Arsenic Bacteria? Methodology and Implications of the Search for Alternative Life Forms*

Philosophy of Experimental Practice (Omiros)
Chair: Pieter Vermaas
Sally Riordan: *The First Determination of the Kilogram, 1790-1799: A Fresh Look at the Theoretical-Observational Divide*
Sjoerd D. Zwart: *Models as Artifacts: The Neutrality Thesis for Engineering Models*
Roger Stanev: *The Justification of Statistical Decisions in Clinical Trials*
Efi Kyprianidou: *On the Nature of Scientific Photography: Questions of Representing and Viewing*
Sophia Efstathiou and Eric Silverman: *Conceptual Frameworks and Interdisciplinarity: Modelling Ageing Populations*

Philosophy of Psychology and Psychiatry (Solon)
Chair: Drakoulis Nikolinakos
Adela Roszkowski: *The Cognitive Impenetrability of Perception and the Theory-ladenness of Observation Debate*
Panagiotis Oulis: *Explanatory Coherence, Partial Truth and the Distinction Between Validity and Utility of Psychiatric Diagnosis*
Thomas Sturm: *Metacognition and the Rationality Debate in Psychology*
Matt Bateman: *Experimental Inquiry in Cognitive Neuroscience*

19.30-20.30 Opening Reception (Palamas Cultural Centre of University of Athens)

THURSDAY, 6 OCTOBER 2011

09.00-11.00 Parallel sessions
Symposium: *Is there a Nature of the Chemical Bond?* (Hypatia)
Chair: Theodore Arabatzis
Michael Weisberg, Julia Bursten, Robin Hendry and Paul Needham

Symposium: *From Evolution to Development and Back: Towards a Developmental Theory* (Socrates)
Chair: Uskali Mäki
Lucie Laplane, Francesca Merlin, Antonine Nicoglou and Thomas Pradeu

Philosophy of Quantum Mechanics I (Aristotle)
Chair: Dennis Dieks
Laura Felline: *It's a Matter of Principle. Principle Reconstructions of QT and Their Contribution to the Understanding of the Quantum World*
Juan Sebastián Ardenghi, Olimpia Lombardi and Martín Narvaja: *Consecutive Measurements and Modal Interpretations*
Albert Solé: *The Redundancy Argument and the Many Interpretations of Bohmian Mechanics*

Richard Healey: *How to Use Quantum Theory Locally to Explain EPR-Bell Correlations*

Mechanisms in Explanation (Omiros)
Chair: Erik Weber
Eleanor Knox: *The Limits of Abstraction: Finding Space for Novel Explanation*
Jaakko Kuorikoski and Petri Ylikoski: *How Organization Explains*
Robert C. Richardson, Fred Boogerd and Frank Bruggeman: *Articulating Mechanisms*
Samuel Schindler: *Mechanistic Explanations: Asymmetry Lost*

Philosophy of the Cognitive Sciences I (Solon)
Chair: Eduard Machery
Lilia Gurova: *Principles vs. Mechanisms in Cognitive Science*
Lena Kästner: *Interventionism Cannot Cross*
Markus I. Eronen: *Pluralistic Physicalism and the Causal Exclusion Argument*
Emma Mª Martín Álvarez, Paco Calvo and Angel Garcia Rodriguez: *Cognitive Mechanisms as Biological, not Physical Mechanisms*

11.00-11.30 Coffee Break

11.30-13.30 Parallel sessions
Symposium: *Symmetries, Superselection and Statistics* (Hypatia)
Chair: Steven French
Adam Caulton, David Baker, Hans Halvorson, Klaas Landsman and Noel Swanson

Symposium: *The Reliability of Climate Model Predictions* (Socrates)
Chair: Roman Frigg
Katie Steele, Charlotte Werndl, Arthur Petersen, Jan Sprenger and Seamus Bradley

Realism and Anti-realism I (Aristotle)
Chair: Anjan Chakravartty
Emma Ruttkamp: *A Novel Defence of the Retrospective Nature of Reference*
Alberto Cordero: *Theory-parts for Realists*
Dean Peters: *Partial Realism, Anti-realism and Deflationary Realism: Can History Settle the Argument?*
Luca Tambolo: *The Normative Naturalist against the Pessimistic Induction*

Science as Collective Knowledge (Omiros)
Chair: Vasso Kindi
Hanne Andersen: *Acting out of Line: On Joint Accept and Unilateral Rescission in Scientific Groups*
Cyrille Imbert: *Collective Science: How not to Lose Scientific Understanding?*
Adam Toon: *Friends at Last? Distributed Cognition and the Cognitive/Social Divide*
Thomas Boyer: *Is a Bird in the Hand Worth Two in the Bush? Or, Whether Scientists Should Publish Intermediate Results*

Philosophy of the Cognitive Sciences II (Solon)
Chair: Lilia Gurova
Víctor M. Verdejo: *Computationalism, Connectionism, Dynamicism and Beyond: Looking For An Integrated Approach To Cognitive Science*

Norman Sieroka: *Neurophenomenology of Hearing: Relations to Intentionality and Time Consciousness*
Lieven Decock and Igor Douven: *Qualia Compression*

13.30-15.00 Lunch (Women's Caucus meeting 14.00-15.00 — Socrates)

15.00-16.30 Plenary lecture (Apollon)
Chair: Friedrich Stadler
Dan Sperber (Institut Jean Nicod, ENS)
The Argumentative Theory of Reasoning and its Relevance to the Study of Science

16.30-17.00 Tea Break

17.00-19.00 Parallel sessions
Symposium: *Can We Really Lewis the Laws of Nature?* (Hypatia)
Chair: Gerhard Schurz
Thomas Müller, Marcus Schrenk, Jesse Mulder and Carl Hoefer

Symposium: *Cognitive and Evolutionary Foundations of Human Sociality* (Socrates)
Chair: Matti Sintonen
Francesco Guala, Benoit Dubreuil, Christophe Heintz, Eduard Machery and Alejandro Rosas

Philosophy of Quantum Mechanics II (Aristotle)
Chair: Miklos Redei
Aristidis Arageorgis and Chrysovalantis Stergiou: *On Particle Phenomenology Without Particle Ontology: How Much Local is Almost Local?*
Foad Dizadji-Bahmani: *Why I am not an Everettian*
Iñaki San Pedro: *Freeing Free Will from Conspiracy*
Dunja Šešelja and Christian Straßer: *Abstract Argumentation Applied to Scientific Debates*

Local Epistemologies (Omiros)
Chair: James MacAllister
Saana Jukola: *Defending the Social View on Objectivity*
M. Cristina Amoretti and Nicla Vassallo: *Situatedness and Objectivity: Scientific Knowledge without Standpoints*
Endla Lõhkivi: *Is Workplace Culture Relevant for Philosophy of Science? A Case Study on Physics and Humanities*
Jouni-Matti Kuukkanen: *I Am Knowledge: Get Me Out of Here! On Localism and the Universality of Science*

Philosophy of Mathematics (Solon)
Chair: Costas Dimitracopoulos
Demetra Christopoulou: *On a Double Aspect of Natural Numbers as Abstract Particulars and/or Universals*
Paola Cantù: *Kant and 20th Century Philosophy of Mathematics*
Mark Colyvan: *A Ricci Curvature Tensor by any Other Name*

FRIDAY, 7 OCTOBER 2011

09.00-11.00 Parallel sessions
Symposium: *New Challenges for Philosophy of Science* (Hypatia)
Chair: Maria-Carla Galavotti
Raffaella Campaner, Theo Kuipers, Daniel Andler, Olav Gjelsvik and Roman Frigg

Symposium: *The Social Organization of Research and the Flow of Scientific Information* (Aristotle)
Chair: Rose-Mary Sargent
Rebecca Kukla, Justin Biddle, Torsten Wilholt, Bryce Huebner and Eric Winsberg

Formal Philosophy of Science I (Thales)
Chair: Jesús Zamora Bonilla
Ilkka Niiniluoto: *Models, Simulation, and Analogical Inference*
Petros Stefaneas: *Theories and Abstract Model Theory*
Gustavo Cevolani, Vincenzo Crupi and Roberto Festa: *More Verisimilar Banking: A Novel Analysis of the Linda Paradox*
Doukas Kapantaïs: *Formal Intuitionistic Semantics for Fitch's Paradox*

Models and Simulations in the Life Sciences (Hippocrates)
Chair: Alex Broadbent
Sara Green: *Exploratory Models - Reverse Engineering in Systems Biology*
Bettina Schmietow and Lorenzo Del Savio: *Cells from Computers: from Ethics to Epistemology*
Tim Räz and Raphael Scholl: *Why Do We Model?*
Emanuele Serrelli: *Mendelian Population as a Model, Intended as a "Stable Target of Explanation"*

Philosophy of Quantum Mechanics III (Solon)
Chair: Vassilis Karakostas
Jonathan Bain: *CPT Invariance, the Spin-Statistics Connection, and the Ontology of Relativistic Quantum Field Theories*
Karim Bschir, Michael Epperson and Elias Zafiris: *Decoherence: A View from Topology*
Gordon Purves: *Lies, Damn Lies, and Quantum Statistics: Confirmation and False Posits*
Mario Bacelar Valente: *Are Virtual Quanta Nothing but Formal Tools?*

11.00-11.30 Coffee Break

11.30-13.30 Parallel sessions
Symposium: *Lawish Generalizations in the Special Sciences* (Hypatia)
Chair: Robin Hendry
Craig Callender and Jonathan Cohen, Julian Reiss, Daniel Steel, Andreas Hüttemann and Alexander Reutlinger

Philosophy of the Life Sciences II (Aristotle)
Chair: Rebecca Kukla
Johannes Martens: *Altruism, Correlations and Causality*

Till Grüne-Yanoff: *Evolutionary Game Theory, Learning Dynamics and Mechanisms*
Marta Bertolaso: *An Apparent Circular Causality to Account for the Phenotypic Stability of the Organism: Insights From the Biology of Cancer*
Alex Broadbent: *A Theory of General Causation for Epidemiology*

Philosophy of Space and Time I (Thales)
Chair: Mauro Dorato
Adán Sus: *The Physical Significance of Symmetries and Conservation Laws*
Erik Curiel: *On the Thermodynamical Character of Black Holes in Classical General Relativity*
F.A. Muller: *Structuralism and Space-Time*

Trust and Peer Review in Science (Hippocrates)
Chair: Henk de Regt
Susann Wagenknecht: *Epistemic Trust: An Empirical Study in Natural Science*
Jeroen De Ridder: *Trust in Science: Nicety or Necessity?*
Laszlo Kosolosky: *The Role of 'Peer Review' in Science: Exploring How and Why the IPCC Blundered on the Melting Rate of Himalayan Glaciers*

Theories of Theories (Solon)
Chair: Paul Hoyningen-Huene
Francesca Pero: *Actual Theorizing and the Model-Theoretic Account*
Rogier De Langhe: *The Problem of Kuhnian Rationality*
Chuang Liu: *A Critique of the Deflationary View on Scientific Representation*
Fabian Lausen: *Heuristic Reductionism and the Concept of a Research Directive*

13.30-15.00 Lunch

15.00-16.30 General Assembly Meeting (EPSA) (Platon)

16.30-17.00 Tea Break

17.00-19.00 Parallel sessions
Symposium: *Perspectives on Spontaneous Symmetry Breaking in and Beyond the Standard Model* (Hypatia)
Chair: Richard Healey
Arianna Borrelli, Koray Karaca, Michael Stöltzner and Simon Friederich

Symposium: *Modelling Social Aspects of Science* (Aristotle)
Chair: Ilkka Niiniluoto
Jesús Zamora Bonilla, Stephan Hartmann, Ryan Muldoon, J. McKenzie Alexander, and Gerhard Schurz

Theories of Natural Kinds (Thales)
Chair: Thomas Reydon
Elena Casetta: *Outlining a Unified Framework for Assessment of the Biodiversity*
Samuli Pöyhönen: *Should I Split or Should I Lump? The Epistemic-Tool Approach tto Scientific Concept Formation*
Miles MacLeod: *What Kind of Kinds are Homologies? Studying Homology Concepts as Significant Kinds*

Realism and Anti-realism II (Platon)
Chair: Andreas Hüttemann
Paul Hoyningen-Huene: *The Ultimate Argument against Convergent Realism and Structural Realism: The Impasse Objection*
Simon Fitzpatrick: *Doing Away with the No Miracles Argument: Realism, Empirical Success and Confirmation*
Paul Teller: *Coherent Scientific Realism*
Murat Baç: *Natural Ontological Misrepresentation and Subtleties of Neo-Realism*

Formal Philosophy of Science II (Solon)
Chair: Theo Kuipers
Franz Huber: *How to Confirm Counterfactuals*
Wolfgang Pietsch: *The Limits of Probabilism*
Peter Brössel: *The Significance of Confirmation*

20.30-22.30 Conference Dinner

SATURDAY, 8 OCTOBER 2011

09.00-11.00 Parallel sessions
Symposium: *Where to Draw the Line Between What's Real and Unreal in Biological Knowledge* (Hypatia)
Chair: Mark Colyvan
Marcel Weber, Kenneth Waters, Steven French and Holger Lyre

Symposium: *Technical Functions and Artefacts in Philosophy* (Aristotle)
Chair: Chrysostomos Mantzavinos
Wybo Houkes, Pieter Vermaas, Mieke Boon, Thomas Reydon and Erik Weber

Epistemic Virtues and Theory Assessment (Thales)
Chair: Alberto Cordero
Milena Ivanova: *Can Theoretical or Intellectual Virtues Solve the Problem of Underdetermination of Theory by Data?*
Kate Hodesdon and Kit Patrick: *Is Theory Choice Using Epistemic Virtues Possible?*
Harvey Siegel: *Relativism and the Strong Programme Reconsidered*
Vincent Ardourel: *Strong Underdetermination of Theories by Data: The Case of Different Mathematical Formulations of a Scientific Theory*

Pluralism and Reductionism (Hippocrates)
Chair: Daniel Andler
Stéphanie Ruphy: *'Foliated' Pluralism: A Philosophically Robust Form of Ontologico-Methodological Pluralism*
Robert Kowalenko: *'Styles of Scientific Thinking Can Kill'*
Anjan Chakravartty: *Realism about Scientific Taxonomy*
Henrik Thorén: *What is an Interdisciplinary Problem?*

Philosophy of the Social Sciences (Solon)
Chair: Stephan Hartmann
Yulie Foka-Kavalieraki and Aristides Hatzis: *Economics, Evolution, and the Brain: From Rational Choice Theory to Ecological Rationality*
Thomas Uebel: *Narratives and Action Explanation*
Uskali Mäki: *On the Performance of the Performativity Thesis*
Jan Faye: *How Do We Understand in Science?*

11.00-11.30 Coffee Break

11.30-13.30 Parallel sessions
Symposium: *Emotion in Scientific Reasoning* (Hypatia)
Chair: Hanne Andersen
James McAllister, Jeff Kochan, Lisa Osbeck, Nancy Nersessian and Sabine Roeser

Ontology and Structural Realism (Aristotle)
Chair: Antigone Nounou
Federico Laudisa: *Can There be a Truly 'Ontological' Scientific Naturalism?*
Mauro Dorato: *How to Combine (And not to Combine) Physics and Metaphysics*
Vincent Lam and Christian Wüthrich: *No Categorical Support for Radical Ontic Structural Realism*
Kerry Mckenzie: *'Humean Structuralism' About Laws*

Theories of Natural Selection (Thales)
Chair: Miles MacLeod
Jonathan Everett: *Evolutionary Theory and Thermodynamics: The Role of Statistics*
David Crawford: *Probability Measures and Biological Fitness*
Fridolin Gross and Cecilia Nardini: *Is Natural Selection a Mechanism?*
Francis Cartieri: *Is Neo-Darwinism in Crisis? Lamarck and Epigenetic Inheritance*

Reduction and Idealization in the Physical Sciences (Hippocrates)
Chair: Mauricio Suárez
Nazim Bouatta and Jeremy Butterfield: *Emergence and Reduction Combined in Infinite Systems*
Mathias Frisch: *Incantations of 'Causation' and Other Philosophical Sins, Or: Rehabilitating Ritz*
Ave Mets: *Measurement Theory, Nomological Machine and Measurement Uncertainties (in Classical Physics)*
Iulian Toader: *The Dappling Effects of Idealization*

Philosophy of Space and Time II (Solon)
Chair: Vassilis Sakellariou
Lisa Leininger: *Presentism, Eternalism, and the Possibility of Temporal Becoming*
Daniel Wohlfarth: *A New View of "Fundamentality" for Time Asymmetries in Modern Physics*
Henrik Zinkernagel: *A Critical Note on Time in the Multiverse*
Matt Farr: *On the Status of Temporal Unidirectionality in Physics*

13.30-14.30 Lunch

14.30-16.00 Springer Lecture in the Philosophy of Science (Vergina)
Chair: Martin Carrier
Nancy Cartwright (LSE)
Evidence, Argument and Mixed Methods

16.00-16.30 Closing

ABSTRACTS

Plenary Lectures

Wednesday, 5 October ▪ 14.30-16.00

Helen Longino
The Sociality of Scientific Knowledge: Not Just an Academic Question

I have argued for a strong interpretation of the social character of scientific knowledge, basing this both on features of the organization of scientific inquiry and on logical features of evidential reasoning. In this talk I will review and update the arguments for this interpretation and urge that it has implications beyond the philosophical circles within which it is debated.

Thursday, 6 October ▪ 15.00-16.30

Dan Sperber
The Argumentative Theory of Reasoning and its Relevance to the Study of Science

I will outline the argumentative theory of reasoning (Hugo Mercier and Dan Sperber, "Why do humans reason? Arguments for an argumentative theory" *Behavioral and Brain Sciences* [2011], 34, 57–111) and consider its relevance to the study of science.

Saturday, 8 October ▪ 14.30-16.00

(Springer Lecture in the Philosophy of Science)

Nancy Cartwright
Evidence, Argument and Mixed Methods

This paper will focus on effectiveness predictions for illustration: predictions that a well-defined policy will produce a targeted outcome here, for us, if we implement it.

Randomized controlled trials (RCTs) are touted as gold standard evidence for such claims. But there is a catch, one the evidence-based policy movement worries about: “There is a risk that with the concentration on, or assumed superiority of, experimental or quasi-experimental methods ..., those types of … interventions not suitable for these approaches will come to be considered less effective or somehow inferior and therefore less ‘value for money’.” So, it is asked, can non-experimental and qualitative evidence support effectiveness claims? How, and how strongly? And how do we combine qualitative and quantitative results to arrive at good predictions?

I think this way of raising the questions grossly misjudges the need for mixed methods and the role they play. I propose we start by asking, what makes RCTs evidence for effectiveness – what makes them evidence at all? To warrant a conclusion, you need a good argument – both sound and valid, and you have to have good reason to suppose that it is a good argument. So you need evidence for each of the premises. How do RCT results fit into a well-supported argument for an effectiveness prediction? In most cases there is no way to build them in as a premise in any sound argument. They enter only indirectly, in a complex subargument that supports one of the main premises, usually a premise to the effect that the policy *can* make a positive contribution here. The other premises, both in the main argument and in the complex subargument, require very different kinds of information, for which experimental evidence is irrelevant. Evidence that secures one subpremise for one premise in the argument does not count for anything towards the conclusion if there is no evidence to support the other premises. Qualitative and non-experimental evidence is thus essential if experimental results are to be evidence at all.

Wednesday, 5 October

16.30-19.00

Models in Explanation

Anna-Mari Rusanen
Information Semantics and the Problem of Imaginary Models

Scientists explore unrealistic and imaginary models and use them to help to explain complex real world target systems. But because the systems described by imaginary models are known not-to exist, it raises the problem of explaining of their empirical and explanatory usefulness. In this paper I'll present how this problem can be reframed in the context of information semantic account of scientific models. According to it the content of a representation is grounded in the information a model carries about its target. This requires a causal-information relationship between a model and its target system, which is implemented by the model-building process. It poses some restrictions for genuinely explanatory models of real world phenomena, and it gives a criterion for distinguishing a "genuine" representation from arbitrary or false mappings. Genuine information carrying representations allow us to obtain information about the intrinsic properties of target systems, completely imaginary, false or arbitrary mappings don't. However, if models are fictional i.e. their target systems do not actually exist then there is no causal information relationship between a model and its target system. How, then, could completely imaginary models carry information about these target systems and help us to represent and to explain real world phenomena? However, very few models are completely imaginary, because they have components that refer to real world entities, even if some other components of models were unreal. In such a case a model is more than a mere imagination, because it captures features of real world entities. Partially imaginary models may be explanatory, if specified parameter values taken by such models carry some information about the real world systems – if not, then in information semantics they would not be adopted as explanatory models of real world entities.

Alisa Bokulich
Explanatory Models vs. Predictive Models: Some Lessons from Geomorphology

Prediction and explanation have long been recognized as twin goals of science, and yet a full understanding of the relations—and tensions—between these two goals remains unclear. Here I examine a field known as geomorphology, which is concerned with understanding how landforms change over time. The complexity of geomorphic systems makes the use of idealized models essential, and these models are typically trying to synthesize processes occurring on multiple time and length scales. There is a growing recognition in geomorphology that the sort of models that turn out to be the best for generating predictions (detailed, bottom-up, physically-based "simulation" models) are not the same kinds of models that are best for generating explanations (highly idealized, cellular or "reduced complexity models"). I examine three cases of explanatory models in geomorphology—a model-explanation of river braiding, a model-explanation of a characteristic coastline evolution, and a model-explanation of the formation of rip currents along planar beaches—and show how they fit my general philosophical account of model-explanations. Because these explanatory models were not designed to provide quantitatively accurate predictions, there arises the question of how such models should be tested/validated. I will examine how geomorphologists are using robustness analyses to test these models and justify them as being genuinely explanatory.

Joel Katzav
Climate Models and Inference to the Best Explanation

I examine the warrants we have in light of the empirical successes of a kind of models I call 'hybrid models', a kind that includes climate models among its members. I argue that these warrants' strengths depend on inferential virtues that are not just explanatory virtues, contrary to what would be the case if inference to the best explanation (IBE) provided the warrants. I also argue that the warrants in question, unlike those IBE provides, guide inferences only to model implications about which there is real uncertainty. My conclusion provides criteria of adequacy for epistemologies of climate and other hybrid models.

Demetris Portides
Idealization and Scientific Models: Reducing the Information Content

Two kinds of idealization have been analyzed from various perspectives by a number of philosophers. Some philosophers blend the two into their notion of idealization, and others refer to the first as idealization and to the second as abstraction. The dubbing of the notions is not however an issue with which I am concerned in this paper. Rather, I am more concerned with highlighting that from the perspective of the reasoning process involved in constructing scientific models, the kind of information content reduction differs in the two cases. More importantly, for my purposes, I wish to highlight the difference between these two kinds of idealization from the third kind. The third way by which information content is reduced in scientific models, idealization by decomposition, has not received much attention in the literature. Decomposition consists in setting apart various clusters of influencing factors. The result of decomposing-idealization is a description that involves distinct clusters of factors thought to be acting in tandem to produce the particular behavior of the system. Idealization as decomposition is the result of setting apart, within our model description, clusters of factors that we assume to influence the behavior of the target system. What is omitted in decomposition is the information that the behavior of the system is the result of a convoluted complex natural mechanism, which is not necessarily the result of independent factors (or mechanisms) acting in tandem to produce the observed behavior. This kind of idealization is most common in Quantum Mechanical modelling. I use some examples from nuclear physics to demonstrate the peculiarities of decomposition and draw some epistemological conclusions that are consistent with its presence in quantum mechanical models.

Science and Democracy

Kristen Intemann and Inmaculada de Melo-Martín
Scientific Dissent, Objectivity, and Public Policy

Many have argued that allowing and encouraging public avenues for dissent and critical evaluation of scientific research is a necessary condition for promoting scientific objectivity. In spite of the importance placed on dissent within science, there is growing concern among scientists and science scholars about the negative effects that dissent can have on public policy. Aware of these potential negative consequences, many scientists have become reluctant to engage in, or be supportive of, even well-grounded dissent, and several science studies scholars have focused their attention on discrediting dissenters and on defending the importance of scientific consensus. We argue that condemnation of scientific dissent is both misplaced and dangerous. It is misplaced because it relies on mistaken assumptions about the relationship between scientific evidence and public policy. In particular, we will show that concerns about dissent are grounded on mistaken assumptions that consensus is necessary and/or sufficient to ground particular public policies. Moreover, criticism of dissent is also dangerous as it is likely to increase illegitimate instances of dissent, can deprive us of resources to criticize special interest science, and can stifle legitimate scientific dissent that is crucial to scientific progress and sound public policy.

José Luis Luján and Oliver Todt
Epistemic and Non-Epistemic Values in Regulatory Science: The Case of Risk Assessment

The debate on the role of values in science is an important issue in the philosophy of science, which has also cropped up in the field of applied science and, particularly, regulatory science. Our paper focuses on how the philosophical analysis of values can help clarify the current controversies related to technological risks. We propose an analysis, from the perspective of values, of the recent controversies related to the role of scientific knowledge in the regulation of technological risks. Based on this analysis, we differentiate three perspectives on cognitive and non-cognitive values in the context of assessing and managing risk. In each of the three perspectives, science plays a specific (as well as critical) role in framing policy decisions: (1) science as arbiter between regulation and innovation, (2) science (based on a modified methodology) for protecting health and the environment, (3) science for generating “inherently safe” alternatives. In the first and third case, cognitive and non-cognitive values are clearly separated, with decisions being based on cognitive values (in the first case), and non-cognitive values (in the third case). In the second case there is an interaction between non-cognitive values and methodological decisions.

Matthew J. Brown
The Democratic Control of the Scientific Control of Politics

I will argue for two popular but apparently contradictory theses: (1) *the democratic control of science* –the aims and activities of science should be subject to public scrutiny and oversight via democratic processes. (2) *Technocracy* –political processes are problem-solving pursuits subject in many ways to the methods and results of science and technology.

Many arguments can be given for (1), both epistemic and moral/political; I will focus on an argument based on the role of non-epistemic values in policy-relevant science. I will argue that we must accept (2) as a result of an appraisal of the nature of contemporary political problems. Technocratic systems, however, are subject to serious moral and political objections; these difficulties are sufficiently mitigated by (1). I will set out a framework in which (1) and (2) can be consistently and compellingly combined.

Rose-Mary Sargent
Early Twentieth Century Debates over Science in the Public Interest

After an extended period of time during which philosophers of science focused almost exclusively on logical and methodological issues internal to science, recent work has sought to reclaim a role for philosophers in wider discussions concerning the pursuit of science in the public interest. The cultural and political reasons behind the positivist retreat to logic have been well documented. In this paper, I look at another contributing factor to the phenomenon –the reintroduction of a strict division between pure and applied science by Bertrand Russell during the early 1920s. Russell's defense of the ideal of a pure, value neutral, science culminated in his extended critique of John Dewey's pragmatic conception of science in the first volume of *The Library of Living Philosophers* (1939). In return, Dewey maintained that a philosopher's focus on pure science represented a "shirking of responsibility". After examining the exchange between Russell and Dewey, as well as some subsequent contributions to the debate over pure science by Reichenbach and Neurath, the paper concludes with a discussion of how the distinction between pure and applied science contributed to the retreat from social engagement. Examining the historical trajectory of the distinction can provide insight into what is at stake in today's current debates. In particular, to the extent that a sharp distinction between pure and applied science is retained, there will continue to be conceptual roadblocks to the full development of a philosophy of science that can serve the public interest.

Elisabeth Nemeth
What is the Role Science Can (And Ought to) Play in Democratic Decision-Making? Harry Collins' "Normative Theory of Expertise" in Historical Perspective

Harry Collins et al. (2002, 2010) distinguish three waves of science studies each of which involves a specific way of looking at the relationships between science and political decision-making: Wave One 'positivism' (from 1950 to Kuhn), Wave Two 'social constructivism' (from Kuhn to 2000), Wave Three (from 2000) which is their own project to develop a "normative theory of expertise and decision-making". It aims at spelling out (1) what makes science different from other forms of knowledge and (2) why this specificity justifies a special role of science and technology in political decision making.

In this paper I will put Collins' project in a broader historical perspective. In 1913, Neurath argued that the way philosophers and scientists conceive of the foundations of science has an important impact on the role science can play in political decision-making. For Neurath, science is a human, historical enterprise which is deeply shaped by contingent decisions of scientists and external factors. This concept of science is, in his view, the pre-condition of a non-illusionary, rational view of the role science can play in modern societies – and therefore also a pre-condition of democracy. In this respect his view is close to Collins'. In contrast to Collins, however, epistemology mattered for Neurath. This difference will be the starting point for questioning the way Collins et al. relate the three Waves to each other.

Philosophy of the Life Sciences I

Kirsten Schmidt
What Genes are not – The Postgenomic Gene as a Process Gene

In the postgenomic age, the term "gene" stands rather for a methodologically useful consensus term than for a well-defined ontological entity. However, the present inability to specify the nature of the gene in an unambiguous way leaves a blank space in the public perception of genetics. Given the huge cultural meaning of the gene concept, this ontological gap must appear frightening. As an attempt to moderate these fears, I would like to approach the ontological status of the postgenomic gene in three steps. First, I will analyse common epistemic trends in modern gene concepts. The increasing conceptual separation of genes from the genomic material of the DNA implies a shift from the genic to the genomic perspective that is frequently accompanied by a shift from the structural to the functional perspective. Moreover, functional approaches tend to emphasize processual aspects of the gene. As I will show in a second step, understanding these trends can help to clarify the ontological status of the gene, because they display what genes are *not*: A gene is not a DNA segment. Genes are not heritable and they do not carry information. Thirdly, I will argue that postgenomic genes are not autonomous molecules that take part in the expression process –instead, what we call "gene" *is* the process. By equating the molecular gene with the expression process, the gene concept encompasses not only a particular DNA sequence but also the cellular, organismic and environmental context that has been largely excluded in the classical molecular concept.

Christopher H. Pearson
Description versus Explanation in Developmental Biology

The research trajectory of developmental biology has trended towards articulating the underlying molecular genetics responsible for organismic development. Alexander Rosenberg has argued that this research trajectory demonstrates an explanatory reduction within developmental biology. In the course of defending this reductionistic position, however, Rosenberg contends further that developmental biology's shift away from the embryological/cellular level of description to that of molecular genetics transforms developmental biology from a non-explanatory discipline to an explanatory discipline; in short, Rosenberg sees developmental biology absent molecular genetics as a descriptive rather than explanatory enterprise. In this paper, I propose that Rosenberg's view of pre-molecular developmental biology is problematic for two interdependent reasons. First, it underestimates the theoretical resources pre-molecular developmental biology may draw upon for explanatory work. Second, the essential element for distinguishing descriptive and explanatory disciplines—that of causality—applies equally to features within pre-molecular developmental biology as it does to post-molecular developmental biology.

Maria Kronfeldner
The Full Slate: Human Nature and Causation

The concept of human nature can be used as a classificatory-descriptive tool (i.e., to sort entities into groups) or as a causal-explanatory tool (e.g. to claim that it is human nature that causes us to be selfish). This paper addresses human nature as a causal-explanatory category. On the basis of an interventionist account of causation, I will suggest that human nature simply cannot be a causal category. In the language of the famous 'slate', human nature is neither a blank nor a full slate. It is a name for the *frame* of an incredibly full and complex slate and not itself a causal category. This is the negative revisionary claim of the paper, developed in its first part. In the second part, I will illustrate that the concept of human nature is nonetheless of relevance for causal explanations, but its role is pragmatic only. In the third part, I shall illustrate why the conceptual clarifications developed in the first two parts are more than just philosophical finger-exercises. I shall describe how the fact that human nature cannot be a causal factor is important for at least one debate in science itself. The negative claim is thus not only a plea for replacing sloppy talking, but helps to understand why, for instance, cultural anthropologists justifiably ignore human nature in their scientific endeavours to understand culture.

Emily Carter Parke
Lessons from Arsenic Bacteria? Methodology and Implications of the Search for Alternative Life Forms

In December 2010, NASA announced the discovery of bacteria that could substitute arsenate for phosphate in their biomolecules. This challenge to the ubiquity of phosphates in biochemistry was met with significant backlash, criticizing the experimental protocol and data, and questioning the claims to its significance independent of whether or not the alleged substitution actually took place. Regardless of the final upshot of this ongoing debate, the logic and methodology of this case are interesting and merit further attention. In my talk, I use this case as a starting point to examine more broadly the issue of what it might mean to change, as NASA put it, our "fundamental knowledge about what comprises all known life on Earth". I suggest a distinction among several different kinds of project in searching for alternative life forms: challenging putative constraints on life, investigating known, potentially biological anomalies, and searching for new anomalies. These projects vary in the type and profundity of lessons they could teach us about the nature and origin of life. I argue that projects like the search for arsenic bacteria, while candidates for significantly changing our knowledge of the familiar biosphere, can go only so far in leading to major conceptual change in our knowledge of what life is and where it came from. The kinds of project that could deliver on this promise face significant practical and theoretical challenges.

Philosophy of Experimental Practice

Sally Riordan
The First Determination of the Kilogram, 1790-1799: A Fresh Look at the Theoretical-Observational Divide

The kilogram is the only scientific unit that remains defined by an artefact. Pointing to a hunk of metal remains the most accurate way to measure mass. But scientists demand more from their measurement standards than accuracy. Metrologists are currently pursuing technologies that will facilitate a more abstract definition of the kilogram. Sentiments heard today echo demands made over two centuries ago, when the kilogram emerged from the upheaval of the French Revolution. At this time, scientists and politicians alike were hankering for the perfect measure: certain, robust and taken from nature. Lavoisier and Haüy were the first to determine the value of the new mass unit in terms of the French pound, in a little-known, water-weighing experiment of 1793. By piecing together the details of their exquisite experiment we come to understand a little better what these scientists were seeking in a natural standard. But we must now wonder why some of the adjustments made to the experiment's data were viewed as harmless and yet others condemned it to obscurity. The distinction between calibration and error-correction becomes blurred. This is one way in which we can express the long-known adage of philosophy of science that observation is laden with theory. In the context of the water-weighing experiment, the battle is played out between certainty and naturalness. We are brought to ask what calibration is and whether it can ever be harmless.

Sjoerd D. Zwart
Models as Artifacts: The Neutrality Thesis for Engineering Models.

Models are increasingly recognized to have an independent and autonomous stance within the scientific enterprise. Moreover they are constructed for specific goals, which, especially in the engineering sciences, need not only be an increase of (theoretical) knowledge, but may have all kinds of other practical purposes. A lot has been written about the value-ladenness of science and the question as to whether science, and in particular scientific theory, is or should be value-free. Surprisingly less attention has been paid, however, to the same question applied to models or modeling, and the restricted relevant literature available displays a surprisingly variety of opinions. In this paper, I address the question as to whether scientific (engineering) models can embody ethical or societal values in the same way as dikes or speed bumps seem to do. To answer this question I compare models to artifacts and apply the neutrality thesis to models. In the end models may be value neutral instruments, only to be put in use for good or bad purposes by their users. The neutrality thesis has met with some fierce criticism from philosophers. Some of them claim that at least some artifacts, such as dikes and speed bumps embody extrinsic final values (Van de Poel, Kroes (forthcoming)). In this paper I will find out if the same holds for some scientific or engineering models.

Roger Stanev
The Justification of Statistical Decisions in Clinical Trials

In recent years, there has been a growing concern about the proper conduct and monitoring of clinical trials. High on the agenda of epidemiologists and biostatisticians is the inadequacy of trial reporting. In my research, I propose a decision theoretic framework—a second order decision framework together with simulations of it—that provides means for representing and evaluating statistical monitoring decisions. Incidentally, the proposal and the set of recommendations for such a framework are not arbitrary. They are sensitive to and based on what researchers often take to be relevant factors, including their own plans for how to conduct trials. (Stanev 2011 is an example of such work) My talk, however, will focus on a specific problem regarding the pair of tasks representation-evaluation: what does it take for a statistical monitoring decision to be considered a good decision? This question is important not only to philosophers and modelers but to anyone who may want to evaluate RCT results. While statistical approaches tend to focus on the epistemic aspects of statistical monitoring rules often overlooking ethical considerations, ethical approaches to RCTs fall short of providing the necessary means for evaluating monitoring rules and early stopping decisions by neglecting the epistemic dimension. In my talk, I answer the question by adopting a framework that incorporates both ethical and epistemic considerations. My paper articulates a comprehensive, but mostly qualitative, approach to the evaluation of statistical monitoring decisions.

Efi Kyprianidou
On the Nature of Scientific Photography: Questions of Representing and Viewing

In the contributed paper I will explore the role of photographic depictions as sources of meaning in the arts and in the sciences. I start by considering the following questions: how do photographs (and images in general) acquire their meaning? How do we learn from scientific and artistic photographs?

Although indexicality and visuality are central themes to the aesthetics of photography, they are absent from the discussions about scientific images. This is probably because in the image making practices in the sciences, indexicality is taken for granted. Scientific photography remains enclosed to an empirical realm, constituted by descriptions, explanations and predictions based on natural laws. Scientific photographs are added in the list, due to their mechanical nature. But how do they find their place in the scientific narration, into the ‘realm of content’? How do they acquire their representational content so that they therefore justify scientific propositions? If, as it has been argued, the difference between the seeing things and seeing things through a photograph generates the aesthetic interest, what is the case with scientific imaging? In virtue of what do scientific images represent?

By scrutinizing the nature and role of scientific photography, we hope to draw some conclusions regarding the nature of truth in the scientific context and the relation between the scientist’s perception and the photographic depiction.

Sophia Efstathiou and Eric Silverman
Conceptual Frameworks and Interdisciplinarity: Modelling Ageing Populations

A hard problem facing interdisciplinary scientific collaboration is communicating across disciplines. We examine this challenge using the case of a project building models by merging social science and complexity science expertise. We discuss how divergent epistemic practices between demography and complexity science affect this collaboration.

We focus on significant conceptual barriers to combining demographic methods with complexity science simulation techniques. Even though both disciplines claim expertise in 'modelling' social systems, there is a significant divergence between considering models as a means of theory-formation and models as a means of prediction. This creates a communication gap between demographic and complexity-science portions of the project, overcoming which demands re-interpreting and re-positioning one's practice on subjects even as simple as what a 'model' *is* (or what it is *for*).

This seems to us an instance of a deep and important philosophical problem. Interdisciplinary work-spaces can produce conceptual trading zones that allow for the formation of new and useful ideas. However for any sensible interdisciplinary *science* to truly begin, concepts founded differently across different research streams and disciplines may need to be unpacked and investigated, and even re-defined or re-named.

Creating new modelling tools in this context involves creating new ideas about modelling. The search for shared disciplinary ground is being progressively pushed back from superficial similarities in shared vocabularies to an investigation, selective challenge and re-formulation of assumptions that shape models at levels prior to the production of any one simulation technology. In that sense, creating interdisciplinary science is an intrinsically philosophical enterprise.

Philosophy of Psychology and Psychiatry

Adela Roszkowski
The Cognitive Impenetrability of Perception and the Theory-ladenness of Observation Debate

This paper criticizes the prevalent use of the notion of cognitive impenetrability in the debate over the theory-ladenness of observation (=TLO). I will pay special attention to Jerry Fodor's classic paper 'Observation Reconsidered' (probably the best-known attack on the TLO thesis) and the more recent work of Athanassios Raftopoulos. Both, Fodor and Raftopoulos try to establish the theory-neutrality of observation by means of drawing on psychological findings which are meant to show that perception is cognitively impenetrable, that is, free from top-down influences. The existence of a cognitively impenetrable level of perception is thought to safe-guard the neutrality of observation from theoretical impact. This paper addresses the following problems: (1) the existence of a cognitively impenetrable level of perceptual processing does not necessarily show that observation is theory-neutral; (2) even on Fodor's and Raftopoulos' account observation can be understood to be theory-laden after all.

Panagiotis Oulis
Explanatory Coherence, Partial Truth and the Distinction Between Validity and Utility of Psychiatric Diagnosis

Among the foundational problems facing contemporary psychiatry, the problem of the validity of its diagnostic constructs, such as e.g. those of schizophrenic or bipolar disorders, remains still not only unsolved but even very poorly understood. Several influential authors have recently claimed that psychiatric diagnostic constructs should be evaluated on the sole grounds of their “utility”, understood as providing “nontrivial information about prognosis and likely treatment outcomes and/or testable propositions about biological and social correlates”. Moreover, in support of their proposal for a robust distinction between the validity and the utility of psychiatric diagnostic constructs, the same authors claimed that whereas the former is a categorical or an “all or nothing” and context-free matter, by contrast, the latter is a matter of degree and, at least in part, context-dependent. In my presentation, I will try to show that the validity of psychiatric diagnostic constructs, understood as the degree of factual truth of idealized conceptual models of human psychopathological reality, is also a matter of degree. Moreover, I will argue that the pragmatic utility of psychiatric diagnostic constructs is parasitic on their validity, being one though not the sole of its indicators or criteria. Finally, I will sketch an alternative scientific realist account of the validity of psycho-diagnostic constructs along with an epistemic index thereof, stressing the need of their integration with mechanistic explanations within an explanatory coherence framework.

Thomas Sturm
Metacognition and the Rationality Debate in Psychology

Defenders of Kahneman & Tversky’s heuristics-and-biases approach (HBA) and the fast-and-frugal heuristics approach (FHA) of Gigerenzer and his colleagues are notoriously divided over fundamentals concerning human rationality. Mostly, this dispute concerns methodology as well as the norms of rationality against which to evaluate reasoning. Neglected is another set of important questions: What distinguishes reasoning from other mental processes? Which processes can be called rational or irrational *at all*? I first introduce the intuition that metacognition – the higher-order knowledge and principles by which we know and select first-order reasoning strategies – plays an essential role in reasoning. Next, I argue that the two approaches tend towards either excessive dogmatism or liberalism about this intuition. While the HBA demands that reasoning always be accompanied by metacognition, the FHA often implies that reasoning can be excellent even when no deliberation, no conscious evaluation of strategies is invested. Finding a firm middle ground between these extremes is a daunting task. One has to do justice to the plurality of kinds of metacognition and of functions of reasoning. Moreover, the question of how metacognition is related to reasoning can be meant conceptually, empirically, or normatively. This leads to new questions for philosophers and psychologists: (1) What kinds of metacognition are constitutive for problem solving to count as reasoning? (2) What kinds of metacognitive rules do people rely on –logical, probabilistic, or heuristic? (3) Is metacognition useful for successful first-order reasoning?

Matt Bateman
Experimental Inquiry in Cognitive Neuroscience

Experimental research in cognitive neuroscience research purports to follow a hypothetico-deductive pattern: a hypothesis is proposed, its consequences are translated into a form amenable to statistical hypothesis testing, and experimental results confirm or disconfirm the hypothesis. The form of statistical inference appears to be hypothetico-deductive, and the value of the research is understood to be primarily confirmation-theoretic. I will argue that experiments play a richer and more dynamic role in cognitive neuroscience than a confirmation-theoretic role and that the hypothetico-deductive model of experimentation is misleading. It is misleading with regards to both the value of the experiments and their methodology. The primary value of experiments often does not lie in the bearing of the experiment on the hypothesis in question. And the methodology of the experiments is often better understood as exploratory rather than confirmatory.

Thursday, 6 October

09.00-11.00

Symposia

Michael Weisberg, Julia Bursten, Robin Hendry and Paul Needham
Is there a Nature of the Chemical Bond?

In modern chemistry, bonds are invoked to individuate chemical substances, to explain their physical properties, and to describe the mechanisms of transformations between them. They play a conceptual and explanatory role in chemistry comparable to that of genes in modern biology. Yet there is a similar lack of clarity and agreement about how, exactly, they are realized at the level of more fundamental theories.

This symposium investigates the phenomenon of chemical bonding from a variety of perspectives in contemporary philosophy of science. In this symposium we aim (1) to survey the landscape of philosophical problems associated with chemical bonding and (2) to develop chemical case studies that can provide useful insights for philosophy of science more generally.

The concept of the chemical bond is associated with a body of structural theory developed to explain a broad range of phenomena that are observed and manipulated by chemists, physicists, biologists, and neuroscientists alike. At the core of all these phenomena lies the transfer of energy, which governs changes in the structure of atoms and molecules, and in turn the formation and annihilation of any chemical substance. These changes are collectively known as chemical reactions, and they play integral if not always starring roles in almost any phenomenon of interest to scientists. Structure, bonding and mechanism are unifying themes across chemistry, material science, spectroscopy and molecular biology. In order better to understand the roles of chemical reactions in systems of scientific interest, it is necessary to get a better handle on the nature of the bonding behaviors that drive these reactions. Our symposium aims to begin this process by discussing a number of cases that raise specific philosophical puzzles in light of questions about how to define or explain particular classes of bonding.

Lucie Laplane, Francesca Merlin, Antonine Nicoglou and Thomas Pradeu
From Evolution to Development and Back: Towards a Developmental Theory

The present symposium investigates the possible foundation of a theory of development in biology. Development is usually defined as the set of mechanisms that generate an organism starting from the egg cell. Yet this symposium will show how this definition has recently been questioned and therefore why the formulation of a theory of development seems all the more necessary. We will address the following questions: When does development begin and when does it end? What is the developing entity? More generally, which kinds of factors are involved in the process of development, and so are part of what we can call the developmental system? The questions above will be addressed having in view to answer to the more general question of what development is. More precisely, in order to go beyond the existing philosophical and biological works on development, we will start by looking at some specific features of development on its own, independently of its potential role in evolution. We will focus our attention on the following features of development: its organization in space and in time, the stochastic character of the biochemical mechanisms involved and its relationship with a variety of environmental factors. The present symposium, even though taking as a starting point development independently of its possible evolutionary impact, intends to move back and contribute, at the end, to the "EvoDevo" debate. Indeed, we will show that it is only by clarifying the kind of biological process development is, and by identifying its spatio-temporal boundaries, that the elaboration of a theory of development may be possible, and the very relevance of development in evolution can be assessed.

Philosophy of Quantum Mechanics I

Laura Felline
It’s a Matter of Principle. Principle Reconstructions of QT and Their Contribution to the Understanding of the Quantum World

We contribute to the debate about the Information-Theoretic Principle Reconstruction (ITPR) program of Quantum Theory (QT) by disentangling the different aims of such program and assessing the resources of ITPRs for the achievement of each of such aims.

1) A possible motivation for the ITPR program is the quest for solid physical bases for QT. We consider the choice of information-theoretic principles and argue that their heavy load of theory and unsure status as physical principles represent a weakness for the program.
2) A second motivation is the search for an explanation of quantum phenomena, independent of ontological, interpretational claims. We illustrate the main features of a typical 'principle-explanation' and characterise it as a top-down, non-metaphysical explanation (but not an explanation by unification). Due to these features, principle-explanations can aspire to successfully explain structural properties of QT, while it cannot explain the occurrence of single events. The latter can only be explained within an interpretation of the theory.
3) Rather than being alternative to the interpretation of QT, ITPRs is sometimes put forward as providing one. Within such a view, information is a new physical primitive and the world is, at its bottom, only information. We argue that the explanatory role of information within the proposed account of principle-explanation does not ground the claim of an ontological priority of information within QT.

Juan Sebastián Ardenghi, Olimpia Lombardi and Martín Narvaja
Consecutive Measurements and Modal Interpretations

The phenomenon of the correlations between the outcomes of consecutive measurements is strongly entrenched in the quantum knowledge of practicing physicists. They usually explain these correlations in a straightforward way by means of the collapse hypothesis. On the other hand, since modal interpretations are no-collapse interpretations, they need an alternative explanation for those experimental results. In this paper we will argue that consecutive measurements are not a threat for modal interpretations since collapse is not indispensable for explaining the correlations arising in that experimental situation. Nevertheless, the same argument will show that modal interpretations need to revise the role assigned to reduced states in their interpretative framework.

Albert Solé
The Redundancy Argument and the Many Interpretations of Bohmian Mechanics

In recent years, supporters of the Many-worlds Interpretation [MWI] have argued that Bohmian mechanics [BM] already has a many-worlds structure built in the wavefunction and that the addition of Bohmian particles to such an ontology is superfluous and redundant. People both endorsing and dismissing the redundancy argument have generally ignored the fact that BM is not an interpretation of quantum theory but a theory on its own that admits many different interpretations. Here, I reconsider the redundancy argument and show that when the issue of interpretation is adequately assessed, new points can be derived, favoring the Bohmian side of the debate. First, there are interpretations of BM that do *not* postulate a wavefunction-based ontology. It is clear that these interpretations cannot be undermined by redundancy and, I claim, the interpretive latitude available here has been unfairly overlooked in the literature. Second, a careful look at the most prominent interpretations of BM committed to wavefunction realism reveals that, within these approaches, the wavefunction is interpreted very differently than within MWI. I claim that this is enough to block the redundancy argument or, at least, to motivate a principled restriction of the functionalist criterion underlying such an argument.

Richard Healey
How to Use Quantum Theory Locally to Explain EPR-Bell Correlations

I sketch a pragmatist interpretation of quantum theory and show how to use it to explain EPR-Bell correlations consistently with relativity. Quantum theory is not a locally causal theory, not because it *violates* Bell's local causality condition based on the intuitive principle that "The direct causes (and effects) of events are nearby, and even the indirect causes (and effects) are no further away than permitted by the velocity of light", but because that condition is simply inapplicable to it. Any agent can use quantum theory to show why EPR-Bell correlations are to be expected, whether the relevant measurement events are time-like or space-like separated. For space-like separated measurements of vertical/horizontal polarization of each photon from a pair in Bell state Φ^+, an agent's explanation of why the distant measurement outcome matches his own appeals neither to a preferred frame nor to any direct connection or influence between these events. Here, as elsewhere, quantum theory helps one explain an initially puzzling phenomenon not by locating it in a causal net but by showing why its occurrence is just what one should have expected in the circumstances.

Mechanisms in Explanation

Eleanor Knox
The Limits of Abstraction: Finding Space for Novel Explanation

Several modern accounts of explanation acknowledge the importance of abstraction and idealization for our explanatory practice. However, once we assume a role for abstraction, questions remain. I ask whether the relation between explanations at different theoretical levels should be thought of wholly in terms of abstraction, and argue that changes of variable between theories can lead to novel explanations that are not merely abstractions of some more detailed picture. I use the example of phase transitions as described by statistical mechanics and thermodynamics to illustrate this, and to demonstrate some details of the relationship between abstraction, idealization, and novel explanation.

Jaakko Kuorikoski and Petri Ylikoski
How Organization Explains

Constitutive explanations explain a property of a whole with the properties of its parts and their organization. The most developed account of mechanistic explanation is due to Carl Craver, who employs Jim Woodward's account of explanation to provide criteria of explanatory relevance for mechanistic information. However, Craver's mutual manipulability criterion can only capture the constitutive explanatory relevance of causal properties of parts and leaves the organization-side of mechanistic explanation unaccounted for. "Organization" cannot be defined as an additional manipulable explanatory variable, because there is no single unique type of organizational dependency linking the organization of the parts to the property of the whole. What is required is a schema or a typology that would provide us with a better grasp of this dependence. In this paper, we link William Wimsatt's (2007) conditions of non-emergence (aggregativity) of a system property to Woodward's theory of explanation to provide such a taxonomy of organizational dependence and thus the criteria of constitutive explanatory relevance of organization. We apply this framework to two cases from social science and systems biology, both fields in which the organization plays a crucial explanatory role: agent-based simulations of residential segregation and the recent work on network motifs in transcription networks in cells.

Robert C. Richardson, Fred Boogerd and Frank Bruggeman
Articulating Mechanisms

We contrast two broadly different approaching toward developing mechanistic explanations. One focuses on modelling system behaviours, without specific attention to information concerning the composition of the system. The other constructs models based specifically on independent information concerning the parts, processes, and organization present. On the former approach, the development or "articulation" of mechanistic models includes four phases, beginning with an initial adequate description of systemic behaviour, and ending with the articulation of an elaborate a causal model. This approach to modelling the behaviour of complex systems has a number of strengths. Most importantly, it emphasizes the idea that we must at least begin with some reasonably robust phenomenon to be explained. It demands a causal model. The latter approach follows a different, more constrained, methodology. These latter models emphasize functional *composition* rather than functional *decomposition*, with more detailed structural data, including kinetic data concerning componential behaviour, and complex networks. Much of contemporary molecular systems biology offers its allegiance more to the detailed modelling of pathways, given better information about component capacities, and somewhat less to its cybernetic ancestors. We will offer an analysis of heuristics in mechanistic explanation of this latter sort and emphasize the dynamic 'fluid' character involved in the process of articulating and elaborating mechanistic models.

Samuel Schindler
Mechanistic Explanations: Asymmetry Lost

The popular mechanistic account of explanation (Machamer et al. 2000) at first glance promises to capture explanatory asymmetry: to explain a phenomenon is to describe the mechanism that *produces* the explanandum phenomenon. Conversely, phenomena do not explain mechanisms. On the original proposal by Machamer et al., however, the nature of the production relation remains unspecified. Recently Craver (2008) has tried to fill this gap in terms of Woodwardian active counterfactuals. Craver shows that the simple active counterfactual 'test' (roughly: would Y change if we were to intervene on X?) is insufficient for individuating mechanisms. Craver proposes that we need to run this test in the opposite direction as well, namely from the explanandum phenomenon to the mechanism: would the mechanism change were we to intervene on the explanandum phenomenon? Craver calls this the "mutual manipulability" criterion for mechanisms, which a reviewer dubbed "one of the main achievements of [Craver's] book" (Levy 2009). As Craver notes himself, however, in a mechanistic account that requires the satisfaction of the mutual manipulability criterion, the relationship between mechanism and phenomenon "is only uncomfortably viewed as causal" (p. 153). This move is critical: it robs the mechanistic account of its apparently built-in explanatory asymmetry. Since Craver's account can be said to be the most elaborated account for explicating mechanistic production relationships, and since explanatory asymmetry is widely considered to be one of the touch stones of explanation, the mechanistic account in its current guise must be deemed incomplete.

Philosophy of the Cognitive Sciences I

Lilia Gurova
Principles vs. Mechanisms in Cognitive Science

A kind of consensus has been formed in the last years that the mechanistic explanations best characterize "the explanatory project of cognitive science" (Bechtel 2010). The only role for law-like generalizations, the proponents of the mechanistic explanatory project claim, is to describe various effects but such law-like generalizations, they say, cannot play any explanatory role insofar as the effects which they describe are themselves in need of explanation.

The aim of this paper is to show that: (1) the mechanistic explanations are not "the only game in town" in cognitive sciences; principle-based explanations have been often advanced to cope with important empirical findings; (2) the principles involved in such explanations are not mere descriptions of the established effects, they rather serve as *explanans* for the described effects; (3) the principle-based explanations in cognitive sciences could not be subsumed under the deductive-nomological (DN) model of scientific explanation: rather than a general premise in a deductive schema, the principles used in cognitive sciences function as an inferential template which is more in tune with Toulmin's (almost forgotten) Wittgensteinian account of the inferential role of general principles in science (Toulmin 1953).

The role of the principle-based explanations in cognitive sciences will be illustrated on the example of the basic level effects, one of the few genuine discoveries (Murphy 2002) which have been made in the research of categorization in the last 30 years.

Lena Kästner
Interventionism Cannot Cross

Scientific investigation into cognitive phenomena is not restricted to same-level (i.e. merely behavioral or merely neural) experimentation. Any serious attempt to make sense of the explanatory practices in cognitive science will therefore have to tell a story that takes cross-level (i.e. top-down and bottom-up) experiments into account. The interventionist account of causation has recently been promoted as promising in this context.

Taking a closer look at both interventionism and the cross-level studies employed in cognitive-scientific practice, I will argue, however, that interventionism cannot keep this promise: within the interventionist framework, there is no convincing interpretation of the kinds of cross-level experiments so popular among empirical cognitive scientists.

Some straightforward possibilities of modifying the interventionist framework will be considered and evaluated with respect to their potential to account for cross-level studies. None of them does the trick, however. Hence, we may have to bite either of three bullets: (i) buy a ragbag ontology, (ii) accept that interventionism does not cash out genuine causal relations, or (iii) become reductive and adopt the position that events on one level are actually identical to events at another level.

Markus I. Eronen
Pluralistic Physicalism and the Causal Exclusion Argument

There is a growing consensus among philosophers of science that scientific endeavors of understanding the human mind or the brain exhibit explanatory pluralism. Relatedly, several philosophers have in recent years defended an interventionist approach to causation that leads to a kind of causal pluralism. In this talk, I explore the consequences of these recent developments in philosophy of science for some of the central debates in philosophy of mind. First, I argue that if we adopt explanatory pluralism and the interventionist approach to causation, our understanding of physicalism has to change, and this leads to what I call pluralistic physicalism. Secondly, I show that this pluralistic physicalism is not endangered by the causal exclusion argument.

Emma Mª Martín Álvarez, Paco Calvo and Ángel García Rodríguez
Cognitive Mechanisms as Biological, not Physical Mechanisms

An interesting dissimilarity between physics and neurobiology concerns the fact that explanations in neurobiology tend towards the uncovering of increasingly particular mechanisms, whereas explanations in physics are often regarded as paradigms of generality. The question is whether cognitive mechanisms resemble neurobiological, rather than physical mechanisms. If so, a corollary of the particularist character of mechanistic explanations in cognitive science is that increasingly particular mechanisms go with particular *explananda*. One should not set cognitive scientists impossible tasks by characterizing target *explananda* in such general terms that no cognitive mechanism could be found for them.

One application of this could be the ‘systematicity of thought’ challenge. If the phenomenon of systematicity is characterized in competence-level terms, no psychology-involving mechanistic explanation will be available. But, by modelling performance with neurobiologically constrained neural networks, cognitive mechanisms with particular lower-level components and activities may be identified. The behaviour of a cognitive system unfolds in time, and matches thus with performance itself, and not with an abstract competence posited externally. If the phenomenon of systematicity is not fixed as a competence, but rather corresponds with more specific systematicity-related *explananda*, the particularist character of mechanistic explanations is vindicated in so far as the space of network solutions is exclusively constrained by the form of the ecological signal that the network is fed with, together with the constraints that arise from the level of implementation. Cognitive scientific explanations, we conclude, boil down then to uncovering mechanisms, much like neurobiology unfolds particular organized structures of components and their activities.

Thursday, 6 October

11.30-13.30

Symposia

Adam Caulton, David Baker, Hans Halvorson, Klaas Landsman and Noel Swanson
Symmetries, Superselection and Statistics

This symposium is an opportunity to share new research in a variety of ongoing issues surrounding particle identity, permutation symmetry, superselection and statistics in quantum theories. The locus of this symposium is the rivalry between competing approaches to deriving the collective behaviour of particles.

One tradition, which we may put under the banner *The Indistinguishability Approach*, begins with the "full" joint Hilbert space and generates the allowed collective behaviours via a superselection rule imposed by a permutation invariance requirement over the algebra of observables *(*aka: *The Indistinguishability Postulate).* On this approach, one derives symmetry types corresponding to the irreducible representations of the symmetric group: fermions, bosons and, for assemblies of three or more particles, *paraparticles*.

According to the other tradition, which we may put under the banner *The Topological Approach*, one instead considers the various inequivalent quantizations of a classical assembly of equivalent particles, whose configuration space has non-trivial topological features due to a prior implementation of permutation invariance applied to *classical* states. According to the folklore, in the case of three or more spatial dimensions, one derives fermions and bosons, but *not* paraparticles. However, in two dimensions, one derives symmetry types corresponding to the irreducible representations of the *braid group:* fermions, bosons, *and* a continuum of alternative statistical behaviours, collectively known as *anyons*.

How do we choose between these competing approaches? The Topological Approach *appears* to rule out paraparticles, which seem not actually to be observed; and (in two spatial dimensions) predicts anyons, which *have* been observed in a variety of systems which approximate two-dimensionality (e.g. the fractional quantum Hall effect as observed in electrons confined to a thin conductive plate). This appears to favour the Topological Approach hands down. But matters are not so simple, as we will investigate.

Katie Steele, Charlotte Werndl, Arthur Petersen, Jan Sprenger and Seamus Bradley
The Reliability of Climate Model Predictions

Climate scientists build complex computer simulation models in order to predict how the climate will evolve over time, given various scenarios for greenhouse gas emissions. The question is: how do we assess which climate models, if any, yield good predictions for future climate variables? In fact, ideally the decision-maker needs to know just *how* confident one should be in the various climate models on offer, and ultimately how confident one should be in the various values the models collectively assign to future climate variables. That is what we draw attention to and make steps to address in this symposium—the possibilities for assessing the *reliability* of climate models and their associated predictions.

Papers by Bradley and Steele & Werndl investigate special issues in confirmation that confound the assessment of climate model predictions. Bradley pursues the significance of robustness: If all climate models in an *ensemble* agree on certain climate predictions, does this have special confirmatory significance? Steele and Werndl investigate model tuning, and address the worry of some climate scientists that evidence used to tune a model (i.e. determine free parameters) cannot also be used to confirm the model.

Sprenger and Petersen pursue a broader line of inquiry with respect to assessing the reliability of climate models: they ask what sort of reliability measures are suitable. Sprenger explores a way of understanding Bayesianism such that it can be applied in a flexible way to climate modelling, namely *instrumental Bayesianism*. Petersen, on the other hand, argues that qualitative measures of reliability or confirmation may be as good as it gets in climate science, given there are at least 3 separate dimensions of reliability—statistical reliability (agreement with data), methodological reliability (extent of model/parameter uncertainty and idealizations) and sociological reliability (trustworthiness of the scientists themselves).

In short, the four papers of this symposium explore the underlying issues of climate model reliability by drawing on and extending work in the philosophy of science, particularly in the realm of simulation models and confirmation theory.

Realism and Anti-realism I

Emma Ruttkamp
A Novel Defence of the Retrospective Nature of Reference

I offer a counter to arguments against the retrospective aspect of selective realist accounts of science. My argument rests on a definition of truth as 'historied reference' which suggests that we cannot be realists about anything except the progress affected by myriad science-reality interactions that are constantly moving on a continuum of increased 'fitness' determined according to empirical constraints. Moreover to reflect this movement accurately, I suggest there is a corresponding continuum, ranging from stark instrumentalism to full-blown realism, on which verdicts about the status of the knowledge conveyed by theories move.

I first discuss Stanford's critique of a selective realist account of science. I then counter his concerns by claiming that identifying which features of theories are success-generating can only be done if it is clear (1) why the 'idle' parts of theories were idle or worthy of rejection and (2) how the theory or existing knowledge claims had to be adapted to make sense of rejecting past 'idle' parts. I thus agree with Psillos that no 'explicit' criterion for selective confirmation exists, although my solution differs from his. I then explain the notion of 'historied reference' as an account of causal reference that is much richer than a triumphant announcement of a single theory's success. In conclusion I show that realist evaluations of science, although retrospective, in my terms are trustworthy and also ensure the trustworthiness of science if they are based on truth-as-historied-reference.

Alberto Cordero
Theory-parts for Realists

The Divide and Conquer approach to scientific realism requires a criterion for specifying theory-parts worthy of realist commitment. Retention across theory-change and being regarded as successful and free of specific doubts are not enough (the ether of light arguably satisfied both conditions). The selected parts must be considered exceedingly likely true as well. This paper argues for a criterion lifted from scientific practice, specifically from the gradual disclosure of unreliable theory components and reliable parts in the natural sciences. It is argued that the resulting identifications spring from critical scrutiny along several overlapping fronts, five in particular: (1) Hostile Probing of the central tenets of a theory, particularly by opponents in reaction to the theory's initial success. (2) Probing of Auxiliary Assumptions, typically conducted by supporters of the theory upon encountering difficulties in its application. (3) External explanation ('elucidation') of theoretical assumptions, achieved by accounting for them in terms of in independently well-established theories. (4) Efforts to identify adequacy conditions for future theories, particularly when a theory faces persistent difficulties and scientists begin to look for alternatives. (5) Explanation of the successes of superseded theories, an achievement that often deepens specific parts of earlier theories by providing a causal and/or structural explanation for some of their characteristic tenets.

Dean Peters
Partial Realism, Anti-realism and Deflationary Realism: Can History Settle the Argument?

Many contemporary realists attempt to distinguish elements of a theory that are *essential* for its predictive success, arguing these are not discarded in instances of theory change. I call this strategy "partial realism":

> In those cases where a theory enjoys novel predictive success, and some element of that theory is essential to that success, then we (i) should expect this element to be retained in successor theories; and (ii) have good evidence that *this element* represents a corresponding feature of the world.

In this paper, I advocate "deflationary realism" (DR), which is identical to partial realism, except that it lacks (ii).

DR is ambiguous in respect of several key terms. For "predictive success", I support the use-novelty account due to Zahar and Worrall. For "essential", I argue in favour of and my own "minimal sub-theory" account. For "retained", I argue for some version of Post's "generalized correspondence principle", but suggest some extensions to it.

Finally, I argue that DR has several advantages over existing competitors. Firstly, it remains agnostic about the metaphysical questions that typically divide realists and anti-realists. Secondly, it makes only empirical claims about the history of science, and so is in principle compatible with anti-metaphysical views like constructive empiricism. Thirdly, it is nevertheless a stronger claim than constructive empiricists are willing to accept. Fourthly, provided the term "essential" is cashed out appropriately, DR is potentially falsifiable by counterexamples from the history of science.

Luca Tambolo
The Normative Naturalist against the Pessimistic Induction

In this paper I explore a line of argument against the Pessimistic Induction that is suggested by Laudan's normative naturalism. I argue that two conflicting versions of normative naturalism, which Laudan never explicitly tells apart, uneasily coexist within his writings. These versions of normative naturalism are dubbed here, respectively, "Reticular normative naturalism" and "Historicist normative naturalism". Although both Reticular normative naturalism and Historicist normative naturalism revolve around the claim that our choice of methodological rules (and of scientific theories) is constrained by our knowledge of how the world works (our factual knowledge), each of them brings with it a different interpretation of the notion of "factual knowledge". Reticular normative naturalism has it that factual knowledge is knowledge of facts concerning the structure of the world, as expressed by our currently accepted theories. On the other hand, within Historicist normative naturalism, "factual knowledge" is equated with "knowledge of facts concerning the history of science"; consequently, history of science is assigned a central role in the assessment of methodological rules (and of scientific theories). I claim that the Pessimistic Induction goes hand in hand with Historicist normative naturalism, but not with Reticular normative naturalism; and as soon as the shortcomings that affect Historicist normative naturalism are exposed, the Pessimistic Induction gets debunked.

Science as Collective Knowledge

Hanne Andersen
Acting out of Line: On Joint Accept and Unilateral Rescission in Scientific Groups

Within the last decade, a substantial literature has developed that discusses the social aspects of scientific knowledge, including the notions of collective knowledge and collective acceptance. While much of the discussion has focused primarily on what it means for a group of scientists jointly to accept a scientific claim, there has still been little focus on one of the major implications of joint acceptance, namely the constraints it poses on unilateral rescission by individual group members from the jointly accepted claim. In this paper I shall provide an analysis of the constraints on unilateral rescission posed by a joint acceptance made by a group. I shall argue that we need to consider several aspects in understanding rescission and rebuke, namely both the epistemological aspect of joint acceptance related to how jointly accepted views are justified, and the normative aspect of joint acceptance related to the obligations related to establishing shared intensions with other human beings.

Cyrille Imbert
Collective Science: How not to Lose Scientific Understanding?

This talk is devoted to trying to clarify under which conditions a scientific group can be said to have and develop scientific understanding of an item of knowledge. In the first part of the talk, I argue that the possession scientific understanding is a specific problem for collaborative science, even if social epistemologists have so far largely ignored it. I emphasize that a plausible account of how groups understand should make clear why the fact that groups as groups may have specific additional understanding is by no means obvious and what the relations between individual and social understanding are.

In the second part, I present some features usually ascribed to individual scientific understanding. I proceed in part three with methodological discussions about how the notion of group understanding should be investigated if it is both to catch some key features that are commonly ascribed to scientific understanding and also be fruitful in order to analyze in what sense groups can develop more or less scientific understanding. In the final part of the talk, I present a deflationary working notion of social understanding that takes into account both the ability of groups to develop collective abilities and the partial individual understanding that individual scientists can develop within collective tasks. I highlight in particular the crucial role of logical independence, modularity and sketchability in the development of group scientific understanding.

Adam Toon
Friends at Last? Distributed Cognition and the Cognitive/Social Divide

Distributed cognition (d-cog) claims that many cognitive tasks are realised not within the minds of individuals but in processes that are ‘distributed’ across social groups, tools and the wider material and social environment. Recently, Nancy Nersessian and Ronald Giere have suggested that adopting this approach might allow us to overcome a longstanding opposition between cognitive and social explanations of science. In this paper I want to explore this idea in detail. While d-cog offers a promising approach, I will suggest that its potential for reconciling cognitive and social theories of science may be limited, for three reasons: First, there are important disputes between cognitive and social theories, particularly concerning scientific representation, on which a d-cog account will remain silent. Second, unless d-cog theorists endorse a radical version of the so-called ‘extended mind thesis’, d-cog would appear to offer an alternative social explanation of science, rather than one which reconciles cognitive and social accounts. Finally, where social explanations can be recast in d-cog terms, this reformulation will not be acceptable to many sociologists of science, since it implies that science is not essentially a social phenomenon.

Thomas Boyer
Is a Bird in the Hand Worth Two in the Bush? Or, Whether Scientists Should Publish Intermediate Results

A part of the scientific literature consists of intermediate results which are considered as first steps within a longer project: scientists often publish a first result in the course of their work, while aware that they should soon achieve a more advanced result from this preliminary result. Should they do so, and publish their intermediate results? As a scientist certainly has some competitors working on the same project, it may be safe indeed to be the first to publish it. But the drawback is that it helps his competitors, who will be on the same footing to compete for the remaining steps. Such a reasoning seems to be implicitly an informal economic argument, and the aim of the paper is to clarify and to assess it. To this end, I investigate it in a rational decision framework, supposing some utility or preferences, and I propose a formal model. It is a sequential model where steps have to be passed in order, and scientists progress from step to step with a probability per unit time; they can choose freely their publishing strategies. Classical questions of social epistemology are tackled, like the possible mismatch between individual and collective rationality, and the effects of non-epistemic motives. The model shows that even individualist scientists should publish when the steps have the same size and difficulty, thus in agreement with the collective demands. When the steps are not homogeneous, I suggest the existence of a minimum threshold about the results worth publishing.

Philosophy of the Cognitive Sciences II

Víctor M. Verdejo

Computationalism, Connectionism, Dynamicism and Beyond: Looking for an Integrated Approach to Cognitive Science

Cognitive science is a discipline in continuous evolution where different and conflicting research strategies are permanently brought to the fore. As a consequence of discussion in the last 30 years or so, cognitive scientists are now apparently required to choose between at least three different overall approaches: the computational, the connectionist, and the (embodied) dynamicist. In this paper, I present an analysis of the aforementioned overall approaches in terms of Marrian levels so as to show that, under certain standard readings, these approaches (1) centre research in one of Marr's levels, taking the other levels to be irrelevant or else secondary; (2) as a consequence of (1), they inevitably lead to incomplete and flawed accounts of cognition. As an alternative to such standard readings, I briefly articulate the not sufficiently emphasized possibility of integrated accounts of cognition at all levels where (a) the problem of incomplete accounts of cognitive phenomena does not arise in the first place and (b) these overall approaches are after all compatible with each other.

Norman Sieroka

Neurophenomenology of Hearing: Relations to Intentionality and Time Consciousness

The aim of my talk is to adopt a neurophenomenological stance and to illustrate the particular relevance of auditory phenomena in improving our understanding of intentionality and time consciousness.

Recently, there has been an increasing interest in relating results from neuroscience and psychology to concepts from Husserlian phenomenology; in particular, those of an extended perceptual present and of time consciousness. However, whereas these discussions have nearly exclusively focussed on vision, I will focus on hearing. For auditory phenomena play a specific role in the neural representation and perception of duration. Other than the visual system, the auditory system exhibits a sensory memory trace and shows the most direct relation between temporal integration and perceptual qualities (temporal pitch). Arguably, these distinctive features of the auditory system are philosophically relevant for several reasons. Within neurophenomenology, the structural features of the auditory memory trace – which, on the phenomenological level, may be described in terms of "immediate memory" and "preattentive sound anticipation" – can be of importance for debates about the status and nature of protentions. On a broader level, issues involved here may also be relevant for discussions about non- or pre-conceptual states and pre-propositional intentionality. Finally, phenomena like temporal pitch may be of interest also in relation to general discussions about perceptual qualities.

Lieven Decock and Igor Douven

Qualia Compression

Qualia inversion scenarios have played a key role in various philosophical debates. Most notably perhaps, they have figured in skeptical arguments for the fundamental unknowability of other persons' phenomenal experiences. For these arguments to succeed, it must be assumed that whether one has normal or inverted qualia may go forever unnoticed. This assumption is now widely held to be false for two reasons. First, in view of the asymmetry of phenomenal colour space, no nontrivial automorphisms within phenomenal colour space are possible. Second, it is assumed that various properties of colour qualia (being unmixed, being fully saturated, belonging to a colour category, or being warm or cool) are intrinsic. We argue that the possibility of undetectable qualia compression is invulnerable to the objections that have been levelled against qualia inversion arguments, and that qualia compression scenarios support a full-blown skepticism regarding other people's color experiences.

Thursday, 6 October

17.00-19.00

Symposia

Thomas Müller, Markus Schrenk, Jesse Mulder and Carl Hoefer
Can We Really Lewis the Laws of Nature?

Ever since its development by David Lewis, the Best Systems strategy has enjoyed great popularity in contemporary philosophy of science as well as in related areas such as metaphysics. It consists in grounding the problematic notion of laws of nature in the totality of unproblematic matters of fact, thus promising to show that we can have laws of nature without being committed to questionable extensions of our ontology or to scientifically inaccessible metaphysical postulates and principles.

But can this promise be fulfilled? Since its inception, the Best Systems strategy has also been confronted with various challenges. The recent debate has extended attention beyond the notion of laws of nature to include the status of the special sciences, of objective chances, and of our familiar objects. This has resulted in a variety of detailed proposals for analysis, but new challenges have arisen as well.

This symposium aims at assessing the current debate in order to arrive at a balanced view of the precise costs and benefits of the Best Systems strategy. Accordingly, there will be two contributions pointing out problems (Mulder, Müller), and two contributions pointing out benefits (Hoefer, Schrenk). There will be ample time for a plenary discussion at the end.

Francesco Guala, Benoit Dubreuil, Christophe Heintz, Eduard Machery and Alejandro Rosas
Cognitive and Evolutionary Foundations of Human Sociality

The increasing integration between psychology, evolutionary biology, and economics is one of the most significant trends in the behavioural sciences. The merger of these disciplines is taking place mainly at the level of theory and methodology, but unsurprisingly is also influencing those areas of philosophy that are closest to the social, biological, and cognitive sciences – like social ontology, the philosophy of mind, and even ethics and political philosophy. Another important new development of the last decade has been the engagement of philosophers with empirical data – as in so-called “experimental philosophy” – and the rise of a new generation of researchers working across the boundary that used to separate empirical from purely conceptual inquiry. This symposium draws together two research agendas, promoting exchange between empirically-minded philosophers interested in the cognitive and evolutionary foundations of human sociality. In particular, it focuses on the role of *social norms* in the emergence of cooperation and the regulation of conflict in human societies. While this topic has attracted an increasing number of philosophers over the last decade, they have approached it from different directions (e.g. from the philosophy of psychology, the foundations of game theory, or the philosophy of biology) relying on different frameworks and concepts, with the result that the exchange of information has sometimes been difficult. We shall try to overcome these hurdles and promote further research in the philosophical foundations of human sociality. Topics to be discussed include: the relation between the size of the group and the human cognitive capacity to track the reputations of fellow group-members; the role of expectations in coordinating social conformity and sanctions; the relation between group identity and social norms; the distinction between moral and conventional norms, and the role of affect in normative behaviour.

Philosophy of Quantum Mechanics II

Aristidis Arageorgis and Chrysovalantis Stergiou
On Particle Phenomenology Without Particle Ontology: How Much Local is Almost Local?

Recently, Clifton and Halvorson have tried to salvage a particle phenomenology in the absence of particle ontology in algebraic relativistic quantum field theory. Their idea is that the detection of a particle is the measurement of a local observable which simulates the measurement of an almost local observable that annihilates the vacuum.

In this note we argue that the measurements local particle detections are supposed to simulate probe radically holistic aspects of relativistic quantum fields. We prove that in an axiomatic (Haag-Araki) quantum field theory on Minkowski spacetime, formulated in a Hilbert space H, there is no positive observable C, with norm less than or equal to 1, satisfying the conditions: (1) the expectation value of C in the vacuum state Ω is zero, (2) there exists at least one vector state Ψ in H in which the expectation of C is different from zero, and (3) there exists at least one spacetime region O such that the non-selective measurement of C leaves the expectation values of all observables in the local algebra $R(O)$ unaltered regardless of the state the system is in.

The result reveals a tension between intuitions regarding localization and intuitions regarding causality: to save “particle phenomena” in the absence of particle ontology, one has to feign “particle” detectors with “good” properties as to locality but “bad” behavior as to causality.

Foad Dizadji-Bahmani
Why I am not an Everettian

Everettian quantum mechanics (EQM) results in multiple, emergent, branching quasi-classical realities, its proponents claim. The possible outcomes of measurement as per ‘orthodox’ quantum mechanics, are, in EQM, all instantiated. Given this metaphysics, Everettians face the ‘probability problem’ - how to make sense of probabilities and recover the Born Rule. To solve the probability problem, Everettians have derived a quantum representation theorem. There is a notable argument against the soundness of the representation theorem based on so-called `branch counting'. Everettians have sought to undercut this argument by claiming that there is no such thing as the number of branches. In what sense is it both true that there is no such thing as the number of branches and that there are multiple branches? Various answers to this question have been given. I first, show that these can be categorised into two kinds: that there are indeterminately-many branches or that there are indeterminably-many branches. I then argue that neither suffices to undercut the argument against the quantum representation theorem. I conclude that the quantum representation theorem is unsound and that the probability problem facing EQM persists.

Iñaki San Pedro
Freeing Free Will from Conspiracy

The aim of this paper is to assess so called "no-conspiracy" condition, or more neutrally "measurement independence", in the context of common cause explanations of EPR correlations. I shall challenge the widespread view that "measurement independence" adequately represents the requirement that EPR experimenters have free will. (In particular "measurement independence" is most commonly taken as a necessary condition for free will.) A number of implicit assumptions can be identified in this regard, all of which can be challenged on their own grounds. As a result, I conclude that "measurement independence"-type conditions are not adequate conditions to reflect the fact that the EPR experimenters have free will. More generally, "measurement independence" cannot be justified by appealing to the preservation of the experimenters' free will when it comes to common cause explanations of EPR correlations.

Dunja Šešelja and Christian Straßer
Abstract Argumentation Applied to Scientific Debates

Abstract argumentation has been shown to be a powerful tool within many fields such as artificial intelligence, logic and legal reasoning. In this paper we enhance Dung's well-known abstract argumentation framework with explanatory capabilities. We show that an explanatory argumentation framework (EAF) obtained in this way is a useful tool for the modeling of scientific debates. On the one hand, EAFs allow for the representation of explanatory and justificatory arguments constituting rivaling scientific views. On the other hand, different procedures for selecting arguments, corresponding to different methodological and epistemic requirements of theory evaluation, can be formulated in view of our framework.

Local Epistemologies

Saana Jukola
Defending the Social View on Objectivity

The paper focuses on so called social view on objectivity, according to which the scientific community has an essential role to play in securing the reliability of scientific knowledge. This conception is contrasted with the more traditional individualistic view that associates objectivity strictly with the actions of individuals: their willingness and ability to base their reasoning on data and logic. It is argued that the individualistic conception does not capture what is needed for securing reliable knowledge, because if fails to take notice of the so called underdetermination problem and it paints too bright a picture of our abilities to avoid mistakes as human beings. I use Helen Longino's theory as my exemplar of the social view, discuss the accusations of relativism that it has faced, and show why these allegations are unwarranted.

M. Cristina Amoretti and Nicla Vassallo
Situatedness and Objectivity: Scientific Knowledge without Standpoints

Feminist standpoint epistemologies of the sciences must be acknowledged to possess some important merits which should not be disregarded. In particular, they correctly emphasize that scientific knowledge is socially situated, while also ensuring the strong objectivity of the sciences. However, the very notion of standpoint – being intrinsically linked to notions of better epistemic reliability, privilege, or advantage – brings with it an unavoidable dilemma: it forces its defenders to choose between embracing essentialism (or at least its awkward and unwelcome consequences) and considering all standpoints at the very same level. Our specific aim is to demonstrate that there is no reason to appeal to any feminist standpoint epistemology of the sciences in order to keep its more significant merits, in particular the situatedness of scientific knowledge and the strong objectivity of the sciences. We shall argue that belonging to a standpoint is not necessary to have a particular perspective on the world and that the democratization of the sciences, their pluralism, is the best tool to ensuring their strong objectivity. Our tentative conclusion shall be that the general idea that scientific knowledge is socially situated, produced, maintained, and transmitted is compatible with a defense of the strong objectivity of scientific knowledge together with its normative character.

Endla Lõhkivi
Is Workplace Culture Relevant for Philosophy of Science? A Case Study on Physics and Humanities

Based on the empirical studies of the workplace culture of physics institutes in 2005-2008, and humanities, specifically, history departments, in 2010-2011, Estonia, I discuss the relevance of cultural findings for the philosophy of science. The comparative analysis has been influenced by Stephan Fuchs' sociological analysis of the scientific styles. For empirical study, the method of culture contrast was applied. Applying this method presumes that the initial contrasts should not be seen as the fixed framework for comparison but as hypothetical categories. Starting from the two styles, a more complicated picture appears in the local analysis, more contrasts and diversities emerge to be identified and explained. The analysis of the cultural contrasts and diversities reveals specific inclusion and exclusion mechanisms which in turn are related to scientists' identities, role models and self-reflections. In this study, the scientific communities are not viewed as in social constructivism – qua consensus communities but instead as culturally fragmented identity groups. This allows one to provide criticism of the local practices, whereas social constructivism in science studies mainly has focussed on the consensus formation procedures describing, e.g. how the core sets resolve controversies, without critical involvement. I claim that a shift of focus from group beliefs and values to interpretation and criticism of the local cultural mechanisms of inclusion and exclusion is necessary for the improvement in both – in the theory of science and in science.

Jouni-Matti Kuukkanen
I Am Knowledge: Get Me Out of Here! On Localism and the Universality of Science

It has become increasingly common in historiography of science to understand science and its products as inherently local. However, this orientation is faced with three problems. First, how can one explain the seeming universality of contemporary science? Second, if science is so reflective of its local conditions of production, how can it travel so effortlessly to other localities and even globally? And third, how can scientific knowledge attain validity outside its context of origin? I will argue that the notion of standardization and theories of delocalization manage to explain the 'globality' of science, but that localism denies 'universality' if it is understood non-spatially. Further, localism limits the validity of scientific knowledge unacceptably inside the laboratory walls or other boundaries of knowledge creation. This is not consistent with scientific practice. I will consider on what grounds extra-local knowledge inferences that transcend the boundaries of locality could be seen as justified.

Philosophy of Mathematics

Demetra Christopoulou

On a Double Aspect of Natural Numbers as Abstract Particulars and/or Universals

This paper addresses a dilemma that arises from the linguistic behaviour of arithmetical expressions in two basic ways: they occur, either as *singular terms* or as *predicates* in arithmetical sentences. However, the two forms of their linguistic behaviour (the *substantival* and the *predicative* form respectively) give rise to different accounts of the ontological status of natural numbers. The *substantival* use of arithmetical expressions supports the interpretation of natural numbers as abstract particulars while the *predicative* use of them either supports the interpretation of natural numbers as universals or it provides for a nominalistic account of the arithmetical language.

The paper takes under consideration those interpretations and sketches their special difficulties. Then it investigates the relation among the *substantival* and the *predicative* form. It applies a reductionist approach in order to distinguish the most fundamental of the two forms of syntactical arithmetical behaviour. A first option is to examine whether the *substantival* form is reducible to the *predicative* form or vice versa. However, the paper concludes that among the two forms there is no prevalent to choose as a reduction basis. A second option is based on Ramsey's arguments against the traditional distinction between particulars and universals. The paper moves on to show that a material equivalence between the *substantival* and the *predicative* form might be established and that the double syntactical behaviour of natural numbers is indicative of their double ontological status. Then it articulates an account according to which natural numbers may be construed both as universals and objects.

Paola Cantù

Kant and 20th Century Philosophy of Mathematics

The paper analyzes some influences of Kantian epistemology on 20th century philosophy of mathematics in order to question three popular beliefs in historiography: (1) Kant's contribution is limited to the introduction of the terminology analytic-synthetic; (2) the foundational debate on 20th century mathematics and logic was mainly devoted to logico-semantical issues rather than to epistemological matters, and thus related to the Leibnizian project rather than to Kantian criticism; (3) the only way to defend Kant's epistemology is to separate it from the application to the science of its time (i.e. from the application to Euclidean geometry and Newtonian physics). The first

belief has been already challenged in recent studies on Kant and analytical philosophy.

In order to challenge claim (2), the paper will consider two traditions that share a strong epistemological interest, and the aim of preserving the spirit of Kantian transcendentalism: intuitionism and formalism. In order to challenge claim (3), i.e. to challenge the belief that the Kantian heritage could be preserved only on condition that the epistemological project be separated from the application to 18th century science, the paper will investigate the echo of the remarks made by Kant on the concept of magnitude. They will be compared with the developments of the axiomatic theory of measurable magnitudes developed by Bettazzi, Veronese and Hölder, and especially with the remarks on the distinction between an abstract and a physical notion of magnitude developed by Bob Hale in a neo-logicist perspective.

Mark Colyvan
A Ricci Curvature Tensor by any Other Name

There is something right about the view of mathematics as "the language of science". Thinking of mathematics as a language is useful in appreciating the significance of, and the difficulties encountered arriving at, a good notational system. Good notation is far from trivial. The development of differential geometry, for example, with its Ricci curvature tensor and the like, is intimately connected with the notation employed. But thinking of mathematics as *merely* language is to ignore the other roles mathematics can play in science. I will consider the role good notation can play in prompting new ideas and new developments in mathematics and science. I will look at the recent work on mathematical explanation and argue that there are genuinely mathematical explanations of empirical facts and the transparency of some of these explanations is dependent upon good mathematical notation.

Friday, 7 October

9.00-11.00

Symposia

Raffaella Campaner, Theo Kuipers, Daniel Andler, Olav Gjelsvik and Roman Frigg
New Challenges for Philosophy of Science

This symposium is organized from within the ESF Research Networking Programme "The Philosophy of Science in a European Perspective" (www.pse-esf.org), which involves scholars from twenty-two European countries and aims at enhancing European tradition in the philosophy of science. The symposium is one of the PSE activities, whose general guiding-topic in 2011 is "The sciences that philosophy has neglected".

Since its very beginning and for a few decades philosophy of science mostly focused on the natural sciences, whose scientific status was regarded as well-established and capable of setting the parameters for discourse about science. More recently, philosophy of science has been widening its range of interests and devoting specific attention to previously neglected disciplines. The symposium aims at showing how contemporary philosophy of science interacts with some emerging fields, considering their most specific issues and toolboxes employed. It will be stressed how a foundational and methodological analysis of medicine, design research, cognitive science, and climate studies is being pursued, among the rest, through a clarification of concepts such as those of scientific explanation, prediction, reduction, and multilevel model building. Close attention will be devoted to how theoretical issues are intertwined with the distinctive practical exigencies and application purposes of these disciplines. It will be argued that this focus on the most innovative trends in philosophy of science is also going to shed some light on what its new directions will most likely be.

Rebecca Kukla, Justin Biddle, Torsten Wilholt, Bryce Huebner and Eric Winsberg
The Social Organization of Research and the Flow of Scientific Information

It is clear that the social organization of research shapes scientific knowledge. More specifically, the social organization of the flow of information - including how information is communicated between researchers, how intellectual property rights function, how studies and grant proposals are reviewed, how publications are designed and authored, and how research is funded - impacts the outcomes of research. Most obviously, the social organization of the flow of information, especially in an era of industry-funded research, can create or preempt opportunities for information to be hidden, forged, or distorted by interests. Issues such as publication bias and access lo proprietary data have received widespread attention. In response, various proposals for increasing transparency in research - such as stricter guidelines for disclosing financial conflicts of interest and calls for public registries of clinical trials - have recently been proposed and implemented.

The primary goal of this panel is to demonstrate that transparency and deception should not be our only epistemological measures when we examine systems for organizing the flow of information in research. There are other ways in which the social organization of the flow of information impacts research outcomes, and we contend that these are both epistemologically interesting and relevant to a philosophical understanding of scientific practice.

Formal Philosophy of Science I

Ilkka Niiniluoto
Models, Simulations, and Analogical Inference

Models and simulations represent target systems by means of relations of similarity or analogy. Two objects or systems are similar if their attributes are close to each other or approximately equal. Two objects are analogous to each other if they are partly identical. From this perspective, it is useful to distinguish similarity models and analogy models as sources of learning about real targets. Similarity models include idealized models which typically represent reality by deformation or caricature: while some irrelevant properties are excluded, some relevant properties are neglected by assigning them extreme values. Inferences from such ideal similarity models would lead at best to truthlike conclusions, which are not true in the actual world, so that true information about the real system has to be obtained by the concretization of counterfactual assumptions. Typical analogical models allow inference from the model to the target system by inductive inference from model data D to generalization C, and analogical reasoning from the model generalization C to the same generalization C about the real system.

Sugden has proposed that economic models are “credible counterfactual worlds”, fictional “parallel worlds” which are realistic in the same sense as novels. Sugden’s idea of model-based induction can be modified by the notion of analogy, but this would be against his fictionalism. Another problem is that idealized economic models are not credible in Sugden’s sense, as they include extreme assumptions like perfect rationality, so that they should be treated as similarity models.

Petros Stefaneas
Theories and Abstract Model Theory

Explanation in science comes in the context of theories. We claim that abstract model theory may provide a flexible framework for the study of scientific theories from the syntactic and the semantic points of view. Abstract model theory is based on an abstraction of Tarski’s concept of formal truth and tries to define in mathematical terms the abstract concept of a logical system. Our approach is based on the well known theory of Institutions. Institutions allow us to abstract from syntactic and semantic details when working on language structures in ‘the large’, without any commitment to any particular logical system. *Theories* over an abstract but concrete Institution may be co-defined via *collections of models*. Given a signature S, an S-theory is a set of S-sentences, and an S-model class is a class of S-models. Every S-theory T determines an

S-model class $T^{\bullet}$, which contains all the S-models that satisfy all its sentences, and every S-model class V determines an S-theory $V^{\bullet}$, which contains all the S-sentences satisfied by all the models in V (Galois connection). We claim that this duality may be used as a formalism to define and study concepts such as *a scientific theory* and its models. Also, it may be used as a multi-language approach to the semantic view.

Gustavo Cevolani, Vincenzo Crupi and Roberto Festa
More Verisimilar Banking: A Novel Analysis of the Linda Paradox

In this paper, we show how the notion of *verisimilitude*, first introduced into philosophy of science by Popper (1963), can be fruitfully applied in the analysis of problems emerging at the interface between epistemology and the cognitive sciences.

We focus on the so called "Linda paradox", a key problem in the experimental study of human reasoning first discussed by Tversky and Kahneman (1983). When faced with the description of a fictitious character, Linda, most people judge the conjunction "Linda is a bank teller and is active in the feminist movement" (B & F) as more probable than the isolated statement "Linda is a bank teller" (B), in contrast with the "conjunction rule" of probability theory, prescribing that $p(B \& F) < p(B)$.

The attempt of providing a satisfactory account of this phenomenon has proved rather challenging. In what follows, we propose a verisimilitudinarian analysis of the Linda paradox, based on the idea that experimental participants may judge B & F a better hypothesis about Linda as compared to B because they evaluate B & F as more verisimilar than B. In fact, while B & F is less likely to be true than B, it may well be a better approximation to the truth about Linda. More precisely, we define an adequate measure *EVs* of the *expected verisimilitude* of the two hypotheses involved, and present some general conditions yielding $EVs(B \& F) > EVs(B)$, thus accounting for the participants' preference for B & F over B as a better hypothesis about Linda.

Doukas Kapantaïs
Formal Intuitionistic Semantics for Fitch's Paradox

In the first part of the paper, I disambiguate some formulae which, by being such (i.e. ambiguous), prevent the realist and the antirealist from having a proper debate on Fitch's proof.

$\neg((\forall p)(p \rightarrow Kp))$ (*)

(*) is meant to formally capture the intuition that not all truths are known and so (*) is meant to negate omniscience. However, it is only through its classical interpretation that this formula successfully captures this intuition. For, according to this interpretation, it reads:

It is not the case that for every state p, if p, then p is known.

Intuitionistically, however, it reads:

One can derive a contradiction from the assumption that one disposes of an algorithm such that one can transform any proof of p into a proof of p-is-known.

In the second part of the paper, I present some models of intuitionistic logic (I call them "S-models") in which $(\forall p)(p \rightarrow Kp)$ is valid without expressing omniscience (for the above mentioned reason), and which (models) further dispose of a formula that is true in the actual world and does capture the intuition that omniscience is *not* the case:

$(\exists p)((F(Kp))\&(\neg Kp))$ (§)

(§) reads: *there is a state that will be known (to obtain) in the future, but is currently unknown (to obtain).*

By so doing, I show the conclusion of Fitch's proof to be intuitionistically valid, but no longer alarming for the antirealist; it does not correspond to omniscience and, moreover, omniscience is overruled in the model by the truth of some other true formula.
S-models are second order temporal Beth models, enriched with some knowledge operator and having an integrated metalanguage.

Models and Simulations in the Life Sciences

Sara Green
Exploratory Models - Reverse Engineering in Systems Biology

The importance of mathematical and computational modeling of biological systems is rapidly increasing in contemporary biology. Different fields within biology experience a manifold increase in the amount of available data, which in addition to biological insight demands skills in mathematics and programming to analyze and model these data.

The amount of data sometimes makes it necessary to build models to "make sense of" the data. In this sense, mathematical models can be described as creative and *question generating*. I conduct a case study of modeling as *reverse engineering*, where the models are (semi-)automatically derived from data to give clues to general properties of the data. Instead of building a system on the basis of design principles, this approach tries to do the opposite: to use a model to search for the general underlying principles in a complex biological system.

I shall argue that modeling can be seen as an open-ended process of creating new spaces of representation, where new epistemic objects are established. My paper will mainly focus on the exploratory side of experimentation, but I will also point to examples of how models in systems biology can be compared to biological data in order to be empirically informative and stabilize epistemic objects. Thus, a diversity of models makes it possible to describe their central function in science through the ambiguous role as question-generating machines *and* answering machines.

Bettina Schmietow and Lorenzo Del Savio
Cells from Computers: from Ethics to Epistemology

The consequences of synthetic genomics have been mainly discussed by ethicists. The claim of artificiality itself instead is seldom addressed: was Venter's lab (2010) entitled to claim to have *created* bacterial cells?

The latter question is not a mere theoretical curiosity about our concept of artificiality: *creativity* is indeed one of the necessary conditions for a patent to be accepted and therefore the issue of patentability of life – when disentangled from its religious background – ultimate relies on this issue. We argue that the long-standing debate about the role of genes in the determination of the organization of organisms could provide the right tools to answer this pivotal question.

Do chromosomes contain the whole genetic repertoire in simple *bacteria*? A *Mycoplasma capricolum* donor cell reverted to a *M. mycoides* phenotype when implanted with a (modified) *M. mycoides* synthetic genome. Hence, organization is at least partially determined by the chromosomal material. Nonetheless, the donor cell material is also necessary to have viable cells and, more importantly, it may be relevant for the organization as well. In fact, it is not known whether the experiment would work with cells that are phylogenetically less related.

We extend along this conceptual line of reasoning concluding that we do not know yet whether, in simple *bacteria*, the genetic repertoire is fully contained in chromosomes. Furthermore, we argue that in the case of other organisms, the negative answer is already widely accepted and that a general answer as it was initially sought in the philosophy of biology simply does not fit what is known.

Tim Räz and Raphael Scholl
Why Do We Model?

Michael Weisberg recently distinguished between model-based science as an "indirect" mode of scientific theorizing and other, more "direct" modes of theorizing. To illustrate his views, Weisberg draws on case studies, most notably on Volterra's model of population dynamics. In the present paper, we take a closer look at the original publications in which Volterra and his collaborator d'Ancona presented their work. This analysis throws new light on the episode. First, we believe that more can be said on the question of why a scientist would choose modeling rather than other practices to approach a given theoretical problem. This leads us naturally to a motivation for modeling, but also to a distinction between "direct" and "indirect" theorizing which differs from Weisberg's. Second, we believe that understanding the motivations for modeling suggests a natural goal for the modeling process. Understanding this goal gives us a framework through which we can understand why Volterra's model was received skeptically, why it ultimately failed when judged on its own stated terms, but why it nevertheless was scientifically useful. As a contrasting example of successful scientific modeling, we will present an analysis (and philosophical re-interpretation) of Darwin's explanation of the origin of coral atolls. Finally, we will consider the empirical confirmation of models, where traditionally much emphasis has been placed on whether the model generates the correct "output". We will argue by example that the key question is generally whether the model produces the correct output for the right reasons, that is, because it represents actual causal structures.

Emanuele Serrelli
Mendelian Population as a Model, Intended as a "Stable Target of Explanation"

Models constitute an increasingly important object of study for philosophy of biology. Yet, no univocal and sufficiently comprehensive definitions of modeling and model are available. Here I adopt a specific notion of a model as a "stable target of explanation", and use it to explore population genetics in a uncommon way. Mathematical population genetics is often referred to as a great set or "family" of models, where "models" mean, arguably, equations of gene frequencies or phenotypic change. In this sense, modeling is seen as an activity of equations specification, tuning, and calculation. The notion of a model as a "stable target of explanation" does *not* apply to population genetics equations. Rather, it is suitable for capturing Mendelian population, i.e. a formal combination space population genetics equations are *about*. One interesting result of my approach is to liken - at least for some epistemological characteristics - a *formal* system to *organic* systems called "model organisms" in experimental biology, like e.g. *Drosophila melanogaster*, or *Caenorhabditis elegans*. A single notion of a model, one that emphasizes model autonomy - with interesting epistemological problems about representation, explanation, and prediction - seems to capture effectively both

Mendelian population and model organisms. Models as stable targets of explanation are systems selected for intensive research, yielding their stability and a cost-effective apparatus of experimental resources; they feature some degree of artificiality, and are never exhaustively known, even in case of complete artificiality.

Philosophy of Quantum Mechanics III

Jonathan Bain
CPT Invariance, the Spin-Statistics Connection, and the Ontology of Relativistic Quantum Field Theories

CPT invariance and the spin-statistics connection are typically taken to be essential properties in relativistic quantum field theories (RQFTs), insofar as the CPT and Spin-Statistics theorems entail that any state of a physical system characterized by an RQFT must possess these properties. Moreover, in the physics literature, they are typically taken to be properties of particles. But there is a Received View among philosophers that RQFTs cannot fundamentally be about particles. This talk will first consider what four alternative approaches to proofs of the CPT and Spin-Statistics theorems suggest about the ontology of RQFTs. These include an axiomatic approach, an approach due to Steven Weinberg, a textbook "Lagrangian" approach, and an algebraic approach. I will next consider the extent to which the ontological implications of these approaches are compatible with the Received View. The discussion will not constitute a conclusive argument against the Received View, but it will suggest that the Received View's approach to ontology is flawed. What we take RQFTs to be about should depend, in part, on what we take the essential properties of RQFTs to be. If we agree that CPT invariance and the spin-statistics connection are essential properties of fundamental states in RQFTs, then we should look to proofs of these theorems to provide clues to the nature of these states. In this endeavor, the Received View's pre-theoretic intuitions that *a priori* militate against particle interpretations may appear out of place.

Karim Bschir, Michael Epperson and Elias Zafiris
Decoherence: A View from Topology

The decoherence programme studies the formation of quantum correlations between the states of a quantum system and the states of its environment and, in particular, the local suppression of interference between preferred states of the system selected through the interaction with the environment. One major conceptual problem with decoherence consists in the decomposition of the universe, described by a global state vector, into “system”, “apparatus” and “environment”. The consistent histories approach tries to bypass the decomposition problem by focussing on histories of the whole universe itself. The major problem with this approach is that there exist many sets of consistent histories which cannot be combined to yield a maximal consistent description.

We introduce a topological approach to decoherence that can be seen as an extension of the consistent histories approach. It captures the relationship between a global description in terms non-commutative algebras of quantum observables and a local description in terms of local Boolean algebras associated with particular measurement contexts. Within this conceptual framework, it becomes possible to understand the notion of environment as a topological localizing scheme at the macroscopic level with respect to a global algebra of quantum observables. The non-commutative global algebra of quantum observables can be interpreted as the realm of all potential states of a system, the “logical environment” so to speak, whereas the local Boolean measurement contexts depict the actualized states (i. e. measurement outcomes). Decoherence is then identified with the process of reduction of global potentialities to localized classical actualities.

Gordon Purves
Lies, Damn Lies, and Quantum Statistics: Confirmation and False Posits

This paper expands upon a normative variational account of scientific fictions that I have introduced elsewhere to discuss the implications that fictions have for the interpretation and confirmation of scientific theories. The main thrust of my argument is a reworking of Cartwright's classic argument that the essential use of falsehoods in theory testing effectively shields a theory from refutation. I show on the one hand how theory confirmation can proceed unimpeded by falsehoods of various types. On the other hand, and more interestingly, I parse apart the different ways that false posits can interfere with the confirmation of a theory, concluding in particular that if a theory can only be empirically adequate when conjoined with a certain type of false posit (a fiction by my definition), which is itself not empirically adequate, then this implies that the model as a whole can be no more than a predictive tool, and thus no realist physical interpretation is permissible. In a strict, realist sense, then, I argue that in these special cases the empirical success of a model is evidence of its falsehood. This latter conclusion is applied to the interpretation of quantum statistical mechanics (QSM), a field of science that has enjoyed substantial empirical success, and show that, granting my account of fictions, that success is actually evidence of the falsehood of the QSM. The problem is in the precise way that QSM appeals to the thermodynamic limit, an unproblematic idealization in classical statistical mechanics, but a necessary fiction in QSM. This conclusion is particularly problematic for interpretations of quantum mechanics that use QSM to find superselection rules to solve the measurement problem. In fact, such an appeal in principle cannot offer an improvement upon simple instrumentalist solutions.

Mario Bacelar Valente
Are Virtual Quanta Nothing but Formal Tools?

The received view in philosophical studies of quantum field theory is that the Feynman diagrams are simply calculational devices. Alongside with this view we have the one that takes the virtual quanta to be also simply formal tools. This received view was developed and consolidated in philosophy of physics works by Mario Bunge, Paul Teller, Michael Redhead, Robert Weingard, Brigitte Falkenburg, and others. In this presentation I will present an alternative to the received view.

Friday, 7 October

11.30-13.30

Symposium

Craig Callender and Jonathan Cohen, Julian Reiss, Daniel Steel, Andreas Hüttemann and Alexander Reutlinger
Lawish Generalizations in the Special Sciences

Many philosophers are convinced that the fundamental laws of physics crucially differ from generalizations in special sciences. Fundamental physical laws are usually assigned the features of being universal, exceptionless, time-symmetric, global and complete, while generalizations in the special sciences are understood to be non-universal, to have exceptions, to be hedged by a ceteris paribus clause, to be time-asymmetric, local and incomplete. In the recent debate (especially in the 2002 volume on ceteris paribus laws by Earman, Glymour and Mitchell), a considerable amount of energy has been devoted to (a) emphasizing the differences between fundamental physical laws and “generalizations” in the special sciences (to the effect that the latter do not deserve to be called “laws”), and (b) to illuminate the meaning of the ceteris paribus clause. These are, certainly, important issues. However, focusing *exclusively* on these questions seems to blur and postpone a more interesting question: given that the special sciences are successful, *how is it possible* that statements in fundamental physics and statements in the special sciences play a *similar* role – *despite* the differences between fundamental laws and special science generalizations?

Despite their different features, laws in fundamental physics and generalizations in the special sciences are important because they serve to pursue the same goals: they are statements used to explain and to predict phenomena, they provide knowledge of how to successfully manipulate the systems they describe, and they support counterfactuals etc. Statements in the special sciences that play these roles in scientific practice, one might call *lawish* statements (similarly, Mitchell 2000). Contrary to the traditional understanding of what it is to be a law, being lawish does neither require universality nor other characteristic features of fundamental physical laws.

In this symposium, we provide metaphysical and methodological accounts explaining how statements in the special sciences can perform a lawish function.

Philosophy of the Life Sciences II

Johannes Martens
Altruism, Correlations and Causality

There are currently two main ways of modeling the evolution of altruism in the field of social evolution theory, namely the inclusive fitness theory based on the analogy of organisms-as-maximizing-agents, and an alternative known as the direct fitness approach. The former has been elaborated by William Hamilton, and explains the evolution of altruism in terms of the indirect benefits it provides to the recipients, weighted by their relatedness to the focal altruists. In contrast, the direct fitness approach explains the evolution of altruism by underlining the role of correlations in natural populations, and proceeds by calculating the fitness effect on the focal recipient of the behavior of all the actors (including the focal recipient), weighted by a correlation coefficient–which should be high enough in order for altruism to evolve.

Both approaches rely on the economic representation of a biological population in terms of “actors” and “recipient”, and are mathematically equivalent. However, because of their fundamental asymmetry, some evolutionists have recently suggested that they were conceptually incompatible frameworks, arguing for the superiority direct fitness approach. In my presentation, however, I show strong reasons to reject their argument. In particular, the direct fitness approach does not provide us with a causal decomposition of the fitness structure that underlies the evolution of strong altruism. Moreover, I argue that when taken literally, it leads us to conflate correlations with the genuine processes at work, and to overlook wrongly the dimension of sacrifice *proper* to strong altruism.

Till Grüne-Yanoff
Evolutionary Game Theory, Learning Dynamics and Mechanisms

Various attempts have been made to draw a strong analogy between cultural evolution and biological evolution – as for example proposed by memetics. In this paper, I propose to analyse such analogy claims with the help of the mechanism concept, as developed in recent philosophy of science. This analytical tool is developed and tested by applying it to a prominent case of such analogy claims – namely the transfer of evolutionary game theory into the social sciences. The mechanism concept contributes to answering this question in three ways. First, it characterises the construal of the EGT formalism in biology and the social sciences, respectively. Second, the mechanism concept helps distinguishing between superficially similar EGT models. Third, the mechanism concept allows comparing different EGT models. Mechanism descriptions can be distinguished on the one hand by the different levels of mechanisms, and on the other hand by the different degrees of abstraction. Categorising different mechanism descriptions along these two dimensions allows assessing their relations to one another, in terms of identity, subsumption or common ancestry.

Marta Bertolaso
An Apparent Circular Causality to Account for the Phenotypic Stability of the Organism: Insights from the Biology of Cancer

Understanding how causality operates at different levels of organization still remains a central question when addressing living beings. When single components come together and form a biological system, they engage in novel behavior and produce novel phenomena through the integration of processes that underlie organic systems.

Paradigmatic in the experimental field, cancer research is now providing interesting empirical evidences and theoretical concepts to deal with these issues. The neoplastic process as a multilevel phenomenon, in fact, seems to shed light on causal relationships among events and biological systems through different kinds of causality.

The aim of this paper is thus to analyze the causal notions used in different explanatory models of cancer and in particular to demonstrate the terms in which the apparent circular causality some of them refer to explains the specific dynamic that support the phenotypic stability of the organism. In this case, the architecture of the tissue is considered an emergent property of the cellular organization: among different levels of the biological structure there is a new mutualistic way of interaction that is essentially reciprocal and causally effective on their behavior and thus their biological identity.

This might allow us to reframe the link between the hierarchical organization of the organisms and causalities by focusing on the biological activity that characterizes them. We will thus clarify in which terms self-determination appears as the last condition for the possibility, at different levels, of the phenotypic stability of the organism through the integration of its functional and molecular properties.

Alex Broadbent
A Theory of General Causation for Epidemiology

The stock example of a general causal claim is “Smoking causes lung cancer”. Epidemiology is the science which established the truth of this claim. Yet the main philosophical analyses of general causation fit epidemiology poorly or not at all, I argue. In particular, I examine two features of epidemiological causal claims. First, I argue that epidemiological causal claims do not make sense if they are uninstantiated. This is contra to prominent (but not all) philosophical theories, which see general causal claims as akin to laws of nature, which are widely thought to be possibly uninstantiated. So Ellery Eells claims that smoking can cause lung cancer even if nobody smokes, or even if everyone smokes but nobody gets lung cancer. Second, epidemiological causal claims are quantified in various ways. Philosophers have paid scant attention to the notion that a general causal relations might be quantitatively measurable, despite the focus on probabilistic causation in this context. I argue that probabilistic theories in particular do not adequately characterise the causal concepts used in epidemiology. I propose a better theory on which general causal claims –at least those made in epidemiology, and arguably in some other contexts too – have more in common with existential than universal quantification. The theory can accommodate quantitative elements in general causal claims. It also explains how uninstantiated general causal claims are problematic, and why philosophers have not noticed.

Philosophy of Space and Time I

Adán Sus
The Physical Significance of Symmetries and Conservation Laws

The empirical significance of symmetries in physical theories has been a matter of discussion in recent times. Although there seems to be no problem with the interpretation of global spacetime symmetries, there is no consensus in relation to the empirical import of gauge symmetries and local spacetime symmetries. Nonetheless, the conventional wisdom seems to be that global but not local symmetries have empirical significance due to the fact that global, but not local, transformations have an active interpretation. The physical intuition linked to this is that some symmetries (gauge and local spacetime ones) connect different mathematical representations of the same physical situation while others connect different physical states.

Furthermore, it is well known that there exists a relationship between symmetries and conservation laws that, for Lagrangian theories, is encoded by Noether's theorems. Here conventional wisdom goes like this: it is global symmetries, through Noether's First Theorem (NFT), that are related to conservation laws. Less known is the fact that for theories with local symmetries, because they necessarily have global subgroups as symmetry groups, Noether's first theorem is also applicable, but this time producing conservation laws with a less clear physical status. In principle, there is a sense in which presence of local symmetries trivialises the conserved quantities obtainable (this is what has been named the Noether charge puzzle) but recent work shows that things are not so simple.

The main objective of my talk will be to show how the discussion about the status of conservation laws helps to clarify the different interpretations of symmetries in physics.

Erik Curiel
On the Thermodynamical Character of Black Holes in Classical General Relativity

I examine the status of the analogy between black hole mechanics restricted to classical General Relativity on the one hand (i.e., with no input from quantum field theory) and classical thermodynamics on the other ("classical" in the sense that no statistical or quantum considerations come into play). Based on the striking formal similarities of the respective mathematical formulae of the laws of classical thermodynamics and those for the mechanics of black holes in stationary, asymptotically flat spacetimes, it is prima facie a strong and deep analogy. But is it of real physical significance in some sense? Standard arguments in the physics literature claim that the analogy is merely formal; one must invoke quantum mechanics in order to show that black holes in fact have physical thermodynamical properties. I argue otherwise, focusing on the analogy between black-hole surface gravity and thermodynamical temperature. Based on examination of the ways that temperature enters into classical thermodynamics and the roles it plays there, I show that black-hole surface gravity enters into and plays the same physical roles already in classical general relativity, with no need to invoke quantum mechanics. This strongly suggests that even in the classical theory on its own, we ought to take seriously the idea that black holes are thermodynamical objects in a physically significant sense, and that the analogy between thermodynamics and general relativity runs very deep on its own.

F. A. Muller
Structuralism and Space-Time

We characterise a view on space-time that is neither a variety of substantivalism nor a variety of relationism but is a variety of *structuralism* by four, or essentially three, principles.

Trust and Peer Review in Science

Susann Wagenknecht
Epistemic Trust: An Empirical Study in Natural Science

This talk aims at developing an understanding of trust among scientists in research teams –an understanding that proves both philosophically fruitful and empirically adequate. Based on a case study, I will analyze natural scientists' perspective on trust in collaborative research. With the help of empirical data from interviewing and observations, I want to explore how trust is actually shaping collaborative knowledge production –how trust works, in an environment that according to a wide-spread ideal should be governed by skepticism.

My reflections on trust are based on Hardwig's work on testimony and trust (Hardwig 1985, 1988, 1991). My study takes seriously what Hardwig calls for: "an epistemological analysis of research teams". Crucial here is the division of epistemic labor. Kitcher (1993) and e.g. Goldman (2002) have discussed the division of labor with regard to the scientific community. I, in contrast, deal with the interdependence of scientists due to division of labor on group level.

I will argue that (1) both trust and control are graded phenomena. Moreover, I will argue that (2) trust is accompanied with skepticism, a form of epistemic distrust. Distrust encourages to setup control measures. Yet, trust and control are not fully mutually completing. Uncertainty remains and researchers have to cope with the continuous insecurity as to whether trust in a particular situation is beneficial to their work or not. Furthermore, I will explain that (3) scientists cope with gradual distrust and lack of control with resort to specific working routines on group level.

Jeroen de Ridder
Trust in Science: Nicety or Necessity?

I will argue that trust is essential in science. Scientists have to take a substantial amount of their colleagues' testimony on faith, in the sense that they have to accept it without themselves (a) possessing proper justification for them and (b) possessing sufficient evidence for their colleagues' reliability to justify acceptance of their claims. The key ingredient in my argument is a sober overview of the practice of contemporary science.

I will then take up recent work by Elizabeth Fricker (2002) and Douven & Cuypers (2009), who argue for a contrary conclusion, namely that scientists typically do have sufficient evidence for the reliability of their colleagues to justify trusting their testimony. Although their arguments correctly point out that scientists do have some evidence for the reliability of their colleagues, the problem is that this evidence comes nowhere near justifying the extent to which scientists actually rely on each other.

In the final part of the paper, I identify a more fundamental problem in both Fricker's and Douven & Cuypers' arguments, which is that they both assume that the point of testimony in science is to share knowledge. I then gesture at an alternative understanding of the role of testimony and trust in science. On this understanding, the point of scientific testimony is to share claims which are backed up by the particular kind of justification that scientific research provides, but which typically fall short of knowledge. Scientists trust each other to do exactly this.

Laszlo Kosolosky
The Role of 'Peer Review' in Science: Exploring How and Why the IPCC Blundered on the Melting Rate of Himalayan Glaciers

In their recent book, "Merchants of Doubt" (2010), Naomi Oreskes and Erik M. Conway showed that "peer review" is a very helpful and crucial tool in establishing scientific results. As for the Intergovernmental Panel on Climate Change, it was in part their extended peer review which made them into a respected scientific organisation on the issue of climate change. Their (latest) Fourth Assessment Report however shows us that their appraised review process fell short by letting mistakes get published. The question is: "How did these mistakes get through peer review?" In this paper, I explain in detail what went wrong, and shed some light on the concept of peer review and its role in scientific practice. Moreover, the responsibilities that arise for (1) scientists, (2) laymen, and (3) the IPCC as organisation will be spelled out.

Theories of Theories

Francesca Pero
Actual Theorizing and the Model-Theoretic Account

The semantic view of theories is considered the "orthodox view" on scientific representation. The model-theoretic account (da Costa and French, 1990; French and Ladyman, 1998), as a formalization of the semantic view, should display what French and Ladyman themselves (1999) have defined as "the hallmarks" of this approach, namely (i) an appropriate formalization of scientific theories (the "*logical analysis*") and (ii) a philosophical analysis of *actual* scientific theorizing (the "*actual content*"). This paper argues that, while the model-theoretic approach fulfils (i) by providing a sensible definition of models as *structures*, it fails with respect to (ii). To show this failure I will use the distinction due to Brading and Landry's (2006) between (a) *presenting* (i.e., to determine the theoretical objects up to isomorphism between the structures which are shared by the models within the hierarchy) and (b) *representing* (i.e., to determine the physical realization of the theoretical objects featuring in the structures). The advocates of the model-theoretic account maintain that models represent insofar as the mathematical relation of morphism holds between the models in the hierarchy, and as the latter applies to the raw data in virtue of such relation. According to Brading and Landry's distinction, this justification of the explanatory power of models is confined to the presentation-level. If the model-theoretic account cannot accommodate the explanatory power of models at the representation-level, how can it provide a philosophical analysis of the actual scientific theorizing? My answer is that it cannot since latching models onto reality is an integral part of actual theorizing activity.

Rogier De Langhe
The Problem of Kuhnian Rationality

The lack of an account of rationality in his "Structure of Scientific Revolutions" was a lacuna which Thomas Kuhn acutely felt. In this presentation I argue that Herbert Simon's notion of "satisficing" provides a formally well-developed and empirically well-established theory of rationality that fits well with Kuhn's general characterisation of science. On this account, Kuhnian scientists are not irrational. Rather they employ the same computational mechanism which allows humans to play chess. I start by considering two rival interpretations of the problem of Kuhnian rationality and introduce Simon's notion of satisficing. I then document how satisficing can be used to interpret the notions of "paradigm" and "incommensurability" in Kuhn's "Structure of Scientific Revolutions" and discuss its implications for understanding scientific change, rationality in theory-choice, relativism and progress.

Chuang Liu

A Critique of the Deflationary View on Scientific Representation

What is scientific representation (SR)? What are models that we see frequently used in science? To these and similar questions, Craig Callender and Jonathan Cohen (2006) give a deflationary answer. They first separate the *constitutional question* from other questions about SR, and then argue that a deflationary answer is the only adequate answer to such a question. The deflationary view says roughly that anything can represent anything else as long as it is properly connected to the fundamental representations (presumably mental states) of the latter. The proper connection is a matter of use and convention. Such a view is also endorsed to varying degrees by, among others, Paul Teller and van Fraassen.

The view has its merit and place in our understanding of SR, but I argue in this paper that it does not answer the constitutional question. I first separate two different readings of the question, one of which is indeed answered by the deflationary claim but the other more important reading – according to which we ask “how we humans represent the world around us, scientifically or otherwise?” – goes far beyond that view. Then I discuss the essential differences between iconic and conventional representations (the latter includes linguistic representation). Without the differences, deflationary view may be said also to be adequate for the second reading. In the end, I use an example of representation by a computer-like creature to show that what SR must depend on how we fundamentally represent.

Fabian Lausen

Heuristic Reductionism and the Concept of a Research Directive

How do we assess the merits of metaphysical stances when it comes to the impact they can have on scientific progress? In my talk, I wish to address this question by introducing the concept of a research directive. I use this concept as a methodological tool for investigating the heuristic fruitfulness of epistemological and ontological claims which are notoriously elusive to empirical testing. This perspective is rooted in Imre Lakatos’ conception of research programs but departs from it in significant ways. Research directives are not judged by the theoretical and empirical progress they exhibit in themselves, but rather by the way they can act as epistemic resources that theories or explanatory paradigms can draw upon. Moreover, I stress that the assessment of heuristic usefulness can often be decoupled from questions concerning the eventual empirical outcome of a specific approach. In this sense, my talk focuses less on the aspect of scientific progress that one could caricature as the “accumulation of true statements.” Instead, I wish to emphasize the aspect of gaining new perspectives and trying to integrate different approaches to well-known problems.

I will focus on a specific research directive that I call heuristic reductionism. This directive mainly draws upon the principles of physicalism and part-whole-asymmetry and encourages the epistemic activities of constructing identities between objects and decomposing systems into their parts. I suggest my theoretical concept as a fresh way of looking at the reductionism debate without getting drawn into the sometimes excessively fierce debates between reductionists and antireductionists.

Friday, 7 October

17.00-19.00

Symposia

Arianna Borrelli, Koray Karaca, Michael Stöltzner and Simon Friederich
Perspectives on Spontaneous Symmetry Breaking in and Beyond the Standard Model

The idea of spontaneous symmetry breaking (SSB) has a status similar to that of symmetry: both notions can be made intuitively accessible with the help of everyday analogies, such as a pencil balancing on its tip and then falling down in one specific direction, but attempting to grasp their meaning in more detail leads into a maze of mathematical formalisms and physical interpretations. SSB emerged as an explicit concept in the 1960s and, after scoring its first successes in the fields of superconductivity and strong nuclear interactions, it came to be employed across the whole domain of solid state and particle physics, as well as in statistical mechanics, unified field theory and cosmology. Best known today is the spontaneous breaking of electroweak symmetry in the Standard Model through the "Higgs mechanism", which purportedly "gives mass" to all its elementary particles.

Despite its increasing significance, SSB has largely remained a problematic notion that cannot be fully grasped in terms of a single mathematical structure or phenomenon. This combination of universality and conceptual difficulties has rendered the notion also an interesting theme for philosophers. While the existing philosophical literature has focused on the role of SSB in solid state physics, the significance of its unifying across sub-disciplines, and its impacts on the ontological and epistemological features of gauge theory, the contributions in our session depart from the role of SSB within elementary particle research and attempt to spell out some of the features which contributed to its success as a many-layered, multi-purpose method. This includes critically assessing SSB's multiple roles as mathematical structure, explanatory strategy, textbook narrative, and methodological tool. The research presented in this session is part of a larger interdisciplinary project "Epistemology of the Large Hadron Collider (LHC)" centered at the University of Wuppertal (Germany).

Jesús Zamora Bonilla, Stephan Hartmann, Ryan Muldoon, J. McKenzie Alexander and Gerhard Schurz
Modelling Social Aspects of Science

Though both formal philosophy of science and social epistemology are fields with a long tradition, there has been relatively little common research in the two areas. Formal epistemology has applied logical and mathematical tools to abstract models that attempted to represent the content of scientific knowledge, but much less to the actions and interactions of scientists themselves. Social epistemology has concentrated in the analysis of empirical case studies and, or to the understanding of scientific processes with the help of sociological, anthropological or social-cognitive theories, but usually not from a formal point of view.

In recent years, however, a growing new field of research applying formal models to epistemology problems which are essentially social is emerging. This paradigm studies the social dimension of the pursuit of acquiring true beliefs and requires philosophical as well as sociological and economic expertise. The insights gained in social epistemology are not only of theoretical interest; they also improve our understanding of social and political processes, as the field includes the analysis of group deliberation and group decision-making. However, surprisingly little work has so far been done on the epistemic properties of group deliberation, belief aggregation and decision-making procedures. To close this gap, the construction and analysis of formal models are especially promising as formal modelling combines representational adequacy with instructive analytical results.

The papers presented in this symposium cover mainly the following problems: (1) the dynamics and properties of deliberation; (2) the social division of cognitive labour and the evolutionary dynamics of research programmes; and (3) the constitution of groups of experts as the outcome of a process of social research.

Theories of Natural Kinds

Elena Casetta
Outlining a Unified Framework for Assessment of the Biodiversity

In order to establish adequate conservation policies for the maintenance of biodiversity, biodiversity has to be understood and measured, and measurement is commonly carried out by counting the species taxa in a defined area. Unfortunately, mainly because of the so-called Species Problem, identifying and counting species taxa is far less easy than it might seem at a first glance.

Kevin de Queiroz in 1998 put forward a solution to the Species Problem. Starting from his insights, I would like to propose a unified metaphysical framework that aims to take into account both species conceptualization (the definition of species) and species delimitation (the over twenty criteria used to delimit species).

I will consider the traditional realist approach to species (the “carving nature at its joints” approach) and I will argue that it is not able to account neither for the indeterminacy of temporal boundaries of species nor for the school-dependency of identity criteria of species. Then I will outline a revised form of realism, Conventional Realism, according to which the boundaries of species taxa are features imposed by a set of species-directed practices on a substratum made up of individual organisms. I will conclude that, by enabling us to account for the Species Problem in this way, Conventional Realism can provide the understanding and the assessment of biodiversity with an effective metaphysical framework.

Samuli Pöyhönen
Should I Split or Should I Lump? The Epistemic-Tool Approach to Scientific Concept Formation

I investigate what I call the *mechanistic theory of natural kinds* and the *splitting-lumping model* of conceptual change suggested by the theory. Carl Craver has recently argued that the mechanistic theory collapses into conventionalism and does not solve the problem of finding natural kinds. I show that this conclusion can be avoided by adopting a *perspectival realist* position that makes the decision of whether to split or to lump relative to the scientific discipline in question. Furthermore, by examining Edouard Machery's work on the notion of concept in psychology, I illustrate how my *epistemic-tool approach* to natural kind concepts can incorporate the splitting-lumping model into a general framework of interdisciplinary knowledge production.

Miles MacLeod
What Kind of Kinds are Homologies? Studying Homology Concepts as Significant Kinds

The purpose of this paper is to open up a new perspective on kind or grouping concepts and their roles in the life sciences, by examining distinctions in their use and application in research contexts that depart from the traditional distinctions made by natural kind categorisations. In this respect it argues that phylogenetic and biological homology concepts are best analysed as *significant kinds*. Homologies are similarities shared due to common ancestry amongst organisms of particular relevance to understanding patterns and process of evolutionary biology. The significant and non-significant kind distinction reflects a distinction between groupings considered likely sources of reliable *group-bound information* relevant to particular goals and those that are used for different epistemic purposes. Applying this concept of kind helps us better understand the exact dimensions of dispute between the two homology concepts where the goals are somewhat shared - as is the methodology of pursuing these goals through identification criteria for homologies that pick out structures that seat and support further generalisations. They disagree rather over the most informative versus most reliable ways of characterising homologous structures in order to achieve this, evoking non-overlapping classes of homologies. As such these concepts are not so easily reconciled, nor can they be pictured as two sides of an *explanans/explanandum* distinction. It also helps us see that it is the very pursuit of evolution in terms of significant kinds that is under challenge with the increasing discovery of homoplasy (convergent similarity) in lineages.

Realism and Anti-realism II

Paul Hoyningen-Huene
The Ultimate Argument against Convergent Realism and Structural Realism: The Impasse Objection

For the sake of argument, three assumptions that are in fact quite problematic are conceded to convergent realism and structural realism. First, a theory space with a metric can be defined containing the relevant sequences of theories. Thus we have a precise framework when talking about theory convergence. Second, the convergence of a sequence of theories can be diagnosed on the basis of a finite number of elements. Thus we are able to make statements about theory convergence even if the number of theories in the sequence accessible to us is finite—which is the case in real life. Third, there is an actually convergent sequence of theories whose convergence we are able to make sufficiently plausible on the basis of the above assumptions. The impasse objection states that the limit theory may be substantially different from the true theory. This prohibits arguing for any sort of realism on the basis of a convergent sequence of theories. This objection hits also all those realists who do not claim the existence of a limit theory but nevertheless base their realism on some stability in the sequence of theories, be it the stability of entities or the stability of structures.

Simon Fitzpatrick
Doing Away with the No Miracles Argument: Realism, Empirical Success and Confirmation

Scientific realists assert that we have good reason to believe that our current best theories in mature sciences are typically approximately true. In recent decades, most of the philosophical debate surrounding the plausibility of scientific realism has focused on the adequacy of the "no miracles argument" (NMA). Indeed, it seems that most contemporary realists and anti-realists have essentially tied the case for realism to the adequacy of the NMA, sometimes even building it into the *definition* of realism. My aim in this paper is to argue that the kinds of moves that realists have had to make in response to objections to the NMA—in particular, the sorts of onerous claims that they have had to make about the history of successful reference and theoretical continuity in mature sciences—demonstrate that it is mistake for them to let the debate be framed in this way. The NMA actually *weakens* rather than strengthens the realist cause. Instead, I will argue that an adequate defence of scientific realism ought to focus on the specific bodies of evidence that support our current best theories, where the relevant notion of "evidence" must transcend the crude predictive and instrumental notions of empirical success at work in the NMA.

Paul Teller
Coherent Scientific Realism

Scientific realism, as it is traditionally understood, is either vacuous or logically incoherent. I explain a coherent reinterpretation that, once presented, is immediately attractive and that is free of the metaphysical issues that worry "anti-realists".

Murat Baç
Natural Ontological Misrepresentation and Subtleties of Neo-Realism

Arthur Fine's "Natural Ontological Attitude" (NOA) aims at finding a common discursive ground on which realism and antirealism could come together. Fine believes that NOA is a useful minimalist position in that once this core position is adopted, the realist and antirealist can add to it in accordance with their philosophical preferences. While this middle-of-the-road attitude of NOA has some prima facie theoretical attraction, it also suffers from certain problems regarding its suppositions about realism and antirealism. According to Fine, while realism is sensitive to non-mental (objective) aspects of the universe, antirealism places the emphasis on the human-made (subjective) aspects or components of knowledge. Such general statements about realism and its rival can be conceded, but Fine also seems to associate realism with the notion of some correspondence truth of a noumenal kind and antirealism with behaviorism or intersubjectivity. This, however, is a misleading interpretation, and one needs to take into account certain prominent ontological and alethic views that have been in circulation in order to appreciate the problems of NOA. In particular, one has to take into consideration how some significant Kantian ideas inform and affect contemporary debates on realism and antirealism in onto-semantic contexts.

Formal Philosophy of Science II

Franz Huber
How to Confirm Counterfactuals

The similarity approach to counterfactuals (Stalnaker 1968, Lewis 1973) gives a precise semantics for counterfactuals, but says little about how to empirically test counterfactuals. The interventionist approach to counterfactuals (Woodward 2003) has a story about how to empirically test counterfactuals, but that story does not square with the semantics of the similarity approach. I will first present a new semantics for counterfactuals and then tell a new story of how to empirically test counterfactuals. I will conclude by showing under which conditions the truth-values of counterfactuals can be reliably inferred.

Wolfgang Pietsch
The Limits of Probabilism

We argue that Bayesian probabilism is applicable only to phenomenological theories, where conventions and empirical hypotheses can be clearly separated, while it fails for abstract theories like physics, where such a separation is not feasible. The argument proceeds as follows: First, it is pointed out that scientific theories always contain conventions besides empirical hypotheses. Second, it is argued that it constitutes a category mistake to ascribe probability to conventions. Third, it is shown that in abstract theories conventions and empirical hypotheses cannot be clearly separated. These three premises allow to conclude that it constitutes a category mistake to ascribe probability to abstract theories and to abstract hypotheses. Therefore, Bayesian epistemology cannot provide a foundation for the methodology of abstract sciences.

Peter Brössel
The Significance of Confirmation

The concept of Correlation is highly significant for Bayesian epistemology. This paper focuses on the study of correlation. Section 2 presents one particular simple correlation measure which is the keystone for the philosophy of science and epistemology. More specifically, section 2 shows how this correlation measure is related to pivotal aspects of scientific reasoning such as confirmation and the explanatory and unificatory power of theories. The intimate connection between correlation and scientific reasoning evokes the question how correlation and truth are related. This question is answered in section 3 of the paper. Section 4 outlines the consequences the presented results have for epistemology and the philosophy of science from a Bayesian point of view.

Saturday, 8 October

9.00-11.00

Symposia

Marcel Weber, Kenneth Waters, Steven French and Holger Lyre
Where to Draw the Line Between What's Real and Unreal in Biological Knowledge

Both realists and antirealists agree that some parts of scientific knowledge should be interpreted realistically, while others shouldn't. What they disagree about is where it should be drawn and how it should be understood. In this symposium, French, Waters, Lyre and Weber will consider where the line should be drawn in biological sciences, an area which has hardly been considered in the realism debate. French will argue that it should be drawn between knowledge about *structures* and claims about *objects*. Waters will draw the line in a somewhat similar place, but argue that what should be interpreted realistically are claims about *objects* and *situated processes*, not claims about fundamental structures. Lyre will effectively lower the line as compared to French, but keep it above that argued for by Waters. Weber will critically examine the positions and arguments advanced by all three and assess if they can they can do justice to scientific practice.

Wybo Houkes, Pieter Vermaas, Mieke Boon, Thomas Reydon and Erik Weber
Technical Functions and Artefacts in Philosophy

Technical Functions: On the Use and Design of Artefacts (Springer, 2010) by Wybo Houkes and Pieter Vermaas, concluded efforts in the project *The Dual Nature of Technical Artifacts* at providing an analysis of technical functions and giving a characterisation of artefacts in engineering.

In *Technical Functions*, function ascriptions to artefacts are analysed against the background of artefact use and design. The use of an artefact is captured as the carrying out of a use plan for the artefact. Design is seen as – primarily – the development of new use plans for artefacts and – only secondarily – the description of the artefacts themselves in blueprints and other instructions for production. A function can then be justifiably ascribed to an artefact on the following three conditions:

An agent *a* justifiably ascribes the physicochemical capacity to ϕ as a function to an item *x*, relative to a use plan *up* for *x* and relative to an account *A*, iff:

I. *a* believes that *x* has the capacity to ϕ;
a believes that *up* leads to its goals due to, in part, *x*'s capacity to ϕ;
C. *a* can on the basis of *A* justify these beliefs; and
E. *a* communicated *up* and testified these beliefs to other agents, or *a* received *up* and testimony that the designer *d* has these beliefs.

This account of technical functions is dubbed the ICE theory of functions, where I, C and E refer to the existing intentionalist, causal-role and evolutionary/etiological approaches to functions.

The symposium consists of presentations by Mieke Boon, Thomas Reydon and Erik Weber, who will review the ICE theory of technical functions as presented in *Technical Functions* and explore the status of technical functions and artefacts in philosophy. The symposium combines a retrospective author-meets-critics approach with an outlook on future research in philosophy of technology.

Epistemic Virtues and Theory Assessment

Milena Ivanova
Can Theoretical or Intellectual Virtues Solve the Problem of Underdetermination of Theory by Data?

This paper challenges the appeal to theoretical virtues in theory choice as well as the appeal to the intellectual virtues of an agent as leading to unique choices. I argue that theoretical virtues cannot justify the choice of one theory at the expense of another theory and are therefore inconclusive in cases of theory choice. I illustrate this point with a discussion of the current problem of underdetermination in quantum mechanics and show that each theory possesses important virtues which would justify choosing it over its rivals. However, appeal only to theoretical virtues is insufficient to resolve the choice between them. I then turn to the employment of intellectual virtues in theory choice and argue that they are also insufficient to single out one agent, who defends a particular theory, and exclude another agent, defending an alternative theory. My suggestion is that the appeal to theoretical virtues as well as the appeal to intellectual virtues is inconclusive and cannot justify the adoption of one theory at the expense at another. I argue that the inconclusiveness of epistemic virtues shows that their employment is a misguided strategy as a solution to the problem of underdetermination because in both cases the underdetermination is shifted to another level, failing to determine a unique outcome of choice.

Kate Hodesdon and Kit Patrick
Is Theory Choice Using Epistemic Virtues Possible?

According to the popular ‘epistemic virtue account’ (EVA) of scientific theory choice, we ought to choose between theories on the basis of their epistemic virtues; empirical fit, simplicity, unifying power etc. We present a powerful and highly general argument against EVA: given plausible assumptions there is no possible rule that the EVA supporter could use to aggregate each theory’s virtues into a non-cyclic ranking. Our argument is based on an application of Arrow’s Theorem, a result whose consequences have been much discussed in the context of social welfare theory. The theorem has only recently been applied to theorem choice in science.

We give a novel development for applications of Arrow’s Theorem that is more appropriate for ranking epistemic virtues than election candidates. In typical uses of the theorem, each epistemic virtue is assumed to be measured with the same depth of information. However, in practice we measure different virtues using different scales, which provide greater or lesser degrees of information. In our presentation we show the affect of access to these different depths of information about epistemic virtues and map out this new landscape with basic diagrammatic proofs.

Harvey Siegel
Relativism and the Strong Programme Reconsidered

Relativism has had a bad name in philosophy ever since Plato offered his famous self-refutation arguments in the *Theaetetus*. Nevertheless, scholars of various orientations have embraced and defended it. A particularly important example is that of the ‘Strong Programme’ (SP) in the sociology of scientific knowledge. In this paper I raise two problems for SP: its fundamental argument for relativism fails; and its relativism is in tension with its insistence on its own scientific status. (1) SP’s basic argument for relativism fails. Its key term, ‘transcendence’, admits of stronger and weaker readings; on the stronger reading its premises are false, and on the weaker reading the conclusion does not follow. While it is true that we cannot judge from a ‘perspectiveless perspective’, wholly independently of our conceptual scheme, we *can* ‘transcend’ our schemes in the sense of *incremental improvement*. (I offer several examples of such transcendence from the history of science.) (2) Central to SP is the claim that *relativism is required for science*. But I show that there is a deep tension between SP’s relativism and its insistence on its own scientific status. (3) Finally, I consider Bloor’s (2004) recent defense of SP, and argue that it does not overcome the difficulties just rehearsed. In particular, I argue that it fails because it fails to upend the ‘epistemic/socially constructed’ and ‘inductive/conventional’ distinctions, the blurring of which is central to Bloor’s case.

Vincent Ardourel
Strong Underdetermination of Theories by Data: The Case of Different Mathematical Formulations of a Scientific Theory

One of the most discussed argument against scientific realism stems from the underdetermination of theories by observational data. According to a strong version of this thesis, i.e. the strong underdetermination of theories (SUT), any scientific theory has an incompatible rival theory to which it is empirically equivalent. While this thesis is commonly viewed as a “highly speculative, unsubstantiated conjecture”, Newton-Smith claims there may still exist a real case of SUT. According to him, the two rival hypotheses “space and time are continuous” and “space and time are merely dense” are compatible with all actual and possible observational data. Therefore, he claims that two theories of classical mechanics grounded on these two rival hypotheses are strongly underdetermined. In this paper, I claim that Newton-Smith did not show a real case of strong underdetermination of theories by data. I maintain that he is wrong in saying that there are two rival scientific theories in his example, and I show that there is only a single theory with two different mathematical formulations: a continuous formulation and a merely dense formulation. I also show that the case of two different mathematical formulations of a single scientific theory is very general in science. However, I claim that no consequence about scientific realism can be deduced from such case.

Pluralism and Reductionism

Stéphanie Ruphy
"Foliated" Pluralism: A Philosophically Robust Form of Ontologico-Methodological Pluralism

The aim of this talk is to elaborate a "philosophically robust" form of scientific pluralism that captures essential features of contemporary scientific practice largely ignored by the various forms of scientific pluralism currently discussed by philosophers. My starting point is Hacking's concept of style of scientific reasoning, with a focus on its ontological import. I extend Hacking's thesis by proposing the process of "ontological enrichment" to grasp how the objects created by a style articulate with the common objects of scientific inquiry. The result is "foliated pluralism", which puts to the fore the transdisciplinary and cumulative ways of proceeding in science, as well as the historical dimension of the genesis of scientific objects.

Robert Kowalenko
'Styles of Scientific Thinking Can Kill'

Ian Hacking's account of 'styles of scientific thinking' attributes three distinct properties to scientific 'styles' that, jointly, amount to a type of social epistemic relativism, as they obfuscate the distinction between scientific and non-scientific thinking and preclude the external evaluation of a style's standards of truth. Drawing on two historical examples—Paracelsus' renaissance medicine and late mediaeval witchcraft trials—as well as an extended contemporary case study—the HIV/AIDS epidemic in contemporary South Africa—I illustrate the deleterious consequences of adopting this theory in the public sphere. The South African case shows, I conclude, that philosophers of science cannot wash their hands off the demarcation problem, and suggest ways to modify Hacking's account.

Anjan Chakravartty
Realism about Scientific Taxonomy

This paper examines the metaphysics of classification from the point of view of the sciences, and more specifically, from the perspective of scientific realism, the most generous view of the epistemic credentials of the sciences. I argue that the default assumption implicit in most treatments of realism, to the effect that the world comprises a uniquely objective natural kind structure – taxonomic monism – is undermined by modern scientific practice. I consider, and find wanting, two objections to this contention: the claim that a plausible reductionism renders it void; and the claim that scientific taxonomy may not describe the actual kind structure of the world after all. The rejection of these claims suggests that realists about scientific taxonomy should be pluralists, but our most detailed accounts of pluralism are all forms of antirealism. I present an account of pluralism for the realist, in three parts: the first concerning patterns of property distribution in the world; the second concerning levels of ontological scrutiny which may profitably admit of pragmatic commitment only; and the third concerning the use of dispositional concepts in descriptions of systems of scientific interest. I maintain that some version of the first of these theses, which I label 'sociability-based pluralism', is a requirement of realist pluralism. The latter two, which I label 'metaphysical nature-based' and 'manifestation-based' pluralism, involve further commitments which the realist need not make, but may wish to make, in light of their explanatory value.

Henrik Thorén
What is an Interdisciplinary Problem?

Interdisciplinary research is sometimes distinguished from its disciplinary counterpart by reference to the problems it solves. The idea is that there is a certain class of problems that are best, or possibly only, solved by interdisciplinary research. This raises some philosophical concerns. For instance, how are such problems to be categorized? Do they make out a genuine category of problems as such, or are they perhaps rather to be conceived of in terms of their sources? There are some possible issues to common construals of how such interdisciplinary problems are to be accounted for. Problems genuinely 'between disciplines' seem undetectable due to the lack of a theoretical backdrop, a necessary component of having the problem in the first place. Another is that, following Kuhn, disciplines tend to re-cast problems in their own terms to make them available to disciplinary problem solving procedures. Hence there is no guarantee that the solutions eventually produced are reconcilable. In this paper a three-fold taxonomy categorizing different kinds of problems is suggested; broad problems, under-specified problems, and cross-cutting problems. Deploying this taxonomy one can point to where in the research process disciplines converge and in what way. A model is suggested that can make sense of the notion of interdisciplinarity as 'integrative though boundary maintaining' with reference to problems that shift between disciplines. It is argued that this has some normative consequences for how interdisciplinary research might be pursued and in what contexts and situations it may be successful.

Philosophy of the Social Sciences

Yulie Foka-Kavalieraki and Aristides Hatzis
Economics, Evolution, and the Brain: From Rational Choice Theory to Ecological Rationality

We believe that the theory of evolution can function as a metatheory for the "behavioral sciences" (i.e., for the purposes of this paper, all the sciences that try to explain and predict human behavior), conjoining them under the umbrella of a general theory. Economics is the first in line to claim and put in good use such a unifying and explanatory theory from evolution as it is concerned with human judgment, decision-making, reasoning and acting within environments of constrained choice, namely, it is concerned with high level human cognition. In this way, we will be able to account for rationality as well, as long as we view the latter as a kind of an evolutionary adaptation to a "transaction costs" environment. We thus see ecological rationality as a dynamic process of an evolutionary adaptation activated within the frame of changing biological and socio-cultural surroundings and involving the process of learning to deal with obstacles in order to achieve one's goals (satisfying preferences). Evolutionary psychology can offer us a theory for the historical causes of the brain's states and functions and in this way it can lead our research of decision making and rationality toward the correct direction and at the same time it can be used to inform the economic model by supplying a common pattern of basic preferences. On the other hand, contemporary cognitive psychology, neuroscience and neurobiology can provide us with the present causes of brain functioning and the mechanisms of learning and brain plasticity.

Thomas Uebel
Narratives and Action Explanation

This talk concerns the epistemological question raised by the project of project of providing causal explanations of actions, a project of central concern to the philosophy of history and to much philosophy of social science: what can assure us that the reasons cited were causally effective as claimed? It will be argued, first, that the problem faced is in fact a two-fold one, since not only (i) a generic justification is required for claim to have given a causal explanation, but also (ii) a specific justification for the claim that a particular set of beliefs desires and intentions played the relevant causal role. Then it will be argued, second, that the second of these problema arises (a) independently of the particular form of non-reductive physicalism adopted; (b) independently of what account of singular causal explanation is given; and (c) independently of how we think of our mastery of the folk-psychological idiom in which our explanations of actions are typically given. Having located the problem, I then consider whether and how the notion of narrative can offer any help: can narratives provide the missing justification for causal explanations of actions?

Uskali Mäki
On the Performance of the Performativity Thesis

The idea that economics has a "performative" relationship with the economy has become popular among many social scientists, suggesting for example that certain models in finance theory "perform" financial markets and agent's behaviour on those markets. The idea has remained obscure in its precise contents and consequences. The paper examines the notion from two points of view: that of the nature of the alleged relationship itself; and that of its implications for scientific realism. *First*, I show that while there are many important causal relations between economics and the economy, these are not authentic Austinian relations of performativity (the latter types of relation only appear as moments in the overall causal structures). Using the term as an umbrella for all such relations, sociologists of knowledge have obscured their nature as well as the difference between causal and constitutive relations. *Second*, I show how scientific realism about economics can be salvaged while granting that the economy is partly economics-dependent. I have suggested elsewhere (*Erkenntnis* 2005) that scientific realism should employ the notion of science-independence (rather than some generic notion of mind- or representation-independence). Yet given that social reality is not science-independent at all, this must be further specified with a distinction between causal and conceptual-constitutive science-independence. If the connections between economics and the economy were of the latter sort, scientific realism would be in trouble - but they are not, as the first part of the argument shows.

Jan Faye
How Do We Understand in Science?

In recent years philosophers have become interested in questions concerning scientific interpretation and understanding. Various authors have argued that scientific understanding can be considered a skill. If we go back to Polanyi we see that possessing a skill is to have “tacit knowledge.” Such a suggestion seems reasonable but is not without problems. A skill cannot be ascribed a predicate like true or false, it is a practice that does not necessarily reflect a rule-following procedure. Skills seem always to be functional. You must have the capacity to do something particular in order for you to have a certain skill. You must be able to realize some specific goal. But understanding need not be functional in the sense that it has a practical purpose. I therefore want to argue that understanding may give rise to skills and that skills are based on understanding. Thus the concept of understanding is just as fundamental as that of a skill.

In my talk I’ll make a distinction between *concrete* and *abstract* understanding. Concrete understanding is embodied as a practical ability of action and perception and by possessing tacit knowledge and, on the other hand, abstract understanding is the result of a purely reflective, intentional capacity of thoughts. I shall argue that abstract understanding has internalist conditions for success, whereas concrete understanding, even viewed internally, somehow involves an extrinsic evaluation. Abstract understanding has not only internally accessible criteria but these are also transparent in the sense that it is impossible to understand without understanding that one understands.

Saturday, 8 October

11.30-13.30

Symposium

James McAllister, Jeff Kochan, Lisa Osbeck, Nancy Nersessian and Sabine Roeser
Emotion in Scientific Reasoning

In recent years, a growing body of influential work by philosophers, psychologists, and neuroscientists of emotion has challenged the prevailing assumption that emotion and reason necessarily conflict with one another. Philosophers of science have, however, been slow in responding to these developments and in pursuing their implications for models of scientific reason. The contributors to this symposium will take some first steps towards exploring this exciting and still largely uncharted territory in philosophy of science. In broad terms, all speakers call for a reconceptualization of scientific reason so as to acknowledge an epistemic function for emotion. We challenge, in particular, the view that emotion plays no role in the justification of scientific beliefs and procedures. Our individual analyses range from the theoretical to the practical, our conclusions from the descriptive to the prescriptive. The aim of the symposium is not to present a unified perspective, but to capture, in some modest way, the deep and far-reaching implications of modern emotions research for contemporary philosophy of science. Topics covered in the symposium include: (1) the role of emotion in theory appraisal and the resolution of scientific dilemmas; (2) the role of emotion in securing epistemic rights and establishing epistemic duties in the laboratory sciences; (3) the importance of integrating emotional self-reflection in the design process in the engineering sciences, especially in high-risk projects; and (4) historical reflections on why philosophers of science have typically resisted the theme of this symposium, and suggestions on how recent developments in epistemology may help to mitigate their worries.

Ontology and Structural Realism

Federico Laudisa
Can There be a Truly 'Ontological' Scientific Naturalism?

It is often stated that scientific naturalism can have two main strands: an *ontological* one and an *epistemic* (or *methodological*) one, a principled distinction whose very possibility we would like to question in this paper. In fact, (any reasonable form of) ontological scientific naturalism imposes constraints on what should be properly accepted as a genuine kind of entity-of-the-world. The decision on what is entitled to belong to the class of the entities-of-the-world, however, depends necessarily on how *scientific theories* account for natural phenomena. It seems therefore that 'ontological' naturalism, to the extent to which it relies on the forms and structures of *actual* scientific theories, is really *epistemic* naturalism in disguise.

Under this hypothesis, the decisions about the task and the scope of a scientific theory often presuppose a sort of *explanatory normativity* – namely a sort of canon concerning what constitutes a ‘legitimate’ explanation (and what does not) – since there often seems to exist no consensus on what the theory should be designed for in the first place. Along these lines, we will discuss the example of the area of the foundations of quantum mechanics, in which there seems to be no naturalistic straightforward way to decide *what there is in a quantum world*, since the controversy concerns not only the details of the several interpretations of the quantum formalism, but also the very explanatory task of the theory.

Mauro Dorato
How to Combine (And not to Combine) Physics and Metaphysics

In this paper I will argue in favour of the view that if physics is to become a coherent metaphysics of nature, it needs an “interpretation”. An interpretation of a physical theory is a two-step process, as it amounts to (1) offering a precise formulation of its *ontological* claims (Sellars’ scientific image) and (2) a clear understanding of how such claims relate to the world of our *experience* (the manifest image). In the first part of the paper, I will criticize some prevalent approaches to the relationship between physics and metaphysics, with some attention to the historical tradition. In the second part I will defend my main claim by presenting and discussing two case-studies, one taken from Everettian quantum mechanics and the other from relativity.

Vincent Lam and Christian Wüthrich
No Categorical Support for Radical Ontic Structural Realism

Radical ontic structural realism (ROSR) maintains that the world ultimately consists of 'free-standing' physical relations without relata. ROSRers have struggled to convert this idea into a functioning metaphysics adapted to fundamental physics because the theories of the latter make overt reference to objects. Jonathan Bain (2011) has recently argued that category theory offers a suitable framework for formulating these theories in a way which cleanses them of objects and thus realizes the ROSRer's vision. To make good on this claim, Bain considers the case of the category-theoretic extension of general relativity. The claim that spacetime points are eliminated relies on the fact that the algebraic counterparts of manifold points cannot be defined within the category-theoretic framework. The trouble is that while the reference to spacetime points is indeed eliminated, this is not the case for any physical objects whatsoever. Furthermore, it seems as if category-theoretic formulations prove to be impotent in determining or describing the structure of particular models of the theory, particularly in a way that connects with experimental practice. Category theory gets a beautifully general and unified handle on fundamental physical theories at the expense of being blinded as to the structure of the objects of a category--it can't see 'within' them. While we consider the questions raised by Bain and the solutions he offers to be of great foundational value, we submit that the radical shouldn't expect that invoking category theory will alleviate –let alone resolve– her ailings.

Kerry McKenzie
'Humean Structuralism' About Laws

John Earman has recently described the lack of consensus on the nature of laws as "the scandal in philosophy of science", noting that there total disarray on how laws ought to be understood. Earman and Holger Lyre have recently added to the disarray by promoting a view of laws that is at once avowedly Humean and broadly 'structuralist'. While different proposals are offered by each, the Humean aspect of both consists in the presence of only categorical properties in their respective supervenience bases, while the structuralist aspect is evident in the weight placed on invariance and symmetry structure in their analyses. If such a programme were to prove successful it may resolve a number of difficulties that plague more standard Humean accounts of law and furthermore it may do so in a satisfyingly naturalistic fashion.

In this paper I will argue that our current understanding of categorical properties (and by implication essentially dispositional properties) is wedded to a classical account of law. As such, the notion of 'categorical' as a feature of properties requires fundamental revision before we can incorporate it into any metaphysics of law post quantum mechanics. I will further argue that once this revision is undertaken fundamental properties fail to comply with either the categorical or the dispositional designations. In light of this, I will discuss the prospects for Humean metaphysics post quantum mechanics and whether issues of modality in structuralism should be articulated in terms of the modal features of properties at all.

Theories of Natural Selection

Jonathan Everett
Evolutionary Theory and Thermodynamics: The Role of Statistics

This paper is a contribution to the debate about whether natural selection and drift explain evolutionary trends dynamically or statistically. The dynamical interpretation treats evolutionary theory in a manner similar to Newtonian physics, in that it considers evolution to be a theory of forces. On this interpretation natural selection and drift are forces, and causes, of evolutionary development, as such evolutionary explanations that cite natural selection and drift are taken to be causal explanations. The statistical interpretation, though, treats natural selection and drift as statistical properties of an ensemble of trial events and explains evolutionary developments without appeal to causes.

An interesting feature of this debate is that both sides appeal to thermodynamical considerations to support their positions. I will argue that this reflects their different attitudes about the role of statistics in scientific theories in general. The dynamical interpretation treats statistics as merely accounting for uncertainty in a causal process, whereas on the statistical interpretation it has the more complex role of permitting an abstraction from the physical situation.

Appreciating the role that this disagreement about the role of statistics in scientific theories in general has to play in the debate about whether evolutionary trends should be explained dynamically or statistically greatly clarifies the issue. In particular it allows us to see that if drift is to play a meaningful explanatory role in evolutionary theory, then we must interpret the theory statistically.

David Robert Crawford
Probability Measures and Biological Fitness

I examine two criticisms of the probabilistic propensity account of biological fitness (PPF): that the PPF account makes fitness claims tautologous and that the PPF account is incomplete because statistical measures are multiply-realizable. Following S. Mitchell's account of contingency in biological law, I argue that these criticisms confuse two different types of contingency: stability and strength. The stability of a fitness claim reflects its spatiotemporal scope. The strength of a fitness claim reflects its indeterminism. The first criticism mistakenly interprets any PPF-style fitness claim as analogous to a conditional in the first-order predicate calculus and misconstrues the probabilistic operator as analogous to a predicate in the consequent. This criticism overlooks the fact that strength claims interpret probability measures in terms of indeterministic laws. The second criticism interprets the multiple-realizability of statistical measures as analogous to the multiple-realizability of ecological fitness claims. The latter is a matter of stability, whereas the former is simply a reflection of the flexibility of applied measures. Indeed, the statistical measures in question, like mathematical expectation, are truncated versions of a Taylor series approximation. It is the probability measure this approximation measures, and not any particular approximation, which serves as the focal measure for the PPF. I conclude that these misconstruals of the PPF obfuscate a key virtue of the PPF approach, its interpretation of stochastic processes in terms of strength, not stability.

Fridolin Gross and Cecilia Nardini
Is Natural Selection a Mechanism?

In this contribution we discuss whether natural selection is a mechanism in the sense denoted by the “new mechanistic philosophy”. On our opinion the debate so far has focused too much on whether explanations in evolutionary biology can be expressed in the same terminology as that used, for example, in molecular biology. More important than to look at the right definition would be to determine whether the explanatory strategies used by biologists in the respective fields differ in important ways.

To the extent that biologists are able to identify the causally relevant factors in episodes of natural selection and go beyond a mere description of evolutionary processes, it is unclear in what respect evolutionary explanations differ significantly from those given in molecular biology.

On the other hand, we think that the idea of a general principle of natural selection, often referred to as “the mechanism” common to all individual episodes of selection, plays a different explanatory role, one that cannot be captured in purely mechanistic terms.

Our conclusion then is that it is sensible to describe single instances of natural selection within the framework of mechanisms, but that there are important differences when it comes to capturing the idea of natural selection as an abstract principle.

On a more general note, we argue that the case of natural selection provides a good example to elucidate the relationship of mechanistic ideas to different and perhaps more traditional kinds of scientific explanation.

Francis Cartieri
Is Neo-Darwinism in Crisis? Lamarck and Epigenetic Inheritance

Mere mention of the name "Lamarck," a name that has become almost universally derogatory among biologists, is enough to arouse hostility. The reasons for this are multiple and nebulous, but in large part it is believed that evidence of Lamarckian phenomena (the inheritance of acquired characters, for example) counts as evidence *against* Darwinism. This project seeks to establish that biologists may suffer misconceptions about the danger modern Lamarckian theses pose to their research programs, and that these misconceptions can inhibit progress in understanding how gene and environment interact to produce a range of phenotypes. In fact, it will be argued that much of the hostility encountered by Lamarckian theses may be related to an attitude, widely held among philosophers of science (famously Popper and Feyerabend), regarding the need for research frameworks to have strong, incompatible, competitive alternatives. The argument here is that such attitudes have been imported into biology and have resulted in the perception that modern Lamarckian theses are always incompatible competitors with modern Darwinian theses. A way forward will be presented that accounts for the compatibility of modern Lamarckian and Darwinian theses in a way that does not compromise the tenets of modern evolutionary theory.

Reduction and Idealization in the Physical Sciences

Nazim Bouatta and Jeremy Butterfield
Emergence and Reduction Combined in Infinite Systems

Emergence and reduction are compatible, despite the widespread "ideology" that they contradict each other. We will develop this viewpoint for two major examples from physics: (i) phase transitions in statistical mechanics; (ii) symmetry breaking and emergent symmetries in quantum field theory. The overarching theme is that the reconciliation of emergence and reduction turns on subtle uses of infinite limits. We will conceptually analyze, by discussing some specific models, how the renormalization group "controls" these infinite limits.

Mathias Frisch
Incantations of 'Causation' and Other Philosophical Sins, Or: Rehabilitating Ritz

This paper critically examines Earman's recent investigation of the so-called "arrow of radiation" and his review of the Einstein-Ritz debate on this issue. I show that Earman (like most commentators before him) misinterprets Einstein's position and that Einstein himself, in characterizing the classical electromagnetic radiation asymmetry, invoked the very production-talk disparaged by Earman. I then argue that Earman's own appeal to statistical considerations to explain the asymmetry is guilty of what Huw Price has called "the temporal double standard fallacy" and is ultimately unsuccessful. Finally, I defend a causal account of the asymmetry against Earman's charge of being ill-defined.

Ave Mets
Measurement Theory, Nomological Machine and Measurement Uncertainties (in Classical Physics)

According to the classical representational understanding, measurement is the basis of physical sciences by enabling numerical treatment of the material reality: it is comparing objects or phenomena according to some chosen attribute(s) and assigning them numbers accordingly, thus reducing the problems of nature to operations with numbers. For this, a nomological machine is needed: an ideal one to guide experiment design that enacts the purported fundamental mathematical laws of phenomena, a material one (laboratory, measuring instruments) to “read” nature’s properties and their values for providing data and building phenomenological laws. Cartwright regards the latter as pertaining to, and true about, the material world, the former as false for not holding in real world situations, and hence phenomena as idiosyncratic as occurring only in restricted artificial laboratory conditions. Woodward, in contrast, regards stable phenomena as pertaining to the world; and data, being affected by peculiarities of material settings, as full of noise and uncertainties and hence idiosyncratic.

I argue that (1) not only fundamental laws, but also phenomenological laws lie; moreover, measurement data – numerical relations – do not pertain to the properties of the (laboratory) world independent of human understanding of them, but both measured attribute and its value are theory-borne, “noise” being a genuine nature of the world; and (2) therefore the (naïve) representational view of measurement and experimentation doesn’t hold and should be replaced with a practice-based or pragmatic view.

Iulian Toader
The Dappling Effects of Idealization

It has been recently argued that although theories are dappled, in the sense that their methods and laws are diverse rather than uniform, the world is not, since methodological and nomological diversity does not entail ontological diversity. In this paper, I criticize this argument from a structural realist perspective. More particularly, I argue that idealizing procedures in quantum physics commit the ontic structural realist to the existence of a dappled world, but that ontological diversity should be conceived of in modal terms.

Philosophy of Space and Time II

Lisa Leininger
Presentism, Eternalism, and the Possibility of Temporal Becoming

In this paper, I argue that objective temporal becoming can *only* be plausibly accommodated within an eternalist framework. The presentist (as well as growing block proponents) accuse the eternalists of not having that “something extra” which allows for the existence of temporal becoming, namely, the successive addition of a slice of reality (and, in the case of the presentist, successive deletion of a slice of reality as well), which serves as the basis of the claim that things “come into existence”. I hold that the eternalist should not want this “something extra” – ultimately, this “something extra” is a component of the presentist (and growing block) account that the eternalist can, and should, do without.

I show that as a consequence of the Principle of Sufficient Cause, we are in the uncomfortable position of holding that a future entity must exist for a present entity to be able to bring it about. It is clear that only the eternalist can make sense of this situation, and I argue that this position is not so uncomfortable after all. In fact, this position better matches our everyday sense of temporal becoming. We do not experience things coming into existence from nothing. Entities do not pop into existence; instead, there is a change in existence of an event in one state to another state.

Daniel Wohlfarth
A New View of "Fundamentality" for Time Asymmetries in Modern Physics

I argue that the understanding of 'fundamentality' should be changed in order to define a 'fundamental' time direction in nature. This will be possible *without* a not time reversal invariant law and without typical boundary conditions (for example a low entropy past).

Because: Suppose L is a fundamental dynamic equation and S(L) is the solution space with dim(S(L))=n. I will argue that, if S(L) fulfils
(i) There is no more than a countable collection $S_i(L)$ of subspaces of dimensions $m_i<n$ such that: if $f(t) \in S(L)$ is time-reversible then $\exists\, i$: $f(t) \in S_i(L)$, and if $f(t) \in S(L)$ is not time-reversible then $\forall\, i$: $f(t) \notin S_i(L)$.
(ii) For time asymmetric solutions $f(t) \in S(L)$, the solution $f(-t) \in S(L)$ refers to the same physical world as f(t) does.
the time asymmetry in S(L) can be understood as a fundamental one.

I will argue that the solution space of the fundamental (in classical cosmology) Einstein equation, under reasonable conditions, satisfies (i) and (ii). Thus, classical cosmology seems to provide a 'fundamental' time asymmetry.

Moreover I will show how and under which conditions we could use such a 'fundamental' time direction to deduce the arrow of radiation in his understanding from e.g. Frisch (2000), Jackson (1999) or Rohrlich (2005). We will see that we could do that by deducing a time asymmetric energy flux, which refers to proper times, from the fundamental asymmetry.

Henrik Zinkernagel
A Critical Note on Time in the Multiverse

The idea of a multiverse has recently become quite popular in modern cosmology. According to some multiverse scenarios our universe is supposed to be just one inflating bubble in an infinitely bigger and older multiverse – with each component expanding differently and having different physical laws. In this and related versions, the multiverse thus purports to reject the common wisdom regarding modern cosmology according to which asking what was before the 'big bang' is considered as meaningless as asking for what is north of the North Pole. This talk will critically examine the notion of a global time in the multiverse – a notion which underlies the idea that some parts of the multiverse are older than others.

In recent analyses of standard, single-universe, cosmology, it has been pointed out that specific assumptions (the cosmological principle and, in particular, the so-called Weyl principle) regarding the distribution and motion of matter must be made in order to set up the cosmological standard model with a global time parameter. Moreover, it has been argued that the physics which supposedly describes the very early universe –

and in particular the supposed quantum nature of matter at this ‘epoch’ – may undermine the mentioned assumptions and hence question the very definition of cosmic time. Relying on these results, and examining various multiverse scenarios, I will argue that the prospects of identifying a physically well-defined notion of global time in the multiverse are dim.

Matt Farr

On the Status of Temporal Unidirectionality in Physics

I assess the thesis of temporal unidirectionality – that time has a privileged direction – and its relation to physics. The first half considers a naturalist approach to temporal unidirectionality implicit in the literature, in terms of a (physical) temporal orientation on a relativistic spacetime. I consider the relation between time reversal and the direction of time, and in particular the respective roles that temporal orientation plays in Malament’s (2004) geometric time reversal, and in Earman’s (1974) and Maudlin’s (2007) accounts of time direction. I argue that these roles are significantly different, and that contrary to several suggestions in the literature, a theory's requirement of a temporal orientation does not in any obvious way provide epistemic access to the existence of a physical temporal orientation.

The second half considers an alternative route to justifying temporal unidirectionality by considering its role in physical explanations. I consider unidirectional and adirectional treatments of the reversibility paradox in statistical mechanics, particularly concerning the Past Hypothesis, and consider whether the former have sufficient merit to provide an inference to the best explanation in favour of temporal unidirectionality. I argue that this approach to justifying temporal unidirectionality, though more promising, is also insufficient.

Index of Speakers

Index of Chairs