SPSP 2015

The 5th biennial conference of the
Society for the Philosophy of Science in Practice

Contents

About SPSP

Philosophy of science has traditionally focused on the relation between scientific theories and the world, at the risk of disregarding scientific practice. In social studies of science and technology, the predominant tendency has been to pay attention to scientific practice and its relation to theories, sometimes willfully disregarding the world except as a product of social construction. Both approaches have their merits, but they each offer only a limited view, neglecting some essential aspects of science. We advocate a philosophy of scientific practice, based on an analytic framework that takes into consideration theory, practice and the world simultaneously.

The direction of philosophy of science we advocate is not entirely new: naturalistic philosophy of science, in concert with philosophical history of science, has often emphasized the need to study scientific practices; doctrines such as Hacking's 'experimental realism' have viewed active intervention as the surest path to the knowledge of the world; pragmatists, operationalists, and late-Wittgensteinians have attempted to ground truth and meaning in practices. Nonetheless, the concern with practice has always been somewhat outside the mainstream of English-language philosophy of science. We aim to change this situation, through a conscious and organized programme of detailed and systematic study of scientific practice that does not dispense with concerns about truth and rationality.

Practice consists of organized or regulated activities aimed at the achievement of certain goals. Therefore, the epistemology of practice must elucidate what kinds of activities are required in generating knowledge. Traditional debates in epistemology (concerning truth, fact, belief, certainty, observation, explanation, justification, evidence, etc.) may be re-framed with benefit in terms of activities. In a similar vein, practice-based treatments will also shed further light on questions about models, measurement, experimentation, and so on, which have arisen with prominence in recent decades from considerations of actual scientific work.

There are some salient aspects of our general approach that are worth highlighting:

(a) We are not only concerned with the acquisition and validation of knowledge, but also with its use. Our concern is both with how pre-existing knowledge gets applied to practical ends, and how knowledge itself is shaped by its intended use. We aim to build meaningful bridges between the philosophy of science and the newer fields of philosophy of technology and philosophy of medicine; we also hope to provide fresh perspectives for the latter fields.

(b) We emphasize how human artefacts, such as conceptual models and laboratory instruments, mediate between theories and the world. We seek to elucidate the role that these artefacts play in the shaping of scientific practice.

(c) Our view of scientific practice must not be distorted by lopsided attention to certain areas of science. The traditional focus on fundamental physics, as well as the more recent focus on certain areas of biology, will be supplemented by attention to other fields such as economics and other social/human sciences, the engineering sciences, and the medical sciences, as well as relatively neglected areas within biology, chemistry, and physics.

(d) In our methodology, it is crucial to have a productive interaction between philosophical reasoning and a study of actual scientific practices, past and present. This provides a strong rationale for history and philosophy of science as an integrated discipline, and also for inviting the participation of practicing scientists, engineers and policymakers.

Organizing Committees

Permanent organizing committee

- Rachel A. Ankeny, University of Adelaide, rachel.ankeny@adelaide.edu.au
- Mieke Boon, University of Twente, m.boon@gw.utwente.nl
- Hasok Chang, Cambridge University, UK, hc372@cam.ac.uk
- Sabina Leonelli, University of Exeter, s.leonelli@exeter.ac.uk
- Joseph Rouse, Wesleyan University, jrouse@wesleyan.edu
- Andrea Woody, University of Washington, awoody@u.washington.edu

Additional members for programming

- Marcel Boumanns, University of Amsterdam and Erasmus University Rotterdam, boumans@fwb.eur.nl

Local organizing committee

- Hanne Andersen, Aarhus University/University of Copenhagen, hanne.andersen@ind.ku.dk
- Randi Mosegaard, Aarhus University, randi@math.au.dk
- Samuel Schindler, Aarhus University, samuel.schindler@css.au.dk
- Henrik Kragh Sørensen, Aarhus University, hks@css.au.dk
- Sara Green, University of Copenhagen, saraehrenreichgreen@gmail.com

Advisory board

- Marcel Boumans, University of Amsterdam and Erasmus University Rotterdam, m.j.boumans@uva.nl
- Nancy Cartwright, Durham University, nancy.cartwright@durham.ac.uk
- Henk De Regt, Free University of Amsterdam, hw.de_regt@ph.vu.nl
- John Dupré, University of Exeter, J.A.Dupre@exeter.ac.uk
- Mary Morgan, London School of Economics, m.morgan@lse.ac.uk
- Margaret Morrison, University of Toronto, mmorris@chass.utoronto.ca
- Nancy Nersessian, Georgia Institute of Technology, nancyn@cc.gatech.edu
- Miriam Solomon, Temple University, Philadelphia, msolomon@temple.edu
- Alison Wylie, University of Washington, aw26@uw.edu

Additional thanks

The SPSP conference is hosted by Centre for Science Studies, Department of Mathematics, Aarhus University. We gratefully acknowledge the technical support of Lars Madsen, MATH-AU, and from the AU administrative divisions for Communication and for Finance.

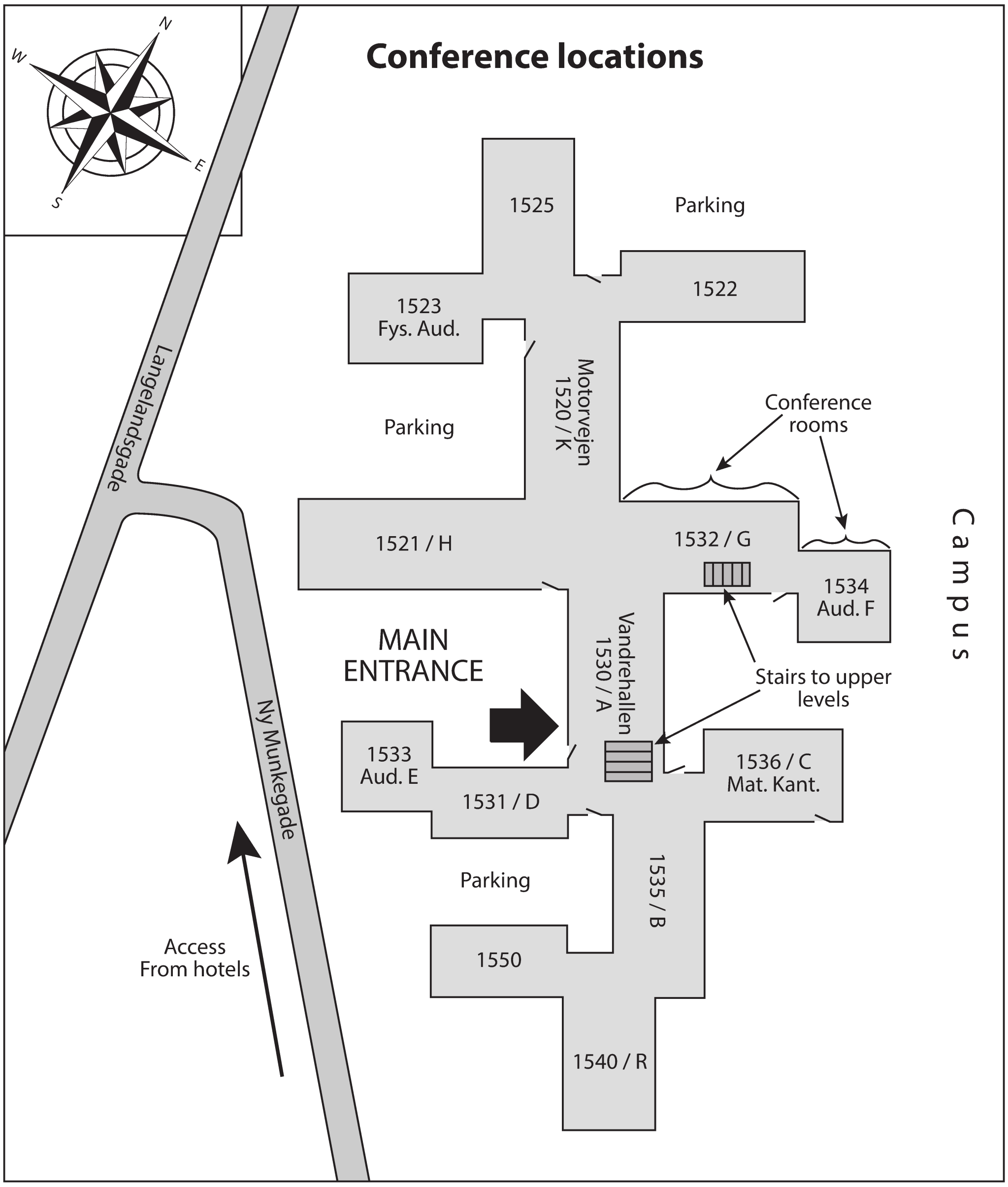
The SPSP2015 has received support by the Carlsberg Foundation and the Faculty of Science and Technology, Aarhus University, and with additional financial support from the research project Philosophy of Contemporary Science in Practice (Danish Research Council, PI: Hanne Andersen), and the Danish Network for Philosophy of Science.

Conference Location and Local Information

The conference will be held at Aarhus University, in the buildings of the Department of Mathematics, Ny Munkegade 118, 8000 Aarhus C. Information on how to get to the university from the city center can be found at the SPSP2015 webpage under Conference Location.

The registration desk is located in front of auditorium **Aud F** and is open on 23 June from 17–19 and on 24 June open from 8. The conference will take place in six rooms on two levels, mostly in building 1532.

- 1st floor (ground floor): Aud F (building 1534), Aud G1, Aud G2
- 2nd floor: Koll. G, G3, G4



Twitter Presence @SocPhilSciPract

The Twitter feed for this conference will be conducted under the hashtag **#spsp2015**. We look forward to lively coverage and discussions, also for the benefit of SPSP members that could not make it to this meeting!

Book exhibit

The book exhibit is to be found between main entrance and the conference rooms (in Vandrehallen).

Session Format and Chairing

Session format: Individual papers in concurrent sessions are scheduled for 30 minutes each in total, with 20 minutes for the talk and 10 minutes for questions immediately following each paper. This scheduling allows people to 'session hop' if they wish, so we also ask chairs to allow a minute for change over at the end of each paper/questions, and to stick to the order of presenters as it appears in the program. This means that some session blocks which are in a 2 hour period will run only for the first 1.5 hours (if they have 3 rather than 4 papers), and so on. Symposia are given more flexibility to run as they wish, but we recommend using a similar format if possible just in case people wish to attend for certain papers and not all.

All conference rooms have projectors. For all other rooms than Auditorium F, laptops need to be brought by the speakers/chair. We encourage speakers in all sessions to put all presentations on one laptop in advance, if possible.

Chairing: In each conference room the chairs will find a chair folder including three laminated pages that can be used as signs to the speaker during the talk. A green page showing the speaker that there are 5 minutes left, a yellow page showing that there are 2 minutes left, and a red sign showing that time is up.

We request that in concurrent sessions, chairs limit introductions to participants to their affiliations, and keep sessions running on time. We also ask that all participants be respectful with regard to the relatively limited question and answer periods, keeping their questions short and on point. As SPSP is about dialogue and networking, we do hope conversations will flow out of the formal sessions into more informal settings during the conference (and beyond!).

Conference coffee breaks and lunch

Coffee, tea and lunch are included in the delegate fees and will be served during the breaks. Vegetarian and vegan lunches will be served together with the regular lunch buffet so please pay attention to the signs at the buffet.

Internet

If your home organization is a part of the Eduroam initiative (https://www.eduroam.org/), you can log on to the Eduroam at Aarhus University using the credentials of your home organization. Please make sure that your devices (laptop, tablet, cell phone) are configured for Eduroam at your home organization before arrival as we cannot assist with Eduroam setup for other institutions.

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If you do not have Eduroam access, please ask for a network code at the registration desk.

What to see and do in Aarhus

Free admission to the ARoS Art Museum: Conference participants have free admission to the ARoS Art Museum (vouchers included in the SPSP2015 conference folder). ARoS is the main art museum in Aarhus. On the roof of ARoS you can also visit the rainbow panorama from here you can move around in a 150 meter long, circular panoramic path with 360° views of the surrounding city. For more information on the museum and special exhibitions, please visit: http://en.aros.dk/.

If you do not wish to use your voucher, we kindly ask that you return the vouchers to the organizers.

Sculpture by the Sea Aarhus 2015: The biggest sculpture exhibition in Denmark is free of charge and invites everybody to enjoy the combination of art and nature. From 5 June to 5 July, artists from all over the world visit Aarhus to exhibit extraordinary sculptures by the scenic shore between Tangkrogen and Ballehage. Along the beautiful scenic southern coast line you can enjoy more than 50 unique sculptures created by artists from all over the world. The many unique sculptures are exhibited on the beach, in the water and in the forest, – free to the public and available 24 hours a day.

Address: Tangkrogen 1, 8000 Aarhus C

For more information, please visit http://www.sculpturebythesea.dk.

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For other things to see, do and eat, we hope that you will all visit the SPSP2015 blog where locals give their recommendations on good restaurants, bars, sports, museums and much more:

https://spsp2015aarhus.wordpress.com

For more general information, please visit the tourist information webpage:

http://www.visitaarhus.dk

Emergencies

In case of emergency, call **112**.

Emergency pharmacy: 'Løve Apoteket' at Store Torv 5, Aarhus C, is open all hours, nights included.

Emergency doctor service: Call +45 7011 3131.

SPSP 2015 General Schedule

Tuesday, 23 June

19:00–21:00	Informal pre-conference social gathering at Dale's Café (on Campus: Høegh-Guldbergs Gade 4, 8000 Aarhus C).
21:00–22:00	Midsummer celebration on Campus ("Universitetsparken")

Wednesday, 24 June

09:00–10:15	Opening Remarks + Plenary Session 1
10:15–10:30	Coffee Break
10:30–12:00	Parallel Sessions 1A–1F
12:00–13:30	Lunch
13:30–15:00	Parallel Sessions 2A–2F
15:00–15:30	Coffee Break
15:30–17:30	Parallel Sessions 3A–3E
17:30–19:00	Welcome to participants at the Steno Museum
19:00–21:00	Reception at the Department of Mathematics

Thursday, 25 June

09:00–11:00	Parallel Sessions 4A–4F
11:00–11:20	Coffee Break
11:20–12:30	Plenary Session
12:30–14:00	Lunch + SPSP newsletter meeting
14:00–15:10	Plenary Session
15.10–15:30	Coffee Break
15:30–17:30	Parallel Sessions 5A–5E
17:30–18:30	Business Meeting

Friday, 26 June

09:00–10:10	Parallel Sessions 6A–6F
11:00–11:20	Coffee Break
11:20–12:30	Plenary Session
12:30–14:00	Lunch
14:00–15:30	Parallel Sessions 7A–7E
15:30–16:15	Closing Discussion

Detailed Programme

Wednesday, 24 June 2015

09:00–09:10 | Welcome (Aud F)

Session chair: Rachel A. Ankeny (University of Adelaide)

09:10–10:15 | Plenary talk (Aud F)

Session chair: Joseph Rouse (Wesleyan University)

The Turn to Practice in Science Studies: An Overall Characterization in Terms of Shifts With an Analysis of the Main Uses of the Term "Practice"

Léna Soler (Université de Lorraine/CNRS)

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10:15–10:30 | Coffee Break (Vandrehallen)

10:30–12:00 | Parallel Session 1A (Aud F)

Session chair: Alfred Nordmann (Technische Universitat Darmstadt)

Organized by: Annamaria Carusi (University of Copenhagen); Alfred Nordmann (Technische Universitat Darmstadt)

Symposium: Similarities Reconsidered: How Achievements of Similarity License Inferential and Constructive Moves in Research Practice

Part 1: Inferential Moves

- Similarity of Functional Behavior – Inferential Strategies in the Engineering Sciences
  Mieke Boon
- From Mice to Men: Homogeneity, Similarity and Relevance in Model-Based Reasoning
  Lara Huber
- Systems of Equivalences: Successes and Failures in Instituting Similarity in Computational Modelling of Hearts and Brains
  Annamaria Carusi

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Part 2 follows Friday at 14:00 (Parallel Session 7A)

10:30–12:00 | Parallel Session 1B (G1)

Session chair: Jessica Carter (University of Southern Denmark); Christopher Pincock (Ohio State University)

Organized by: José Ferreirós (University of Sevilla); Jessica Carter (University of Southern Denmark); Henrik Kragh Sørensen (Aarhus University)

Symposium: Philosophy of Mathematical Practice

- Fourier Series as an Interface Between Mathematics and Physics
  Christopher Pincock
- Strategies of Tuning. A New Look on Mathematization
  Johannes Lenhard
- Representations and Understanding in Mathematics
  Jessica Carter

Session abstracts on page 32–35

10:30–12:00 | Parallel Session 1C (G2)

Session chair: Sjoerd D. Zwart (Delft University of Technology)

- Cognitive Constraints, Complexity And Model-building
  Miles MacLeod; Nancy Nersessian
- The Role of Interactions in Determining Biological Systems Structure and Function
  Mihaela Pavlicev; Robert Richardson
- Systems Medicine: Visions and Controversies
  Sara Green

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10:30–12:00 | Parallel Session 1D (G3)

Session chair: Timothy Tambassi (Università del Piemonte Orientale)

- On the Emergence of a Prediction Culture in Climate Modeling
  Matthias Heymann
- Uncertainty, Non-epistemic Values and the Intergovernmental Panel on Climate Change
  Erin Nash

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10:30–12:00 | Parallel Session 1E (G4)

Session chair: Leen De Vreese (Ghent University)

- Cancer: From One to Multiple Diseases
  Gry Oftedal; Anders Strand
- Psychiatric Classification Between Science and Practice
  Anke Bueter
- Inductive Risk, Epistemic Risk, and Overdiagnosis of Disease
  Justin Biddle

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10:30–12:00 | Parallel Session 1F (Koll G)

Session chair: Hasok Chang (University of Cambridge)

- Death of the Scientific Author, Contributors Too
  Barton Moffatt
- Privacy, Informed Consent, and Participant Observation
  Julie Zahle
- Facing Animals
  Sophia Efstathiou

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12:00–13:30 | Lunch (Vandrehallen)

13:30–15:00 | Parallel Session 2A (Aud F)

Session chair: Marcel Boumans (University of Amsterdam and Erasmus University Rotterdam)

Organized by: Michiru Nagatsu (University of Helsinki)

Symposium: Critical Perspectives on the Practice of Evidence-Based Behavioral Public Policy

- Behaviorally Informed Policy and Decision Theory
  Jaakko Kuorikoski; Samuli Pöyhönen
- Boosts Versus Nudges: How to Pick the Right Policy Tool
  Markus Feufel; Till Grüne-Yanoff; Caterina Marchionni
- What's Wrong With Evidence-Based Approach?: "New Development Economics" and Behavioral Development Economics
  Judith Favereau; Michiru Nagatsu

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13:30–15:00 | Parallel Session 2B (G1)

Session chair: Mieke Boon (University of Twente)

- Toward a Tool for Supporting Interdisciplinary Research Teams in Developing Shared Ontologies
  Julie Mennes
- Boundary Work: Nanoscience Meets Philosophy
  Julia Bursten
- Inter-experimentality in the Discovery of the Acceleration of the Universe
  Genco Guralp

Session abstracts on page 47–49

13:30–15:00 | Parallel Session 2C (G2)

Session chair: Sabina Leonelli (University of Exeter)

- Replication and Data-Sharing in the Social Sciences: Progress and Challenges
  Stephanie Wykstra
- Explaining Rare Events in Political Science: A Mixed Methods Approach
  Sharon Crasnow; Stephan Haggard
- Economics Imperialism in Social Epistemology: A Critical Assessment
  Manuela Fernández Pinto

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13:30–15:00 | Parallel Session 2D (G3)

Session chair: Joseph Rouse (Wesleyan University)

- Toward Semiotic Modelling of Experimental Practices
  Robert Meunier
- Individuation, Individuality, and Experimental Practice in Developmental Biology
  Alan Love
- Context Dependencies and Multi-level Explanations in Biological Sciences
  Marta Bertolaso

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13:30–15:00 | Parallel Session 2E (G4)

Session chair: Holly VandeWall (Boston College)

- Precaution in Scientific Model Building: The Case of the Threshold of Toxicological Concern Approach in Food Toxicology
  Karim Bschir
- The Risk of Using Inductive Risk to Challenge the Value-Free Ideal
  Inmacuala de Melo-Martin; Kristen Intemann
- Estimation of Systematic Uncertainty as Robustness Analysis
  Kent Staley

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13:30–15:00 | Parallel Session 2F (Koll G)

Session chair: Hasok Chang (University of Cambridge)

- To Know is to Identify: Forging a Realist Criterion for Astrophysical Entities
  Alan Heiblum
- Theseus and the Zymes
  Dana Tulodziecki
- Science, Philosophy, and Applied Ontology: A Common Project for a Unitary Description of Reality
  Timothy Tambassi

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15:00–15:30 | Coffee Break (Vandrehallen)

15:30–17:30 | Parallel Session 3A (Aud F)

Session chair: Alan Love (University of Minnesota)

Organized by: Sabina Leonelli (University of Exeter)

Symposium: Data Practices in Biology and Biomedicine

Part 1: Data Flows and Epistemic Implications of Databasing

- The Evidential Scope of Databases in Cancer Genetics
  Emanuele Ratti
- Data Integration Between Databases
  James A. Overton
- Mapping Biological Knowledge. From Particular Data to General Phenomena
  Federico Boem
- The Flow Metaphor and Data Ecosystems
  Gregor Halfmann

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Part 2 follows Thursday at 09:00 (Parallel Session 4A)

15:30–17:30 | Parallel Session 3B (G1)

Session chair: Annamaria Carusi (University of Copenhagen)

Organized by: Sophie Van Baalen (University of Twente)

Symposium: Philosophy of Science in Clinical Practice

- From the ‘Revolution’ to the ‘Renaissance’: Science, Philosophy, Rhetoric and the EBM Debate
  Michael Loughlin
- Causation in Scientific Methods and the Medically Unexplained
  Rani Lill Anjum
- A Critical Medical Humanities Approach to the Clinical Practice and Science of Breathlessness
  Jane Macnaughton
- Evidence Based Medicine Versus Expertise: Understanding Epistemic Actions in Clinical Practice
  Sophie van Baalen

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15:30–17:30 | Parallel Session 3C (G2)

Session chair: Andrea Woody (University of Washington)

- Diagrams as Both Representation and Practice in Developing Mechanistic Cell Models
  Yin Chung Au
- Computer Data Processing and Its Impact on the Interpretation of Digital Images
  Vincent Israel-Jost
- Diagrams of Sound and Vision
  Sabine Brauckmann; Sara Franceschelli

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15:30–17:30 | Parallel Session 3D (G3)

Session chair: Mieke Boon (University of Twente)

- Expanding the Experimental Realm: An Account of Descriptive and Functional Experimentation in the Natural Sciences
  Stephan Guttinger
- Is Rigorous Measurement of Statistical Evidence Possible?
  Veronica Vieland
- Measurement and Metrology Post-Maxwell: A Historical, Philosophical, and Mathematical Primer
  Daniel Mitchell

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15:30–17:30 | Parallel Session 3E (G4)

Session chair: Joseph Rouse (Wesleyan University)

- Collaboration And Explanatory Models
  Melinda Fagan
- Model Coupling in Resource economics: Conditions for Effective Interdisciplinary Collaboration
  Michiru Nagatsu; Miles MacLeod
- Authorship, Collaboration, and Joint Commitment
  Haixin Dang
- Philosophy of Citizen Science in Practice
  Kristian H. Nielsen

Session abstracts on page 75–79

17:30–19:00 | Welcome to participants (Steno Museum for the History of Science and Medicine)

19:00–21:00 | Reception (Department of Mathematics)

Thursday, 25 June 2015

09:00–11:00 | Parallel Session 4A (Aud F)

Session chair: Alan Love (University of Minnesota)

Organized by: Sabina Leonelli (University of Exeter)

Symposium: Data Practices in Biology and Biomedicine

Part 2: Towards a Pluralist Understanding of Data Uses

- Data, Models and Data Models
  Sabina Leonelli
- Contrasting Approaches in Mitochondrial Evolution: Data-Emphasis, Data-Ignorance and Its Consequences
  Thomas Bonnin
- Data, Infrastructures and Materials: Repertoires in Model Organism Biology
  Rachel A. Ankeny; Sabina Leonelli
- Commentary
  Hans-Jörg Rheinberger

Session abstracts on page 60–66

09:00–11:00 | Parallel Session 4B (G1)

Session chair: Stephanie Wykstra (Innovations for Poverty Action)

- From Cells to Society (and Back): Epistemic Challenges and Political Implications of a Novel Approach in Public Health
  Alexandra Soulier; Caroline Guibet-Lafaye
- A Change in Practice?: A Reevaluation of Mechanistic Reasoning and Clinical Experience in Post-grade Evidence-Based Medicine
  Sarah Wieten
- Pluralism Into the Pasteur's Quadrant: From the Study of Human Behavior to Cancerology
  Baptiste Bedessem; Stéphanie Ruphy
- Meta-analysis and the Ideals of Objectivity
  Saana Jukola

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09:00–11:00 | Parallel Session 4C (G2)

Session chair: Julia Bursten (University of Pittsburgh)

- Making Sense of Theoretical Practices: Scripts, Scruples and the Mass of the Universe
  Jaco de Swart
- Situating Styles of Reasoning
  Adam Toon
- Performing Medical: Transforming Institutional Identity at the Jackson Laboratory
  Ekin Yasin
- Practice Theory and Pragmatism in Science & Technology Studies: Convergence or Collision?
  Anders Buch

Session abstracts on page 82–85

09:00–11:00 | Parallel Session 4D (G3)

Session chair: Lena Kästner (Humboldt Universität zu Berlin)

- Laws and Mechanisms: The Convergence of Two Explanatory Accounts in Neuroscientific Practice
  Philipp Haueis
- Reverse Inference, the Cognitive Ontology and the Evidential Scope of Neuroimaging Data
  Jessey Wright
- The Explanatory Payoffs of Multiple Realization in Cognitive Neuroscience
  Maria Serban

Session abstracts on page 86–88

09:00–11:00 | Parallel Session 4E (G4)

Session chair: Anna de Bruyckere (Durham University)

- Bridging the Gap Between Well-Being research and Policy
  Alicia Hall
- Science-Based Policy-making in an Interdisciplinary Perspective
  David Budtz Pedersen
- Knowledge Creation in the Congressional Research Service
  Holly VandeWall
- Industrial Intellectual Property Law as Technology
  Ave Mets

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09:00–11:00 | Parallel Session 4F (Koll G)

Session chair: Andrea Woody (University of Washington)

- An Empirical Based Classification of Engineering Projects
  Sjoerd D. Zwart; Marc J. de Vries
- Incorporating Growth of Knowledge Frameworks in the Science Curriculum
  Sibel Erduran; Zoubeida Dagher
- Reconceptualizing the Nature of Science for Science Education
  Zoubeida Dagher; Sibel Erduran
- The Place of Contextual Knowledge in the Design of a Software Platform for Teaching and Learning: Making the Case for an Empirical Strategy in Software Design With Distributed Cognition
  Klara Benda

Session abstracts on page 92–95

11:00–11:20 | Coffee Break (Vandrehallen)

11:20–12:30 | Plenary talk (Aud F)

Session chair: Andrea Woody (University of Washington)

Investigating Discovery Practices: Studies of Bioengineering Sciences Labs

Nancy J. Nersessian (Harvard University)

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12:30–14:00 | Lunch (Vandrehallen) | Newsletter Meeting (all welcome!)

14:00–15:10 | Plenary talk (Aud F)

Session chair: Mieke Boon (University of Twente)

Philosophy of Clutter

Marcel Boumans (University of Amsterdam and Erasmus University Rotterdam)

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15:10–15:30 | Coffee Break (Vandrehallen)

15:30–17:30 | Parallel Session 5A (Aud F)

Session chair: Andrea Woody (University of Washington)

- Understanding Scientific Practices as Discursive Niche Construction
  Joseph Rouse
- Representation and Correspondence as Dead Metaphors
  Hasok Chang
- Scientific Practices and the Problem of Concept Formation
  Laura Georgescu
- The Consequences of Putting the Philosophy of Science Into Practice
  Robert Frodeman

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15:30–17:30 | Parallel Session 5B (G1)

Session chair: Sabina Leonelli (University of Exeter)

Organized by: Hans Radder (VU University Amsterdam)

Symposium: Practising Philosophy of Science in the Public Interest

- The How and Why of Philosophy of Science's Societal Impact
  Hans Radder
- Should Scientific Ontologies Reflect Public Interests?
  David Ludwig
- The Social Relevance of the Philosophy of Climate Science
  Anna Leuschner
- A Satanic Mill for Science?
  Daniel Hicks

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15:30–17:30 | Parallel Session 5C (G2)

Session chair: Sara Green (University of Copenhagen)

Organized by: Morgan Thompson (University of Pittsburgh)

Symposium: Mechanistic Explanation Meets Scientific Practice

- Mechanist and Non-mechanist Modes of Discovery: A case for phenomenal intervention in neuroscience
  David Colaco
- Explanatory Relations
  Daniel C. Burnston
- Norms for Mechanistic Explanation Available in Practice
  William Bechtel
- Limiting the Scope of Mechanistic Explanation
  Morgan Thompson

Session abstracts on page 105–108

15:30–17:30 | Parallel Session 5D (G3)

- On the Epistemic Roles of Simulations in Cognitive Modeling
  Maria Serban
- Hermeneutic Marginalisation and Economic Policy Modelling
  Anna de Bruyckere
- About "Numerical Experiments"
  Julie Jebeile
- An Information-Theoretic Model of Scientific Reasoning
  Agnes Bolinska

Session abstracts on page 109–112

15:30–17:30 | Parallel Session 5E (G4)

Session chair: Leah McClimans (University of South Carolina)

- Knowledge and Its Limitations in Otolaryngology
  Anaïs Rameau
- Neglected Tropical Diseases: A Case for Epistemic Pluralism
  Erman Sozudogru
- Kinds and Degrees of Scientific Understanding in Medicine
  Leen De Vreese
- Biological Organization, Diseases and Normativity in Medicine
  M. Arantzazu Etxeberria

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17:30–18:30 | Business Meeting (All Welcome!) (Aud F)

Friday, 26 June 2015

09:00–11:00 | Parallel Session 6A (Aud F)

Session chair: Adam Toon (University of Exeter)

Organized by: Chiara Ambrosio (UCL)

Symposium: Aesthetics in Scientific Practice

- Why Do Scientists Find Beautiful Theories Aesthetically Attractive?
  James W. McAllister
- Who is Afraid of Mimesis?
  Chiara Ambrosio
- Resemblance and Its Discontents in Art and Science
  Mauricio Suárez
- 'Creative Similarity' in the Understanding of Science and Art
  Julia Sánchez Dorado

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09:00–11:00 | Parallel Session 6B (G1)

Session chair: Inmacuala de Melo-Martin (Weill Cornell Medical College)

Organized by: Evelyn Brister (Rochester Institute of Technology)

Symposium: Interdisciplinarity, Sustainability Science, and Philosophy of Science Beyond the Disciplines: Commentary on Robert Frodeman's Sustainable Knowledge: A Theory of Interdisciplinarity

- Interdisciplinarity, Sustainability Science and the Philosophy of Science: Robert Frodeman's Sustainable Knowledge
  Paul B. Thompson; Danielle Lake
- Interdisciplinarity, Rigor, and Deaccelerating the Growth of Knowledge
  Evelyn Brister
- Why Has Applied Philosophy Run Out of Steam
  David Budtz Pedersen
- Sustainable Knowledge: Philosophy of Science in the Field
  Robert Frodeman

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09:00–11:00 | Parallel Session 6C (G2)

Session chair: Joseph Rouse (Wesleyan University)

- Mechanistic Explanations of Physical Laws: How Do They Provide Understanding?
  Erik Weber; Joachim Frans
- Mechanisms vs. Difference-Making
  Lena Kästner; Lise Marie Andersen
- Reaction Mechanisms in Chemistry: A Comparison Case for Accounts of Scientific Explanation
  Andrea Woody
- Explanation, Inferences, and Chemical Reactions: A Mechanistic View for Scientific Practices
  Juan Bautista Bengoetxea; Oliver Todt; José Luis Luján

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09:00–11:00 | Parallel Session 6D (G3)

Session chair: Holly VandeWall (Boston College)

- The University Museum: A Microcosm for Studying Transdisciplinary Challenges
  Line Breian; Johannes Persson
- Scientists as Experts: Understanding Trustworthiness Across Communities
  Heidi Grasswick
- Expert Witnesses in a Trial Against Experts: Of Causal Links and Scientific Responsibility in the L'Aquila Case
  Federico Brandmayr
- Why Scientists Cannot and Should Not Be Sincere
  Stephen John

Session abstracts on page 127–130

09:00–11:00 | Parallel Session 6E (G4)

Session chair: Maria Serban (University of Pittsburgh)

- Design Explanation and Idealization
  Dingmar van Eck
- Unified and Disunified Strategies for Explaining Parameter Robustness
  Nicholaos Jones
- Not Null Enough: Causal Null Hypotheses in Community Ecology and Comparative Psychology
  William Bausman; Marta Halina
- Essentialism, Evolutionary Theory and Human Rights
  Edit Talpsepp

Session abstracts on page 130–134

09:00–11:00 | Parallel Session 6F (Koll G)

Session chair: Hasok Chang (University of Cambridge)

- The Epistemological Role of Systematic Discrepancies
  Teru Miyake
- (Re-) Discovering Elementary Particles at Cern by Diagnostic Causal Inferences
  Adrian Wüthrich
- What Would Be a Cultural Logic of Conceptual Discovery?
  Jouni-Matti Kuukkanen
- Theoretical Bias of the Standard Research Practice in Social Psychology
  Taku Iwatsuki

Session abstracts on page 134–137

11:00–11:20 | Coffee Break (Vandrehallen)

11:20–12:30 | Plenary talk (Aud F)

Session chair: Sabina Leonelli (University of Exeter)

On Materiality and Scientific Objects

Hans-Jörg Rheinberger (Max Planck Institute for the History of Science)

Abstract on page 137

12:30–14:00 | Lunch (Vandrehallen)

14:00–15:30 | Parallel Session 7A (Aud F)

Session chair: Mieke Boon (University of Twente)

Organized by: Annamaria Carusi (University of Copenhagen); Alfred Nordmann (Technische Universitat Darmstadt)

Symposium: Similarities Reconsidered: How Achievements of Similarity License Inferential and Constructive Moves in Research Practice

Part 2: Constructive Moves

- Varieties of Similarity
  Alfred Nordmann
- Image-Based Inferences in Engineering-Sciences: Which Role Does the Concept of Similarity Play?
  Sabine Ammon
- The Epistemic Functions of Computational Simulation Images: A Case Study in Nanoscience
  Catherine Allamel-Raffin

Session abstracts on page 24–31

14:00–15:30 | Parallel Session 7B (G1)

Session chair: Marcel Boumans (University of Amsterdam and Erasmus University Rotterdam)

Organized by: Leah McClimans (University of South Carolina)

Symposium: Nomothetic and Idiographic Approaches to Quality of Life Measurement

- A Lay of the Land: Nomothetic and Idiographic Approaches to Quality of Life Measurement
  John Browne
- Epistemic and Ethical Problems with Nomothetic and Idiographic Quality of Life Measures
  Leah McClimans
- Applying Tal's Model-Based Account of Measurement to Nomothetic Quality of Life Measures
  Laura Cupples

Session abstracts on page 137–140

14:00–15:30 | Parallel Session 7C (G2)

Session chair: Justin Biddle (Georgia Institute of Technology)

- Connecting Feminist Standpoint Empiricism to Cognitive Neuroscience
  Vanessa Bentley
- The Epistemic Significance of Scientific/Intellectual Movements
  Kristina Rolin

Session abstracts on page 140–142

14:00–15:30 | Parallel Session 7D (G3)

Session chair: Sabina Leonelli (University of Exeter)

- Upper Level Ontologies, Metaphysical Commitments, and the Production of Questions
  Brandon Boesch
- What Are Biological Mechanisms? A View From Scientific Practice
  Daniel Nicholson

Session abstracts on page 142–143

14:00–15:30 | Parallel Session 7E (G4)

Session chair: Maria Serban (University of Pittsburgh)

- Material and Social Conditions for the Development of Mathematics
  Morten Misfeldt; Mikkel Willum Johansen
- Generating Certainty in Mathematical Practice: A Case Study in an Ethnography of Current Research Mathematics
  Stav Kaufman
- Mathematization in Practice
  Davide Rizza

Session abstracts on page 144–146

15:30–16:15 | Closing Discussion (Aud F)

Session chair: Hasok Chang (University of Cambridge)

Abstracts

The abstracts appear in the same order as in the programme, with the exception of the two two-day Symposiums where the presentation abstracts for the entire symposia are listed consecutively (see page 24 and page 60 respectively).

Plenary talk

Wednesday, 24 June 2015 at 09:10–10:15 in Aud F

Session chair: Joseph Rouse (Wesleyan University)

The Turn to Practice in Science Studies: An Overall Characterization in Terms of Shifts With an Analysis of the Main Uses of the Term "Practice"

Léna Soler (Université de Lorraine/CNRS)

Abstract. It is generally acknowledged that science studies underwent a change that began in the 1970s and was later often called the "turn to practice" (or "practice turn"). This occurred in one form or another in all studies that take science as their object, whatever their perspective (philosophical, historical, sociological, or other) and whatever the field of interest (physics, biology, mathematics, engineering sciences, etc.). In this talk, I attempt an overall characterization of this practice turn. The aim is to point to general trends, beyond the diversity of orientations and the possible particularities depending on the fields of interest. The corresponding trends are framed in terms of shifts, so as to emphasize the contrast with anterior so-called "traditional" ways of approaching science against which actors of the practice turn have motivated and defined their aims, methods, and views. The analysis of the shifts specify and disentangle different uses of the term "practice(s)", associated with different messages conveyed by the appeal to practice(s), that are more or less explicitly involved in the practice turn.

After some preliminary remarks about problems of delimitation and the status of the proposed characterization, each shift is successively introduced and clarified through a number of pivotal constitutive contrasts. Three shifts are identified as central and are analyzed in detail – through each covers aspects that could be conceptualized as other shifts, possibly as sub-shifts. The first shift consists in moving away from accounts of science that are based on a priori conceptions of science and are "too" idealized, and in looking for empirically-based and empirically-adequate accounts of science. This shift can be viewed as the most general formulation of the criticism directed by the practice turn against traditional studies of science. All the other shifts convey a particular version of the first shift. The second shift moves from scientific products to scientific processes (relying on senses of "product" and "process" that will be examined). And the third shift moves from science as contemplation and re-presentation of the world, to science as intervention and transformation.

For each shift, I specify the sense(s) of "practice(s)" that are at stake, I analyze the main substantial and methodological messages conveyed by referring to "scientific practice" in these senses, and I consider some paradigmatic ways in which the shift in question has been instantiated in the science studies. I also indicate some important relations between the different shifts and between the different uses of the term "practice(s)". Finally, I sketch some more or less generally accepted lessons about science that can be learned from the practice turn.

Parallel Session 1A and Parallel Session 7A

The symposium is split into two parts, time, place and chairs follow below.

Organized by: Annamaria Carusi (University of Copenhagen); Alfred Nordmann (Technische Universitat Darmstadt)

Symposium: Similarities Reconsidered: How Achievements of Similarity License Inferential and Constructive Moves in Research Practice

Synopsis. Inferences flow from data to an explanatory conclusion, as well as in the other direction from theory to predicted values. This is a matter of inductive and deductive logic, probabilistic reasoning, Bayesian networks, and the like. Each in their own way, these conceptions of presuppose some degree of homogeneity among data, some way of treating them as similar. A philosophy of inferential practice needs to consider the varieties of technologies and techniques by which similarity is achieved and thus the conditions established for inferential moves. These include very basic conceptual tools like methodological maxims, inductive principles, conservation laws that delimit the domain of inquiry ("ex nihilo nihil", "natura non fecit saltus", "the future is like the past", "no matter is lost or created, just rearranged in space"). Similarity may also be achieved through elaborate technical frameworks of reasoning like those of measurement theory or probabilistic inference ("numbers can be assigned to qualities," "in the long run, relative frequency approximates objective probability"), and through routines of collecting, standardizing, curating data and making them commensurable, e.g., in natural history museums or databanks.

Especially in contemporary practice, this set of strategies and techniques includes the mutual assimilation of technologies of observation and of modeling such that one can move back and forth between experimental and simulated situations. So, indeed, if the technological conditions warrant it, it becomes possible for purposes of explanation and prediction to infer a shared underlying dynamic from mere visual similarity. This common and accepted practice cannot be accounted for in terms of the methodological canon of the philosophy of science. It is this practice, in particular, to which the papers in the proposed pair of panels will be dedicated.

The first panel will consider the question of how to reconstruct and justify appeals to similarity in explanatory inference. Among the fields where these practices can be investigated are theoretical chemistry and materials research where simulations of laboratory experiments take on the role of explanation. Also, it is a common feature where models are taken as models for rather than models of, e.g., when animal models stand in for human disease processes. Arguably, this form of explanation from similarity serves as an epistemic ideal in Systems Biology and Synthetic Biology as well as the Human Brain Project and any research that surrenders the demand for intellectual tractability to machines and judges only the overall performance of the machine as a simulacrum.

The second panel will consider how the construction of systems of equivalences offers access to new phenomena, and allows us to study objects of design, that is, objects that do not yet exist. Here, the so-called emerging technologies, engineering and architecture come into view where similarity can underwrite not only inferential but thereby also constructive moves.

Part 1: Inferential Moves

Wednesday, 24 June 2015 at 10:30–12:00 in Aud F

- Similarity of Functional Behavior – Inferential Strategies in the Engineering Sciences
  Mieke Boon
- From Mice to Men: Homogeneity, Similarity and Relevance in Model-Based Reasoning
  Lara Huber
- Systems of Equivalences: Successes and Failures in Instituting Similarity in Computational Modelling of Hearts and Brains
  Annamaria Carusi

Part 2: Constructive Moves

Friday, 26 June 2015 at 14:00–15:30 in Aud F

- Varieties of Similarity
  Alfred Nordmann
- Image-Based Inferences in Engineering-Sciences: Which Role Does the Concept of Similarity Play?
  Sabine Ammon
- The Epistemic Functions of Computational Simulation Images: A Case Study in Nanoscience
  Catherine Allamel-Raffin

The abstracts for both parts follow below.

Part 1: Inferential Moves

Wednesday, 24 June 2015 at 10:30–12:00 in Aud F

Session chair: Alfred Nordmann (Technische Universitat Darmstadt)

Similarity of Functional Behavior – Inferential Strategies in the Engineering Sciences

– Mieke Boon (Department of Philosophy, University of Twente, m.boon@utwente.nl)

Abstract. Two intriguing questions in aiming to understand the engineering sciences are: how is scientific research epistemologically related to technological innovation, and how is it possible that scientific research in the engineering sciences purposefully creates / invents new physical phenomena (such as new material properties in biomedical engineering or in nanotechnology). What kinds of inferential strategies enable these kinds of inventions?

It will be argued that the invention of a new physical phenomenon in the engineering sciences involves mutually related epistemological activities: (1) its conception (e.g., 'artificial photosynthesis,' which is the physical phenomenon of artificially producing electricity and/or useful chemical compounds from sun-light, similar to photo-synthetic processes in nature); (2) the conception of causal-mechanistic model(s) of how the phenomenon could possibly be generated (e.g., causal-mechanistic model of biochemical processes involved in photo-synthesis); and (3) the conception of how the physical phenomenon could possibly be generated by technological instrumentation.

How does similarity as an inferential strategy play a role in those epistemological activities? Bengoetxea et.al. (2014) have argued that the concept of similarity is a useful epistemological tool for performing basic tasks in science, such as: learning, inductively generalizing, and making predictions. In chemistry, similarity is used to establish classification patterns that are based not only on fundamental structural features derived from physical theory, but even more on all relevant chemical aspects useful to scientists (cf. Giere 2010). By means of these classifications chemists can obtain sophisticated sets of concepts that permit both the representation of properties and phenomena, as well as the prediction of new properties and entities. Hence, according to Bengoetxea et.al. (2014), in chemistry new properties and entities are predicted by means of concepts, which themselves have been obtained from similarities between relevant chemical aspects. Similarity put this way, involves as an epistemological strategy Newton's second rule of philosophizing ("to natural effects of the same kind the same causes should be assigned, as far as possible"). However, it will be argued that this account does not sufficiently explain how it is possible that scientists invent properties in the materials sciences. The predictive power of concepts formed by means of similarity requires further explanation, and a more comprehensive explanation must take into account the epistemological activities mentioned above (also see Boon 2012 and forthcoming). Research on artificial photosynthesis will be used to show similarity as an inferential strategy that lead towards such inventions.

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From Mice to Men: Homogeneity, Similarity and Relevance in Model-Based Reasoning

– Lara Huber (Institut für Philosophie, Bergische Universität Wuppertal, lara_huber@gmx.de)

Abstract. Models and simulations commonly are regarded as key strategies of scientific inference: Models are said to be 'sources of genuine science-extending existential hypotheses' (Harré 1970), simulations are discussed to 'increase the range of phenomena that are epistemically accessible to us' (Frigg & Reiss 2009). Besides well-known queries, such as if and how models represent target phenomena (which they are said to model), more recent debates reflect upon issues of validation and verification: With regard to explanatory inference it has been stated that model and target systems should share relevant similarities (i.e. Hesse 1963, Parker 2009). Here, what is considered 'relevant' depends on the particular question an experimental system wants to answer.

According to Parker and others relevant similarity justifies inference much more adequately than ontological equivalence. The latter refers to models that are 'made of the same stuff as the real world' (Morgan 2005).

Given the overall framework of biomedicine, to justify inferences from animal-based models of (human) diseases is a multifaceted issue, as a recent study taking the case of Alzheimer mice illustrates (Huber & Keuck 2013): Here, a three-fold validation process includes, (a) the selection of means and targets of modelling (appropriate organism; relevant research parameters), (b) issues of internal validation within the laboratory, such as strategies of manipulation and control, which are ranging from standardised intervention into experimental organisms to practices of stabilised replication of given features in a species. Also issues of external validation are involved, given that the generation of a specific animal model is achieved only if the experimental potentiality of an organism is realised with respect to a certain target of modelling that is proven to be clinically relevant. Furthermore, (c) validation processes relate to the applicability of animal-based approaches to patient-based (clinical) research.

Against this background key aspects of model-based reasoning and inference in biomedicine are addressed. The paper elaborates on practices of identifying relevant pathogenic processes and securing homogeneity, i.e. of transgenic experimental organisms. Especially, it explores in how far homogeneity as an epistemic end of standardisation could be regarded as prerequisite of (relevant) similarity: Relevant similarity, as research into Alzheimer's Disease suggests, is not an object of mere assumption or stipulation, but has to be instantiated on the basis of experimental techniques, and thereby proven.

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Systems of Equivalences: Successes and Failures in Instituting Similarity in Computational Modelling of Hearts and Brains

– Annamaria Carusi (Centre for Medical Science and Technology Studies, University of Copenhagen, carusi.annamaria@gmail.com)

Abstract. In previous work, I have claimed that even though comparisons of computational modelling and laboratory experiments or other sources of data are couched in terms of 'resemblance', 'correspondence' and 'match' apparently after the main activity of modelling has taken place, the process of constructing the grounds of comparability is in fact a core part of the modelling process (Carusi 2014, Carusi, Burrage and Rodriguez 2013). This means that in the scientific practices of experiment-facing modelling, there is not a straightforward matching or checking for correspondences in a 'face-off' between models on one hand and experiments on the other, as though these were externally related, independently constituted entities. Rather, there is a gradual and essentially temporal process, over several iterations, of establishing a system of equivalences between the different aspects of the process, which are seen as internally related, co-constituted parts (Chang 2004, Rouse 2002). Systems of equivalences – significantly in the plural – play a strong role in shaping what counts as similarity in a modelling domain, and therefore also condition what might be called an inferential style in that domain, and underpin what is meant by terms such as 'representation' in its discourse. The presentation focuses on the characterisation of the notion of systems of equivalences, inspired by the philosophy of vision, art and symbolic systems of Maurice Merleau-Ponty (for example, 1973): they are mediated and embodied in the symbolic systems and technologies of the modelling domain; epistemic and normative, as they provide the framework for interpretation and significance that serve as criteria of comparability; ontological with respect to the constitution of the features of the modelling domain. Very importantly, systems of equivalences must be socially shared in order to play any of these roles. I draw on apparently successfully established modelling domains by drawing on a case study in computational cardiac electrophysiology. However, failures to share systems of equivalences can lead to scientific controversies such as has recently been the case in the Human Brain Project. The neuroscientific community has been vociferous in its objections to the amount of European funding that has been given to this project. I analyse the reasons for these objections through a discourse analysis of the documents relating to it, including letters that have been written by the various parties, papers published by the main proponents, and the different modes of visualisation used. I pay especially close attention to the claims made concerning the alleged equivalences between the human brain and computational artefacts, be they for 'science' or for 'engineering' purposes. I claim that the notion of system of equivalences sheds light not only on domains that are apparently successfully constituted, but also on those that are not.

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Part 2: Constructive Moves

Friday, 26 June 2015 at 14:00–15:30 in Aud F

Session chair: Mieke Boon (University of Twente)

Varieties of Similarity

– Alfred Nordmann (Institut für Philosophie, Technische Universität Darmstadt, nordmann@phil.tu-darmstadt.de)

Abstract. At least from its beginnings in the late 19th century, philosophy of science emphatically excluded from its methodological canon appeals to similarity. In the tradition of Kant, physicists like Heinrich Hertz and philosophers like Ludwig Wittgenstein maintained that the truth or falsity of models or pictures does not depend on their similarity to what they represent or depict. Indeed, all we can know about these models is their predictive or explanatory succcess – but we do not and cannot know whether their likeness extends beyond the agreement, say, of a predicted and an observed fact.

In contemporary discussions about similarity, partial or complete isomorphisms (e.g., Giere 1999, Suárez 2003, French 2003) the Kantian rigor about limits of knowledge is liberalized – to be sure, an oil painting of a sunset is quite dissimilar from an actual sunset, but this should not prevent us from acknowledging that painterly realism differs from abstract art in that it produces likenesses. This acknowledgment, in turn, gives rise to a program where different degrees of similarity might be used to judge veracity.

This shift misses the point, however, of the original need to reject appeals to similarity. It served to distance modern science from magical thinking. Similarity animates the pre-modern prosaic world decribed by Foucault (1970), it is a central methodological category of the so-called pseudo-sciences of astrology, alchemy, physiognomy, homeopathy. Is does not signify a representational relation (more or less similar in terms of visual or structural likeness) but a kinship relation according to which similar things participate in a shared reality, and this relation is thought to be causally significant. On this account and pace Goodman (1972), similarity is sui generis and not reducible to “sameness in some specifiable respect, difference in others.”

Against this conceptual backdrop, the paper considers the recent contributions by Bengoetxea et al. (2014) and Weisberg (2013) on similarity in chemistry. It will show that they treat similarity only as a representational notion and therefore miss out on the fact that technologies of modelling and visualization establish kinship relations that underwrite and warrant the reappearance of a notion of similarity that had been exorcised by modern conceptions of science.

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Image-Based Inferences in Engineering-Sciences: Which Role Does the Concept of Similarity Play?

– Sabine Ammon (Institut für Philosophie, Technische Universität Darmstadt, ammon@phil.tu-darmstadt.de)

Abstract. Looking at design and construction processes in engineering sciences, we find a plethora of different kinds of images: sketches, drawings, plans, diagrams, and renderings (Ferguson 1992, Henderson 1999, Ewenstein & Whythe 2009).They are crucial means to develop novel artefacts and design-knowledge; their production allows processes of reasoning in order to establish the rightness of the design and to gain knowledge of the yet non-existent. In my contribution I will examine examples of image-based reasoning in engineering sciences in order to determine to which extend these procedures rely on the concept of similarity.

The argument is based on the assumption that design processes involve modes of genuine knowledge production by specific techniques, methods, and strategies – which are anchored in visual-spatial reasoning and thinking (Tversky 2005, Hegarty & Strull 2012). These techniques, methods, and strategies help to single out problems, isolate open questions, supply procedures to approach tentative solutions, as well as to refine and test them until they hold up convincingly. Little by little, in hard-won steps and iterative loops, the rightness of the design is tested within the process of drawing. It is revised, discarded or strengthened until eventually, from the struggle for rightness, a secure knowledge can be stabilized. And this knowledge finally allows for the construction and building of the artefact. To ensure this outcome, many factors may have an impact. They deliver restrictions, frameworks, or simply guide the direction of the ongoing process: When exploring the design problem, selecting among variations or assessing potential results, several factors come into play, such as the coherence of the design, its consistency with well approved bodies of knowledge, the relevance of certain parameters, the anchoring of partial results in existing design experience, the range of the intended solution, or its effects on the overall setting.

These forms of reasoning rely on a domain specific implementation of epistemic strategies. Such epistemic strategies are for example the reduction of complexity, variation and comparison, identification of relevant parameters, the development of criteria of assessment, externalizing and explaining, or a search for mistakes. In the engineering sciences, especially visuo-spatial techniques, methods, and tools enable to pursue these strategies. They are implemented in techniques such as layering and contrasting juxtaposition, projecting, scaling, or interrelating design manifestations. Their underlying epistemic dynamic can be described by a broadly conceived concept of visual similarity.

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The Epistemic Functions of Computational Simulation Images: A Case Study in Nanoscience

- Catherine Allamel-Raffin (Institut de Recherches Interdisciplinaires sur les Sciences et la Technologie, Laboratoire d'Analyses des Sciences et des Technologies, Université de Strasbourg, catherine.allamelraffin@unistra.fr)

Abstract. Images of different kinds are widely used in nanoscience. Firstly, relying on the analysis of my own ethnographic studies, I will propose a classification of images produced in a nanoscience laboratory: primary images, secondary images and computational simulation images. Primary images are produced by instruments that acquire data that are then transduced by a specialized algorithm linked to a computer which in turn generates a topological or associated depiction of the object under investigation. The instruments which are used to obtain primary images are, in the present case study, the transmission electronic microscope, the scanning tunnelling microscope, the atomic force microscope, and so forth. Secondary images issue from the primary images and retain their foundational data. They require the introduction of a computer graphics program specialized in image processing. Computational simulation images represent computational output as form. Computation thus operates on two levels: it calculates physical phenomena and then, in a second phase, that output is numerically processed through algorithms and emerges as images. Each class of images fulfils different epistemic functions. In a second part of my talk, I will focus on the computational simulation images' functions: they can be used as an alternative to real experimental processes (because these are too expensive, time consuming or impossible to achieve); they can help to explain and to predict physical processes and ultimately, they may constitute an aid for decision making in case of controversial results produced by different instruments. In a third part of my talk, I will underline that such imaging practices entail their own sources of problems: for example, some computational simulation images may contain false information or lose some relevant information. One solution consists in comparing the computational simulation images to primary images or to secondary images. This strategy leads us to another problem: how can comparisons be made, and which types and degrees of similarity are needed between computational simulation images, primary or secondary images? Frequently, the compared images are both inserted in the final scientific publications. By doing so, the aim of the researchers is not to provide an absolute truth, but robustness – in other words, a convergent network of evidence.

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Parallel Session 1B

Wednesday, 24 June 2015 at 10:30–12:00 in G1

Session chairs: Jessica Carter (University of Southern Denmark); Christopher Pincock (Ohio State University)

Organized by: José Ferreirós (University of Sevilla); Jessica Carter (University of Southern Denmark); Henrik Kragh Sørensen (Aarhus University)

Symposium: Philosophy of Mathematical Practice

Synopsis. The Association for the Philosophy of Mathematical Practice (APMP) was founded 4 years ago with the aim of fostering "a broad outward-looking approach to the philosophy of mathematics which engages with mathematics in practice (including issues in history of mathematics, the applications of mathematics, cognitive science, etc.)". In view of the common interests with SPSP, it seems natural to strive for establishing links with your society and stimulating shared knowledge and common action. It is with that aim that the APMP as such wants to propose a Symposium in the context of the next SPSP meeting in Aarhus. We would like to display some of the work of our associates and to take advantage of their presence at the SPSP meeting to explore options for common work.

Thinking about topics that might best suit the interests of SPSP, it seemed rather obvious that questions about the relations between mathematics and different aspects of scientific practice would be ideal. Not just the issue of the applicability of mathematics (which tends to be ideology-laden from its very formulation) but more generally the relations –back and forth–between math and science.

The proposed Symposium is an outcome of that perspective. It consists of three papers, each to be presented in 30 min. The contributors have proposed topics that explore the variety of aspects from which the interaction science/mathematics can be explored – from Fourier series and their dual role/justification in mathematical physics and pure mathematics, to strategies of tuning in computer models as a central ingredient in contemporary science, to attempts to deepen understanding of relativity theory by means of a mathematical reinterpretation.

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Fourier Series as an Interface Between Mathematics and Physics

– Christopher Pincock (Ohio State University, chrispincock@gmail.com)

Abstract. A Fourier series is a means of representing a function as an infinite sum of trigonometric functions (sines and cosines). While such series appear sporadically in the eighteenth century, it is only with J. Fourier (1768–1830) that they become an object of theoretical study in their own right. Fourier deployed these series with masterful effect in The Analytical Theory of Heat (1822) to solve many of the outstanding problems posed by the heat equation. In this paper I will discuss some of the ways that the introduction of Fourier series changed the practice of mathematics and physics in the nineteenth century.

Historians of mathematics and physics have independently remarked on the revolutionary impact of Fourier's work. For mathematics, Ferraro has argued that the introduction of Fourier series was a major factor in the rejection of the "formal concept" of series that was central to the work of Euler. Once accepted as genuine objects of mathematics, Fourier series prompted many difficult questions about tests for convergence and other aspects of their rigorous application (Bottazzini 1986). Equally important claims have been made for the significance of Fourier's innovations for the development of physics. In their biography of Fourier, Dhombres and Robert (1998) present their subject as the "creator" of mathematical physics. One aspect of this creation is emphasized by Fox (1974) as well as Buchwald and Hong (2003): Fourier's successful work on the heat equation contributed to the downfall of the then dominant Laplacian approach to physics. On a Laplacian approach, the components of a mathematical representation must be motivated by a direct interpretation via "a microphysical explanation grounded in short-range forces" (Buchwald and Hong 2003). Fourier called this restrictive program into question by showing how his successful representations had only

an indirect interpretive significance. This opened the door to a more malleable program of relating sophisticated mathematical tools to complex physical systems.

I build on this historical work by arguing that these innovations in mathematics and physics are intimately connected. On my reconstruction, Fourier series achieved their initial legitimacy as mathematical entities largely due to their successful application to physical problems. This encouraged mathematicians to further refine and improve on Fourier's own somewhat vague pronouncements. As this process of rigorization continued, later generations of physicists were confidently able to extend Fourier's flexible approach to applications in new domains. I illustrate the main elements of this account through what has become a standard textbook problem: how deep should a wine cellar be so that it remains cool in the summer and warm in the winter? This case reveals one way that mathematical practice and scientific practice are coupled and how a success in one field can prompt important innovations in another field.

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❧

Strategies of Tuning. A New Look on Mathematization

- Johannes Lenhard (Bielefeld University, johannes.lenhard@uni-bielefeld.de)

Abstract. Mathematization has been identified as an essential ingredient in the development of modern science. Dijksterhuis (1961) or Koyré (1968, 1978) are thoughtful and standard historical references. According to such perspective, heroes like Galileo and Descartes established the viewpoint that mathematics and mathematically formulated laws provide an approach to the pertinent structures of phenomena. This line of reasoning – interconnecting mathematics, laws, and structures – is strong also in the philosophy of science.

I would like to present a quite different outlook on how mathematics is used as a tool in the sciences. I shall like to concentrate on a type of mathematical modeling strategies that is closely connected to using the computer as an instrument. More precisely, the issue in this talk is parameter tuning and its importance in mathematical practices.

Tuning is a well-known part of modeling and of building artifacts in general. However, it is mostly ignored in philosophical accounts, presumably because tuning counts as an ad hoc measure for counteracting minor shortcomings of the model – necessary, but insignificant. This view is inappropriate, especially when looking at computer-based modeling. There, tuning has become a central element. It is employed in systematic ways so that mathematical models can advance to fields that otherwise would not be amenable to mathematization. Instead of determining what would happen in a highly

idealized system, one wants to predict or manipulate the actual behavior of a certain system. Such behavior is usually influenced by a host of relevant, but not completely known, factors. My point is that mathematization is not prevented, but rather helps to deal with these situations. In particular, mathematical models allow for tuning.

Strategies of tuning will be discussed along the examples of chemical process engineering and modeling of atmospheric convection. Both examples critically hinge on computer-based tuning strategies that involve specifying good tuning parameters, and finding economically feasible feedback-loops for adjusting parameters in a model, or model-network. In both examples, the models contain errors and insufficiencies – which is the normal case in scientific practice, I think. Tuning a parameter according to the overall behavior of the model then means that the errors etc. get compensated (if in an opaque way). Tuning is a tool that utilizes the plasticity and adaptability of sub-models and their coupling, rather than their structure.

Based on the analysis of these cases, I want to defend the near-paradoxical – and hopefully controversial – claim that tuning is a mathematical practice for working with inconsistent models. Models can be called inconsistent, because they have no common theoretical framework. They build a façade in the sense of Wilson (2006) whose apparent consistency only emerges in the course of tuning. Thus mathematics is not restricted to consistent formal systems, quite the opposite is the case: It serves as a tool for dealing with inconsistent parts and their complex interactions. Else it would be much less relevant in contemporary sciences.

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❧

Representations and Understanding in Mathematics

- Jessica Carter (Department of Mathematics and Computer Science, University of Southern Denmark, jessica@imada.sdu.dk)

Abstract. In the first part, I shall use Peirce's semiotics, in particular his notions of icons and indices, in order to describe how it is possible to handle complex mathematical proofs or expressions. This will be illustrated by a result from contemporary mathematics, where the value of a complex expression is found by gradually breaking it down into simpler expressions. I claim that we handle proofs by using an interplay between different kinds of representations. One role, that these representations play, is to enable us to break down proofs into manageable parts and thus to focus on certain details of a proof, by removing irrelevant information (Carter 2010). The role of icons and indices in this process will be explained. Put briefly the role of icons, signs that represent because of likeness, is to ensure that there is likeness between the parts when the expression is broken down, and the role of the index, acting as a signpost, ensures that the parts may be reassembled in the end.

The second part will link the above description to a notion of mathematical understanding. There are many ways to characterise understanding. In this paper I will only consider one, that is, where the motive given for understanding a certain subject matter is to further development in that subject. In this sense understanding is linked to fruitfulness. I shall take understanding to be characterised by finding ways to:

1. Handle a field/subject matter – given our cognitive set-up – in order to
2. Reveal structure of the subject matter.

These descriptions will be regarded in view of the above process of breaking down a proof into manageable parts and in relation to Peirce's characterisation of icons.

Parallel Session 1C

Wednesday, 24 June 2015 at 10:30–12:00 in G2

Session chair: Sjoerd D. Zwart (Delft University of Technology)

❧

Cognitive Constraints, Complexity And Model-building

- Miles MacLeod (University of Helsinki)
- Nancy Nersessian (Harvard University)

Abstract. Models and modeling have pride of place in contemporary philosophy of science. However, in discussion of modeling methods and choices scant attention is paid to the way in which these are motivated in practice by particular cognitive constraints. Discussions on model-based reasoning that track the role of mental models, visualization and analogy in the model-building process provide notable exceptions (see Nersessian, Thagard, Giere, Magnani). In part, the neglect of cognitive constraints in philosophy stems from a lack of understanding of the intricate role these and other cognitive processes play in reducing complexity during model construction. In cases where researchers are up against significant complexity without well-structured theoretical or methodological protocols for handling it these processes carry a significant load. Satisficing limits the kinds of epistemic goals and aims researchers take on, promotes abstraction and idealization techniques, as well as promoting the distribution of cognition where possible to computational simulation and technologies.

In this talk we provide a case study from integrative systems biology using data from our 4 year ethnographic study of model-building practices in two systems biology labs. Complexity is a dominant concern for modelers in systems biology for a variety of reasons, not least the actual complexity of the biological systems they address. We show that distributed model-based reasoning plays a central role in the model-building process and researchers consistently rely on mental modeling to infer network structure and simplify their problems in order to make them tractable. Cognitive constraints such as working memory constraints may well limit the scale of networks that can be tackled and the epistemic goals that can be formulated with respect to them. Researchers in these field in fact acknowledge that cognition plays a decisive role in what they can and can't do, and in turn the need to formulate cognitively manageable problem-solving strategies. One instance of this is the strategy of mesoscopic modeling (Voit et al., 2012, Voit, 2013). Mesoscopic modeling works to simplify and abstract both higher systems-level and lower molecular-level relationships in the form of models of medium size. This enables researchers in practice to work with relatively simplified systems that facilitate the inferences and model-based reasoning they need to produce accurate system models. However with these models in place, hierarchical learning can be applied through experimenting and simulating a model to understand important causal relations within the system and more reliably expand and correct these models despite the complexity of the systems. Given the importance of such strategies for handling complex systems, we have good grounds for asserting the strong role cognitive constraints play in methodological choice in this context.

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The Role of Interactions in Determining Biological Systems Structure and Function

- Mihaela Pavlicev (University of Cincinnati Medical School)
- Robert Richardson (University of Cincinnati)

Abstract. Systems biology encountered a recent boost with extension of technologies that are able to efficiently generate and handle large amounts of reliable data. However in theoretical sciences, the questions of complex systems' structure and behavior, of their functionality and adaptability in the face of large number of interacting parts, has long been central. Integrating the newly available data with this knowledge is essential in order to facilitate the questions asked on data on the one side, as well as to meaningfully focus the development of theory. While integration of theory and data is an agreed upon goal, it is by no means a trivial task.

The common idea of systemic approaches is that systems are more than the sum of their parts and hence cannot be deduced from information about the latter. This emergent quality is often attributed to specific relationships or interactions between parts, and between parts and environment, which only come into view when the system is considered as whole, or in different contexts. The rationale behind such description is that these relationships structure the system in a constructive way that constrains how parts contribute to a common function (e.g. hierarchy or modularity are such structures). Yet what are the specific features of interactions that contribute to emergent system behavior?

Systems biology shares the problem of complexity with social studies, as well as ecology. In addition to evolutionary genetics, we draw on insights from theoretical and experimental work in social dynamics and ecological systems with the aim to identify the properties of interactions that are at the center of complex systems function. Our ultimate goal is to determine the character of pivotal interactions. This will enable us to develop an approach to identify the pivotal interactions in systems-biological problems.

❧

Systems Medicine: Visions and Controversies

- Sara Green (Department of Science Education, University of Copenhagen)

Abstract. Developments within the life sciences increasingly reflect the need for new interdisciplinary strategies to deal with the 'grand challenges' in society. An example is the current expansion of systems biology to systems medicine: a biomedical research field that aims to provide a better understanding of complex diseases and to account for variation among individual patients by developing personalized models of 'digital patients'. The proponents envision that computational integration of new data types will revolutionize biomedical research and health practice through prediction and prevention of a number of diseases. Yet, the emerging literature on systems medicine reveals strong disagreements on the best way to advance systems medicine research and on the wider implications of these strategies for science and society. This paper analyzes the basis for these disagreements and examines the methodological and theoretical challenges highlighted by those skeptical of the strategies of systems medicine.

I focus in particular on challenges of integrating models and data in comprehensible large-scale models, and the associated controversies regarding the role of genomics for understanding complex diseases. The development of systems medicine, as it is currently conceived, is dependent on access to and collection of new data types such as whole-genome sequencing and continuous measurement of disease-related variables such as blood pressure, heart rate, blood sugar levels, protein markers etc. Accordingly, data-collection and model development will be conducted by different groups of basic science researchers, clinicians, practicing health service personnel, and individual patients. Patients are expected to actively engage in data collection as 'consumers' of health-technologies for self-monitoring of health-relevant information. The integration of scientific research and disease-preventing health strategies is a potent resource for understanding disease from a perspective that

accounts for interdependencies between different diseases and for conditions specific for individual patients. But several researchers have raised concerns about the possibility of turning the vast amount of information into medically useful models. While some see genomics as a powerful tool for a new medicine, tailored to individuals with specific genetic profiles, others argue that making sense of the effects of gene mutations requires a better understanding of the organization of higher levels such as tissues and the whole human body. I demonstrate how such disagreements are often rooted in different views on the ontology of diseases, and in differences in epistemic standards for modeling. For instance, it is currently debated whether cancer is a cell-based disease (caused by mutations) or a tissue-based disease (caused by failure of higher-level organization). Furthermore, it is unclear how patient-specific information can be adequately related to relevant reference classes and to what extent large-scale models will provide evidence needed for health professional to make an informed decision. Analyzing the debate on the prospects of systems medicine, I examine how scientific disagreements relate to different epistemic and ontological standards for how we may best approach and perceive the functioning and malfunctioning of living systems.

Parallel Session 1D

Wednesday, 24 June 2015 at 10:30–12:00 in G3

Session chair: Timothy Tambassi (Università del Piemonte Orientale)

❧

On the Emergence of a Prediction Culture in Climate Modeling

– Matthias Heymann (Aarhus University)

Abstract. Computer simulation was adopted quickly in the atmospheric sciences since the early 1950s. Successes in weather and climate simulation increased the attraction and authority of this approach. Even though Edward Lorenz pointed out the sensitivity of simulation models on initial conditions in his famous paper of 1963, the inherent problems of sensitivity and uncertainty did not hamper atmospheric scientists to expand computer models and computer simulation as a valuable new scientific practice and to base long-term projections and warnings on them. The problem of climate change is a case in point. Climate simulation gained increased attention since the early 1960s. A growing number of groups, first in the USA, later also in Australia and Europe, engaged in the development and use of climate models. In various official reports the issue and potential risk of climate change was discussed. In 1979, based on the results of climate simulation atmospheric scientists reached agreement that climate change "on a regional and global scale may be detectable before the end of this century and become significant before the middle of the next century" (WMO, 1979, p. 714). The so-called Charney Report of the U.S. National Academy of Science came to similar conclusions in the same year. Already at this point, climate modeling had become a dominant resource for the production of predictive knowledge about climate.

Underlying the dominance of climate modelling and its uses in the production of predictive climate knowledge are fundamental decisions about which types of knowledge are important, which epistemic standards are used to judge that knowledge, and which applications of that knowledge are regarded as useful and socially relevant. First, interests in climate (including research questions and methodologies, types of climatic knowledge, and knowledge production) were more diverse than the present dominance of modelling suggests. Second, Climate models initially served heuristic purposes to investigate and better understand atmospheric processes. Only in the 1970s, a new generation of climate modellers pushed the development of climate models for the long-term prediction of global warming, an endeavour which has been successful but which was initially controversial. This shift from heuristic to predictive climate modelling involved new presumptions, interests, and epistemic standards which emerged and stabilized in specific historical and cultural contexts. Predictive modelling did not only entail different applications of models and different uses of modelling results, it

involved different priorities and research tasks as well as different research practices and strategies. The production of predictive knowledge required a pooling of resources to problems defined by this ultimate goal. Theory needed to be developed and adjusted for the specific scope of prediction, scientific problems needed to be prioritized and those problems estimated less fundamental for meaningful prediction relegated to later treatment, models needed to be adjusted to the goal of long term prediction and relevant data needed to be collected and processed accordingly.

Based on examples from the USA, the UK and Germany, this paper aims at investigating how and why a new culture of climate prediction emerged in the course of the 1970s. It will explore the strong interaction of scientific and political interests, which paved the way for an application of climate models for predictive use, and which supported a consensual framing of climate change and a partial merging of scientific and political agendas. It seeks to provide answers to questions such as: How and why did predictive modeling gain acceptance? To what extent was this pushed by scientific and political interest? Which practices did predictive modelling entail? Which controversies did it cause within the climate modelling community? The paper will show that climate modelling strategies and practices – even though based on the same basic principles – differed considerably in different countries and in different modelling groups. It will illuminate differences of interests, perceptions and practices within the climate modeling community. The paper will be based on historical research and contribute to the philosophical understanding of the shaping scientific practice in climate modelling.

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Uncertainty, Non-epistemic Values and the Intergovernmental Panel on Climate Change

– Erin Nash (Centre for Humanities Engaging Science and Society at Durham University)

Abstract. In 'The social epistemology of the Intergovernmental Panel on Climate Change' (forthcoming in the Journal of Applied Philosophy), Stephen John argues that due to the ineliminable issue of inductive risk, there are circumstances in which peoples' ex-ante political commitments can actually provide them with good reasons to not defer to scientific testimony. John states that it is possible that one's political commitments might be such that she adopts an extremely high epistemic standard for accepting any claims about climate change (ie. she would demand more evidence in order to accept the scientists' assertions). This includes the science of Intergovernmental Panel on Climate Change (IPCC), although John believes that such cases of non-deferral would be very unlikely, due to the high epistemic standards used by the IPCC: "…it remains "conceivable that non-experts' political commitments might still provide them with good reason to fail to defer to the IPCC's testimony" but "…such cases are likely to be extremely rare".

Despite opening up this space in the 'value-free ideal' debate, John does not provide an account of the criteria that would need to be satisfied in order for such cases, however rare, to be recognised as well-founded and principled. In this paper I will provide a preliminary account of such criteria, closing this loophole, which I am concerned can be easily exploited.

In addition, I will also argue (contra John) that there are certain circumstances under which one cannot appeal in any way to their non-epistemic values as good reasons not to defer to scientific testimony. I highlight two claims the IPCC make which exhibit such conditions and explain why.

Parallel Session 1E

Wednesday, 24 June 2015 at 10:30–12:00 in G4

Session chair: Leen De Vreese (Ghent University)

❧

Cancer: From One to Multiple Diseases

- Gry Oftedal (University of Oslo)
- Anders Strand (University of Oslo)

Abstract. Diseases are increasingly researched on the molecular level as well as redescribed and explained by way of molecular mechanisms. One result is that what before was considered one or a few diseases are split into a multitude of diseases or disease subtypes in a fine-grained fashion according to molecular variations, mutations, or gene expressions.

Cancer is traditionally classified according to actual tumor site, or to the site of cancer origin. In contrast, there is a trend in contemporary cancer research of categorizing cancer types and subtypes in terms of genetic mutations, receptor types or other molecular level features. There is an ambition to understand and explain cancer development in a more fine-grained manner, and to develop new and better cancer treatments.

We investigate this change in description and understanding of cancer with an attention to the separation between treatment focus (the goal to treat cancer) and explanatory focus (the goal to find and understand cancer causing mechanisms). We shed light on how these different angles, although they often go hand in hand, may give rise to different takes on cancer categorization where the latter is more substantial and the former more instrumental than the other. We discuss how categorizations of cancer subtypes often are based on the possibility of diagnosis and intervention which may or may not overlap with the mechanism of disease development.

Targeted cancer therapies may intervene directly in disease mechanisms, they may intervene via a disease cause without intervening directly in the mechanism in which this cause plays a role, or they may intervene via a factor not directly related to the disease process at all.

In research on nano-carriers, the more relevant situation seems to be the second case, where a causal factor in cancer development is used as a handle (to attach the carrier to the cell) but where the drug works on a relatively coarse level by killing the cell when it divides. It will vary to what degree the causal factor used as a handle is implied in cancer development (and this does not really matter), and although differences between cells when it comes to such handles represent actual differences between cancer cells, these differences may or may not correspond to differences in developmental history and prognostic relevance of the different cells.

In principle, variation in biomarkers can be disconnected from the cancer causing mechanisms in the various cells and clones due to effects of passenger mutations. On this background, we argue that molecular categorization of new cancers or cancer subtypes based on genetic variation and thereby variation in targetable ligands and biomarkers, is mainly instrumental in the sense of helping diagnosis and treatment, and is not necessarily based on features explaining or reflecting the origin and development of the disease.

❧

Psychiatric Classification Between Science and Practice

– Anke Bueter (Leibniz University Hannover)

Abstract. The current classification system in psychiatry (as exemplified by the DSM) exhibits severe problems, and its recent revision, culminating in the DSM-5, has left many disappointed. On the one hand, there are controversial debates on the criteria for individual diagnoses and the question, whether they pathologize normal feelings and behavior, for example in the cases of ADHD or depression. On the other hand, there are also numerous critics who question the overall system. It is often argued that the heterogeneity of groups picked out by the DSM's polythetic criteria, the excessive rates of comorbidity, and the lack of predictive success of the DSM diagnoses indicate a severe lack of validity. This lack is commonly attributed to the DSM's phenomenological approach to classification, the current system being based on co-occuring, observable symptoms. The main proposal for improving the situation is to change the classification from a phenomenology-based one to an etiology-based one that groups symptoms according to our best scientific theories about their underlying causes. Proponents of such an etiological revolution often present it as a move forward towards a more scientific, evidence-based nosology. Even more cautious criticisms often seem to assume that the change towards a more valid etiological classification is only a matter of time, awaiting further research results.

What I want to show is, first, that the question of the classificatory basis is not one that can be answered by empirical evidence alone. Instead, it requires judgments on what level of evidence is needed to justify changes as well as judgments on what kind of evidence is most important. Second, in making these judgments we need to weigh the needs of clinical practice and scientific research.

Regarding the question of how much evidence is enough to legitimate a more radical revision, it is important to note the DSM's multiple purposes. While it aims to be a suitable basis for research, it also thoroughly shapes psychiatric practice. Changes can be very consequential in that they affect patient's diagnoses and possibly treatment, might impact questions of reimbursement, and even change public views of mental disorder and normality. Therefore, before one starts a revolution, there should be solid evidence that this will improve the situation in terms of science as well as health care. What exactly that means is moreover not a purely scientific question but calls for value-judgments on the weighing of inductive risks and consequences of possible errors.

In consequence, the needs of clinical practice and of scientific research do at present stand in conflict with each other. While the former calls for a conservative approach and high standards on evidence before every radical change, the DSM's problems as a basis of scientific research are indeed severe and call for pluralistic explorations of possible alternatives and causal explanations. The central difficulty in psychiatric classification is, accordingly, not just a lack of validity or a lack of evidence, but lies in trade-offs between the different demands of research and practice.

❧

Inductive Risk, Epistemic Risk, and Overdiagnosis of Disease

– Justin Biddle (Georgia Institute of Technology)

Abstract. Philosophers interested in the role of values in science have focused much attention on the argument from inductive risk. In the 1950s and 1960s, a number of authors argued that value judgments play an ineliminable role in the acceptance or rejection of hypotheses (Hempel 1965; Rudner 1953). No hypothesis is ever verified with certainty, and so a decision to accept or reject a hypothesis depends upon whether the evidence is sufficiently strong. But whether the evidence is sufficiently strong depends upon the consequences (including ethical consequences) of making a mistake in accepting or rejecting the hypothesis. Recent philosophers of science have not only revived this argument; they have also extended it. While Rudner and Hempel focused on one point in the appraisal process where there is inductive risk – namely, the decision of how much evidence is enough to accept or reject a hypothesis – more recent philosophers of science have argued that there

is inductive risk at multiple points in the research process. Douglas (2000) argues that inductive risk is present in the choice of methodology (e.g., the setting of a level of statistical significance), the characterization of evidence, and the interpretation of data. Wilholt (2009) argues that there is inductive risk in the choice of model organism. The upshot of these and other extensions of the Hempelian/Rudnerian argument from inductive risk – which I will call the classical argument from inductive risk – is, again, that the research process is shot through with inductive risk. Indeed, it can seem that, as a result of these extensions, there is inductive risk at any point in the research process at which a decision must be made.

While I applaud the extensions of the classical argument from inductive risk, and while I think that they provide valuable insights into the ways in which value judgments operate in the appraisal of research, I will argue that some of the purported extensions of the classical argument do not fit cleanly within the schema of the original argument and that, for the sake of conceptual clarity, they should simply be treated as different arguments. I will discuss the growing problem of overdiagnosis of disease due to expanded disease definitions in order to show that there are some risks in the research process that are important – and that should be taken seriously by philosophers of science – that very clearly fall outside of the domain of inductive risk. Finally, I will introduce the notion of epistemic risk as a means of characterizing such risks. This more fine-grained taxonomy of risks in the research process will help to clarify the different roles that values can play in science.

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Parallel Session 1F

Wednesday, 24 June 2015 at 10:30–12:00 in Koll G

Session chair: Hasok Chang (University of Cambridge)

❧

Death of the Scientific Author, Contributors Too

- Barton Moffatt (Mississippi State University)

Abstract. Authorship is a key concept in the system of scientific communication. It is the primary vehicle for determining professional credit and reward for scientific achievement. Authors are also the people held accountable when problems are found in research. There is growing concern about unethical authorship practices across all research domains, and professional associations and journal editors are creating and revising explicit authorship policies to guide researchers. Much of the difficulties surrounding authorship are due to the fact that authorship is both used to communicate within scientific communities and to serve as markers of productivity outside of those research communities. Often these functions are at cross-purposes.

Some scholars have proposed moving to a system that lists everyone's contributions to a research project on each paper produced. There are several different contributorship proposals. In one, each author lists his or her contributions. In another, everyone's contributions are listed even if they

are not listed as authors, and still others propose to get rid of authors altogether, merely listing contributions. Many journals are now requiring that all authors list their contributions. The problem is that there is no evidence that requiring contributor listings has eliminated unethical authorship practices. Is it worth making researchers take extra time to specify contributions if they are not adding value by reducing undesirable behavior?

One reason why authorship guidelines are failing to achieve their moral purpose is that there is not a clear understanding of the pressures that lead to bad authorship practices. There is little discussion of the role authorship plays in the political economy of the research laboratory and whether the concept of authorship is still suited to play this role. Part of the problem is the mismatch between authorship and necessary scientific roles like getting research funding and doing the scut work. Many accounts of authorship do not grant authorship for those types of contributions, creating a situation where necessary jobs are unrewarded. In this paper, I argue that it is time to replace the category of author in favor of a system that allocates three types of credit: intellectual, labor and funding. Eliminating the category of author in favor of a credit model would have two main advantages. First, it would allow for credit to be given for different types of contribution in a way that is in accord with scientific practices. Second, it would be an improvement over the contributorship proposals because it allows each research group to define who should get credit on a project, which reduces the possibility of non-scientific institutions and actors not properly valuing research contributions.

❧

Privacy, Informed Consent, and Participant Observation

– Julie Zahle (University of Copenhagen)

Abstract. One the one hand, it is widely agreed that the social researcher must make sure to respect the privacy of the individuals who form part of her study. On the other hand, not much attention has been paid to the notion of privacy in a research context and to the question of how exactly the researcher may ensure that she does not invade the privacy of the individuals she studies.

In this paper, I am concerned with the proposal that the social researcher may use the device of informed consent to ensure that the privacy of the individuals she studies is not invaded when she generates data about them. This suggestion is rarely examined in any detail. I do so while focusing on the social researcher who produces her data by carrying out participant observation. I argue that the use of informed consent does not suffice to protect individuals' privacy: When carrying out participant observation, the social researcher must take further measures in order not to violate individuals' privacy.

The paper falls in two parts. In part I, I distinguish between two forms of privacy, situational and informational privacy, and explicate how the social researcher may invade both these forms of privacy when carrying out participant observation. Further, I explain the idea of informed consent and show how it works to protect individuals' situational and informational privacy. Finally, I point to some difficulties that the administration of informed consent, as a privacy protection device, may run into.

In part II, I examine the practice of carrying out participant observation with a view to determining whether the use of informed consent suffices to protect individuals' situational and informational privacy. I point to three situations that very commonly occur once the social researcher has obtained the research participants' informed consent and started carrying out participant observation:

(a) The social researcher changes, or considerably narrows down, her research question as she goes along producing her data.
(b) The social researcher produces data in situations, or collects information about matters, that do not obviously fall within the scope of the informed consent.
(c) The research participants get used to the social researcher's presence, or they get to know her better and start trusting her with the result that they sometimes forget, or do not take it, that the social researcher is (also) producing data for her research.

In these common types of situation, I argue, having earlier obtained the research participants' informed consent does not preclude their situational and/or informational privacy from being invaded.

At the same time, I explicate what further measures the social researcher needs to take in order to ensure that she respects the research participants' privacy. More specifically, I outline how she should make sure, in all three types of situation, that the individuals in question implicitly or explicitly accept her research activities.

❧

Facing Animals

– Sophia Efstathiou (Philosophy and Religious Studies, Norwegian University of Science and Technology, NTNU, sophia.efstathiou@ntnu.no)

Abstract. This paper explores animal research practices using the phenomenological notions of the 'face' and 'animality'. Based on my ethnographic study of laboratory research using rat models, I argue that relating animal model results to humans relies not only on rational grounds to anthropomorphise those rats but also on the felt experience of the rats as animals with faces. Whether or not we want to "know" animals by looking at their faces, the possibility of so doing is implicated in our quest for knowledge through animal models. This makes our responsibilities to them as others with faces hard to escape, as Emanual Levinas discusses for the case of humans (Guenther 2007). Yet at the same time facing animals is managed through various, what I call, 'technologies of effacement' including laboratory attire, handling techniques, and processing rituals.

Following Merleau-Ponty, David Morris (2007) proposes that 'animal faces' are of special significance in the world as experienced by us, using our onto-logic as animals. Reading faces lets us recognize how we-each-other 'are', opening up a realm of invisible, mental or emotional 'being' to the realm of the visible, physical being. Faces are visible surfaces communicating what is internal or invisible. They are special surfaces that can manifest something inferred from the realm of the invisible. Morris juxtaposes looking at faces with looking at the internal workings or organs: even if we imagined ourselves having transparent skins, making all our internal processes seen, we would still need to look in each-others' faces to say how we 'are'.

I argue that knowing through animal research is inextricably tied up with the possibility of knowing animals through their 'face': understood as the surfaces facing us – faces and bodies. Experimental design in animal studies negotiates between conceiving of the animals as "faceless" expendable, laboratory material, bought, quality checked and discarded once used, and as animals we "face", whose behaviours, pains and bodies we relate to ours. Even if experiments focus on aspects of an animal other than its face, animal experimentation involves encounters with the animals as others with faces and knowing the animals through their faces.

I examine how researchers negotiate the dual role of the animal as faceless and as an other with a face. Specifically I focus on the sacrifice stage of an osteoporosis study using ovariectomised rat models. During sacrifices the living animals waiting to be sacrificed are kept at a distance from the animal being sacrificed to prevent them from smelling the blood on the surgical table and becoming agitated. Manifestly it is the other animals that are to be protected through this decision. However it arguably keeping the living animals at a distance prevents human researchers from facing those animals. My results show that researchers get on with work more easily once they focus on their specific tasks, while emotions are harder to control when looking at the animals waiting to be sacrificed. Keeping living animals apart from animals under operation also means keeping humans from facing these other animals.

Animal experimentation aims to get at humanly relevant answers. However the categorical and felt alignment between rats and humans as animals with faces will by default raise ethical questions.

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Parallel Session 2A

Wednesday, 24 June 2015 at 13:30–15:00 in Aud F

Session chair: Marcel Boumans (University of Amsterdam and Erasmus University Rotterdam)

Organized by: Michiru Nagatsu (University of Helsinki)

Symposium: Critical Perspectives on the Practice of Evidence-Based Behavioral Public Policy

Synopsis. Critical perspectives on the practice of evidence-based behavioral public policy: in this symposium, we critically examine different aspects of evidence-based behavioral public policy. The three papers provide theoretical, methodological, and practical analyses of the popular evidence-based behavioral policy in both poor and rich countries, drawing on actual practice of experimental, behavioral and development economists, as well as cognitive psychologists and policy makers. The papers also offer several concrete proposals to improve scientific practice and its applications.

❧

Behaviorally Informed Policy and Decision Theory

– Jaakko Kuorikoski (University of Helsinki)
– Samuli Pöyhönen (University of Helsinki)

Abstract. Nudge and boost are two proposals to improve social outcomes by influencing individual decisions. We argue that these approaches offer a limited basis for behaviourally informed policy because of their focus on individual behavior. This is problematic, because (1) with some well-known exceptions, individual rationality is neither necessary nor sufficient for aggregate rationality; and (2) social outcomes typically involve complex interactions between agents ignored by nudge, boost and their respective theoretical foundations in the psychological research on individual decision-making..

The behavioral sciences used to gather evidence for behavioral policies need to pay more attention to meso-level social coordination and to the effectiveness of possible interventions aimed at influencing it. Nudge and boost must be seen as complementary instruments to what we call design interventions targeted directly at institutions and social structures.

Why has the debate between nudgers and boosters been framed so as to leave out the possibilities of intervening on the rules of interaction (market and norm design)? Moreover, the debate completely ignores a vast body of empirical studies on the effectiveness of different techniques of behavior change relating to a variety of social and health issues. We argue that these oversights are due to problems in transferring knowledge over disciplinary boundaries.

It is not surprising that both nudge and boost have more or less ignored system-level effects, since both are rooted in experimental psychology of individual decision making, and the debate between them has been framed as being fundamentally about the scope of decision theory. Prospect theory underlying many of the nudge policies is an attempt to tie economics back to psychology according to the picture of a direct bottom-up interface between psychology and economics, the individual and the systemic. This compatibility of prospect theory with economics has probably been one of the principle reasons why nudge policies have been so easily accepted: there is an existing conceptual slot into which deviations and biases, and their implied nudges, can be inserted. However, this convenient interface also leads naturally to the assumption that correcting deviations in rationality at the individual level leads automatically to improvements at the system-level.

In contrast, advocates of boost attack the core ideas of decision theory in claiming that it represents a wrongheaded picture of (individual) rationality and consequently leads to ineffective (and morally suspect) policy recommendations. Fast and frugal heuristics aim to provide a more psychologically plausible picture of processes of rational decision-making in a complex world. However, theory of fast-and-frugal heuristics does not offer any systematic theory for reasoning about the system-level effects of boosts. This lack of a clear interface between fast and frugal heuristics and existing economic models has likely been a major factor hindering the adoption of the boost approach among

social scientists and policy makers. Furthermore, the missing shared theoretical ground between research on behavior change in social psychology (see. e.g Michie et al.: Behavior change wheel) and the decision-theoretic focus of nudgers and boosters explains the divided state of behaviorally informed policy research. We argue that both parties to the debate would benefit by treating decision theory more as a language in which to reason about social behavior, rather than as a substantial explanatory theory of individual decision making that one should either defend or attack on empirical or normative grounds.

❧

Boosts Versus Nudges: How to Pick the Right Policy Tool

– Markus Feufel (Charité University Hospital)
– Till Grüne-Yanoff (Royal Institute of Technology)
– Caterina Marchionni (University of Helsinki)

Abstract. In recent years development economics has undergone an "empirical turn" (Angrist and Pischke, 2010), namely the extensive use of randomized field experiments (RFE) to produce evidence. Abhijit Banerjee and Esther Duflo at the Jameel Abdul Latif Poverty Action Lab (J-PAL) are the two leaders of this movement. They characterize J-PAL's approach as a "new development economics" (Banerjee, 2005) based on the unique use of RFEs in order to (1) produce evidence on the effectiveness of development programs and then (2) use these evidence to guide policy makers.

Although the "new development economics" claims to be "theory-free", its practice is largely informed and guided by the framework of behavioral economics: the J-PAL's researchers focus on the behaviors of the poor, assuming that the poor, like the rich, suffer from various cognitive biases that hinder rational decision making, thereby keeping them trapped in poverty. RFEs are thus designed mainly to assess the power of different "nudging" devices to counteract these cognitive biases. We show how this approach operates in practice by examining a paradigmatic RFE study of this type, Pascaline Dupas' experiments on measures to increase the use of bednets to fight malaria (one of the main causes of death in developing countries). We then argue that the continuous failure of the series of her experiments to find any effective nudge to change the behavior of the poor is partly but importantly due to the individualistic perspective on decision making, which the study inherits from behavioral economics. That is, the practice of allegedly "theory-free" RFEs in fact suffers empirically from the implicit theoretical perspective that takes as the main explanatory/causal factor individual decision making in isolation from interactive, social and institutional contexts.

The failure of Dupas' experiments and the unfulfilled promise of the "new development economics" more generally suggest that the evidence-based development economics movement may gain from shifting the focus from isolated individual decision makers to aggregate choices in social and institutional contexts. There are different ways of implementing this perspective shift, such as interdisciplinary collaborations with non-experimental social scientists such as anthropologists and area studies researchers to better understand the contexts in which the poor make decisions. Here we propose an alternative, i.e. to reconsider experimental practice in behavioral economics upon which the new development economics is built. Our proposal is motivated by the new practice called "experimetrics" (Bardsley and Moffatt 2007) or "behavioral econometrics" (Andersen et al. 2010), which adopts econometric techniques to explicitly model heterogeneity of data generating processes in the population. This opens up a way to understand how interactions of people with different beliefs and preferences result in aggregate results, and how the same individuals change behavior from one context to another. These are key knowledge to effective policy interventions, which however RFEs have failed to provide so far. We propose that the proponents of RFEs drop the rhetorical emphasis on its "evidence-based" nature and shift the individualistic perspective on poverty, which, upon careful examination of different strands of experimental practice, we argue, turns out to be not only unnecessary but unsatisfactory as a guiding tool for policy.

❧

What's Wrong With Evidence-Based Approach?: "New Development Economics" and Behavioral Development Economics

– Judith Favereau (University of Helsinki)
– Michiru Nagatsu (University of Helsinki)

Abstract. Recent theoretical efforts in modelling boundedly rational agents are having considerable impact on policy making, leading to the design of interventions that take into account the cognitive limitations of decision makers. Yet considerations of bounded rationality have not led to a uniform kind of public policy proposal. In this paper, we distinguish two kinds of policy proposals, nudges and boosts, which arise from theories of bounded rationality, and provide a framework that allows assessing, in a given context, which policy type is more likely to achieve a specific goal.

Nudges are inspired by the heuristics and biases approach championed by Kahneman, Tversky and others, whereas boosts are promoted by the ecological rationality paradigm of Gigerenzer and colleagues. According to the heuristics and biases program, while heuristics may lead to good decisions and behaviours in some circumstances, often they must be regarded as irrational from the normative viewpoint of probability theory. Nudges constitute interventions on the decision environment that circumvent the limiting effect of cognitive biases by facilitating desirable, and deterring undesirable, behaviours. For example, it is assumed that many people would wish to save more for their retirement if they seriously considered their needs at an older age. However, due to an alleged mixture of presence-bias, inertia and visceral influences, they often fail to do so. Consequently, nudge proponents have suggested changing the retirement savings default for new employment contracts, which assume that employees make a high monthly savings contribution, unless they actively choose against it.

By contrast, the ecological rationality program argues that the rationality of heuristics depends on their fit with the decision environment. For instance, if a heuristic is biased to process only some but ignore other cues (frugality), it is regarded as ecologically rational if the few processed cues are valid predictors of a desirable criterion while unprocessed cues do not increase the heuristic's predictive power. Consequently, boosts aim at aligning the decision environment and the kinds of heuristics people use by changing the decision environment and/or by extending people's heuristic toolbox. For instance, fast and frugal decision trees may be designed to match the cue structure of an environment and evaluated against alternative algorithms. If they show improved performance over current practice, they should be taught to decision makers.

In this paper we leave aside theoretical questions regarding whether the two kinds of intervention follow directly and exclusively from the competing theoretical approaches, as well as normative considerations about their legitimacy and desirability. Instead, we treat them as complementary policy tools and assess the conditions under which they are more likely to achieve a specific goal in a given context. We argue that the effectiveness of nudges and boosts hangs on several dimensions and thereby identify the kind of evidence needed to establish it. Our approach provides a common framework in which to evaluate the applied value of two popular concepts of bounded rationality and point to ways in which some of the disputes about nudges and boosts may be settled empirically.

Parallel Session 2B

Wednesday, 24 June 2015 at 13:30–15:00 in G1

Session chair: Mieke Boon (University of Twente)

❧

Toward a Tool for Supporting Interdisciplinary Research Teams in Developing Shared Ontologies

– Julie Mennes (UGent)

Abstract. Today, scientific research projects often can be characterized as 'interdisciplinary projects', i.e. projects that integrate knowledge and know-how from different (sub-)disciplines. However, interdisciplinarity comes with all kinds of problems. This paper addresses frequently reported problems related to interdisciplinary communication. Consider, for example, the following observation by a cultural and a physical geographer:

"Given different epistemological approaches of different disciplines, finding a way to work together to produce a coherent output is exceedingly difficult, not least because disciplines have developed their own specific technical languages reflecting these differences." (Jones & McDonald 2007, p. 491)

One cause of impeded interdisciplinary communication is terminological ambiguity, i.e. within the context of an interdisciplinary project, some terms are shared by multiple disciplinary jargons, yet it is unknown whether the concept behind each them is the same in all jargons. In other words, interdisciplinarians might be unknowingly using different ontologies, thereby inhibiting clear and efficient communication. As interdisciplinarians already experience heavy workloads and generally are not trained to deal with communication problems, they would clearly benefit from using a tool that enables to deal with terminological differences. What follows is the outline of such a tool.

The tool has two goals: (i) to identify terminological ambiguities, and (ii) to resolve these ambiguities. The first goal entails three subgoals. First, the shared terms are to be identified. Next, it should be checked whether the concepts underlying shared terms vary across the source disciplines. Finally, the differences between the underlying concepts need to be articulated in detail.

To reach the first goal, the tool makes use of natural language processing techniques. For the first subgoal, term extraction software is used to generate lists of key terms for each disciplinary jargon whereupon the set of shared terms is determined. To reach the second subgoal, the tool starts from Harris' hypothesis, stating that words occurring in similar contexts have the same underlying concept, where 'context' is understood as a number of words before and after a term (1968). The different concepts underlying a shared term are determined by checking the concordance with other terms (with underlying concepts) by means of statistical filtering. The third subgoal requires representing all concepts underlying a shared term and comparing them to determine the differences. Based on the identified contexts of shared terms, representations are generated using a combination of (i) Thagard's diagrams containing kind and part-whole relations between concepts (1992), (ii) Kuhn's relations of similarity and dissimilarity (1977), and (iii) Chen and Barker's work on attributes and values of concepts (2000).

The second goal comes down to developing a new ontology in which every shared term has only one underlying concept which integrates the former variety of underlying concepts in such a way that it both accommodates the needs of the project and remains acceptable for all of the involved researchers. To reach this goal, the generated representations will be used to set up a moderated group discussion.

❧

Boundary Work: Nanoscience Meets Philosophy

– Julia Bursten (University of Pittsburgh)

Abstract. Nanoscience is an inherently interdisciplinary field of study. Because it developed around a scale, rather than a set of laws or phenomena, it invites research programs from fields as diverse as materials science, biology, physics, chemistry, engineering, and design. For instance, gold nano-cubes are synthesized and characterized by chemists and physicists; modeled on computers by mechanical engineers; studied for their color-changing properties in stained glass by art historians, designers, and materials scientists; and manipulated for smarter drug delivery by chemists and biologists.

This scale-centric character of nanoscience means that knowledge in nanoscience is often grouped not along disciplinary lines, but rather around instrumentation techniques (as Mody (2011) has argued), around individual materials, as described above, or around particular applications. Consequently, the structure of knowledge in nanoscience is better understood as clusters of Galisonian "trading zones," rather than a taxonomy of laws, theories, models, and heuristics. These trading zones permit contributions from diverse research perspectives—including those from history and philosophy of science.

I have spent over 3 years working with a nanoscience laboratory with the aim of understanding the structure of knowledge in nanoscience. Through this work I have become convinced that philosophers and historians of science can impact the development of new knowledge in nanoscience alongside practitioners in STEM fields. My talk shows how contributions from history and philosophy of science can provide new knowledge in nanoscience by describing how philosophical reflection on the concept "surface" led to reforms in experiment design in my lab.

❧

Inter-experimentality in the Discovery of the Acceleration of the Universe

– Genco Guralp (Johns Hopkins University)

Abstract. As interdisciplinary fields such as climate science, genetic engineering, or behavioral economics gain prominence in contemporary scientific practice, an increasing number of investigators of scientific knowledge began to probe problems that arise in this context. One example of these problems is the question of disciplinary integration, both at the epistemic and social levels. However, most of these studies deal with the problem from a theoretical or formal perspective. In my paper, I offer an approach to the integration problem via the "new experimentalist" thesis that "aspects of experiment might offer an important … resource for addressing key problems in philosophy of science." (Mayo, D. "The New Experimentalism, Topical Hypotheses, and Learning from Error," in PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association, Vol. 1994, Volume One: Contributed Papers (1994), pp. 270–279, p. 270.)

The experimental episode that I focus on is the discovery of the acceleration of the universe, which was made by two independent research collaborations in 1999 (to be awarded a Nobel Prize in 2011). The aim of the paper is to examine how one of these collaborations, known as the Supernova Cosmology Project (SCP), had to deal with several significant obstacles stemming from inter-disciplinary integration, in order to reach their final knowledge claim. The SCP team is centrally located at the Lawrence Berkeley National Laboratory (LBNL) at the University of California, Berkeley and all the founding members of the team came from a particle physics background, which governs the evidential culture of LBNL. This particle physics culture proved to be a key epistemic factor for the SCP research. Using the concept of inter-experimentality, understood as the study of the interaction of differing practices of empirical confirmation in inter-disciplinary contexts, I analyze both the formation history of the team and their attempts to be recognized as competent epistemic actors within the astronomy community, which regarded them as particle physicists invading astronomers' turf.

There were two forms of integration problems that the SCP had to deal with. Internally, the particle physicists and the astronomers within the SCP had to work together throughout the data collection and analysis procedures. Externally, the group had to legitimize itself within the astronomical community, who were the main judges of their work. In both these cases, there arose problems. For example, it became clear very early on to the astronomers in SCP that the particle physicists of the group did not know enough astronomy and made elementary mistakes during the data collection processes at the telescopes. Moreover, in several oral history interviews I conducted, astronomers of the SCP complained about the "over-confidence" of the particle physicists that the statistical analysis would take care of the problems that arise in data collection. The evidential cultures of particle physics and astronomy clashed within the team.

Externally, the SCP had to overcome a cultural barrier to launch a successful research program. For several years, the group had enormous difficulty in obtaining observation time at the big telescopes. As they were perceived as "outsiders," the astronomy community was reluctant to grant access to the SCP researchers for "they did not know what they were doing" (This remark was made by a senior astronomer who is an expert in supernova research. He later joined the rival group, known as the High-z, which was composed only by astronomers). In order to legitimize their methods, the SCP had to publish a premature paper explaining their research endeavor to the astronomical community, using questionable data. Even though the results of the paper later turned out to be wrong, particle physics trained members of the group still think that it was a necessary move, for it gave them both communitarian and institutional validation for further research (SCP also had issues of losing support from their home institution, LBNL, due to not having produced results for a number of years).

In the body of my paper, I draw several lessons from this episode, following a historical presentation that documents the above claims. I argue that we need to be attentive to inter-experimentality in order to understand empirical confirmation in inter-disciplinary research. More specifically, I claim that perceived disciplinary hierarchies and different cultures of evidence play key roles in the confirmation practices of inter-disciplinary research and in certain contexts, this may impede progress. Both in the micro-level contexts of data collection, statistical analysis and announcement of the results, and the macro-level settings of institutional support and access to research facilities, the sociology of inter-experimentality is seen to have direct epistemic consequences, as I aim to demonstrate.

Parallel Session 2C

Wednesday, 24 June 2015 at 13:30–15:00 in G2

Session chair: Sabina Leonelli (University of Exeter)

❧

Replication and Data-Sharing in the Social Sciences: Progress and Challenges

– Stephanie Wykstra (Innovations for Poverty Action)

Abstract. Research transparency is increasingly a priority for funders, associations and journals in the social sciences. As researchers share more of their data and statistical code in public repositories, the research community has greater access to the research materials underlying published results.

Research transparency is valuable for many reasons, among them: (1) researchers are apt to be more careful in double-checking their analysis if they know they will share their statistical code and data, (2) there is potential for re-use of data for secondary analysis and meta-analysis and (3) research transparency permits replication of research results.

In my paper, I will focus primarily on the topic of replication, and specifically on the relation between replication and publicly available data. "Replication" is a term used in many ways; here it will be used to refer to re-analysis and robustness checks of published research results. Recent cases

such as the replication of Reinhart's and Rogoff's highly-cited research in economics (Herndon et al. 2013) illustrate the value of such efforts. While the difficulty of publishing replications has limited how many of them are done and shared, here have been a variety of projects in the social sciences in recent years to increase replications.

Here I want to pinpoint one question which is of particular salience to applied epistemology: what are the minimal research materials which must be shared, in order to allow for external parties to make use of these materials to assess the reliability of the analysis and conclusions drawn in a paper? Given that one goal of the open science movement is to improve the evidential status of published research, it is worthwhile to investigate whether the research materials that are typically shared allow us to better assess a paper's conclusions.

By looking closely at funder and journal requirements, we see that they most often call for data and code "underlying published research results" to be shared. This includes end stage analysis code, along with variables used the reported results. While this is a step in the right direction, I will argue that there are several important and basic questions – among them how analysis variables were constructed and whether there was selective reporting of outcomes – which these materials do not allow one to assess. I maintain that if the research transparency movement is to lead to better and more transparent evidence about research results, we should form our data-sharing requirements with an eye to replication. While journals may not be in a position to request full collected datasets, funders are often in a better position to do so.

I will present the example of the Arnold Foundation's data-sharing policy. By explicitly using language that requires full study datasets among other materials, the policy addresses the concern above. While there are many challenges – e.g., giving researchers credit for their data, funding them appropriately to undertake the task of data-sharing, and how data should be curated – I will argue that the research transparency and open data movement would greatly benefit if funders adopted requirements similar to that of the Arnold Foundation.

❧

Explaining Rare Events in Political Science: A Mixed Methods Approach

- Sharon Crasnow (Norco College)
- Stephan Haggard (University of California San Diego)

Abstract. In A Tale of Two Cultures: Qualitative and Quantitative Research in the Social Sciences, Gary Goertz and James Mahoney argue that there are fundamental differences quantitative and qualitative research traditions in political science. These differences include different sets of values, beliefs, and norms that result in different research procedures and practices and thus the different traditions might be characterized as constituting different cultures. The result is that while within-tradition conversations are often rich and productive, across tradition conversations are typically "difficult and marked by misunderstanding"(2012, 1). Such a characterization challenges the recent attention to mixed method research in the social sciences, suggesting deeper incompatibilities.

While it may be the case that there are ultimately "two cultures," we suggest how quantitative and qualitative causal process observations may be combined for the study of one class of phenomena: rare events. Many phenomena in political science (and perhaps other disciplines) are extremely complex events that occur only rarely: such as wars, civil wars, revolutions, financial crises, genocides, famines. Causal hypotheses about the mechanisms that give rise to such phenomena are similarly complex and pose challenges to standard quantitative methods such as multiple regression techniques. This paper outlines a general mixed methods approach to the study of rare events. This method combines statistical analysis with an approach to process tracing that investigates all cases of the phenomenon in question.

We develop this method through its use in exploring a particular sort of rare event – the reversions of democracies to autocracies in the third wave of democratic development (Haggard and Kaufman forthcoming). We briefly illustrate the method using our case. We next consider the elements of the method and the various ways they contribute to understanding the phenomenon. For example, we give an account of causal process tracing in political science, examining recent literature on

the topic (Beach and Pederson 2013, Bennett and Checkel 2014). Our account of process tracing highlights the role of theory (hypothesis) in specifying what counts as evidence that the process in question operating in this case. It also examines the way that the interplay between quantitative and qualitative methods refines hypotheses and the categories to which cases belong. We conclude that a mixed methods approach of the kind that we outline contributes to knowledge production in a variety of ways, potentially bridging the “two cultures.”

❧

Economics Imperialism in Social Epistemology: A Critical Assessment

– Manuela Fernández Pinto (University of Helsinki)

Abstract. In the late 1980s social epistemology emerged as a subfield of philosophy dedicated to the study of the social dimensions of knowledge, with a particular emphasis on scientific knowledge. Prominent among the original approaches to social epistemology was the use of economic models to account for the social character of scientific knowledge production while preserving science’s epistemic goals, such as the acquisition of truth and objective knowledge about the world—an approach that Hands (1997) has called the Economics of Scientific Knowledge (ESK). Kitcher (1990) and Goldman & Shaked (1991) made two of the first contributions to ESK, building analytic models in rational choice theory to explain how scientists can make epistemic achievements through an efficient division of cognitive labor, despite following non-epistemic interests, such as the aim for personal credit. Or in other words they aimed at showing that science’s epistemic goals are not necessarily trumped by social factors.

At the same time in which social epistemologists started to use conceptual and methodological tools from economics, Dupré (1995) raised important doubts regarding economics’ imperialistic tendencies, claiming that “as scientific methodologies move further away from their central areas of application their abstractions become ever grosser, and their relevance to the phenomena become ever more distant,” and also that “…alien intellectual strategies may import inappropriate and even dangerous assumptions into the colonized domains” (380).

Economics’ imperialistic tendencies have been a matter of extensive debate. Unquestionably economics has broadened its scope well beyond “economic” phenomena to explain other “social” phenomena in the realms of political science, behavioral science, sociology, geography, and the law. But the appropriateness of such imperialism has been a controversial topic for social scientists, whose views range from an uncritical appraisal of economics’ scientific methods to a radical rejection of the trend.

Despite expanding economics explanatory scope, social epistemology has not yet been examined as a case of economics imperialism. This paper aims at filling this gap. Following recent philosophical contributions to the conceptual and normative framework of scientific imperialism (Dupré 1995, 2001; Clarke & Walsh 2009, 2013; Mäki 2009, 2013; Kidd 2013), I examine whether social epistemology can be considered a case of economics imperialism and determine whether economics explanatory expansionism appropriately contributes to this philosophical subfield or not. I argue first that ESK approaches to social epistemology count as a case of economics imperialism under a broad conception of the term, and second that we have good reasons to doubt the appropriateness of the incursion of economics into social epistemology, insofar as ESK’s attempt at explanatory unification fail to express significant human interests.

The paper is divided in five sections. The second section presents the recent philosophical literature on scientific imperialism and introduces a normative framework for the evaluation of these cases (following Mäki 2013). The third section examines social epistemology’s interdisciplinary transfers with economics, especially through the development of ESK. The fourth section evaluates such transfers according to the criteria presented in the second section and highlights the shortcomings of the ESK approach regarding its significance for non-epistemic human interests. The last section presents some concluding remarks.

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Parallel Session 2D

Wednesday, 24 June 2015 at 13:30–15:00 in G3

Session chair: Joseph Rouse (Wesleyan University)

❧

Toward Semiotic Modelling of Experimental Practices

- Robert Meunier (University of Kassel)

Abstract. Starting from the assumption that scientific knowledge production is a complex process that can be subject to explanations and modelling, the talk introduces a semiotic approach to modelling experimental practices. The aim is to show how in experimental systems phenomena are produced as signs and how these signs are weaved in a web of other semiotic entities, such as instruments, images and the words in which the knowledge gained about these phenomena is expressed. The semiotic reconstruction will mainly draw on Nelson Goodman´s theory of symbols as developed in his Languages of Art. A hallmark of Goodmans theory is that it understands labels (words, images or objects when they denotate) as referring to objects, which is essential for understanding the way symbols function in science, without taking referents as given. In fact labels and objects are delineated within a system of alternative labels and objects (just as signs and content in structuralist semiotics, which however avoids to include objects and a reference relation). Furthermore, reference is not limited to the relation of labels to objects (denotation), but includes the possible relation of objects to labels (exemplification), which seems particularly important in science. In this way many referential relations among the components of an experimental system – the objects or processes studied, manipulatory tools, measuring instruments, images and words -, can be identified. The result is a view of scientific knowledge as forming locally coherent semiotic networks. However, such a network is introduced through the action of scientists. To connect the actions that bring about and are at the same time enabled by the experimental system in the semiotic nexus of the system, James Gibson´s notion of affordance will be employed. Actions that are afforded by objects are organized

in systems of alternatives, just as the labels exemplified by these objects. Some actions that are afforded by given objects introduce differences, that is, they introduce new symbols systems in which alternative objects, labels and follow-up actions are delineated and coordinated.

Apart from answering philosophical questions about the meaning of scientific terms or the way epistemic objects appear in experimental systems, the approach allows to address historiographic questions. In particular through comparing experimental practices described in the same semiotic language, we can detect fundamental differences, which can explain specific historical constellations. For example, in the first decades of the 20th century, the new discipline of genetics and the not much older strands of experimental developmental biology were not well integrated, despite the fact that they seemed to address the same phenomenon – the reproduction of the form of organisms. Comparing their experimental practices, however, reveals that the terms they used to refer to aspects of form (as grown or as inherited respectively), gained their meaning in very different material semiotic systems and networks. In this talk I will sketch semiotic reconstructions of typical experiments in both disciplines to illustrate the approach and show how it can be employed in historiographic studies.

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Individuation, Individuality, and Experimental Practice in Developmental Biology

– Alan Love (University of Minnesota)

Abstract. Philosophical analyses of individuals in biology have focused on theories of individuality that either account for what a biological individual is (Clarke 2013) or provide different dimensions of biological individuality (Godfrey-Smith 2009). The primary considerations in these discussions derive from evolutionary theory, understood as a fundamental framework that governs all of biology, where the capacity of an object to undergo selection is paramount. Less attention has been paid to how individuals are determined in practice, rather than in theory, and what those individuation practices look like in different investigative contexts, especially experimental contexts. I argue that individuation in biological science is governed by specific scientific problems that differ across biology (Love 2008, 2014, forthcoming). These problems lead to variable and divergent conceptualizations of what qualifies as an individual. The result is a pluralist perspective on individuality in the life sciences where different kinds of individuals are tracked in experimental practices (Griesemer 2007). I use the problem agenda of growth in developmental biology to illustrate this situation in the context of an experimental inquiry into the coordination of relative sizes between the whole organism and its constituent parts (Oliveria et al. 2014). Molecular and morphological practices used for tracking individuals and their components through ontogeny are not dependent on evolutionary theorizing. The problem-relative nature of biological individuation dissolves the so-called "problem of biological individuality" (Clarke 2010), which is an artifact of monist philosophical assumptions about scientific knowledge, and captures more accurately how biologists engage in successful practices that contribute to the manipulation, prediction, and explanation of biological individuals.

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Context Dependencies and Multi-level Explanations in Biological Sciences

- Marta Bertolaso (University Campus Bio-Medico of Rome)

Abstract. Reflections on how different explanatory enterprises and projects emerge and characterize scientific practice have been developed. Quite recently, various cases of multilevel research in molecular life sciences have been reviewed (O'Malley et al 2014) and it has been shown how diverse multilevel systems raise significant philosophical questions about explanation, modelling and representation, especially the demand for integrative methods to produce new knowledge. However, an analysis of the structure of biological explanation has been often overlooked. In particular, the strict relationship between the structure of biological explanations and their context dependencies has been difficult to capture or has been considered as a methodological recommendation (Darden and Craver 2009).

The strategy I adopt in order to disentangle different kinds of context dependencies in biological explanations, is to analyse the explanatory import of the context argument when the question is on biological behaviours, understood as dynamic processes, which imply an organizational and adaptive dimension characterized by an inter-level regulatory phenomenology. Examples are taken from cancer research. Firstly, I analyse how the context issue emerges and is accounted for, within a debate on the possibility of reducing biological explanations (Fox Keller 2010; Dupré 2010). Secondly, I clarify how such conceptual and empirical context dependencies integrate the explanatory accounts in biological sciences. The focus of the analysis shifts on the structure of the biological explanations instead of on the way reductions are performed, that is on what kinds of relata fulfil the requirements for these explanations and on the nature of their relationship.

My final thesis is that different epistemological approaches, which are always multi-level in nature, are possible when dealing with organizational and adaptive processes. I will also show how a satisfactory local explanatory model always has two main substantive components –a conceptual and an explanatory one- with each component having a closely related logical/epistemological aspect. Which of them is more relevant depends on the epistemological perspective that is adopted. This kind of relevance is always relative and well explains why mechanistic and systemic perspectives in biological sciences are not only complementary but often imply each other.

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Parallel Session 2E

Wednesday, 24 June 2015 at 13:30–15:00 in G4

Session chair: Holly VandeWall (Boston College)

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Precaution in Scientific Model Building: The Case of the Threshold of Toxicological Concern Approach in Food Toxicology

– Karim Bschir (ETH Zurich)

Abstract. Scientific uncertainty is inherent all research endeavors, and often poses major practical challenges for the application of scientific knowledge in decision-making. In many contexts, however, uncertainties not only play a role in the application of scientific models but also in their development. I will argue that precautionary principles must be applied already in the development of scientific models and not merely when they are used to inform decision-making. If this conclusion holds, it constitutes a good example of how scientific practice can benefit from philosophical considerations. I will contextualize my claims by discussing a case in the field of food toxicology.

Over the past years, improvements in analytical methods have lead to the detection of an increasing number of previously unknown substances in food products. Estimates say that we might be looking at several thousands, if not tens of thousands of chemicals. Their presence might be due to degradation processes, migration from packaging material, or impurities in the manufacturing process. Usually, they are present in low and very low concentrations and the toxicity as well as the potential large-scale effects on human health of these substances are unknown. Assessing the risk of these non-intentionally added substances in food products has become a major challenge in toxicology.

One of the current best scientific approaches for the evaluation of potential risks of incidental low-concentration substances is the so-called Threshold of Toxicological Concern (TTC) concept. The TTC provides a probability-based risk assessment tool, which rests on the assumption that it is possible to derive risk estimates for substances with unknown toxicity from toxicological data of structurally similar substances.

I will explain how the approach works, what it is supposed to accomplish, and what kinds of uncertainties arise in the context of its application. I claim that the TTC provides a useful tool for assessing quantifiable uncertainty (i.e. risk), but that there exist additional uncertainties, which cannot be treated using probability-based approaches. They include uncertainties about unconceived outcomes, uncertainties regarding the underlying theoretical assumptions (e.g. about structural similarity or about the extrapolation of animal toxicity data to humans), or controversies in the scientific community. These uncertainties are very often intimately connected to normative questions.

I will conclude that it remains questionable whether the TTC provides an adequate tool for the assessment of potential health hazards if one evaluates the approach against the standards of the precautionary principle. In accordance with Sprenger (2012)*, I will argue that precaution has substantial implications for model building in science. I will use the case of the TTC to establish the more general claim that the precautionary principle should not merely be seen as a decision rule, but that it should play an important role in responding to model uncertainty. Precaution must be applied already at the stage where we evaluate the epistemic robustness of scientific models.

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❧

The Risk of Using Inductive Risk to Challenge the Value-Free Ideal

- Inmacuala de Melo-Martin (Weill Cornell Medical College)
- Kristen Intemann (Montana State University)

Abstract. The ideal of value free science has come under increasing criticism in philosophy of science. Although not a new one, a significant challenge stems from what has come to be known as the 'inductive risk argument' (Douglas 2009). According to this argument, because acceptance or rejection of hypothesis is unlikely to happen with certainty, scientists must consider whether there is enough evidence to do so. This involves considering not only the likelihood of error, but also how bad the consequences of error would be. When the consequences are related to public policy, this requires evaluating the ethical consequences for those potentially affected. It is thus necessary and desirable for scientists to make ethical value judgments about what sorts of errors are acceptable. Moreover, because scientific reasoning is affected by uncertainty at different stages, proponents of the inductive risk argument maintain that ethical and social value judgments are necessary not just at the moment of acceptance of theories or hypothesis but also at earlier stages of the research such as those involving the characterization of the evidence and its interpretation (Douglas 2009).

Although the argument from inductive risk has been embraced by many as a challenge to the value-free ideal of science, we contend that such is not the case. We argue that for an account of the role of contextual values in scientific decision-making to successfully challenge the value free ideal, it must address two appropriate concerns motivating such an ideal. First, it must tackle an epistemological worry that the use of contextual values in scientific reasoning will lead to wishful thinking. Second, it must address a political concern that having scientists making social and ethical value judgments in research undermines democratic values. The argument from inductive risk aims to address the epistemological concern by narrowly limiting the legitimate role of contextual values in scientific reasoning. This move, however, hinders proponents' ability to justify the claim that contextual values are necessary in scientific decision-making. Insofar as the necessity of contextual values is undermined, then the inductive risk account of values in science is on par with the value-free one. Moreover because, contrary to the value-free ideal, proponents of the inductive risk argument seem unable to address the political concern, the value-free ideal seems preferable. We argue that a successful challenge to this ideal must ultimately reject the assumption that values cannot legitimately play evidentiary roles in order to more adequately overcome both the epistemological and political concerns that motivate the ideal. In the final section we show how this might be carried out.

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Estimation of Systematic Uncertainty as Robustness Analysis

- Kent Staley (Saint Louis University)

Abstract. In numerous disciplines, when scientists report quantitative experimental results, they distinguish between the statistical uncertainty and the systematic uncertainty associated with their measurements, and provide quantitative estimates of both. In this paper I focus on the practice of estimating systematic uncertainty as carried out within experimental high energy physics (HEP). I argue that the estimation of systematic errors in HEP should be regarded as a form of quantitative robustness analysis, understood (following Wimsatt 1981) in terms of four component procedures: (1) analysis of a variety of independent processes; (2) identification of invariants in the outcomes of those processes; (3) determination of the scope and conditions of such invariance; and (4) analysis and explanation of relevant failures of invariance. My analysis employs as an interpretive heuristic the secure evidence framework, developed in Staley 2004 (see also Staley 2014) as an approach to explaining the evidential value of robustness.

Although the quantitative estimation of systematic uncertainty is a common practice in many disciplines, it has received little attention from philosophers of science (but see Tal 2012). By providing a philosophical explication of this practice in terms of robustness analysis, I hope to clarify its epistemological purpose, thus providing potential guidance in ongoing debates amongst particle physicists over the appropriate methodology for estimating systematic uncertainty.

Philosophical neglect of this issue is unfortunate, for discussions of systematic uncertainty open a remarkable window into experimental reasoning. Even cursory presentations of systematic uncertainty estimates will note the main sources of systematic uncertainty. More careful reports detail both the ways in which systematic uncertainties arise and the methods by which they are assessed. Such discussions require forthright consideration by experimenters of the body of knowledge that they bring to bear on their investigations, the ways in which that knowledge relates to the conclusions they present, and the limitations on that knowledge. This process is epistemologically crucial to the establishment of experimental knowledge.

Moreover, philosophical insight regarding the estimation of systematic uncertainty could have significant practical value. Presently, there is no clear consensus across scientific disciplines regarding the basis or meaning of the distinction between statistical and systematic uncertainty, despite some concerted efforts surveyed in this paper. Scientists in HEP and other fields also debate the proper statistical framework in which systematic uncertainty should be evaluated, a debate with important philosophical aspects. It is the contention of this paper that some progress may come from regarding the estimation of systematic uncertainty as an instance of robustness analysis applied to a model of an experiment or measurement, the epistemic value of which concerns the security of evidence claims subjected to such analysis.

The plan of the paper is as follows: I begin with a discussion of the distinction between systematic and statistical uncertainty, then turn to debates in HEP regarding the appropriate statistical framework for the estimation of systematic uncertainty, thus highlighting the importance of philosophical insights for scientific practice in this regard. I then outline the secure evidence framework to be employed in my analysis. I present my argument for viewing systematic uncertainty estimation as quantitative robustness analysis, showing how such analyses in HEP target both inferential and measurement robustness (as these terms have been articulated by Woodward 2006) and conclude with some tentative suggestions regarding how the present analysis might illuminate the scientific debates previously mentioned.

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Parallel Session 2F

Wednesday, 24 June 2015 at 13:30–15:00 in Koll G

Session chair: Hasok Chang (University of Cambridge)

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To Know is to Identify: Forging a Realist Criterion for Astrophysical Entities

– Alan Heiblum (Department of History and Philosophy of Science. University of Cambridge)

Abstract. The current panorama of cosmology provides rich material for the philosophy of science. Theory and data do not match as might be expected. General relativity and classical mechanics predict vastly more material in the universe than what is found in astronomical observations. To explain this excess of gravitational potential, for almost a century scientists have advanced the assumption of an unobservable matter called dark matter. This ad hoc hypothesis enjoys the approval of the majority of the scientific community despite the fact that it has not been detected, nor has its modelling been able to provide a coherent explanation for all observed astronomical phenomena.

The problem of the dark matter is the problem of its identification. But what does it mean to identify dark matter? Identify as a general problem has two faces, than we can call the missing identity and the missing object. In the former the existence of the entity is known while its identity remains unknown. In the latter, you know what you are looking for, but you don't know if you will find it. Thus, dark matter poses a significant challenge to cosmology because both elements are absent. We don't have a definitive list of candidates nor the certainty of its existence.

To know is to identify. According to this position, science is a sophisticated system to identify the unidentified. The distinction between identified and unidentified objects may seem Kantian. This is not the case. An unidentified object is not unknowable; it is inexhaustible. This posture is very close to the one of Niiniluoto "The point is to illustrate the idea that, as soon as we choose a language L, it is THE WORLD itself which chooses the structure WL. It may be more natural to say that THE WORLD contains unidentified facts, while WL contains identified facts" (Niiniluoto 2002, 224). The difference lies in the importance ascribed to language. In my view, the world is not going to choose structures hanging by our words. To crystallize identifications, intervention is needed. "Epistemic things, according to my conception, are invested with meaning; they are not just 'named'" (Rheinberger 2005, 408). As long as "dark matter" is just a name used to describe a theoretical problem, we will have no progress in our knowledge about the entities that populate the universe.

If natural science is more a matter of creating phenomena than "saving" them, what happens in those domains, such as astrophysics, at the limit of our experimental capacity? Are we doomed to anti-realism? Here, I will claim a clear "no". First I will support the thesis of realism concerning experimental entities as Hacking did in 1983 in his Representing and Intervening. But then, I will depart from him when by way of a too narrow reading of his own argument, he declares himself an anti-realist about astrophysical entities (Hacking 1989). The main argument I will offer is the following one: the choice is not only between creating or salving phenomena; there is also simulation. Simulations are not merely representations, even though they are not full interventions. In my view we could use simulations as the missing link to clarify the reality of the unreachable objects that populate the heavens. Thus, realism has a strong case in astrophysics when triangulates the creation, salvation and simulation of phenomena. My aim is to show how this criterion would works in the particular case of dark matter.

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Theseus and the Zymes

– Dana Tulodziecki (Purdue University)

Abstract. The pessimistic meta-induction targets the realist's claim that a theory's (approximate) truth is the best explanation for its success. It attempts to do so by undercutting the alleged connection between truth and success by arguing that highly successful, yet wildly false theories are typical of the history of science, and, thus, that a theory's success cannot be a symptom of its truth (cf. Laudan 1981, 1984).

There have been a number of prominent realist responses to the pessimistic meta-induction, most notably those of Worrall (1989), Kitcher, (1993), and Psillos (1999). All of these responses try to rehabilitate the connection between a theory's (approximate) truth and its success by attempting to show that there is some kind of continuity between earlier and later theories, structural in the case of Worrall, and theoretical/referential in the cases of Kitcher and Psillos.

In this paper, I will show that both the realist's focus on continuity and the anti-realist's focus on undercutting the connection between approximate truth and success are problematic and, in fact, misguided. I will do so by means of a case-study from the history of medicine that I discuss in some detail: the so-called zymotic theory, a mid-19th Century version of the miasma theory of disease.

First, I will argue that neither realists nor anti-realists can account for this case: there is no continuity of any kind between the zymotic theory and the germ theory that realists are interested in; however, despite the fact that nothing responsible for the zymotic theory's success is retained by the germ theory, it is also a mistake to view the switch from one to the other as one of radical discontinuity. The situation is better viewed as being analogous to an extreme version of Theseus's ship: even though no part of the original ship is retained, there is still a continuous, very slow and gradual process leading from the earlier to the later ship. In the case of the zymotic theory, this involves changing the miasmatic and anti-contagionist concept of a zyme until it is virtually indistinguishable from a germ. I will argue, however, that the two 'ships' – the zymotic theory and the germ theory – end up being so different that it is not clear how one could even go about comparing them along realist or anti-realist lines: neither the various notions of realist continuity nor the notion of approximate truth can be made sense of in this case, or can even be viewed as applicable to it. Furthermore, attempting to press this case into the standard realist/anti-realist mould forces one to lose exactly what is most interesting about it.

I end by using the zymotic case to make some more general points that show that the framework of the realism/anti-realism debate is not well-suited to doing justice to actual historical cases, and conclude by drawing some lessons from the zymotic example about the relationship between philosophical frameworks and historical cases more generally.

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Science, Philosophy, and Applied Ontology: A Common Project for a Unitary Description of Reality

– Timothy Tambassi (Università del Piemonte Orientale)

Abstract. In this paper, mutual interactions between science and philosophy are analysed from the point of view of contemporary applied ontology. Firstly, we shall address the question as to whether science needs philosophy, offering some perspectives that might be helpful in developing a synergetic relationship between these different domains. Secondly, we shall point out how it is possible to bring together the work of scientists and philosophers from a practical perspective. In particular, we shall focus our attention on the GEOLAT project, which offers a practical exemplification of the interaction between science and philosophy in the contemporary debate.

The aim of GEOLAT project is to make accessible the Latin literature through a query interface of geographic/cartographic type. Since all texts written in the classical period are rooted in geographic space, they all contain references to geographic places in some respect. Therefore, it becomes interesting to use a web resource that includes references to geographic context. Most research is based on the use of a gazetteer in which a place is normally represented by point locations. The limited spatial semantics associated with these approaches narrows the scope of their ability to retrieve useful resources for spatial queries.

All these information are collected in a comprehensive and informative geographical ontology, which plays a central role in intelligent spatial search on the web and serves as a shared vocabulary for spatial mark-up of Web sources. This ontology plays a specific role in representing information in four different domains: contemporary and ancient geography, informatics, Latin literature, as well as philosophical ontology of geography. The examination of this ontology allows us to rethink the relationship between science and philosophy on new bases, considering these disciplines as parts of a common project for a unitary description of reality.

Parallel Session 3A and Parallel Session 4A

The symposium is split into two parts, time, place and chairs follow below.

Organized by: Sabina Leonelli (University of Exeter)

Symposium: Data Practices in Biology and Biomedicine

Synopsis. This double symposium is devoted to fostering a philosophical understanding of data handling practices and their epistemic implications for knowledge production. Rather than focusing only on data generation and interpretation, we analyse ways in which data are processed and shared for future use, which have so far been largely overlooked within general philosophy of science and even within the philosophy of science in practice. The first half of the double symposium focuses on the ways in which data are disseminated within research, and particularly how databases are used to structure and facilitate data flow across a variety of scientific contexts, and what repercussions this has for the ways in which data are produced, analysed, interpreted and traded among research environments. The second half of this double symposium examines several ways in which data are used in biological practice, which are not captured by the traditional emphasis on data production and data interpretation within philosophy of science. As the speakers will illustrate, data can be modeled in a variety of ways; their mere presence, or absence, can determine which research directions are taken and which theoretical approach is supported; and their relation to laboratory materials is essential to determining their usefulness and meaning as evidence. Focusing on data as key components of research practices sheds light on the nature of the relationship between data, models, theories and the world, and on the crucial epistemic role of the material basis of evidence within the life sciences.

Part 1: Data Flows and Epistemic Implications of Databasing

Wednesday, 24 June 2015 at 15:30–17:30 in Aud F

- The Evidential Scope of Databases in Cancer Genetics
  Emanuele Ratti
- Data Integration Between Databases
  James A. Overton
- Mapping Biological Knowledge. From Particular Data to General Phenomena
  Federico Boem
- The Flow Metaphor and Data Ecosystems
  Gregor Halfmann

Part 2: Towards a Pluralist Understanding of Data Uses

Thursday, 25 June 2015 at 09:00–11:00 in Aud F

- Data, Models and Data Models
  Sabina Leonelli
- Contrasting Approaches in Mitochondrial Evolution: Data-Emphasis, Data-Ignorance and Its Consequences
  Thomas Bonnin
- Data, Infrastructures and Materials: Repertoires in Model Organism Biology
  Rachel A. Ankeny
  Sabina Leonelli
- Commentary
  Hans-Jörg Rheinberger

The abstracts for both parts follow below.

Part 1: Data Flows and Epistemic Implications of Databasing

Wednesday, 24 June 2015 at 15:30–17:30 in Aud F

Session chair: Alan Love (University of Minnesota)

Part Synopsis. This double symposium is devoted to fostering a philosophical understanding of data handling practices and their epistemic implications for knowledge production. Rather than focusing only on data generation and interpretation, we analyse ways in which data are processed and shared for future use, which have so far been largely overlooked within general philosophy of science and even within the philosophy of science in practice. The first half of the double symposium focuses on the ways in which data are disseminated within research, and particularly how databases are used to structure and facilitate data flow across a variety of scientific contexts, and what repercussions this has for the ways in which data are produced, analysed, interpreted and traded among research envirnoments.

The Evidential Scope of Databases in Cancer Genetics

- Emanuele Ratti (Dipartimento di Scienze della Salute, University of Milan, Department of Experimental Oncology, Istituto Europeo di Oncologia, Milan, emanuele.ratti@ieo.eu)

Abstract. Contemporary experimental biology (in particular molecular biology) makes extensive use of data stored in databases. Sabina Leonelli claims that storing data in databases expands the evidential scope (i.e. the range of claims for which something can be taken as evidence) of data themselves. In order to be properly understood, this claim should be embedded into the framework provided by Bogen and Woodward in their seminal paper Saving the Phenomena. Bogen and Woodward argue that science is not interested in formulating systematic explanation about data, but rather about phenomena. According to this view phenomena are stable regularities for which data serve as evidence. However, data should be somehow 'filtered' to eliminate confounding factors (e.g. spurious or meaningless regularities, noise, etc.) in order to reveal phenomena. How data are processed varies from experiment to experiment, in the sense that any experimental context has its own peculiar confounding factors that complicate the identification of phenomena. Therefore the evidential scope of data is local, in the sense that it depends on the particular way data are produced. Leonelli challenges this idea by saying that data, when stored in biological databases, become nonlocal and their evidential scope is enhanced. In this talk, I will start from Leonelli's point and I shall further develop it into an account of how data stored in biological might expand their original evidential scope. Therefore the question of this talk is: in which sense the evidential scope

of data sets is enhanced once they are re-used? In what follows I shall argue that data stored in databases are not properly data, but they display many important features which are usually ascribed to phenomena and – moreover – they are stored as representing specific phenomena. By drawing from the distinction between phenomena types and phenomenon tokens I shall show that data, when stored in biological databases, are transformed into phenomena type. By 'packaging data', curators of databases actually make an operation of idealization over data. Data stored in databases can be used as types of (idealized) phenomena to be compared to other bits to data. Therefore the relation between data and databases might be understood as the model/world relation extensively analyzed in the philosophy of science. Specifically my case in point will be cancer genomics, where data stored in databases are used to eliminate specific confounding factors. In such a 'data intensive' enterprise, computational algorithm might potentially detect several phenomena but not all phenomena are actually useful for a particular research. First I will show exactly how databases are structured in cancer genomics. Then, I shall illustrate how, by using databases such as COSMIC or TCGA, it is possible to identify 'phenomena' of interest in large data sets, and to eliminate uninteresting ones. In particular, I will draw on a case where databases are used to establish whether some genes are cancer genes in a specific type of cancer (i.e. lung). Therefore by comparing 'regularities' detected in a lab to 'regularities' stored in databases (which are already characterized as specific phenomena), one can establish whether a regularity is a token of a specific phenomenon type. By being used as one horn of a typical model/world relation, data stored in biological databases expand the range of claims for which they are taken as evidence.

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Data Integration Between Databases

– James A. Overton (Knocean.com, james.overton.ca, james@overton.ca)

Abstract. The Immune Epitope Database (IEDB) has been painstakingly curated from more than 16,000 publications – nearly all the papers ever published in immunology, allergy, and autoimmune research. An epitope is the part of a molecule that is recognized by the immune system, and the heart of the IEDB is the set of more than 119,000 epitope records, covering positive and negative results from more than 700,000 individual experiments. The value of this costly data is vastly increased when it is linked to other resources, such as GenBank, UniProt, the NCBI Taxonomy, and the Gene Ontology. Every such resource is built on a set of constraints and assumptions, often implicit. Integration invariably means bringing these constraints and assumptions into conflict. In every case we find that external resources contain both too much and not enough data for our needs: too many taxa in the NCBI Taxonomy but too few transgenic mouse strains; too many proteomes in UniProt but not enough information about viral polyprotein cleavage products. We must select and extend the data from the external resources, respecting their constraints while bringing them in line with our own. As we resolve these conflicts we change, and hopefully clarify, the meanings of our data, exposing exceptions and errors that have to be re-curated. Integration on this scale breaks new ground, raising many questions and demanding careful reflection on the science, the data, and their use. In this paper I present my perspective on these practises, both as philosopher of science and as a member of the IEDB team.

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Mapping Biological Knowledge. From Particular Data to General Phenomena

– Federico Boem (Dipartimento di Scienze della Salute, University of Milan, Department of Experimental Oncology, Istituto Europeo di Oncologia, Milan, federico.boem@ieo.eu)

Abstract. In biomedical research an experimental result can be grounded on the consistency of the methods adopted and on the locality of its production, namely, the experimental conditions. As also remarked by Jacob "in biology, any study [. . .] begins with the choice of a 'system'. Everything depends on this choice: the range within which the experimenter can move, the character of the questions he

is able to ask, and often also the answers he can give" (Jacob 1987). Thus biological findings seem to be strictly dependent on the locality of their production. The possibility of a generalisation is very problematic in biology and the claims about biological phenomena beyond the locality of data is often difficult within traditional approaches. Ontologies seem to overcome such a locality (see for instance Leonelli 2009) since they can exploit the knowledge produced in a specific context and make it disposable to another (even very different) one. To put it differently ontologies broaden the Jacob's notion of experimental system to the entire realm of biological knowledge. However in these terms such a difference could be just an evocative picture. My proposal is to provide an epistemic justification for the unifying power of ontologies. In particular, I will focus on Gene Ontology (GO) structure. By examining both the epistemic reasons for its implementation and the type of analysis provided by GO, I will show how such a tool resembles some features of a map but nevertheless constitutes something new in the epistemological scenario. Not entirely a theory, more than a model (but structurally similar to it), I will argue that GO a novel category within the epistemic repertoire. I then claim that the knowledge provided by GO should be seen as a more or less effective tool through which we can discriminate, among an enormous amount of data, a convenient way of organising those empirical results which were at the basis of the GO analysis. Accordingly, such a specific status will be better specified given that GO is both conventional, as the result of epistemic interactions towards a common agreement, and normative, since the tool shapes the representation of knowledge as it will be perceived by other, future researchers. In conclusion I will suggest that GO is an orienteering tool on which scientist can map their data on a wider context and then, thanks to this, elaborate new experimental strategies. GO is then a map for making the conceptual content of a particular experimental condition comparable across different research contexts. Such a map is essential not as a way to confirm experimental results but as a way to compare experimental results with the theoretical background (the so called 'big picture'). Lastly, I will face the fact that ontologies are considered a unification tool. In taking into account the possibility of such a generalisation (beyond the locality of data production), I will show that GO does not create, per se, a unification for the theoretical content. My proposal is then to clarify what and how exactly GO is unifying.

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The Flow Metaphor and Data Ecosystems

– Gregor Halfmann (Department of Sociology, Philosophy and Anthropology & Exeter Centre for the Study of the Life Sciences (Egenis), University of Exeter, gh337@exeter.ac.uk)

Abstract. "Data flow" is a common term used in the context of data-intensive sciences, which have managed to turn the production of scientific data into an automated information stream from the observation or experimentation instrument, to online databases, and into the researcher's office computer. The flow is, however, only one of several widely used metaphors, which relate in various ways to the image of a data ecosystem (Star and Griesemer, 1989; Baker and Bowker, 2007; Parsons et al., 2011), and which carry ontological implications at the same time. While being a powerful instrument for conceptualisations of data management and knowledge production, the flow and ecosystem metaphors' implication that data can move as smoothly, effortlessly, and cohesively as a natural river is also criticised (Edwards et al, 2011; Leonelli, forthcoming). I contend that an ecosystem's processuality, its adaptability and complexity contradict with data's desired robustness and its character as mineable and quantifiable product. Moreover, the epistemic role of data and data management practices in particular remain largely unclear in light of this blurred ontological image of data.

My paper will take two approaches to elucidate this image: I will firstly discuss the ambiguous ontic implications of data and data ecosystems with respect to the dichotomy of process ontology and substance or object ontology. These positions serve as two extremes that mark the limits of a field, in which conceptions of data management systems and examples of data ecosystems from case studies can be situated and put into relation. My second approach is based on empirical ontological study, which takes into account recent developments in science and technology studies, which have highlighted the possibility of ontologically differing enactments of entities and pluralisation depending on situation (Lynch, 2013). My paper analyses data practices in contemporary ocean sciences

with respect to data flows, ecosystems, and their ontological enactments. In contrast to climate sciences' global infrastructures (Edwards, 2010), oceanographic data is produced and processed in systems with larger ranges of complexity and automatization. Oceanographers not only produce heterogeneous data from remote and often inaccessible areas; they create methods to produce robust and reusable data of ocean phenomena, which are themselves highly complex and processual.

Part 2: Towards a Pluralist Understanding of Data Uses

Thursday, 25 June 2015 at 09:00–11:00 in Aud F

Session chair: Alan Love (University of Minnesota)

Part Synopsis. The second half of this double symposium examines several ways in which data are used in biological practice, which are not captured by the traditional emphasis on data production and data interpretation within philosophy of science. As the speakers will illustrate, data can be modeled in a variety of ways; their mere presence, or absence, can determine which research directions are taken and which theoretical approach is supported; and their relation to laboratory materials is essential to determining their usefulness and meaning as evidence. Focusing on data as key components of research practices sheds light on the nature of the relationship between data, models, theories and the world, and on the crucial epistemic role of the material basis of evidence within the life sciences.

Data, Models and Data Models

– Sabina Leonelli (Department of Sociology, Philosophy and Anthropology & Exeter Centre for the Study of the Life Sciences (Egenis), University of Exeter, s.leonelli@exeter.ac.uk)

Abstract. This talk discusses the notion of 'data model', its current role in philosophy of science and what focusing on these objects can teach philosophers about the complex relationship between data and models. My discussion is grounded on an empirical approach to philosophical analysis, in which the discussion of the epistemic role of data and models is grounded on a study of how contemporary scientists are using these research components to explore the world and reason about it. I start by arguing that data models have often been seen as oversimplified/idealised version of actual dataset whose main function is to make data useful as evidence for theoretical claims (for instance, in a 2006 review paper Roman Frigg defines them as a "corrected, rectified, regimented and in many instances idealized version of the data we gain from immediate observation, the so-called raw data"); and that capturing the relation between data and models in this way has inhibited a close philosophical investigation of the status of data in scientific research and its relation to modeling. Building on ongoing empirical work of how data are circulated and modeled in contemporary plant science, I then reflect on the status of data models, the extent to which they can be viewed as 'representations' of a target system, the possible differences between data models and 'simple' datasets (and their respective roles as communication and exploration tools within and across scientific communities), and the crucial importance of these tools towards the achievement of scientific understanding.

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Contrasting Approaches in Mitochondrial Evolution: Data-Emphasis, Data-Ignorance and Its Consequences

– Thomas Bonnin (Department of Sociology, Philosophy and Anthropology & Exeter Centre for the Study of the Life Sciences (Egenis), University of Exeter, tb391@exeter.ac.uk)

Abstract. The consensus view in the evolutionary history of Mitochondria is that this intracellular structure emerged from endosymbiosis. This idea asserts that mitochondria, once free living α-proteobacterias, were integrated by another cell, and were progressively specialized to become an organelle, mostly serving as a "power plant" for the host cell. While this broadly sketched scenario is now widely accepted, there is much disagreement on its details. The object of this talk is to understand the difference in the data used by the two main antagonists in this debate, how these different sets of data result in differing hypothesis, and the consequence in the philosophical debates that it bears. On one side is the 'hydrogen hypothesis', first defended in a 1998 Nature article by William Martin and Miklos Müller. This hypothesis takes place in an oxygen-deprived (anaerobic) environment, where a hydrogen-consuming archaebacterial took advantage of a hydrogen-producing α-proteobacteria and eventually integrated it within its cytoplasm. A subsequent amount of gene transfer between the host and symbiont increased the host's metabolic versatility, and allowed this association to thrive in oxygen-containing (aerobic) environments. The acquisition of the mitochondria in this scenario is seen as key to the formation of eukaryotes and to the subsequent increase of complexity observed in these organisms. The other camp's main protagonist is Thomas Cavalier-Smith, which elaborates since 1975 his 'phagotrophic hypothesis'. The phagotrophic hypothesis argues that the key event in the acquisition of mitochondria, as well as in the development of eukaryotes, is the acquisition of a system of internal membranes which is crucial to the evolution of phagocytosis. Phagocytosis made possible the integration of an α-proteobacteria which was progressively enslaved to become a mitochondria. The energy efficiency increase provided by the presence of the mitochondria is seen as one step among others in the evolution of eukaryotes, secondary in importance to the membrane innovations that helped the integration of the different organelles and the nucleus. I will argue that the former hypothesis restricts the scope of its data on genomic ones, despite being secondarily supported by metabolic constraints. On the contrary, the second hypothesis is based on more varied sources of data, and in this case genomic data are more treated as secondary sources which are fitted in a model built with cellular biology and fossil records data. The philosophical assumptions lying behind the 'hydrogen hypothesis' are of importance for recent debates in philosophy of biology, mainly in the contestation of the Tree of Life hypothesis, and many voices now defend that evolutionary relations between species are of a network nature. We will see that this contestation is also grounded in a similar genome-centred perspective that provokes the ignorance of other kind of data. With this talk, I would therefore to illustrate how the usage of data can shape philosophical discussions, and assess the impact that a more diversified approach, like the one of Cavalier-Smith, can have.

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Data, Infrastructures and Materials: Repertoires in Model Organism Biology

– Rachel A. Ankeny (Department of History, University of Adelaide (Australia), rachel.ankeny@adelaide.edu.au)
– Sabina Leonelli (Department of Sociology, Philosophy and Anthropology & Exeter Centre for the Study of the Life Sciences (Egenis), University of Exeter)

Abstract. How effectively communities of scientists come together and co-operate is crucial both to the quality of research outputs and to the extent to which such outputs integrate insights, data and methods from a variety of fields, laboratories and locations around the globe. This paper focuses on the ensemble of material and social conditions within which organismal research is situated that makes it possible for a short-term collaboration, set up to accomplish a specific task, to give rise to

relatively stable communities of researchers. We refer to these distinctive features as repertoires, and investigate their development and implementation in a key case study in contemporary biological sciences, namely how research on individual organisms evolved into model organism communities. We focus particularly on the ways in which the epistemic value of data as evidence is shaped by the features of the materials via which data have been generated, as well as the ready availability of access to these materials. This is a typically overlooked aspect of data epistemology, affecting both how data is encoded in databases and how its provenance is portrayed and interpreted when data are re-used, integrated and/or developed in further research. We conclude that whether a particular project ends up fostering the emergence of a resilient research community is partly determined by the degree of attention and care devoted by researchers to material and social elements beyond the specific research questions under consideration.

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Commentary

– Hans-Jörg Rheinberger (Max Planck Institute for the History of Science)

Parallel Session 3B

Wednesday, 24 June 2015 at 15:30–17:30 in G1

Session chair: Annamaria Carusi (University of Copenhagen)

Organized by: Sophie Van Baalen (University of Twente)

Symposium: Philosophy of Science in Clinical Practice

Synopsis. Since its inception in the early 1990's, evidence based medicine (EBM) has been promoted as a way to make clinical practice more scientific. Exponents have appropriated concepts and terminology from the philosophy of science to explain the nature and appeal of EBM. Such appeals range from references to scientific revolutions and paradigms (accompanied by references to Kuhn) to claims of a more clearly empiricist and positivist nature, including the idea that empirical evidence can be a 'neutral arbiter' between competing explanatory approaches. EBM famously 'de-emphasises' intuition, clinical experience and pathophysiologic rationale in favour of objective evidence from RCTs and their statistical analysis. Critics of EBM have questioned the consistency of these appeals to diverse and incompatible traditions in the philosophy of science, and challenged the naive positivism of the idea that we do not need to know 'why' something works but simply 'what works'. EBM is said to be based on a narrow view of science, focusing on quantitative, clinical evidence and rule-following instead of basic science, theories and judgments, and a narrow view of what it means to be ill, focusing on what can be known of disease instead of how disease is experienced by patients.

These criticisms raise a wide range of questions about the value of EBM and its alternatives. Are they well-placed, and what alternatives exist? Does a return to emphasising judgement and 'discernment on a case-by-case basis' represent a return to an uncritical acceptance of authority-based decision making, which EBM set out to replace? Does a return to emphasising the importance of underlying theoretical perspectives and explanatory mechanisms represent a return to a clinical science in which treatments are introduced based on a theoretical, molecular or causal explanations rather than some evidence of their value in practice? Until we have a clearer sense of what we mean by science, and the sense in which clinical practice ought to be 'scientific', we cannot develop a viable account that considers clinical trial methodology and how doctors can best incorporate scientific evidence in clinical decision-making and reasoning, taking into account the individual and personal nature of decision-making in clinical practice. Philosophy of science can make a valuable contribution to medical science and practice by understanding and improving medical research methodology, but also by developing an account of medical reasoning, and guiding clinicians in the use, appraisal and integration of information from different sources. A special role is reserved for "philosophy of science in practice", as the specific tasks and limitations of clinical practice should be taken into account. In this symposium, we would like to explore how philosophy of science can improve the epistemological work in the clinical (research) practice.

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From the 'Revolution' to the 'Renaissance': Science, Philosophy, Rhetoric and the EBM Debate

– Michael Loughlin (Department of Interdisciplinary Studies, Manchester Metropolitan University, M.Loughlin@mmu.ac.uk)

Abstract. Evidence-Based Medicine (EBM) was introduced in the early 1990s as a 'radical' and 'revolutionary' new 'paradigm', a 'movement' destined to remodel medicine, which henceforth would be 'based on evidence'. In the new 'era', objective science in the form of Randomised Controlled Trials and their systematic analysis would replace the medieval quackery of the past as the 'base' for medical practice. Yet from the outset there was a tension between the apparently contentious nature of any 'revolutionary' doctrine and the assumption of protagonists that the case for EBM was so 'unquestionable' as to permit no credible opposition. Following critical interrogations of what exponents meant by 'evidence', and the plausibility of 'basing' all practice upon this conception of evidence, key defenders of EBM produced various 'clarifications', effectively redefining EBM in terms so 'unquestionable' as to be platitudinous. When critics focussed on EBM's implications for the role of judgement, value and context-specificity in clinical decision-making, protagonists continued to 'clarify', rather than retract or modify any substantive claims, speaking of 'integrating' these factors (though typically without providing an account of how precisely they would be integrated). Now a new 'EBM Renaissance Group' calls for a return to 'real EBM', which incorporates 'expert judgement', 'individualised evidence' and which 'makes the ethical care of the patient its top priority'.

The "brand name' theory of EBM is the view that, throughout the 22 year history of the EBM 'movement', critics made the error of treating as a thesis or proposal what is in fact a brand name, associated with a range of products and publications as well as career and funding opportunities for its exponents. While it is difficult to assess the causal role of EBM in improving outcomes for patients, EBM is nonetheless an extremely successful academic movement. As one of its founders commented in 2009, 'funding agencies have accepted EBM with remarkable enthusiasm' and its terminology has spread 'like fire', way beyond the medical arena into areas as diverse as education and social work. This success can be analysed with reference to methods employed by other academic movements purporting to discover the 'base' or 'centre' for practices, including the quality movement in management theory. Ironically, willingness to commit certain basic rhetorical fallacies seems to be the key to establishing longevity in an academic movement of this sort.

Philosophers and scientists often pride themselves on being party to a discourse more rational than popular debate. However, all attempts by theorists to make a positive impact on practice are mediated by the economic and political contexts in which theoretical and practical debates interact. Research in this area needs to acknowledge the difficulties in balancing meeting the demands of the policy agenda with retaining intellectual integrity and making contributions which are of genuine use to practitioners.

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❧

Causation in Scientific Methods and the Medically Unexplained

- Rani Lill Anjum (CauseHealth – Causation, Complexity and Evidence in Health Sciences, Norwegian University of Life Sciences, rani.anjum@nmbu.no)

Abstract. Scientific methods are supposed to guarantee the quality of our research. But they do more than this. They define what counts as evidence, what counts as a cause, and what counts as a result. Any science that looks for causes must therefore do so with a pre-understanding of what causation is. This understanding if often tacit and unexamined, yet it forms the basis of our scientific practice. In medicine, for instance, population studies such as randomised controlled trials (RCTs) are thought to offer the strongest evidence of causation. But this method is based on a difference-making notion of causation and also on a frequentist interpretation of probability. These are not neutral or unchallenged philosophical theories.

Does it matter scientifically how we understand causation philosophically? To a great extent, I argue. About 30 percent of all symptoms reported to doctors in Europe and other industrialised countries today are so-called medically unexplained (MUS). These include conditions such as chronic fatigue syndrome (CFS/ME), irritable bowel syndrome (IBS), low back pain (LBP) and fibromyalgia (FM). MUS researchers have not been able to find a common set of causes, a definite psyche-soma division, or even clear-cut classifications for these conditions. Each patient seems to have a complex and unique combination of symptoms and a unique expression of the condition.

These conditions are often depicted as outliers: atypical illnesses where standard causal explanation fails. They are then approached as epistemic problems, where a solution can be found by doing more of the same. In contrast, we take the problem of MUS to be a symptom of a deeper philosophical problem: how to detect causation in cases of complexity and heterogeneity.

Hume thought we could only understand causation as a relation of regularity between discrete, essentially unconnected types of event. From this, an orthodoxy has developed which has affected the way causation is understood within the medical model: 1) robust correlations, 2) difference-making, 3) probability raising, 4) same cause, same effect. This paradigm is tacitly accepted in many scientific methodologies, especially in the health sciences. Evidence based medicine is premised on the idea that what is true of a given population should be directly applicable in individual clinical decisions. What works for most people should also work for the patient. Such external validity only holds if we assume that individual propensities can be derived directly from statistical frequencies.

An alternative to this orthodoxy is a recently developed theory of causation, called causal dispositionalism. This theory emphasises complexity, context-sensitivity, tendency, singularism and holism. While these features are problematic for the orthodox understanding of causation, they are central to MUS and other complex diseases. By changing our philosophical framework for understanding causation, we must also change our scientific practice. This includes upgrading the status of clinical experience and mechanistic knowledge. Methodologically, this means that experimental methods and N of 1 studies should be favoured over statistical methods.

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A Critical Medical Humanities Approach to the Clinical Practice and Science of Breathlessness

- Jane Macnaughton (Centre for Medical Humanities, Durham University, jane.macnaughton@durham.ac.uk)

Abstract. Breathing is ubiquitous and an often neglected aspect of embodied life. It is only when breathing turns into breathlessness that it becomes noticeable, and, if acute or chronic, a medical problem. This paper examines how current critical medical humanities research can shed new light on the symptom of breathlessness. I suggest that there is an epistemic gap between clinical knowledge about the physiology of breathing and breathlessness and the cultural significance of breath and breathing which seeps into the clinic in ways hitherto unexamined. In a project which has been funded by the Wellcome Trust at the Universities of Durham and Bristol (UK) we propose to bring the two forms of knowledge into dialogue, by engaging clinicians with knowledge gleaned by medical humanities, humanities and social science work on breathing and breathlessness. This project suggests that an understanding of the complex cultural, existential and spiritual meanings of breath will contribute to better clinical understanding of this common yet neglected symptom.

There are significant challenges presented by attempting practically to engage clinicians in what the humanities (literary/cultural insights as well as philosophical) have to offer their science and practice, and the first of these is actually finding and route of entry through questions that the epistemic approach of biomedicine has found it difficult to answer. In the case of breathlessness, there are three key issues that are of concern to clinicians. First, conditions causing breathlessness are a significant global health burden. Chronic obstructive pulmonary disease (for example) is currently the 4th largest cause of death in developed countries. Crucially, in the UK at least, a significant number of those who suffer from this condition remain undiagnosed and untreated for a range of reasons, including stigma associated with smoking. Second, breathlessness as a symptom has very few treatment options when therapy for the underlying cause has been exhausted. This is partly because the clinical approach focuses not on the symptom (what is experienced by the patient) but on the pathological cause (what can be demonstrated by clinical science and investigation). There is a clear mismatch between measured lung function and experienced breathlessness, which puzzles clinicians, but which has led to new modeling of the mechanisms of breathlessness through neuroimaging. This takes me to the third issue of focus for clinicians: the neuroscience of breathlessness. This relatively new science is predicated on the fact that breathlessness is under not only involuntary but also voluntary control, and that sites of interest must be sought not only in the brain stem but also in the cortex in functional MRI studies. The route map for this science has so far been pain studies, but there are significant phenomenological differences between pain and breathlessness that may mean this approach may not yield accurate results.

This paper will give an overview of how the humanities (in a broad sense) can become entangled with clinical science and in particular enable more accurate approaches to brain imaging in the field of breathlessness research.

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Evidence Based Medicine Versus Expertise: Understanding Epistemic Actions in Clinical Practice

– Sophie van Baalen (Department of Philosophy, University of Twente, s.j.vanbaalen@utwente.nl)

Abstract. Evidence Based Medicine (EBM) was introduced with the aim to make clinical decisions more "scientific." One way to realize this is the "hierarchy of evidence," in which evidence obtained from (systematic review of) randomized controlled trials (RCT's) is placed on top, and "basic science" and "expert opinion" below. Recently, we have argued that knowledge from basic sciences is crucial in clinical decision making, as it allows doctors to bring together heterogeneous pieces of information (including outcomes from RCT's and specificities of the patient) to construct a coherent "picture" of the individual (Baalen and Boon, 2014). We consider this ability one of the key intellectual challenges of doctors. The constructed "picture" is consequently used as an epistemic tool in reasoning about the diagnosis and treatment of that patient. Furthermore, instead of deferring their responsibility to rule-based reasoning and strict clinical guidelines, as promoted by EBM, doctors have epistemological responsibility (van Baalen and Boon, 2014).

In this paper, I will argue that, instead of referring the professional expertise of doctors to the bottom of the hierarchy, their specific expertise should be given a central role in thinking about their

epistemological responsibility. First, I will claim that constructing a coherent "picture" requires a great deal of expertise. The danger of this claim is that it might initiate a return to "authority-based" decision making. Therefore, secondly, I claim that thinking about this "picture" as an epistemic tool for reasoning about diagnosis and treatment enables to clarify what the professional expertise of doctors consists of, avoiding that too much emphasis is put on "authority".

Collins and Evans (2007) argue that an important aspect of expertise is "tacit knowledge", which cannot be expressed in formal language, but has to be attained through "enculturation". The structure of medical education (formal education in basic knowledge of the human body, diseases and treatment, followed by years of apprenticeship learning as intern, resident or fellow) reveals that medicine should also be considered an expertise of this kind. Nevertheless, to ensure a certain quality of clinical decision making, referring to tacit knowledge is not enough, and a closer examination of the professional expertise of doctors is needed.

Besides skills (e.g. communication and surgical skills) and epistemic content (e.g. basic knowledge, knowledge of treatments and up-to-date knowledge of medical science), thinking about epistemic challenges and epistemic tools reveals that another aspect of expertise is crucial for medical professionals, namely, epistemic actions. This includes the gathering of relevant information from the patient, literature and other sources, critical assessment of information, and medical reasoning. Therefore, I will argue that epistemic actions are crucial for constructing a coherent picture.

In this paper, I will argue that explicating the tacit aspect of expertise through analysis of epistemic actions allows to assign expertise a central role in clinical decision-making, without having to refer to "subjective" qualities, like "intuition" or "authority". Secondly, it allows to teach young doctors relevant skills to become medical experts. Last, it offers a viable alternative to EBM that warrants a certain quality of clinical decision-making by developing the epistemological responsibility of doctors, instead of prescribing algorithmic reasoning.

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Parallel Session 3C

Wednesday, 24 June 2015 at 15:30–17:30 in G2

Session chair: Andrea Woody (University of Washington)

❧

Diagrams as Both Representation and Practice in Developing Mechanistic Cell Models

- Yin Chung Au (UCL Department of Science and Technology Studies)

Abstract. This study has done both quantitative and qualitative analyses on the diagrams of mechanistic models for cell phenomena, circa 1970s–2005. The results from examining over 3,500 papers across eight journals show that mechanism diagrams play a crucial role in the practice of developing mechanistic explanations for cell biology. This study argues that mechanism diagrams serve as both representation and practice in model-development. Such a role transcends the dichotomy between static visual representations and dynamic research processes. This study borrows the phrase "state of becoming" from cartography to characterise the mechanism diagrams in cell biology. Below introduces the two aspects of this state.

The first aspect is the historicity embedded in biological diagrams. The components of mechanism diagrams not only represent the knowns – including entities, activities, and their relationships – but

also represent how these components have come to be the knowns. The process includes continual data-gathering, model-building, and error-correcting etc. Moreover, it involves the inter-field and inter-level interactions between different perspectives for the same phenomena. This is due to the complex nature of biological mechanisms. This study treats mechanism diagrams as a communicative device that acts in the research dynamics. The communication takes place not only between different researchers in the field but also between the same researchers at different stages of model developing. Meanwhile, the development of the representational signs has its history, too. This study imports art-historical method to examine the visual elements in biological diagrams, showing that the visual conventions in mechanism diagrams are not given but have evolutionary processes. Neither the represented nor the representation is fixed. They have been developed and are still open to evolution. Cell biologists are both the author and the viewer with trained interpretation. Such interpretation decodes the meanings of conventional signs, and simultaneously decodes the historicity embedded in the making of these conventions.

The second aspect is the deep involvement of mechanism diagrams in both the defining of research arenas and the intervention in the mechanisms. Both activities extend into the future and cannot be separated from the aforementioned dynamics of communication. Mechanism diagrams engage the user through bringing about new problems and activating the future research. Bechtel (2013) has demonstrated this with diagrams that contain question marks. This survey also includes many examples that visualise the underdetermined parts of the models. The diagrams integrate the knowns and the unknowns for visual reasoning. The unknowns are visualised so that they can become actual in the future. My analysis on visual elements and compositions of mechanism diagrams suggest that cell biologists reason with diagrams while developing the visual languages. Through such reasoning, the mechanisms of interest are constantly defined and redefined, greatly according to the pragmatic purpose to control the mechanisms.

In sum, mechanism diagrams, while appearing as static representations, are constantly in the state of becoming. This is because they embody the research dynamics, and that the making of them is an important part of knowing and controlling what they represent.

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Computer Data Processing and Its Impact on the Interpretation of Digital Images

– Vincent Israel-Jost (Université Catholique de Louvain-la-Neuve)

Abstract. Digital data processing has many particularly interesting applications in scientific imaging. Today, as most imaging instruments have become digital — they produce data in numeric form, as lists or matrices of numbers – images can be mathematically reconstructed in 3D from 2D projections, or blur and various artifacts can be corrected for by algorithms. These practices have become so common in scientific imaging that it is now more appropriate to talk about imaging systems, which combine an instrument and a computer, than about imaging instruments that are rarely used in an autonomous way.

The analysis of the practices of image processing has led to very little philosophical investigations. So far, most contributions have dealt separately with instruments (e.g. (Hacking 1981) on the microscope) or with computer models and simulations (Winsberg 2001, Humphreys 2009). In the literature on instruments, there is no reference to the possibility to add computer processing to the raw data produced by the instrument, either because many of the most famous contributions on the topic precede the digital era, or because this possibility has been ignored since. The contributions on models and simulations, which are more recent in general, have focused on the use of computers alone, and particularly on the experimental character of results that have been obtained through the exploration of computer models of phenomena. In this communication, I will consider jointly instruments and computers to study the role that image processing plays in the empirical investigation. More precisely, I will take the point of view of agents (the investigators) and ask in what respect image processing is beneficial for them. Why, for instance, is it desirable to use images that have been obtained less directly and that have been altered by algorithms?

The main thesis regarding the interest to perform image processing is that it renders images less demanding for the investigator who is in charge of interpreting them. In spite of a further

sophistication in the production of images, which could lead to more elements to take into account into the interpretation, algorithms of data processing are aimed to facilitate interpretation. In fact, I will argue that they realize a kind of pre-interpretation, because they perform certain tasks that would otherwise be under the investigator's responsibility: subtracting noise or removing artifacts are done mentally during the interpretation when images haven't been processed. As a result, there is an economy of skills and knowledge brought by image processing. The rest of the communication will discuss potential dangers of these new practices, especially with regards to the objectivity of processed images.

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Diagrams of Sound and Vision

- Sabine Brauckmann (University of Tartu)
- Sara Franceschelli (ENS Lyon)

Abstract. In an essay about the atomistic iconography Christoph Lüthy's presents globules as visual symbols for distinct theories of matter. In a similar vein we want to put up for discussion diagrams of cell lineages and the notational systems of modern computer music (e.g., Ligeti, Lutoslawski, Xenakis). Our point of departure is to challenge the conviction that the eye is the most important tool for recognizing patterns and forms in nature. For, the ear also identifies spatial patterns, or the gestalt of a sonification that the composer depicts in a specific notational system, creating audible sounds by inaudible structures. To foster our argument, we will compare cell lineage diagrams around 1900 to the graphic notational systems of music in the 1960s. For ordering and classifying dividing cells, the cytologists created 'data displays' that resemble the graphic systems of modern music. We will survey Carl Naegeli's approach to figure cell-formation of peat mosses, Wilhelm Hofmeister's attempt to include geometric figures into Naegeli's arithmetic series and Maupas' diagram of Paramecium. When comparing their diagrams, it looks as if the biologists had developed sophisticated graphic methods for representing emergent shapes nearly 100 years before the mid-20th century appearance of graphical music notation. The hypothesis therefore is that the cell diagrams (e.g., Hofmeister, Maupas) resonate with the computational notational systems of algorithmic music composition. For, both notational systems, or data displays, provide a structurally similar form of reasoning, regardless of whether we observe an object (cell) or perceive a sense data (sound). For example, processes of cell divisions partly resemble the theoretical approaches of computer musicians tracing the sound as processes of fraction, interval and spinning turns of semi-development. Moreover, the combination of these notational practices also reminds of Helmholtz' work on the 'acoustic image' in the 1860s and his transdisciplinary approach of 'inclusive research'. A preliminary argument we want to explore here is that the shape of sound in sensu Helmholtz represents the acoustic equivalent of a dynamic motion that is fixed in space by notation and coding (Boulez). Our objectives are (1) to entangle sounds (sonification) and cells, and (2) to disclose the epistemic, aesthetic, and methodic similarity, or difference between these gestalten, which come into being either by software, or by the narratives of wet experimenting in the laboratory.

Parallel Session 3D

Wednesday, 24 June 2015 at 15:30–17:30 in G3

Session chair: Mieke Boon (University of Twente)

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Expanding the Experimental Realm: An Account of Descriptive and Functional Experimentation in the Natural Sciences

– Stephan Guttinger (Egenis, University of Exeter)

Abstract. Since the 1980s philosophers of science have moved away from a narrow understanding of experimentation as a test instance for theory-derived hypotheses. A key product of this shift in perspective was the idea that besides theory-driven experimentation (TDE) there is also 'exploratory experimentation' (EE), a practice free of theory-guidance that can be used to explore new phenomena or regularities that are not captured by existing theories. Intriguingly, the TDE/EE distinction seems to be where the process of expanding the picture of the experimental realm has stopped, as it has not been supplemented with further/alternative distinctions between experimental practices.

In this paper I want to take up again the task of expanding our understanding of the realm of experimental practices by looking more closely at a distinction that is often used by scientists but which has so far not found much attention in philosophy of science, namely the distinction between descriptive and functional experimentation (DE and FE resp.). The goal will be to spell out what this distinction amounts to in functional terms (i.e. the role the different practices play in the scientific context) and to identify some of the distinguishing features of the different practices.

To develop a more detailed understanding of the DE/FE distinction I will analyse an experimental system that can be used for both practices, namely the in vitro binding assay. The analysis of the different uses of this system will show that the DE/FE distinction does not map 1:1 onto the TDE/EE distinction, implying that it is indeed an independent category of experimental practices.

The analysis of the in vitro binding assay will also show that an understanding of the role FE plays in scientific practice has to be tightly linked to Robert Cummins' account of functional analysis; using FE scientists don't just hunt for causal connections but for causal roles that particular entities or processes play in a larger system. DE on the other hand does not give causal knowledge about the system of interest, even though it makes use of the same interventions as FE. It is this difference between general causal insight and insight into the causal role of an entity or process that marks one of the key differences between FE and DE. This characterisation of the different practices also highlights the need for a more elaborate understanding of how 'intervention' or 'manipulation' relates to causal insight generated in the experimental sciences.

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Is Rigorous Measurement of Statistical Evidence Possible?

– Veronica Vieland (The Research Institute at Nationwide Children's Hospital)

Abstract. Statistical analysis is an increasingly important component of scientific research in the biological sciences and elsewhere, particularly in the era of genomics and other data-intensive areas of investigation. Statistics can serve many purposes (hypothesis testing, parameter estimation, etc.), but for working scientists, the primary outcome of interest of a statistical analysis is often the strength of the evidence for or against hypotheses of interest on given data. Arguably, this is what drives the ubiquitous scientific practice of interpreting the p-value as if it were a measure of evidence, and more than that, as if it were a calibrated measure – that is, one that can be meaningfully compared across experiments, across time points as data accrue and even across experimental domains. It is well known that relying on p-values as evidence measures can cause errors in the interpretation of data, and easy to show that these errors can be substantial, leading to entirely wrong conclusions.

Yet the practice persists, indicating a compelling scientific need for evidence measurement in the absence of a satisfactory statistical measurement procedure.

Statisticians and philosophers alike have considered the nature of statistical evidence in a literature extending back to the early 20th century, and various definitions of statistical evidence have been proposed (e.g., the likelihood ratio [LR] or Bayes factor [BF]). But if there is any general consensus on the subject, it seems to be that rigorous measurement of statistical evidence is, in general, impossible. In this paper I will motivate the underlying problem anew from the nomic measurement perspective [Chang, Inventing Temperature], starting with a fundamental measurement question: How can we be sure that our measure of evidence is correctly mapping onto the underlying quantity of interest, the evidence itself? We need a way to rigorously map observable (or computable) features of statistical systems (such as LRs or BFs) onto the true underlying evidence via some function. But how can we discover or verify the function without first having some independent means of knowing what is the true evidence? Posing the question in this way suggests the relevance of precedents from physics, especially development of the Kelvin temperature scale; it also invokes measurement theory as developed by Suppes, Narens et al., which has to my knowledge never been invoked in this context. I will argue that the problem of evidence measurement is tractable, but only once we take a step back from standard statistical precepts and adopt the measurement perspective. I will focus here on philosophical aspects of "live" (not yet settled) nomic measurement problems, which present an opportunity for philosophers of science to make a direct and very practical contribution to the day-to-day practice of scientific research.

❧

Measurement and Metrology Post-Maxwell: A Historical, Philosophical, and Mathematical Primer

– Daniel Mitchell (University of Cambridge)

Abstract. It is well known to historians that the Committee on Electrical Units convened at the International Electrical Congress of 1881 endorsed the CGS electromagnetic system of units for practical electrical measurement. Narratives of processes of electrical standardization typically move on to describe the intricate measurements required to establish the magnitudes of the units, particularly the ohm, and the development of associated material standards, in the 1880s and beyond. Those that tackle a more extended time period typically structure their narrative around key decisions taken at international meetings, which, quite naturally, informed subsequent experimental work at the local level.

This congress-centric historiography, however, has left much animated discussion about systems of electrical measurement and their scientific merits unexamined, particularly when practical needs predominated over strictly scientific ones. Such discussion incorporated a wide range of established mathematical principles and empirical laws, novel mathematical notations and physical theories, plausible physical conceptions, and even the latest quantitative data concerning electrical and magnetic media. No single actor could have laid claim to mastery over all these aspects of the field. Misapprehension abounded as metrology slid into metaphysics.

This paper is intended as a primer to encourage historians and philosophers of science to explore the issues that came to light, many of which were either left unresolved or remain subject to debate today. It centres on the French response to Maxwell's Treatise, which provided an essential touchstone for the many works on absolute electrical units that appeared during the 1880s. Maxwell's new form of dimensional analysis as presented in the Treatise left many issues open, not least the viability of the mathematical grammar itself, and the variety of possible inferences and interpretations to which it gave rise.

The disciplinary separation of mathematical and experimental physics in France, as well as the distinctness of electrical science from electrical practice, resulted in an impressive diversity of analysis. When combined with characteristically French philosophical sensibilities, this provides a conceptually-rich point of entry into Europe-wide discussions concerning the scientific foundations of electrical metrology and measurement during the late-nineteenth century and beyond.

The paper starts with the implicit attribution of the claim that 'resistance is a speed' to members of the BA Committee on electrical units by French mathematicians, who were skeptical of the practice-oriented British preference for the electromagnetic system (and, more generally, Maxwell's electrodynamics). I investigate the veracity of this attribution, which concerns the possible interpretations of dimensional formulae: operational, physical, and mathematical. This leads me to a similar analysis of the various interpretations and derivations of the physical constant nu, and finally into electrical ontology through the relationship between charge and current, and the role of the medium in electrical and magnetic effects. In this way I lay the foundations for a new reading of Maxwell's Treatise in which his concern with systems of electrical units, dimensional analysis, and methods of measurement connects with the familiar story about field theory and the electromagnetic theory of light.

Parallel Session 3E

Wednesday, 24 June 2015 at 15:30–17:30 in G4

Session chair: Joseph Rouse (Wesleyan University)

❧

Collaboration And Explanatory Models

– Melinda Fagan (University of Utah)

Abstract. It is well-established that collaboration among researchers is prevalent in scientific inquiry, past and present. Philosophically-informed case studies examine how researchers collaborate, or fail to, across fields and disciplines in physical, life and social sciences (e.g., MacLeod and Nersessian 2014). Andersen and Wagenknecht (2013) propose a general framework for examining scientific collaboration in terms of patterns of epistemic dependence relations, which highlights integration and connection of knowledge from disparate sources. Their social epistemic approach links empirically-based studies of scientific collaboration with philosophical debates about collective scientific knowledge and cooperative activity (reviewed in Fagan 2012a).

This paper extends the social epistemic framework to engage scientific explanation. Recent accounts of mechanistic explanation are informed by careful study of experimental practices of mechanism discovery, particularly in biology and neuroscience (e.g., Bechtel and Richardson 1993, Machamer et al 2000, Bechtel and Abrahamsen 2005, Darden 2006, Craver 2007). However, philosophical discussion is dominated by causal aspects of mechanistic explanation, linking to classic debates about the nature of causality, methods of acquiring knowledge of causes, and the ontic status of mechanisms. This paper explores aspects of mechanistic explanation that have received far less attention.

Building on earlier research (Fagan 2012b), I argue that mechanistic explanation in life science exhibits the same basic structure as scientific collaborations in a social epistemic framework: a 'lower level' of non-interchangeable units organized in a dynamic pattern of specific interactions, which constitutes a 'higher level' system. In biological models, the units are molecules and macromolecular structures, while the system is a cell, tissue, or organism. In models of collaboration, the units are individual members, the system a scientific group. Theories of the chemical bond offer a third case, accounting for the structure and properties of molecules in terms of the arrangement of interacting (paired) electrons and their relations to atomic nuclei.

All three cases share several features: multiple levels, with explanation 'directed' from lower to higher; heterogeneous units and interactions at the lower level; and a crucial role for organization in linking levels. I discuss how these features contribute to the epistemic payoff of successful explanation, traditionally characterized in terms of successful prediction, simplicity, or control. The explanatory structure of multiple levels involves, I propose, equipoise among various epistemic

payoffs for explanation. The model simplifies the lower level by displaying how heterogeneous components fit together into an overall system, like the fragmented mess of a jigsaw puzzle's thousand pieces forms a coherent image as the puzzle is completed. On the other hand, the overall system is explained in terms of its parts, which we can know through concrete experimental manipulation, everyday practical experience, mathematical theory, or some combination thereof – methods that enable prediction and control of the parts. Insofar as the parts' organization is amenable to the same methods, behavior of the whole system can be predicted/controlled. So there is a balance of epistemic payoffs among different levels: top-down simplicity, bottom-up prediction and control. I conclude by discussing how this view of explanation can enrich our understanding of scientific collaboration.

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❧

Model Coupling in Resource economics: Conditions for Effective Interdisciplinary Collaboration

- Michiru Nagatsu (University of Helsinki)
- Miles MacLeod (University of Helsinki)

Abstract. Recent years have seen a burgeoning interest in promoting and analyzing interdisciplinary collaboration in the natural and social sciences by researchers and university administrators. There is now a substantial collection of academic work and policy documents available on the subject. Most interdisciplinarity research so far has been the domain of science policy and science and technology studies. This research has focused on the institutional, organizational and social dimensions of scientific research that promote or inhibit interdisciplinary interactions, while developing policy frameworks and guidelines for structuring scientific institutions and organization to promote interdisciplinary interactions (see e.g. Gibbons et al. 2004). What is largely missing however is actual case-based study of how the available cognitive resources of different scientific fields and disciplines – their extant theories, modeling templates, experimental and evidential resources – get combined to create functional collaborative platforms for investigation and problem-solving, as well as precise descriptions of what is gained from these combinations. Discussions have identified the need for researchers to integrate values, goals, methods and so on in order to collaborate, but with little

concrete guidelines or case studies of how this can happen conceptually and methodologically (see Mattila 2005 as one of the few existing case studies of this kind).

We have here an opportunity for philosophers to actually contribute their expertise on the conceptual and methodological side of scientific processes, to formulate more informed policy criteria on how to construct effective interdisciplinary collaborations. While the philosophical literature studying the explanatory affordances of different types of conceptual and methodological integration is starting to grow, there are yet few philosophical investigations exploring how effective a strategy of integration might be in creating functional collaborative problem-solving platforms given the constraints and difficulties of interdisciplinary research. Our goal in this paper is to demonstrate that conceptual frameworks developed in order to integrate background models and model-building practices can be structured in ways that facilitate collaborative responses to problems. Such frameworks can thus be measured and analyzed according to their ability to facilitate effective and gainful collaborative responses.

We identify and examine one such relatively clear conceptual framework for integrating ecological and economic models developed through successful collaborative interactions between groups of resource economists and ecologists. Their interdisciplinary interaction relies upon what we call a coupled model framework. After a brief introduction to current interdisciplinary studies (section 2) and integration in economics and ecology (section 3), we show how various features of this framework serve to demarcate the nature and structure of the collaboration required between ecologists and economists (section 4). Further using this case we apply two informal measures for assessing the degree to which this conceptual framework generates effective collaboration in practice; by assessing 1) the features of the framework that facilitate efficient collaborative interaction in practice given the various constraints of working across disciplinary boundaries (collaborative affordances), and 2) the gain these approaches afford through the agency of collaboration in comparison with what could be achieved working purely with one's disciplinary resources and skills (collaborative gain) (section 5). From this information we draw several lessons on both the affordances of this kind of methodological set-up for interdisciplinary research in ecology and economics, and interdisciplinarity more broadly (section 6). We conclude by summarizing our arguments (section 7).

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Authorship, Collaboration, and Joint Commitment

– Haixin Dang (Department of History and Philosophy of Science, University of Pittsburgh)

Abstract. In this paper, I argue that philosophers of science ought to pay more attention to issues surrounding scientific authorship. Science has become an increasingly collaborative enterprise. Today the overwhelming majority of scientific papers published are multi-authored. Authorship allocation is a fraught issue among scientists. Different journals have taken different policies towards how collaborators are treated as authors. For example, in 2009 Nature has revised their authorship policy to require senior members of collaborations to formally take responsibility of the content of the paper and also require authors to explicitly list their contributions to the paper. A statement of author contributions is now required for publication. The ICMJE (International Committee of Medical Journal Editors), on the other hand, requires a specific set of requirements to be met for authorship and emphasizes the condition that all listed authors must be accountable for all aspects of the work. Further problems arise when the work is produced by an extremely large group of scientists, as in the case in high-energy physics. Journal editors are struggling with defining authorship, credit, and accountability in a time when these concepts are being challenged by how scientists collaborate. The modern fragmentation of scientific authorship has been examined by historians and STS scholars (i.e. Biagioli & Galison 2002), but little discussed by philosophers, besides Wray (2006). In this paper, I work towards a further philosophical understanding of scientific authorship and collaboration in contemporary science. I take disputes in authorship allocation as an indicator of how collaborations function.

In the first part of the paper, I outline the relationship between different kinds of collaborations and authorship policies (both formal and de facto). Drawing from my larger project on the epistemology of scientific collaborations, I define scientific collaborations as a specific way collaborators

share goals and intentions, but most importantly information and data. I argue that differences in authorship allocation correspond to how information and data are generated, shared and interpreted. Here I will introduce a taxonomy of collaborations according to their structure. Large-scale collaborations, like ATLAS, treat authors in an egalitarian way because information is circulated throughout the collaboration strategically in which each member of the collaboration plays a role in generating and/or verifying results. Small-scale collaborations, like labs, can have a more top-down structure in which senior members play a more crucial role in directing how information is shared among the collaboration.

In the second part of the paper, I use Gilbert's concept of joint commitment to examine issues surrounding scientific authorship. Co-authors can be said to have joint committed to the content of their paper. Joint commitment is established among collaborators as they negotiate their shared goals and intentions. I will discuss the process of bringing about joint commitment in collaborations. But the final section of the paper will focus on the normative force of joint commitment. Authorship not only designates credit, but also responsibilities. Collaborators hold each other to accountable for their contributions and place epistemic trust in each other. I will argue that ultimately we need a more robust account of joint commitment to capture the complexities of authorship.

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❧

Philosophy of Citizen Science in Practice

- Kristian H. Nielsen (Aarhus University)

Abstract. There are many ways in which to practice citizen science. This paper first makes a distinction between three types of citizen science and provides contemporary examples of each type. It then links the diversity of citizen science in practice to its philosophical and sociological interpretations. It will be argued that citizen science embeds divergent, often conflicting, assumptions about the means and ends of science and the role of the citizen/scientist in contemporary democracy. The philosophical and practical challenges of citizen science are to understand how and why these conflicting meanings co-exist and interact (Lewenstein, 2004).

A typology of citizen science based on Lewenstein (2004):

- Citizen science1: Scientific work undertaken by members of the general public, often in collaboration with or under the direction of professional scientists and scientific institutions. Examples of citizen science1 include amateur astronomy, bird counts, distributed computing, and gamification.
- Citizen science2: Participation of nonscientists in decision-making about policy issues that have scientific or technological components. Examples are consensus conferences, citizen juries, and protest movements.
- Citizen science3: Participation of scientists and engineers in public debate and policy-making. Examples include the Intergovernmental Panel on Climate Change and other bodies established to link science and policy-making.

Citizen science1 has fuelled debates about the nature of scientific expertise and the (proper) practice of science. Scientists normally are defined by means of their specialist knowledge and certified competencies. This definition is challenged by citizen scientists1 with little or no formal training in science who appears to be able to contribute to scientific research. Harry Collins and Robert Evans (2002) coined the term "interactional expertise" to denote situations in which laypersons acquire enough expertise to talk the language of specialized science, but still are not able to produce actual

science. Other sociologists of science such as Michel Callon and coworkers have argued that citizen scientice1 is part and parcel of the emergence of new modes of research where what counts as expertise and competence is open to negotiation and change (Callon, Lacoumes, & Barthe, 2009).

The challenge of citizen science2+3 turns on the role of science and expertise in public affairs. Alan Irwin (1995) for example used the term "citizen science" not only to understand how environmental issues often generate counter-expertise, ambivalent public attitudes towards science, and reflexivity about risks, but also to open for more equal relationships between scientific and nonscientific understandings and expertise. In particular, he argued that the role of science in public affairs often is heterogeneous and context-dependent. Examples such as consensus conferences where citizens and experts meet to deliberate environmental and ethical issues show that citizen science2 sometimes is mixed with citizen science3.

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Parallel Session 4B

Thursday, 25 June 2015 at 09:00–11:00 in G1

Session chair: Stephanie Wykstra (Innovations for Poverty Action)

❧

From Cells to Society (and Back): Epistemic Challenges and Political Implications of a Novel Approach in Public Health

- Alexandra Soulier (INSERM)
- Caroline Guibet-Lafaye (CNRS)

Abstract. Since the recent growth of social epigenetics, a post-genomic science which examines within a molecular framework the impact of environmental context and social behaviors on human physiology, to move "from cell to society" has become a tempting program for public health researchers. Emerging frameworks explicitly link epigenetic regulation to social regulation in an attempt to capture social-to-biological transitions and make use of these explanatory models to guide public intervention.

The union of epigenetics and epidemiology is particularly fecund in social epidemiology, the discipline that explains how socio-structural factors influence the distribution of states of health within a population. The additional support brought by biology to the search for social determinants of health extends the analytical power of social epidemiology and hereby entails two (interrelated) effects:

- Epidemiologically-produced associations (that link social exposure to health and behavioral outcomes) once anchored into molecular analyses are provided with a firm explanatory basis. With the support of biological evidence, social epidemiology gains scientific legitimacy as explanations move from providing associations to proving causality;

– The deciphering of the biological mechanisms that render bodies' development and ageing dependent from social conditions brings novel opportunities of intervention over life. Based on the perception that social environments and behaviors are mediums for gene regulation, public health agents develop innovative forms of social management, beyond individuals' agency.

Either in its explanatory or regulatory version – as a matter of scientific exploration or public intervention – the "from cells to society" approach hypothesizes the continuity of biological and social phenomena and the commensurability of biological and social knowledge. A metaphysical and epistemic commitment is therefore required to develop an integrative framework that intends to follow the (assumed) molecular paths that connect the 'social' to the 'biological' and to make sense of their connection in a causative fashion. The level of synthesis required to achieve such inter-theoretic explanation relies on hypotheses that cannot be said parsimonious. Hence our questioning as to the enthusiastic commitment to the "cells to society" approach in public health.

The following contribution consists in an attempt to examine the epistemic challenges and political implications of the recent rapprochement between social epigenetics and social epidemiology in public health. We propose to examine the social epistemology of the approach called "from cells to society" from different perspectives. From a historical point of view, the integration of molecular insights in epidemiological frameworks can be presented as an opportunity to "harden" the causal claims of epidemiology and to provide a strategic response to the identity struggles of the discipline. This rapprochement has however consequences in terms of research practices and the conception of the modes of intervention. We therefore propose to test the hypothesis that biological and social knowledge are commensurable through a comparison of the nature of 'the social' that social epidemiology explores in comparison with the material objects that stand for 'the social' in epigenetics' laboratory practice. These philosophical insights serve as a basis for our (bio)political analysis of the modes of public health intervention that rely on the regulation of social environments.

❧

A Change in Practice?: A Reevaluation of Mechanistic Reasoning and Clinical Experience in Post-grade Evidence-Based Medicine

– Sarah Wieten (Durham University)

Abstract. In early forms of Evidence Based Medicine, mechanistic reasoning and experience were sharply critiqued in comparison with the "gold-standard" of evidence, randomized controlled trials. EBM supporters argued that these critiques were necessary in light of the authoritarian pedagogical strategies of medical school, where students were taught to do as their more experienced teachers did, and the horrors of past medical practices based on mistaken conceptions of the body's mechanisms. Both philosophers and clinicians pushed back against this denigration of experience and mechanistic reasoning, arguing, among other things, that RCT's failed to live up to EBMs high expectations, and that the problem with historical dangerous medical methods was a poor understanding of the body's mechanisms, not that they relied on mechanisms at all. In response, later versions of EBM added these sources of medical knowledge into the hierarchy of evidence, although at very low positions in various rankings.

The most recent of these reconceptualization of EBM are the GRADE standards of evidence, which are now taken to be the unified account of EBM. Indeed, some critics of EBM have been chastised for not engaging with GRADE, but continuing to critique other earlier versions of EBM.

In this paper, I will examine the version of EBM expressed through GRADE to see if its treatment of clinical experience and mechanistic reasoning, important components of the clinical encounter in practice, vary from earlier versions of EBM. I will argue that the GRADE system's treatment of clinical expertise and mechanistic reasoning is not fundamentally different from the earlier EBM stances on these topics, and so philosophical critiques on these topics apply equally to the new system. Lastly I will discuss what implications this continual denigration of clinical expertise and mechanistic reasoning means for clinical practice and where, if anywhere, they might fit in to a future account of EBM.

❧

Pluralism Into the Pasteur's Quadrant: From the Study of Human Behavior to Cancerology

- Baptiste Bedessem (Laboratoire Philosophie, Pratiques et Langages)
- Stéphanie Ruphy (Laboratoire Philosophie, Pratiques et Langages)

Abstract. The plurality of representations in science is an important source of epistemological and metaphysical controversies. One of the major conflicts develops around the following questions: can we reach a unified vision of the external world? Or science can only provide us a patchwork of disjointed explanations, more or less compatible?

Out of the purest metaphysical plan, it appears that the concrete impact of these interrogations strongly depends on the field considered. For instance, the (non)-existence of plurality has not the same consequences in cosmology and in biomedicine. In the second case, a constitutive tension exists in between the desire to explain and the necessity to cure and it seems that plurality may not be accepted in the same way at each extremity of this spectrum -in a biologist's or in a clinician's mind. As a consequence, the question of plurality should be linked to the opposition between « pure » and « applied », or « use-inspired », research.

We first present in this paper the recent work of H.Longino, about the study of human behavior, as an interesting approach to illustrate this view To her, theoretical attempts at explaining human behavior and its correlative pathologies tend to generate sterile debates focused on irreconcilable positions, such as nature and culture. Longino argues that this attitude directly comes from a rejection of pluralism. By searching a unique explicative model for human behaviour, scientists naturally create fundamental opposition to justify the exclusivity of their approach. Yet, to her, irreducible pluralism has to be preferred to monism as a more relevant epistemological guideline. By contrasting this tendency of fundamentalist scientists with the clinical approach, Longino builds the idea of pragmatic pluralism.

To do so, she uses the works of the psychiatrist K-S.Kendler to defend the epistemic value of an approach focused on clinical practice. This one would bypass the monism rising from basic science by building descriptions of mental illness which are not grounded on theoretical concept, but on the effects of the therapeutic agents used in clinical practice, through the use of the interventionist model of causality, developed by J.Woodward.

Our paper aims to show how the field of cancerology can bring interesting additional remarks to the work of H.Longino. First, it appears that the controversy which opposes the two main theories of cacinogenesis (SMT and TOFT) has generated a sterile conflict between reductionism and holism, symmetrical to the debate between nature and culture denounced by H. Longino.

Secondly, some elements taken from the history of the fight against cancer tend to comfort the idea of an epistemic value of use-inspired research. This form of investigation, directly influenced by clinical needs, is efficient to generate global, valuable, knowledge. This idea has to be compared to the conclusions of T.Wilholt about research in industrial context.

Besides, use-inspired research seems to be less sensitive to monism than fundamental biology of cancer, indicating that the « Pasteur's Quadrant » could then be a « pluralist » space.

❧

Meta-analysis and the Ideals of Objectivity

- Saana Jukola (University of Jyväskylä)

Abstract. Meta-analysis is a method of synthesizing information from two or more studies by using statistical techniques. In evidence-based medicine and policy, meta-analyses are often placed on the top of the evidence hierarchies, which represent the assumed strength of different types of evidence. Meta-analyses are thought to provide more precise information on the effects of treatments than individual studies (Cochrane Collaboration 1.2.2.) and to amalgamate evidence in a less biased way

than other means of synthesizing studies. This is because the formal rules of meta-analysis are supposed to ensure the objectivity of the process.

In his article (2011), Jacob Stegenga has argued that meta-analyses fail to be objective because conducting them involves making judgments. For instance, when choosing what primary data to analyse, a researcher needs to consider at least the following questions: What methodological quality criteria should the included studies meet? How to solve the problems caused by publication bias? (Stegenga 2011: 500–504.) In this paper, I show that Stegenga's reasoning is based on the so-called procedural ideal of objectivity, according to which judgments necessarily threaten objectivity. I shall argue that the ideal of procedural objectivity as the guiding rule in medical research should be abandoned. This is because the ideal, on the one hand, is practically unattainable, and, on the other hand, does not help to evaluate all of the practices that are relevant in producing reliable medical knowledge. For instance, Stegenga himself discusses publication bias and the lack of evidential diversity, i.e., basing treatment guidelines on evidence from randomized controlled trials only. The ideal of procedural objectivity does not fully capture why these issues are problematic, and thus does not give us tools for counteracting them.

The use of the concept "objective" is eminently complicated, as recent philosophical (e.g., Douglas 2004) and historical (e.g., Daston & Galison 2007) analyses demonstrate. In this paper, the focus is on the practical consequences of different understandings of what kinds of practices ensure objectivity, particularly in the context of medical research. By introducing a case in research on the possible suicide risk related to the use of selective serotonin reuptake inhibitors, I demonstrate the weaknesses of the procedural ideal of objectivity. In addition, I show why the so-called social view on objectivity succeeds better in accommodating 1) the way in which scientific research necessarily involves judgments, 2) the possible risks involved in research, and 3) the influence that the institutional context has on research activities. Adopting the social view helps us to see why the evidence produced by meta-analyses may be more reliable than the results of some other means of amalgamating evidence without having to adhering to the unattainable ideal of procedural objectivity.

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Parallel Session 4C

Thursday, 25 June 2015 at 09:00–11:00 in G2

Session chair: Julia Bursten (University of Pittsburgh)

❧

Making Sense of Theoretical Practices: Scripts, Scruples and the Mass of the Universe

- Jaco de Swart (University of Amsterdam)

Abstract. The scientific activities we could signify as "theoretical" – activities involving theories, formalisms, equations, and calculations – have enjoyed relatively little attention in studies of science in practice. As Bruno Latour put it: "almost no one has had the courage to do a careful anthropological study of formalism" (Latour, 1987, p. 246). Although there are some interesting exceptions, it seems that this 25-year-old observation has still not lost its accuracy. In this paper I take this observation seriously, and elaborate on some recent ideas of Latour to illustrate that a new and more performative terminology will provide tools to better approach theoretical practices.

These tools, I argue, can be found in Latour's most recent project, "An Inquiry into Modes of Existence" (2013), in a context that is not obviously related to the study of science. In the sections on 'Organisation' and 'Morality'

Latour analyzes acts of calculation as they appear in economic activity, where they are used to "express preferences, to establish quittances, to trace ends [and] to settle accounts" (Latour, 2013, p. 465). He deploys the notion of scripts – constraining narratives – and Frank Cochoy's notion of qualculation – quality-based judgements – to make sense of what he refers to as the scruples of organisational, moral and economic action. Although the context in which these notions are applied is different, I seek to demonstrate that the notions of scripts, scruples and qualculations are very suitable to study theory as a scientific practice.

To make this explicit, I make use of an example from early twentieth century physical cosmology: a short paper of Einstein and De Sitter (1932), and the application of what now are known as the "Friedmann equations" to calculate the mass density of the universe. The arguments, assumptions and calculations involved in this work exemplifies how activities in the production of theoretical knowledge can be understood in terms of the piling up of scripts and the coping with scruples. It becomes clear that extending Latour's new work to a context of theoretical science can indeed offer a valuable set of tools that helps to shift attention towards a more performative assessment of theory in practice. More specifically, I argue that the activity of making objects adequate, the process of adequation, plays a central role in such an analysis of the performance of theory. Contrasting this perspective with Latour's earlier focus on centres of calculation and their bookkeeping, I hope to create room for the practices of theoretical sciences to be followed more closely.

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Situating Styles of Reasoning

– Adam Toon (University of Exeter)

Abstract. In a series of influential articles, Ian Hacking has argued that we may identify a number of different styles of reasoning within scientific practice, each with its own history (e.g. 1982, 1992, 2012). Furthermore, in his earliest paper on styles, 'Language, Truth and Reason' (1982), Hacking argues that styles of reasoning lead to a form of relativism. His argument for this claim appeals to positivist theories of meaning: if the meaning of a proposition depends upon the style of reasoning appropriate to establishing its truth or falsehood, then the birth of a new style brings new propositions into being as candidates for truth or falsehood. As a result, styles cannot be subjected to independent criticism, since the propositions they evaluate have no meaning outside of the style.

In his more recent work on styles, Hacking has placed less emphasis on their relativistic implications. Two other developments are also important for the present paper. The first is that Hacking is keen to stress that styles of reasoning are not styles of thinking, since "thinking is too much in the head" and omits "the manipulative hand and the attentive eye" (1992, pp. 3 – 4). Styles involve an "embodied creature [that] uses not just its mind but its body to think and to act in the world" (2012, p. 600). The second important development is that Hacking now links styles of reasoning to a burgeoning form of inquiry that he calls cognitive history. This is "the study of how an organism with certain cognitive capacities, on a planet like this, developed (etc.)" (2012, p. 607), exemplified by works such as Renfrew, Frith, and Malafouris' The Sapient Mind: Archaeology Meets Neuroscience (2009, OUP).

Recently, one of the main proponents of such work, Lambros Malafouris, has argued that an appropriate theoretical framework for these studies can be found in a range of recent work in cognitive science, which goes by names such as situated, embodied, extended, and distributed cognition (Malafouris, 2013). Each of these approaches stresses the importance of interaction between the brain, body and environment in our cognitive processes. In this talk, I will ask how these frameworks might be brought to bear upon styles of reasoning, thereby underpinning Hacking's own emphasis on the role of the body in scientists' reasoning. Interestingly, I will argue, this approach to understanding styles might also be thought to give rise to a form of relativism, since work in situated cognition suggests that people are unable to engage in certain thought processes in the absence of particular

external, material devices. I will examine this 'situated' reading of styles of reasoning in detail, and ask how the relativism that emerges from it might differ from Hacking's own view.

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❧

Performing Medical: Transforming Institutional Identity at the Jackson Laboratory

- Ekin Yasin (New York University)

Abstract. Based on fieldwork conducted at a leading genetic research facility, the Jackson Laboratory, this paper tracks the institution's transformation from a genetic research laboratory into a genomic medicine center. Located in Bar Harbor, Maine the Jackson Laboratory has been at the front of research that examines the genetic causes and treatments of human cancer – by studying it on mice. Recently, Jackson Laboratory undertook a new project of building a genomic medicine center in Farmington, CT which promises to do translational cancer research in collaboration with University of Connecticut's Cancer Center.

In this paper I chronicle and think about the transformation of this institution's identity. I describe administrative and communication team's rush to re-present themselves as the institution becomes relevant to more donors who starts to see clear links between animal based genetic research and human well being. From purchasing stock images of cancer patients who have no real links to the institution to producing promotional videos with patients who have not been treated by the institution, the laboratory devised a number of new tactics which I call in this paper performing medical. By this term, I refer to transformative stage such institutions find themselves in – they are closer to the field of medicine as the research being conducted on animals more rapidly can be linked on research being done for humans. However this translation is not yet immediate and the proximity to the field of medicine is a novel undertaking.

In order to understand the performance of the medical I focus on three tactics. The first one of these tactics is concealment. How is it that the personalization of medicine and genetic research on human cancer is so tied to laboratory animals yet there is a consistent desire to conceal this relationship? Is there a systematic unease about the practice of scientific research? Is this unease more visible now that this field closer in time and practice to the field of medicine? The second tactic is re-narrating. Whilst the laboratory rebranded itself as a scientific mecca of genomic medicine the time the institution still has a confused relationship to the field of medicine. The Communication team at the laboratory has to create novel connections as the pressure for funding rises. How can a scientific research facility communicate their research's relevance to patient's and patient's family? What is the best name to give to this type of scientific research? How can the institution sell the idea of a cure in the future often not attainable during the lifetime of patients? The last tactic is collaborating. This last tactic reorients the culture of laboratories. With the rising possibilities of translation and collaboration amongst disciplines of science and medicine performing medical becomes an imperative at the laboratory. In this way the spirit and action at the Jackson Laboratory contrasts the laboratory environment Woolgar and Latour (1979) has described. The concerns for funding and the rapid expansion results in the laboratory as a space not merely for "production of papers" but instead a space where connections to donors has to be made periodically. For this goal scientists have to collaborate with a staff of story-tellers and marketing specialists to invent new ways to speak to a new audience.

❧

Practice Theory and Pragmatism in Science & Technology Studies: Convergence or Collision?

– Anders Buch (Aalborg University Copenhagen)

Abstract. Science & Technology Studies (STS) and social science has made a turn, a 'practice turn', and the notion 'practice theory' has made its way into the field of STS. But it is notable that proponents of this turn and theory rarely mention American pragmatism as a source of inspiration or refer to pragmatist philosophy. Reading through the practice theoretical STS literature the vista seems to come very close to positions occupied by classical American pragmatists.

In this paper, I invite you on a journey, which I have just begun, to find out not why contemporary scholars of practice theory as for example Rouse, Schatzki and Reckwitz refrain from including the pragmatist legacy in their writings. This question would probably either be entirely speculative or maybe even not very interesting? Rather, I want to explore what these two apparently similar ways of theorizing do to the study of science and technology, or to some of these studies. It is impossible to cover all STS studies inspired by practice theory, and I probably have not found all the studies drawing on pragmatism. It is in the spirit of both practice theory and pragmatism to reach out, to try to bridge ideas by talking to other traditions rather than shut themselves off in a closed closet (Bernstein, 1989; Nicolini, 2013), and as one of the contemporary pragmatist philosophers says with reference to Dewey's "Experience and Nature" (1925 [1981]): "To be human is to be engaged in practices" (Boisvert, 2012: 109).

To back up my argument, I begin by an introduction to some of the proponents of practice theory and of pragmatism. Regarding the latter, I primarily present work by Dewey because this is what I am most familiar with. Although I recognize that practice theory and pragmatism differ on fundamental philosophical issues in relation to the normative evaluation of action, I show that the two intellectual traditions have much in common when it comes to what they do to STS studies. After this introduction to practice theory, my paper will proceed in the following steps. Firstly, I will briefly survey practice theoretical and pragmatist contributions to STS studies in order to discern their respective accounts of practices and human activity. Secondly, I will trace these accounts back to Dewey's and Schatzki's philosophical reconstructions of the concept of 'practice' and 'action' in order to tease out differences and similarities between pragmatist and practice theoretical understandings. Thirdly, I will – mainly trough the work of Joseph Rouse – vindicate that the seeming collision points between practice theory and pragmatism (mainly in relation to conceptions of 'normativity' and 'naturalism') can in fact be overcome. I will argue that a pragmatist approach can add valuable resources to a practice theoretical 'toolkit' of studying and representing science and technology.

Parallel Session 4D

Thursday, 25 June 2015 at 09:00–11:00 in G3

Session chair: Lena Kästner (Humboldt Universität zu Berlin)

❧

Laws and Mechanisms: The Convergence of Two Explanatory Accounts in Neuroscientific Practice

– Philipp Haueis (Berlin School of Mind and Brain, Max Planck Institute for Human Cognitive and Brain Sciences)

Abstract. This paper belongs to a larger project entitled "meeting the brain on its own terms", which aims to show how exploratory experiments in human brain research—particularly functional neuroimaging—can help neuroscientists to develop new concepts and to formulate principles of brain organization (Author 2014). In this paper, I propose that organizational principles can be seen as specific kinds of neuroscientific laws. Mechanistic accounts, in contrast, hold that biological explanations are not law-like because they pick out properties that are contingently produced by evolution, allow for exceptions under nonstandard conditions, and vary in scope depending on the research context (Craver 2007). Such criticisms target philosophical conceptions—like the deductive-nomological model—according to which scientific laws are universally quantified sentences describing the states of affairs in their domain of application without exception. Instead of addressing the metaphysical question of what scientific laws are, however, pragmatic accounts (Lange 2000a) have given priority to the roles that laws play in scientific practice (e.g., support of counterfactuals or inductive confirmation).

A comparison of Craver's mechanistic and Lange's pragmatic-nomological account of explanation with regard to the role of generalizations in neuroscientific practice reveals a convergence on three levels. Firstly, Lange has refuted arguments against laws in functional biology by defending a normative conception of ceteris paribus laws and by arguing that the explanation of functions that an organism presently exhibits are independent from its evolutionary history (Lange 2000a, 2002). By transferring these arguments to neuroscientific explanations, I show that Craver's concept of mechanism fulfills Lange's formal criteria for natural laws (compare also Craver and Kaiser 2013). Secondly, both authors defend the autonomy of the special sciences by arguing that nonfundamental explanations pick out causally efficacious, higher-level phenomena (Craver 2007, ch. 6) and that generalizations with independent counterfactual stability pick out different forms of necessity (physical, biological, psychological etc., cf. Lange 2000, ch. 3). Thirdly, mechanism sketches—partial descriptions of the causal structure of the mechanisms—guide the experimental search for mechanistic parts of the explanandum phenomenon. They therefore fulfil the same role as Lange's conceptual outlooks, from which researchers can predict new patterns with a law that makes otherwise empirically equivalent predictions with another law of the same domain (e.g., the Boyles-Charles and van-der-Waals law for gas behavior under normal pressure, cf. Lange 2000b). Sketching a mechanism or applying a conceptual outlook prospectively commits researchers to certain experimental results, so that they can require revision if the anticipated results do not occur.

Two examples will finally show that the results of my comparison can be applied to neuroscientific practice. I will briefly discuss how functional connectivity patterns explained by Hebb's law—"neurons that fire together, wire together"—are counterfactually stable under alternative evolutionary trajectories. I also sketch how the discovery of unknown neurotransmitters first seemed to refute Dale's principle, which asserts that a neuron releases the same neurotransmitter at all synapses. By adopting a new conceptual outlook, however, neuroscientists were able to extend the principle to phenomena like transmitter co-release.

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❧

Reverse Inference, the Cognitive Ontology and the Evidential Scope of Neuroimaging Data

- Jessey Wright (University of Western Ontario)

Abstract. Recent work in cognitive neuroscience has been aimed at developing a reliable cognitive ontology (a one-to-one mapping between brain regions and cognitive processes) and characterizing the validity of reverse inference (the ascription of a cognitive function from information about brain activity). A cognitive ontology specifies a set of mental functions and identifies the regions (or networks) of the brain that implement those functions (Price & Friston 2005). A complete cognitive ontology would permit reasoning from function to region and from region to function. However, most of the analysis techniques in neuroimaging are only suited for attributing involvement in a cognitive process to a region of the brain. Indeed, reverse inference, the opposite procedure whereby investigators infer the engagement of a cognitive process from brain activity, is considered a 'fallacy' (Poldrack 2006, Machery 2013). Claims of selective association (e.g., that the amygdala is the 'fear area') need to be backed up with evidence which shows that activity in the region of interest reliably determines if a particular cognitive function is engaged. This evidence would help resolve philosophical concerns about the pluripotentiality of brain regions and the plausibility of a complete cognitive ontology (Klein 2010). It has been proposed that pattern classification analysis (PCA), can provide this evidence (Poldrack et al 2014).

Whether or not PCA can provide the evidence needed to develop a formal cognitive ontology, and so warrant reverse inferences, will depend on the evidential scope of the analysis results. This, I argue, is determined by the data manipulations required to produce those results. Data must be manipulated into evidence and all data manipulations involve the suppression of information. The nature of the resulting evidence (what it can be said to be about and how good it is) will be determined, in part, by what is suppressed by the analysis techniques used to produce it. I contrast PCA with subtraction, the most common analysis technique used to analyze neuroimaging data. By identifying the information in the data suppressed by each technique I show that pattern classification provides better evidence for reverse inferences because the results have the appropriate evidential scope. Subtraction analysis invokes assumptions that prohibit reliable inferences from the activation of a brain region to a particular mental function (i.e., prohibit reverse inference). PCA invokes different assumptions because it suppresses different information. PCA characterizes the informational content of the measured patterns of brain activity, which permits reverse inferences (with some caveats). Thus, it provides the needed evidence for the selective association of a cognitive process with a pattern of brain activity. This has further implications for the structure of the sought after cognitive ontology.

I conclude (1) that the inferential problems with reverse inference are an artifact of the data manipulations used; (2) that a cognitive ontology supported by evidence from pattern classification analysis maps cognitive processes to brain activity profiles, and not merely brain regions; and (3) by characterizing how data manipulations constrain the evidential scope of experimental results.

The Explanatory Payoffs of Multiple Realization in Cognitive Neuroscience

– Maria Serban (University of Pittsburgh)

Abstract. Multiple realization designates a relation which holds between some systemic or macro-property exhibited by one or several complex systems and a class of heterogeneous micro-properties of the same system(s). Assuming that we have an articulated stable higher-level theory and a theory pitched toward the lower-level of organization of the target system, the doctrine of multiple realization claims that there are one-to-many mappings from the unified (and perhaps homogeneous) higher-level properties to the heterogeneous lower-level properties of the system. Within philosophy, the multiple realization doctrine has been traditionally taken to license a pretty strong thesis about the autonomy of psychology from neurobiology and to set an antireductionist agenda for philosophy of cognitive science in general (Putnam 1965; Fodor 1974). However, critics of multiple realization have contested the strong anti-reductionist consequences of the thesis. Their objections targeted both the conceptual arguments for multiple realization (Sober 1999) and the lack of empirical support for the doctrine within cognitive neuroscience (Bechtel and Mundale 1999).

In response, I argue that current scientific research provides ample support for the multiple realization thesis in both biology and cognitive neuroscience. Drawing a comparison between the degeneracy thesis and the multiple realization thesis allows us to refine some of the features and implications of adopting multiple realization as a viable research hypothesis in cognitive neuroscience (Figdor 2009). Within biology, degeneracy designates the ability of structurally different elements to perform the same function. This has been shown to be a ubiquitous feature of complex biological systems at different levels of organization from the genetic, cellular, system, to population levels (Tononi, Sporns, and Edelman 1999; Edelman and Gally 2001; Price and Friston 2002; Mason 2014). Besides capturing the idea that disjoint and disparate structures can have in certain contexts similar (or even the same) functions or behavioral consequences, the theoretical treatment of degeneracy allows for a mathematically precise way to measure degrees of degeneracy in biological networks and to distinguish genuine cases of degeneracy from redundancy and pluripotentiality. Using the measures developed in the study of degeneracy helps clarify the central claim of the doctrine of multiple realization, namely that the micro-properties which differentiate the multiple realizers are not relevant for the explanation of the target higher-level behavior or property.

In order to illustrate the methodological and explanatory payoffs of the multiple realization thesis I rely on research on the phenomenon of recovery of language functions after brain damage. This case study illustrates that the collaboration between different cognitive modeling paradigms (the lesion-deficit model, functional imaging studies of normal adult subjects and developmental models of brain function recovery) provides ample support for the multiple realization or degeneracy of higher-level cognitive functions. In this context, I show how the thesis of multiple realization promotes a pluralist methodology which generates hybrid (or mixed-level) explanatory strategies for explaining the properties and behaviors exhibited by complex biological systems at higher (and more abstract) levels of organization (Richardson 2009). The more general lesson is that multiple realization supports an integrationist model of intertheoretic relations in cognitive neuroscience.

Parallel Session 4E

Thursday, 25 June 2015 at 09:00–11:00 in G4

Session chair: Anna de Bruyckere (Durham University)

❧

Bridging the Gap Between Well-Being research and Policy

– Alicia Hall (Mississippi State University)

Abstract. Scientific research on well-being has increased substantially in recent decades, leading to a growing interest in applying the findings of this research to public policy. Because of the potential of well-being research to guide the allocation of societal resources and affect people's lives, it is important that we carefully assess whether the conception of well-being as operationalized in the social and medical sciences is something we ought to pursue, and whether researchers in these diverse fields are studying the same concept or are instead interested in many different 'well-beings.' Philosophy of science, then, can make an important contribution to this field of study. However, questions have been raised about the relevance of philosophical theories of well-being to this research. Recently, Alexandrova (2012) has argued that traditional theories of well-being are of little use here, and that instead some sort of pluralist approach should be applied to the empirical study of well-being.

In this paper, I explore scientific pluralism about concepts of well-being in research. Well-being is both a normative and a functional concept, and so we need to be clear about what we aim to achieve in doing well-being research in different areas (e.g., doing well for an elderly cancer patient often means something very different from doing well for a developing child). Many of the prevailing philosophical theories of well-being are difficult to apply in research contexts. For instance, subjective theories of well-being, wherein a person's well-being is dependent on her attitudes or desires, typically contain idealizing constraints, but it can be difficult if not impossible to know in practice whether these constraints have in fact been met. There are good reasons for believing that a pluralist account may be best for describing the substance of well-being for research purposes.

However, because of the interest in applying the results of well-being research to public policy, we need some way of comparing across different contexts to make decisions at the societal level. To do this, we need some unified account of well-being that can be applied to diverse areas of scientific research. Unlike traditional theories of well-being, however, this account should be procedural rather than substantive. Rather than listing a set of necessary and sufficient conditions for when a person counts as living well, it should focus on how we deliberate about well-being and when we can be justified in believing that something will be prudentially beneficial for someone. I briefly describe an example of such an account and show how it can be applied in specific areas of interest in scientific research and public policy. Finally, I note how empirical research in turn can be used to develop and improve a procedural account of well-being for use in scientific research.

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❧

Science-Based Policy-making in an Interdisciplinary Perspective

– David Budtz Pedersen (Humanomics Research Centre, University of Copenhagen)

Abstract. In recent years there has been significant debate about the definition and role of scientific experts in advanced liberal societies. Some scholars have noted that experts are mediators between science and government, or between science and the lay public. Others have focused on the hybrid epistemological and cognitive character of expertise and the reliability of expert testimony (Brewer 1998; Selinger & Crease 2006; Maasen & Weingart 2005; Lentsch & Weingart 2011). In this paper, I take a different look at the discussion and focus on certain problems internal to the definition of expertise. More specifically, the paper challenges the notion, prominent among scholars in Science and Technology Studies, that expertise is to be identified primarily as technical knowledge. The selective use of technical expertise in policy-making and science advisory systems represents a serious challenge for wider conceptions of societal change. Only very rarely is expertise from the social sciences and humanities used in public policy-making (Bocking 2013; Budtz Pedersen 2014). As Sergio Sismondo observed in his SPSP 2013 keynote: "We see in the current knowledge regime a substantial concentration of power in few hands and strong incentives to flood the market with knowledge that serves narrow interests" (Sismondo 2013). Yet, with the recent refocusing of science funding agencies and research institutions on solving the "grand challenges" of society, such as food security, energy safety, environmental change and healthy ageing, more effort is needed to ensure that expertise from the social sciences and humanities (SSH) inform policy-making in a meaningful way. In effect, I claim that an interdisciplinary approach to expertise will have substantial positive effects on the perception and legitimacy of policy interventions, including the perceived lack of democratic legitimacy in science-based decision-making (Bovens 2006; Hulme 2011; Stehr 2013). Using the framework of "trading zones" as suggested by Collin & Evans (2007) and Collins (2010), the paper explores different strategies for including SSH research within a philosophy of scientific expertise. At the core of this framework is the idea that interdisciplinary collaboration should be managed through the medium of "interactional expertise." The paper concludes that it is only by promoting interactional expertise (i.e. the capacity to interact and exchange disciplinary perspectives) that SSH researchers and policy-makers can engage in effective dialogues, and ensure that the provision of expert knowledge responds to the complexity of real-world problems.

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❧

Knowledge Creation in the Congressional Research Service

- Holly VandeWall (Boston College)

Abstract. It would be difficult to find a group of U.S. researchers who have been forced to defend their claims of objectivity with the regularity and rigor as those who staff the U.S. Congressional Research Service. The CRS, often referred to as “Congresses’ Think Tank,” was founded (as the Legislative Reference Service) in 1914. In direct contradiction to the Mertonian goal of autonomy in selecting questions for research they are asked for very specific information from Congress. Their goal, as described in their own mission statement is to provide analysis that is “timely, objective, authoritative, and confidential” on any subject about which a member of the U.S. Congress should feel like inquiring – a tall order indeed.

While CRS area of analysis extend well beyond the scientific, one of their research divisions, Resources, Science and Industry, is of particular interest to philosophers of science. This division produces a vast array of publications every year, which in 2012 alone included “Changes in the Arctic: Background and Issues for Congress”, “The National Institute of Standards and Technology: An Appropriations Overview”, “EPA Regulations: Too Much, Too Little, or On Track?”, “An Overview of the ‘Patent Trolls’ Debate”, and “Airport Body Scanners: The Role of Advanced Imaging Technology in Airline Passenger Screening.” Many of these publications are brief updates on the status of current law. But a significant percentage CRS reports are the work of multiple authors working across disciplinary boundaries to provide analysis that brings together data that has not previously been assessed as a whole.

It might be argued that this is not scientific research as such – the authors of these reports do not have their own labs; their work is entirely literature review. But using the specific examples of the 2010 on “Deforestation and Climate Change,” the 2013 report on “Environmental Regulation and Agriculture” and the 2014 report on “Asian Carp and the Great Lakes Region” I will argue that CRS research reports contribute novel interdisciplinary work by experts in their fields. Because these reports exhibit knowledge creation in a form that has unusually direct political ramifications I will argue that philosophers of science in practice ought to pay closer attention to the epistemic significance of these documents.

❧

Industrial Intellectual Property Law as Technology

- Ave Mets (University of Tartu)

Abstract. My aim is to treat industrial intellectual property (IP), particularly plant-based patenting, in a framework of technology meant in the broader sense as (prospective) ontology considered in a practice-based philosophy of science as a structure of culture and world picture.

Technology is an ontology – a way to see what there is in the world. At the same time it determines future ontology by prescribing conceptual and material conditions for what yet can and need to be brought into existence. Human conceptual state guides his actions, including technology as human doing. Contemporary scientific technology is guided by the analytic-mathematical enframing, guiding the dissecting of nature into “elementary parts” to be manipulated separately to achieve certain predictable ends. The broader notion of technology takes it to be the changing of any part of the world (material, social, conceptual, theoretical) according to preconceived aims, and the world in technological view to be the sum total of possible resources.

This account of technology bases my case study in two respects: 1) concerning industrial IP as applicable narrowly on scientific-technological products of material technologies, e.g. chemical conceptualisation of plants, endowing them the shape subsumable under IP law; law models its object in certain legal terms, patent law presupposes both legal and scientific-technological terms; 2) IP law as a social technology: it models the (social) phenomenon that it is about (creativity), being an idealised representation of it; it thereby shapes the way how that phenomenon is seen in the society and thence designs future treatment of it. So nature is theoretically or conceptually turned into a technological artefact accountable for with scientific and technological terms and through them informed legal terms, and the social phenomenon 'creativity' is defined by and for legal aims such as property, rights, autonomy, and only exist for legal (and political) spheres as far as thus defined.

I primarily aim to delve into epistemological and social aspects of IP law: what are the philosophical prerequisites (a) to define something as IP, (b) of the requirements to patentability; (c) what is the effect of technological and legal definition of nature upon cultural practices and world picture.

Industrial IP law – plant based technologies

The chosen case study concerns part of nature that is an object of cultural significance and normal human perception. Plants have various roles in culture, of which agricultural and medicinal are interesting here as most conspicuously aspects of scientific research, technological application and legal regulation based on those. Many new technologies have evolved out of traditional technologies concerning those plants. (Chemical) science and technology reduce plants to compounds of substances, changing their role in culture: scientific descriptions are not available to traditional practices and technologies, creating basis for technological exploitation in new ways and thus for legal regulation that disregards traditional knowledge. The case study will thus inform the narrower and broader concepts of technology and scientific-technological world picture and ontology to be undertaken in the paper.

Parallel Session 4F

Thursday, 25 June 2015 at 09:00–11:00 in Koll G

Session chair: Andrea Woody (University of Washington)

❧

An Empirical Based Classification of Engineering Projects

– Sjoerd D. Zwart (Delft University of Technology)
– Marc J. de Vries (Delft, Technical University)

Abstract. In this paper we will present a classification of engineering projects. It is based on more than ten years of experience with bachelor end projects carried out at the Faculty of 3mE (mechanical, maritime and materials engineering) of the Delft, Technical University. Students of this faculty have to collaborate in groups of four carrying out an engineering project during the last six months of their bachelor. These projects, which originate in the research groups of the faculty, are not just applications of standard engineering procedures. They require creatively combining many topics learned during the preceding years of study. The project questions are open and their answers are unknown to the supervisors.

During the first years of this problem-based learning exercise the students were only provided with (1) hypothesis formulating and testing methodology, standard within descriptive knowledge production in the natural sciences. Soon in turned out however that many proposed projects were (2) directly design related, or (3) focusing on normative design knowledge formulation. The extensions (2) and (3) did broaden the methodological attention considerably but (1), (2) and (3) did not cover the methodological needs of all the projects. We had at least to add (4) the outlines of modeling projects, (5) the methodology in optimization operations and finally methodologies used in (6) sheer mathematical or information theory projects, which concentrated on formal proofs or algorithms.

These distinctions left us through the years with a six-element classification of engineering projects, which, as we will argue, cover many and perhaps most knowledge-related engineering projects in practice. The crucial point of our classification scheme is the differences between the various goals and the way to achieve them.

In the spring of 2013 we started with a course "Research Design" at our University's Graduate School. Our classification turned out to cover most of the diverging PhD projects presented although they were frequently combinations of some categories. These combinations can be represented suitably with radar charts the six axes of which identify the categories just described. Such charts prove to provide excellent methodological X-rays of the individual PhD-projects in engineering.

This practice-based paper, which covers more than 700 bachelor and 100 PhD projects, is part of a larger exercise in which we will study the various engineering methodologies for solving fundamental and applied problems. It serves theoretical and practical purposes. As far as we know no engineering methodology handbook exists, which gives methodological advice about the entire gamut of problems engineers encounter when carrying out their projects. The first purpose is therefore to provide such a manual for at least educational purposes; the second one is to study and describe the various ways in which engineering practices and different kinds of engineering knowledge are theoretically interrelated. We hope that the latter will also shed light upon the intricate relations between the practical and the descriptive sciences.

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Incorporating Growth of Knowledge Frameworks in the Science Curriculum

– Sibel Erduran (University of Limerick)
– Zoubeida Dagher (University of Delaware)

Abstract. School science has been dominated by what seems to be an 'essential tension' between two competing curriculum emphases: one focusing on the products of science in the form of propositional knowledge of particular theories, laws and models, and another focusing on scientific processes that in many cases deteriorated to an emphasis on science process skills. Problems associated with the first type of emphasis is rooted in the manner in which products of science are taught in a disconnected fashion without giving learners a sense of the relations between different forms of scientific knowledge; how scientific knowledge grows; and what criteria, standards and heuristics drive growth of scientific knowledge. As Schwab (1962) pointed out decades ago, students need to understand both the substantive and syntactic structures of science. The substantive structure refers to "a body of concept-commitments about the nature of the subject matter functioning as a guide to inquiry", while the syntactic structure refers to "the pattern of the discipline's procedure, its method, how it goes about using its conceptions to attain its goal" (Schwab, 1962, p. 203). Communicating both structures in curriculum and instruction is a desirable goal in science curriculum and instruction. Contemporary calls for curriculum reform (e.g. the Next Generation Science Standards, NGSS Lead States, 2013) resonate with some of Schwab's notion of structures of science, as they call for re-organizing and integrating science concepts around three dimensions: scientific and engineering practices, cross-cutting concepts and disciplinary core ideas. The question still remains as to how the growth of scientific knowledge can be coordinated in the science curriculum. The purpose of this paper is to investigate a timely topic on how scientific knowledge including its development can be captured in the science curriculum such that students acquire understanding of growth of scientific knowledge. Drawing on the rich scholarship in philosophy of science (e.g Giere, 1999; Mayr, 2004; Press, 2009) we propose a pedagogically relevant growth of knowledge framework involving theories, laws and models (TLM). The framework can serve as a metacognitive tool for designing or enacting a more coherent science curriculum. In particular, TLM provides 1) a visual tool that can have pedagogical utility, 2) supports the cognitive and epistemic goals of current science education reforms, 3) can be customized to different subject areas in science, and 4) acknowledges continuities and discontinuities in growth of scientific knowledge. Such a growth of scientific knowledge framework goes beyond the traditional 'atomistic' differentiations in science education between laws and theories, and focuses instead on a whole set of relationships between different forms of scientific

knowledge. Such holistic consideration of theories, laws and models is more likely to assist learners in understanding growth of scientific knowledge.

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❧

Reconceptualizing the Nature of Science for Science Education

- Zoubeida Dagher (University of Delaware)
- Sibel Erduran (University of Limerick)

Abstract. Recent science curriculum reforms continue to advocate the inclusion of the nature of science in science education. For example, the Next Generation Science Standards (NGSS Lead States, 2013) call for reorganizing and integrating science concepts around three dimensions: scientific and engineering practices, cross-cutting concepts and disciplinary core ideas. While this tripartite emphasis invites reconfiguring several meta-assumptions about science into the curricular landscape, it does not offer clear pathways for so doing. Rich scholarship in philosophy of science can provide some useful insight into how characterization of the nature of science can be clarified in science education. Using Wittgenstein's Family Resemblance Approach (FRA), Irzik and Nola (2014) proposed using broad categories that address a diverse set of features that are common to all the sciences. FRA conceptualizes science in terms of a cognitive-epistemic system and as a social-institutional system. The analytical distinctions are meant to "achieve conceptual clarity, [and] not [serve] as a categorical separation that divides one [dimension] from the other. In practice, the two constantly interact with each other in myriad ways" (Irzik & Nola, 2014, p. 1003). Science as a cognitive-epistemic system encompasses processes of inquiry, aims and values, methods and methodological rules, and scientific knowledge, while science as a social-institutional system encompasses professional activities, scientific ethos, social certification and dissemination of scientific knowledge, and social values. Our work expanded it in a way that offers a pedagogical framework for supporting the development of a more sophisticated and grounded view of the nature of science for teachers and learners.

In the paper, we review literature from philosophy of science (e.g. Giere, 1999; Mayr, 2004; Press, 2009) to illustrate the characterization of each of the FRA categories. Re-conceptualizing the nature of science for science learning and instruction is not about the replacement of some specific statements from NGSS with 11 categories. The approach we propose in applying an expanded version of the FRA is rich and nuanced and has direct implications for structuring science content for learners. The NOS content draws on overarching principles from which objectives can be developed and adapted to different settings and grade levels. These overarching principles invite teachers and learners to be active participants in seizing opportunities for understanding science in a more contextualized and relevant way.

Identifying the components of science as a cognitive-epistemic and social-institutional system is a beginning step in the design of curricula. The pedagogical strategies that accompany the realization of the FRA framework need to also be considered. There are implications for teacher education as well, in terms of familiarizing science teachers in the content of topics that they may have taught in a decontextualized fashion. There is thus the task for teacher educators in extending the framework for professional development purposes to enable teachers to incorporate FRA components in their science lessons.

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The Place of Contextual Knowledge in the Design of a Software Platform for Teaching and Learning: Making the Case for an Empirical Strategy in Software Design With Distributed Cognition

– Klara Benda (Georgia Institute of Technology)

Abstract. Human-centered computing has expressed a sense of marginalization with respect to the scenes where software is made, and a related unease about the nature of its contributions. It has been suggested that the practice of using ethnography toward the formulation of requirements and implications for design is limiting. The presentation outlines an alternative epistemic strategy based on a distributed cognition account of the making of a software platform for teaching and learning within an open source community of institutions of higher education. The suggested strategy parallels practice-based, constructivist accounts of science in an emphasis of the mediating role of conceptual models in the scientific understanding of the world. My central claim is that insofar as knowledge about the contexts of use is taken up in the generative modelling processes of designing, the empirical strategy of human-centered computing should be derived from the understanding of the conceptual processes of design.

My analysis draws on Hutchins' framework of distributed cognition, which views conceptual change as distributed over time, among people, and between humans and artifacts, as well as its application in Nersessian's account of scientific research as distributed conceptual modelling. Conceptual change in the sciences has been described in terms of universal human cognitive capacities as a model-based reasoning process. According to the mental modelling hypothesis of cognition, humans create simplified structural representations of phenomena, which can be mentally manipulated for the purposes of simulating possible or future situations. Familiar representations can become generative of new models, resulting in conceptual change. Distributed cognition brings to this analysis the notion that cognition is cultural, i.e. the models used in reasoning are shared and passed on among the participants, and the material environment participates centrally in this process.

The case study describes the process of distributed conceptual modelling from which a new software platform has emerged. Central to this process was the emergence of a socially and materially distributed design space from a series of prototyping projects, which configured participants and software prototypes around the loose and open-ended agenda for building a new platform. The open-ended design space prompted a temporally extended process of sense-making with conceptual models and prototypes. Participants were formulating, sharing and discussing thought experiments for a coherent model of user experience, and visited their knowledge about the contexts of use to collectively fuel and test the model building process.

While human-centered computing thinks of its empirical contribution as preceding design both in a temporal and logical sense, the case study suggests a reverse relationship. Participants were pulling in knowledge about the educational context as needed to support their sense-making for the purpose of design, discussing experiences from personal memory and tapping into community archives of similar discussions in the past. The conceptual organization of the knowledge was also in line with the model-building efforts. This implies the viability of an empirical strategy, which embraces conceptual models and the mediation of cultural experiences, and instead of producing accurate contextual descriptions for individual design projects, seeks to make available a rich pool of cultural models for broader cultural domains of experience.

Plenary talk

Thursday, 25 June 2015 at 11:20–12:30 in Aud F

Session chair: Andrea Woody (University of Washington)

Investigating Discovery Practices: Studies of Bioengineering Sciences Labs

Nancy J. Nersessian (Harvard University)

Abstract. This presentation will discuss a 14-year ethnographic research project investigating the cognitive practices that have been leading to scientific discoveries in four bioengineering sciences labs, two in biomedical engineering that conduct experiments with physical simulation models (tissue engineering and neural engineering)and two in integrative systems biology labs (one that does only computational modeling in collaboration with bioscientists and the other that does modeling and conducts bench top experiments to further their modeling). My research group conducted open interviews, field observations of the researchers at work, and collected various archival data, including draft and published papers, research proposals to funding agencies, records of lab meetings, power point presentations prepared for various purposes, and dissertation proposals. I began this line of research out of the conviction that 1) philosophers should not cede studies of science labs to sociology of science since these are also cognitively rich domains and 2) much of what goes on in scientific discovery practice that is relevant for philosophical analysis is not preserved for the historical record. I will discuss significant insights into discovery and problem solving – around topics of philosophical interest such as method development, modeling and simulation, concept formation and change, and explanation – that could only have been gleaned from data of the day-to-day research processes that the ethnographic interviews and observations provide. This kind of research also has significant potential for making philosophy of science relevant to and collaborative with scientists in facilitating their research agendas. As an emerging interdisciplinary fields these areas face many challenges such as how to organize research labs, how to facilitate cross-disciplinary collaborations, and how to train researchers. I will discuss how our investigations have been providing bioengineering researchers with insights into these challenges.

Plenary talk

Thursday, 25 June 2015 at 14:00–15:10 in Aud F

Session chair: Mieke Boon (University of Twente)

Philosophy of Clutter

Marcel Boumans (University of Amsterdam and Erasmus University Rotterdam)

Abstract. Philosophy of science in practice is philosophy of clutter, and not of theory. While the world of the theory is clean and clear, the world of research practice is messy and noisy. Most of the research time is spent on cleaning, filtering, and similar tidy-up activities. Clutter leads to errors. Hence, the more can be cleaned up, the more accurate the research results will be. But clutter is heterogeneous in the sense of its composition, idiosyncratic with respect to its environmental conditions, and sticky, that is, hard to separate from the object of study. Due to the nature of clutter, no theory can account completely for the practice of research. For the epistemological understanding of practice one has to study other documents, namely almanacs, dictionaries, guides, handbooks, instructions, reports, teaching materials, tutorials, and yearbooks. Although for the study of tacit knowledge ethnographic methods seems to be most appropriate, these latter documents provide rather detailed accounts of these idiosyncratic practices. An exemplary document is G. Girard (1990), 'The washing and cleaning of kilogram prototypes at the BIPM.' For the same reason as there is no theory of clutter, the treatment of clutter cannot be done by only mechanical procedures. Typical

for this kind of documents is that they also instruct about essential non-mechanical activities as "rub fairly hard by hand", or "give a few taps on the instrument." Because there are no standards for how hard to rub or how many taps, these judgments are often based on visualizations. (I will not discuss the equally interesting judgments based on smelling, hearing, tasting and touching.) These judgments require training and accumulated experience with the specific practice. Philosophy of science in practice therefore is a philosophy of idiosyncrasy and trained senses.

Parallel Session 5A

Thursday, 25 June 2015 at 15:30–17:30 in Aud F

Session chair: Andrea Woody (University of Washington)

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Understanding Scientific Practices as Discursive Niche Construction

– Joseph Rouse (Wesleyan University)

Abstract. An important recent development in evolutionary biology recognizes niche construction as "a second major participant in evolution, after natural selection" (Odling-Smee, Laland, Feldman 2003, 12). Niche construction is an ecological inheritance: along with genes and epigenetic resources from their parents, organisms inherit a transformed environment exerting different selection pressures via the cumulative effects of other organisms' activities on their developmental and selective environment. Niche construction is often regarded as primarily abiotic, but behavioral niche construction occurs wherever organismic behavior affects the next generation's developmental environment in ways that reliably reproduce that behavioral pattern. Recognizing the biological significance of niche construction thereby also blurs traditional boundaries between biological and cultural evolution.

In a forthcoming book (Rouse 2015), I argue that language and other aspects of human conceptual understanding arose and are sustained in significant part through behavioral niche construction. This paper brings together four important, interrelated consequences of this account of conceptual capacities for a broadly naturalistic understanding of scientific practice:

1. What the sciences primarily contribute to human conceptual capacities is not a body of accepted knowledge claims, but an expansion and reconfiguration of the next generation's capacities to perceive, act toward, and reason about aspects of their environing world. The sciences bring into the Sellarsian "space of reasons" objects, phenomena, conceptual patterns, and causal relations previously opaque to human understanding, while also closing off or reconceptualizing what had once seemed intelligible aspects of the world.
2. This heritable reconfiguration of human conceptual capacities integrally incorporates experimental and technological practices. Novel phenomena (Hacking 1983, ch. 13) and experimental systems (Rheinberger 1997) provide new, regulated settings for articulating conceptual patterns, in concert with new verbal formations and mathematical modeling. Where Morgan and Morrison (1999) speak of theoretical models as "mediators" between theoretical concepts and the world, a niche constructive approach takes scientific understanding to be doubly mediated by theoretical and "experimental" (including clinical, field, or technological) models.
3. The primary mode of scientific conceptual articulation as niche construction opens and sustains domains of research by the holistic articulation and stabilization of conceptual norms. By sustaining the empirically defeasible lawlike invariance of conceptual relationships and their appropriate application within specific material settings, the sciences enable patterns of reasoning and action that are not just stipulative constructions, but answerable to the possibility of sustaining them coherently in ongoing interaction with a niche constructed environment.

4. Scientific reasoning within those domains acquires scientific and more broadly conceptual significance from "heteronomic" relations to other conceptual domains and practices. Such relations to other scientific domains and projects, and to broader aspects of human life and culture, are integral to the conceptual character of scientific understanding. Only by maintaining openness to broader conceptual accountability do scientific practices retain a "two-dimensional" normativity characteristic of conceptual understanding, as about something, in independently articulable respects. This recognition constrains the apparent disunity of science displayed by the diverse scientific domains and their mediating models and experimental systems.

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❧

Representation and Correspondence as Dead Metaphors

- Hasok Chang (University of Cambridge)

Abstract. The traditional philosophical idea that a scientific theory "represents" the world is a metaphor, grounded in other epistemic activities that are actually representational. For example, if we make a drawing of an object that we see, it can be said straightforwardly that the drawing represents the object. The relationship between a theory and the world (or the relevant part of it) is not truly representational. To the theory, we have full direct access; to the world, we do not. In contrast, in real representation there is clear accessibility to both sides. The very idea of the "external world" is a metaphor ("external" — outside of what?), imagined after the phenomenal objects which we observe and make representations of. The rest of the metaphorical structure follows easily: the theory represents the imagined object, with a correspondence between various aspects of the theory and various imagined properties of the object. This metaphorical correspondence is the elusive truth sought by scientific realists.

In line with the program of the study of scientific practice articulated previously (Chang 2011), I propose to consider what we do when we represent something. In the simplest kind of case, we take note of some particular observed features of an object, and create another object that has those same features. Something re-presented has to be present (or be presented) to us in the first place. Representing is the construction of an artificial object (which may be a formal system) that serves to express specific features of an observed natural object, in order to facilitate the achievement of certain epistemic aims. When a theory "represents" the unobservable world, we do typically begin with some observed features of the situation but the theoretician introduces many other features. In order to check the faithfulness of the "representation", we would need to have independent access to the alleged features of the world, which we do not. So, rather than representation, what we have here is the activity of construction. These points will be illustrated through the case of the Rutherford–Bohr atomic model.

The external world, representation, correspondence — these concepts, as they normally occur in epistemological discussions, are metaphors. Moreover, they are dead metaphors, in the sense that they are by now so ingrained in the discourse that they are not even recognized as metaphors and routinely get mistaken as literal expressions (see Bowdle and Gentner 2005; Goldberg 2011, ch. 4). The problem with dead metaphors is that they no longer serve the creative and exploratory functions

of metaphors (on which see Hesse 1966), which require an awareness that the application of the expression in question is not literal, or at least uncertain if taken literally. Dead metaphors are at best useless and harmless, at worst misleading. I will finish with a discussion of how we might move beyond the dead metaphors of representation and correspondence. One option is to remove them, in the hopes that we may see more clearly what the non-metaphorical situation is. Or would there be benefits in keeping them but rendering them actively metaphorical?

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❧

Scientific Practices and the Problem of Concept Formation

- Laura Georgescu (Ghent University)

Abstract. The shift in philosophy of science from a model of science as a body of propositions to a model of science as systems of practices allowed for novel perspectives on many old philosophical queries about the scientific enterprise. One such philosophical query was the creation of novel scientific concepts. In a philosophy of science that focuses on the propositional structure of science, a fundamental distinction is maintained between the activities that bring about a scientific concept and the theoretical role of a scientific concept—and the former are taken to be irrelevant for a philosophical understanding of a given concept. Such a view is shored up by a treatment of concepts as (in principle) fully graspable entities, in at least some invariant core, which provided the basis for treating the conceptual realm as its own independent object of philosophical analysis, separate from practice.

The acceptance of scientific practices as a subject worthy of analysis in philosophy of science completely transformed how the problem of novel concept formation in science is dealt with. Firstly, a philosophy of scientific practice turns the question of how a scientific concept is formed from a non-starter into something philosophically fruitful. Secondly, whatever answers there might be are likely to be found in the various practices scientists are involved in—from practices of experimenting and observing to practices of visual and/or mathematical modelling, and so on. On these lines, Nersessian (2008) and Rouse (2011) have argued that instances of scientific concept formation are not purely intra-linguistic and sudden events, but that making conceptual sense of scientific experiential situations is a tortuous process that is lengthy, difficult and which appeals to diverse methodological strategies in order to articulate a communicable and well-supported scientific concept.

Thus, on the practice reading, scientific concepts are taken to be context sensitive insofar as (1) the practices are the loci where new scientific concepts are formed; (2) scientific practices are integral to the concept formation process; (3) the loci to which a scientific concept is projectable beyond its context of formation are extensions and continuations of precisely those scientific practices that afforded the concept to be formed in the first place. In this paper, I focus on (2). I first show that the formulation of a concept of magnetic field was the result of experimental and representational practices that treated magnetism as a spatial array of dispositional properties—practices of mapping phenomena in controlled settings. That scientific practices are integral to concept formation is well established in the literature (e.g. Chang 2011; Rouse 2011). I note however that such accounts show how scientific practices influence concept formation, but not that they do. I argue that the latter is necessary if one wants to avoid the charge that scientific practices are already presupposed as

integral in reconstructions of historical cases rather than accounted for, and I conclude with an attempt to offer such an argument.

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The Consequences of Putting the Philosophy of Science Into Practice

– Robert Frodeman (university of north texas)

Abstract. Consider the following sketch of 20th and now 21st century philosophy of science. As Reisch (2005) argues, the original impulse behind the Weiner Kreis was social as well as epistemological in nature. Nonetheless, by the post-war period mainline philosophy of science had become strongly internalist in orientation. One effect of the failure of mid-century philosophy of science to take the larger cultural effects of technoscience seriously was the creation of science and technology studies in the 1960s.

At the same time, Kuhn's Structure (1962) initiated the long slow march of the philosophy of science away from an internalist focus and toward taking history and culture seriously. The founding of SPSP can thus be seen as the next logical step in this process—a response to the deficiencies of mainline 20th century philosophy of science by emphasizing questions attendant to the actual practice of science in the real world.

But does SPSP actually practice its practice? Who is the audience for its insights—philosophers, or the wider world? Who comes to its meetings, or submits papers for consideration? Has SPSP managed to break out of the charmed circle of what I have called disciplinary philosophy (Frodeman 2014)?

This talk raises these issues by asking, what are the consequences of putting the philosophy of science into practice? This question can be broken down into two elements, what I will call the institutional and the theoretical. While loathe to separate the two—this separation, I will argue, is much to blame for the irrelevance of the philosophy of science to the larger world—I will focus my remarks on the latter, and ask: what are the theoretical consequences of actually practicing the philosophy of science?

I will argue that the first consequence is that philosophic rigor must itself be seen as pluralistic in nature. The rigor of disciplinary work (i.e., work directed toward other philosophers) is different from, but neither better nor worse, than the philosophic rigor appropriate for real world exigencies. This is a point that bioethicists have long understood. In his 1973 article "Bioethics as a Discipline," Daniel Callahan already saw that doing philosophical thinking with physicians, scientists, and other stakeholders demands "rigor…of a different sort than that normally required for the traditional philosophical or scientific disciplines." Bioethics today (de facto, if not de jure) exists in disciplinary and in non-disciplinary forms that synergize with one another.

This suggests that we should not be forced—as a matter of general principle, and as a matter of gaining tenure and promotion—to value one standard of rigor over another. In response, over the last decade I have offered the neologism of 'field philosopher' to describe what an alternative sense of philosophic rigor might look like. Field philosophy is addressed primarily to non-disciplinary peers in evolving contexts of use. And its disciplinary activities are oriented toward sharing lessons learned in order to improve non-disciplinary contributions. In addition to disciplinary criteria of success, field philosophers are judged by their contributions to policy processes and public debates. And rigor is defined by balancing epistemological thoroughness with other criteria such as timeliness, cost, and relevance.

Parallel Session 5B

Thursday, 25 June 2015 at 15:30–17:30 in G1

Session chair: Sabina Leonelli (University of Exeter)

Organized by: Hans Radder (VU University Amsterdam)

Symposium: Practising Philosophy of Science in the Public Interest

Synopsis. Over the past decade, politicians and science policy organizations have increasingly demanded science (including the social sciences and humanities) to have a 'social impact'. Accordingly, funding agencies and science policy organizations have included such impact criteria in their assessment procedures. In practice, if not in theory, this often amounts to a requirement to demonstrate that the proposed research shall either have some economic value or shall contribute to the solution of a concrete problem of a specific target group. Thus, this policy strongly promotes applied research.

These developments are also highly relevant for philosophers of science. From a philosophy of science perspective, there are three possible responses. First, one may deny the legitimacy of the requirement and reclaim the value of basic science, in the sense of knowledge for its own sake. Second, one may acknowledge the value of basic science (including fundamental philosophy) for the individual scientists and scholars, but at the same time emphasize that it also constitutes and serves a public interest. Thus, this response rejects the claim (implicit or explicit in much current science policy) that only applied research can be of public interest. Third, one may argue that the application of philosophy of science to socially significant issues has been wrongly neglected during many decades. Accordingly, one may focus on specific problems faced by science in society and attempt to contribute to their solution, or at least their clarification, from the perspectives of ontology, epistemology, methodology, social philosophy, or (research) ethics.

We think that the first response is both unjustified and unfruitful. Given the big role of science in society, excluding this subject from philosophical reflection is artificial and reinforces the current, marginal position of the discipline of philosophy of science. The second and third responses (which are, or should be, compatible) see reflection on the role of science in society as a basic responsibility for philosophers of science. This includes and even requires fundamental philosophical research. For instance, research on why 'social' impact should not be reduced to creating economic value or solving concrete, short-term problems and, more basically, research on what constitutes a (long-term) public interest.

The symposium will include four papers. Each of the papers discusses and illustrates the public interest of philosophy of science. They include both general analyses of the ways in which philosophy of science can be of public interest and concrete cases showing how this may work out in practice.

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The How and Why of Philosophy of Science's Societal Impact

– Hans Radder (VU University Amsterdam)

Abstract. The question of whether, and if so how, academic philosophy can contribute to the resolution of societal problems is often seen either as very difficult or as irrelevant. Underlying this assessment is the view that philosophy is an abstract, theoretical endeavour that cannot, or only with great difficulty, be applied to the concrete, practical issues in the wider society. In this paper, I argue against postulating such a theory-practice gap. While it is correct that philosophy is primarily theoretical, there is no fundamental gap because our life-worlds also include theoretical, and even philosophical, notions and issues.

From this perspective, valuable contributions to debates on these notions and issues by philosophers, including philosophers of science, are not at all impossible or irrelevant but rather challenging and rewarding. An important consequence of the fact that there is philosophy in (societal) practices is that 'having an impact' requires a two-way interaction rather than a one-way application of academic philosophy to practical problems. We start by studying the nature and role of philosophically relevant

notions and issues in societal practices, investigate whether these notions and issues can be clarified with the help of our philosophical knowledge and skills, and submit the results of our academic investigations through participation in the relevant societal debates.

I will illustrate these general ideas with two examples. The first concerns the philosophical issue of genetic reductionism, applied to the case of (human) motherhood. New reproductive technologies have enabled what is called 'surrogate motherhood' (Schermer and Keulartz 2002). This has led to a non-trivial differentiation in the notion of motherhood, especially in the case of gestational surrogacy: is the 'real' mother the woman who has 'commissioned' the baby and will raise it, the 'genetic' parent who has donated the egg cell, or the woman who has gestated and delivered the baby?

The second example, concerning current patenting practices in (academic) science, shows that the proposed approach is not limited to ethics. If we study these patenting practices in detail, we encounter a variety of philosophically relevant notions and issues (Radder 2013). For instance, natural entities and theories or concepts are excluded from patentability. Therefore, it is crucial to establish which kind of things are natural and which artificial, and which entities are theoretical or conceptual rather than material or physical.

In the case of both examples, we will see that philosophers of science may significantly contribute to the debate on the relevant issues on the basis of their philosophical knowledge and skills.

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❧

Should Scientific Ontologies Reflect Public Interests?

- David Ludwig (VU University Amsterdam)

Abstract. While it is widely agreed that some areas of scientific practice (e.g. research funding) should reflect public interests, scientific ontologies are often considered to be internal scientific issues. Whether a scientific entity exists does not depend on public interests and ontological issues should be clearly separated from social concerns. One may therefore suspect that any consideration of public interests in scientific ontologies presupposes a radical and highly implausible constructivism. The aim of this talk is to develop a framework for the incorporation of public interests in scientific ontologies that does not presuppose any implausible constructivist or conventionalist claims.

My starting point are current debates about scientific kinds that build on assumptions about property clusters and inductive reasoning. Scientific ontologies are not conventionally constructed but reflect empirical discoveries about the cluster structure of reality that allows scientists to make relevant predictions. While this argument is often used to defend moderate accounts of natural kinds, I argue that it also supports the claim that scientific ontologies are underdetermined by empirical evidence. For example, current controversies about race and genes reflect the large variety of genetic ontologies that are compatible with our empirical knowledge. Genetic properties can be clustered in countless ways that support different inductive inferences. Human genetic diversity can be organized in many ways and therefore requires that scientists incorporate epistemic and/or social considerations in the choice of their ontological frameworks.

In a second step, I argue that epistemic values should not have priority over social values in debates about scientific ontologies. Epistemic values are often considered prior in theory choice because they are assumed to be truth-indicative: a theory with certain epistemic virtues is more likely to be true while a theory with certain social virtues is not more likely to be true. However, choices between scientific ontologies are often not about truth in the first place. For example, competing

genetic ontologies organize human diversity differently but there is little point in calling one of them true and the other one false. I therefore argue that there is no good reason to exclude social values from ontological choices or to consider them to be of only secondary importance.

Finally, I propose two models of incorporating public interests in scientific ontologies. First, well-ordered ontologies attempt to balance epistemic and social concerns on the basis of the current state of empirical knowledge. Second, radical ontologies focus on particular social concerns that are widely ignored in research. I suggest that both models serve different functions in scientific practice. Well-ordered ontologies provide a helpful ideal in science policy and in applied ontology building. Radical ontologies are vehicles of social critique that aim to empower marginalized voices in science.

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❧

The Social Relevance of the Philosophy of Climate Science

- Anna Leuschner (Karlsruhe Institute of Technology)

Abstract. Due to its economic, ecological, and social relevance climate science is under strong societal pressure: despite huge uncertainties climate scientists are forced to provide reliable information to policy-makers as fast as possible. Philosophers of climate science explore to what specific scientific and societal challenges climate science is exposed:

First, they have provided methodological insights into the question to what extent climate models and simulations can be reliable despite data uncertainties and a limited understanding of both the physical functions of the climate system (particularly feedbacks, e.g. from clouds or permafrost) and the development of socio-political conditions (e.g., Biddle & Winsberg 2010; Lloyd 2012). I'll argue that these discussions provide the public and policy-makers with important information on the methodological reliability of specific results of climate science.

Second, they have been concerned with problems of consensus finding and policy advice in climate science. The concept of well-ordered science has been discussed in this context as well as the question whether climate scientists have a specific responsibility, e.g. to speak and write "for the broader public" (Kitcher 2011: 164), or "to combat, piece by piece, the misrepresentations brought in support of the recent attacks on the integrity of climate scientists and of the IPCC" (Keller 2011: 26). This discussion sheds light on the particular responsibility of both the IPCC and individual climate scientists.

Third, they have discussed how science and society should deal with climate change denial and manufactured doubt (e.g., Biddle & Leuschner forthcoming; de Melo-Martin & Intemann 2014). I'll discuss whether the attacks on climate science that are sponsored by the oil and gas industries are detrimental – be it epistemically (by hindering the scientific discussion and leading to skewed scientific results) or morally (by postponing climate change mitigation measures).

I'll conclude that by providing science, politics, and the public with these substantial information, the philosophy of climate science sheds light on the social relevance of philosophical reflection and contributes to a more comprehensive understanding of the problems and potentials of policy-relevant sciences.

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❧

A Satanic Mill for Science?

- Daniel Hicks (University of Western Ontario)

Abstract. In the early twenty-first century, the social value of scientific research is often understood in terms of the production of wealth. In other words, scientific research is a commodity: it is done or produced for profitable sale on the market and, like any other commodity, its value is measured by the exchange value that it commands on the market.

This paper criticizes this 'commodity conception of science' by recalling Karl Polanyi's (1944/2001) notion of a 'fictitious commodity'. Polanyi argued that there is a fundamental tension within any self-regulating, market-based economic system. On the one hand, such a system requires that all aspects of production be regulated by the market. So specifically, land and labour are all treated as commodities. But labour 'is' actually human beings and the activities that constitute our lives. Human beings are not actually produced for sale, and so are not commodities. Instead, labour is a 'fictitious commodity'. Furthermore, this institutionalized fiction has ethically pernicious consequences. As Polanyi puts it, "the alleged commodity 'labor power' cannot be shoved about, used indiscriminately, or even left unused, without affecting also the human individual who happens to be the bearer of this peculiar commodity."

I argue that Polanyi's notion of a fictitious commodity applies equally well to scientific research. Descriptively, much (though not all) scientific research is conducted for the sake of improving human well-being, not for sale; and so it is not actually a commodity. Ethically, treating scientific research as though it were a commodity has pernicious consequences similar to those of treating labour as though it were commodities. I illustrate this with the development trajectory of genetically modified [GM] crops. While GM crop development and use is rationalized with heroic rhetoric of 'feeding the world', almost all actual GM crops are used for pest control.

Time permitting, I respond to a possible objection. Polanyi's critique might be read as assuming something like the distinction between pure and applied science, and as arguing that applied science is, as such, ethically problematic.

In response, I draw on Dewey's discussion of the maxim 'the end justifies the means' (1939/1988), and MacIntyre's discussion of 'compartmentalization' in modern society (2006, among others). In light of these analyses, the problem with fictitious commodities is not, as such, applying them to other uses. Rather, the problem is with treating human lives and scientific research as though they were 'only' commodities; the way in which profit concerns take priority over all the other values. In this way, the mistake of commodifying science is symmetrical to the mistake of giving absolute priority to epistemologically 'pure' science.

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Parallel Session 5C

Thursday, 25 June 2015 at 15:30–17:30 in G2

Session chair: Sara Green (University of Copenhagen)

Organized by: Morgan Thompson (University of Pittsburgh)

Symposium: Mechanistic Explanation Meets Scientific Practice

Synopsis. Advocates of the new mechanistic philosophy of science have often emphasized practice-oriented aspects of mechanistic explanation, especially processes of discovery and experimentation. In this symposium, however, we argue that close attention to practice illuminates limitations of the standard characterization of mechanistic explanation advanced in philosophy of science. The canonical picture of mechanistic explanation is that biologists discover a phenomenon, relate it to a mechanism, decompose the mechanism into component entities (parts) and activities (operations), and show that together they generate the phenomenon. The resulting account of a mechanism is judged to be good if it picks out the entities and activities that actually produce the phenomena in the world. This, however, leaves out many important features of the practice exhibited in fields of biology that are rightly described as involved in mechanistic explanation. By focusing on specific examples of scientific practice, this symposium will identify shortcomings of the canonical picture and suggest ways to develop accounts of mechanistic explanation that better fit scientific practice.

The quest for mechanistic explanation is often portrayed as beginning with the delineation of a phenomenon that then becomes the target of explanation. But delineating phenomena is often a complex process of discovery that involves experimental manipulations and can themselves address the questions that scientists are posing. David Colaco will begin the symposium by exposing practices in which researchers intervene to manipulate phenomena, not parts of a mechanism, to solve the problem posed in their research. While such interventions can occur as a prelude to developing mechanistic explanations, they also occur in contexts in which developing mechanistic explanations is not the goal. Whether the focus is on phenomena themselves or the parts and operations of mechanisms, most scientific research projects focus on what Daniel Burnston, in the second talk, characterizes as explanatory relations. These establish dependency relations between variables that characterize phenomenal or component properties, and in many cases identify relations between what might be viewed as activities within a mechanism and aspects of the phenomenon. These relations, however, are not just preparations for mechanistic explanations—they are crucial to evaluating proposed mechanistic explanations and are often sought in their own right.

In the third talk William Bechtel will pick up on the issue of evaluating explanations. Among accounts of mechanistic explanation that emphasize norms, the focus is often on the mapping of accounts of the mechanism onto the mechanism operative in nature. But such mapping is not something to which scientists have access; rather they can only appeal to evidence and other epistemic considerations available to them. In the case of mechanistic explanation, this involves not only evidence supporting claims about the components but also evidence that the mechanism could actually account for the phenomenon, which sometimes takes the form of explanatory relations as discussed by Burnston. The final talk by Morgan Thompson turns to the question of what is the importance of "mechanistic" in "mechanistic explanation." Some mechanists have contended that all explanations are mechanistic, making the adjective "mechanistic" redundant. Thompson argues for restricting the scope so as to emphasize the distinctive contents and norms of the practice of mechanistic science. Such restrictions are important for a practice perspective as they allow philosophers to focus in on the distinctive practices pursued by scientists engaged in mechanistic explanation.

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Mechanist and Non-mechanist Modes of Discovery: A case for phenomenal intervention in neuroscience

– David Colaco (Department of History and Philosophy of Science and Center for the Neural Basis of Cognition, University of Pittsburgh, djc60@pitt.edu)

Abstract. Recent accounts in the philosophy of scientific discovery (e.g. Craver and Darden 2014) have endeavored to apply the framework of the mechanist program to explain how discovery often occurs in the life sciences, including neuroscience. That is, discovery in fields like neuroscience is described as the discovery of mechanisms and their activities. I argue that, while there are certainly cases that fit this characterization, it does not exhaust all cases from the discipline. There are many cases in which discovery is not best framed in terms of mechanisms. With this in mind, I will sketch out an alternative mode of discovery. Using examples from the investigation of the behavior of coordination and its relation to the motor cortex, I will show how the important framing item is not a mechanism, but rather a specific phenomenal-level problem or issue the researchers wish to solve or ameliorate. With this in mind, I will show that successful phenomenal intervention – cases in which researchers manipulate the phenomena of interest in order to resolve the problem that frames their research – is a powerful mode of discovery. While this mode may lead to the uncovering of mechanisms as a byproduct, the experimental manipulations need not be characterized in terms of them. Rather, they are directed at intervening on the phenomenon, not the components of the mechanism. I will draw out the differences between the phenomenal and mechanistic modes, and how their differences reflect the differences in the character of the experimental practices associated with them. The two modes are not incompatible, and may sometimes go together. Nevertheless, sometimes they do not, and, without an account of the non-mechanist mode, it is difficult to explain many legitimate instances of discovery in the fields of neuroscience.

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❧

Explanatory Relations

– Daniel C. Burnston (Department of Philosophy, Center for Circadian Biology, and Interdisciplinary Program in Cognitive Science, University of California, dburnsto@ucsd.edu)

Abstract. Mechanists often suggest that a fully-developed mechanistic explanation, as portrayed in a mechanism diagram or schema, is the end-goal of a research program. This view entails that other ways of representing system properties, including data graphs, are at best subsidiary in the project of giving explanations. Data graphs perhaps constrain hypothesizing about, or provide evidence for, particular mechanistic accounts, but they are not themselves explanatory. I give a practice-based argument that this standard view is false. Many research papers in active science offer explanations despite not presenting any mechanistic diagrams or schemas at all, or employing them in a heuristic way as a search for other explanatory representations. I focus on one such type of representation, which I call "explanatory relations." Explanatory relations are quantitative relationships between variables or system properties, and are often shown in individual data graphs that exemplify the relationship taken to be important. I discuss cases of the search for explanatory relations in mammalian chronobiology to argue, first, that the role of explanatory relations in explanation is not

reducible to giving constraints on or evidence for hypotheses about the mechanism—i.e., the parts, operations, and organization of the system producing the phenomenon. Second, I argue that it is equally inaccurate to describe explanatory relations in terms of robust generalizations or laws. What is important in representations of explanatory relations is the pattern of quantitative relationships between variables exemplified in a data graph. These are often vital in explaining aspects of the phenomenon. These arguments reveal a flaw in standard approaches and debates about mechanistic explanation: practice reveals that what is important for understanding explanation in biology is not what kinds of representations are most fundamental to explanation. Instead, understanding practice requires analyzing the content and employment of different forms of representation, and how they relate to each other in explanations in particular contexts.

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Norms for Mechanistic Explanation Available in Practice

– William Bechtel (Department of Philosophy, Center for Circadian Biology, and Interdisciplinary Program in Cognitive Science, University of California, bechtel@ucsd.edu)

Abstract. One task for philosophical accounts of explanation is to identify what differentiates good and bad explanations. Focusing on mechanistic explanation, Craver argues that this requires an ontic account of explanation in which the actual activities of entities constituting the mechanism generate the phenomena. Scientists' representations of mechanisms provide good explanations only insofar as they map onto the ontic explanations. Scientists, however, do not have access to ontic explanations except as mediated by their representations and yet they face the challenge of differentiating good and bad explanations. Traditional philosophy of science points to a number of considerations to look for in the normative assessments generated by scientists such as fit with other well-supported explanations, both mechanistic (e.g., at other levels of organization) and non-mechanistic (e.g., evolutionary descent). Moreover, there is an obvious source of evidence relevant to assessing mechanistic hypotheses—whether there are parts of the sorts proposed and whether they can, in appropriate circumstances, perform the operations posited. This suggests the mapping relation that advocates of ontic explanation invoke, but it is important to focus on how scientists secure evidence for parts and operations. But especially important for mechanistic explanations is evidence that posited mechanisms could produce the phenomenon in question. Often this takes the form of demonstrating explanatory relations as discussed by Burnston. In scientific practice, a multitude of research projects advancing different explanatory relations are invoked to support a given proposed mechanistic explanation. Another form is the demonstration, typically through mathematical modeling, that the mechanism would generate the phenomenon. In practice researchers often develop simplified models designed to elicit the core explanatory relations that enable the mechanism to generate the phenomenon. I will illustrate these modes of assessment using recent research on circadian rhythms.

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Limiting the Scope of Mechanistic Explanation

– Morgan Thompson (Department of History and Philosophy of Science and Center for the Neural Basis of Cognition, University of Pittsburgh, mot14@pitt.edu)

Abstract. Although some mechanists worry that limiting the scope of mechanistic explanation to only a subset of all explanations will "marginalize" it, I argue that only by limiting the scope of mechanistic explanation can accounts of mechanistic explanation describe scientific practices and norms in an informative way. When alternative theories of explanation are proposed based on specific examples from the biological sciences, many mechanists reply in the following two ways: (i) the purported counter-example is not actually explanatory and so the alternative theory is not a theory of explanation or (ii) the purported counter-example is actually mechanistic and so the alternative theory of explanation is not an actual alternative to mechanistic explanation. These two responses are

unhelpful not only in the dialectic of the debate, but also in terms of providing descriptively adequate and normatively satisfying theories of explanation. Mechanists have begun discussing examples of network models in graph theory—graphs consisting of nodes and the connections between nodes to describe the structure of a system and system-level properties—to illustrate networks in the brain (Sporns 2010) or protein networks (Alon 2007).

Craver (2014) provides the first response when he argues that network models are not a new kind of explanation, but rather a descriptive tool useful for scientists to describe organization and one that might contribute to mechanistic explanation. In an ethnographic study of two systems biology labs, MacLeod & Nersessian (2015) found that these labs often aim to model a system for interventions on a particular aspect of the system, usually at the expense of distorting other parts of the model through the process of parameter-fitting. I argue that these models do not fit into Craver's phenomenal-mechanistic dichotomy and so his version of mechanistic explanation is not descriptively adequate.

Zednik (2014a, 2014b) responds in the second way by suggesting that networks models indeed provide mechanistic explanations. This response requires the mechanist to expand many aspects of the mechanistic explanation picture to the point of triviality and at the expense of respecting scientific practices. In particular, these mechanists often treat nodes in network models as straightforward components in a mechanistic explanation, which ignores the fact that nodes are defined by the modelers often arbitrarily (e.g., random parcellation schemes). Further, node choice significantly affects the extent to which certain system-level properties (e.g., small-worldness) appear in the model (Zalensky et al. 2010). I argue that limiting the scope of mechanistic explanation allows it to be a more descriptively adequate account of scientific activities (e.g., explanation, modeling) and also provides more consistent, contentful norms for philosophers and scientists interested in successful explanations. Reducing the scope of mechanistic explanation allows the theory to contribute—along with other theories of explanation—to a more descriptively adequate account of modeling and explanation in the biological sciences.

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Parallel Session 5D

Thursday, 25 June 2015 at 15:30–17:30 in G3

❧

On the Epistemic Roles of Simulations in Cognitive Modeling

– Maria Serban (University of Pittsburgh)

Abstract. The task of explaining how various brain structures achieve the complex cognitive functions and behaviors observed in living organisms faces the major challenge of bridging the gap between higher-level or abstract descriptions of psychological properties and behaviors and lower-level accounts of the structure and organization of neural systems at various levels of organization. The Human Brain Project, successor of the Blue Brain Project (Makram 2004) promises to create a framework which allows for the integration of experimental data and theoretical hypotheses targeting different levels of organization of biological organisms and their psychological functions. One of the strategic objectives of the project is to generate powerful simulations of the mouse and human brain which would complement the existing experimental data by connecting different levels of biological organization, and enabling in silico experiments that cannot be carried out in the laboratory. Promoters of the program claim that the simulation of neurobiological models at different levels of description, such as abstract computational models, point neuron models, detailed cellular level models of neuronal circuitry, molecular level models of small areas of the brain, multi-scale models that switch dynamically between different levels of description, will help experimentalists and theoreticians to choose the appropriate level of detail for asking new questions and exploring new hypotheses about the cognitive architecture of the brain and the neural realizers of different cognitive functions.

This type of project raises a series of important philosophical questions about the epistemic roles that large scale computational simulations play in the development of successful cognitive theories. How can simulations facilitate our understanding of the mechanisms underlying various cognitive functions like spatial perception, face recognition, reading, or language learning, among others? A preliminary response is that computational simulations allow formalization and testing of multiscale cognitive models. As such they constitute ways of exploring the limits of current theoretical proposals that cannot be directly assessed in an experimental setting. Another epistemic advantage of using computer simulations within cognitive neuroscience is that they allow the integration of cognitive models developed at different spatial and temporal scales thus producing a type of synthetic knowledge which is critical to understanding psychological phenomena and their neurobiological underpinnings.

However these epistemological and methodological advantages have also been challenged on the grounds that simulations make the relations between different levels of biological organization epistemically opaque. In addition, simulations are criticized for occluding the lack of proper empirical support for certain theoretical models used in cognitive neuroscientific research. For instance, advocates of the Brain Initiative emphasize the need to develop better technologies for collecting more data about the neuronal structures underlying different cognitive functions. They claim that only in light of a complete experimental knowledge we can hope to provide an empirically adequate explanation of the observed psychological patterns and behaviors.

Despite the theoretical challenges facing the simulations method, I claim that the latter allows the development of hybrid explanatory strategies which help advance our understanding of how biological organisms like ourselves can achieve the impressive cognitive features and complex behaviors observed on a daily basis. Drawing on a class of models used in language acquisition and language learning studies, I defend the epistemic advantages of using computational simulations for the purposes of investigating the neural bases of cognition.

❧

Hermeneutic Marginalisation and Economic Policy Modelling

– Anna de Bruyckere (Durham University)

Abstract. This paper discusses economic policy advice: economic policy advisory bodies that co-shape economic policy design, implementation, and reform. Examples include the Netherlands Bureau for Fiscal Policy Analysis and the Belgian Federal Planning Bureau, though internationally, the types and roles of advisory bodies vary significantly. Typical domains of advice include tax systems and welfare infrastructure—issues which are contested, value-laden, and conceptualisable in more than one way.

My talk starts from the observation that the distinctly epistemic dimension to policy modelling remains underexplored. Other disciplines have studied the politics, culture, and sociology of economic policy advice (for Dutch examples: [1][2]). Yet, I argue that the epistemic tools constructed and used in policy advice – with 'tools' as shorthand for 'models, theories, calculation methods, data gathering protocols' etc. – deserve attention in their own right, for how they shape our possibilities for meaningful thought, speech, and action.

Drawing on macroeconomic case-work in healthcare and population aging modelling and informed by the literature on the normative and social dimensions of modelling and quantification (e.g., [3][4]), my own contribution lies in sketching what I call a phenomenology of economic policy modelling and the ethico-political questions such model use poses. I argue that it shapes our sense of policy desirability, legitimacy, feasibility, and effectiveness—thus being intimately tied up with our construal of their ethical dimensions, too.

To this purpose, I extend and adapt Miranda Fricker's concept of hermeneutic marginalization (HM) [5]. I define HM in this context as follows: the ways in which the epistemic tools of economic policy agencies can come to disadvantage certain approaches to, or positions regarding, a specific institution or policy relative to other approaches or positions. I distinguish two types of marginalization.

First, epistemic tools shape perceptions of what is fundamental and supposedly 'real'. I call it objectual marginalization when these come to hamper serious uptake of positions or approaches that conflict with the model's reality. Second, economic modelling practices have come to set standards for rigorous, serious discourse. Approaches or positions which take an issue to be better expressed in ethico-politically thicker, less technical ways, may be disadvantaged by their 'deviating' discourse. I call this discursive marginalization.

'HM' thus aims to articulate how modelling influences our judgments of what positions and approaches are 'apt' for policy issues at hand, scrutinising the epistemic rationality of policy modelling practices more generally. It allows for exploring the non-neutrality of epistemic practices of policy advisory bodies without reducing them to ideology or politics.

I argue that without attention to the epistemic tools of economic policy modelling and their cognitive effects, philosophical scrutiny of policy modelling remains incomplete (as does philosophical inquiry into scientific modelling practices more generally). The question then arises, not so much whether to use scientific knowledge and methods for policy making, but how to do so and doing it well.

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❧

About “Numerical Experiments”

– Julie Jebeile (Université Paris-Sorbonne)

Abstract. It is commonly assumed that knowledge obtained by models does not have empirical origin and is in this sense not as reliable as empirical knowledge. However, doubts have arisen whether knowledge generated by computer simulations could not legitimately be considered empirical knowledge since they bear a strong resemblance to experiments in many respects (see, e.g., Guala 2002; Morgan 2003, 2005; Winsberg 2003). From a commonly-held view, based on similarities between simulation and experiment, one should be allowed to extend certain epistemic properties of experiments to simulations. But, once we acknowledge the similarities between computer simulations and experiments, can we conclude from them that simulations generate empirically reliable knowledge as experiments do? In this paper, I identify these similarities, and examine whether, in accordance with the analogy, they give simulations and experiments the same epistemic properties.

I first investigate four common features shared by simulations and experiments which are often highlighted by philosophers:

(i) Simulation and experiment allow for exploration: simulation consists in mathematically exploring the empirical implications of the underlying model, while experiment consists in exploring the phenomena by providing observations and measures.

(ii) Scientists intervene on both of them: (i) implies that they intervene on the simulation program or the experimental setup.

(iii) Both sometimes make it possible to visualize the system under study (this is not always the case since though, for example, there is no phenomenon to visualize in the experiments of particle physics).

(iv) They both sometimes function as black boxes. An experiment functions like a black box when the experimenter does not know some (or all) physical processes at work in the observed phenomenon. A computer simulation also works as a black box due to the complexity of the program and the speed of the computational process, which makes the process opaque.

I then examine whether these similarities give simulations the two main epistemic functions usually assigned to experiments, i.e. either producing new empirically reliable knowledge or possibly contradicting our best theoretical assumptions.

From this study, I contend that the similarities between simulation and experiment give the scientist at most the illusion that she is facing an experiment, but cannot seriously ground the analogy. In other words, it is not in virtue of these similarities that simulations can provide empirically reliable knowledge. The reason for their epistemic function has to be found elsewhere in the verification and validation of the model content.

I conclude that the analogy between simulation and experiment does not work, but it does not mean that experiment is always epistemologically superior to simulation. While some philosophers (e.g. Mary Morgan and Ronald Giere) take for granted this empiricist presupposition, I show that such a presupposition holds less frequently that we might think.

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❧

An Information-Theoretic Model of Scientific Reasoning

- Agnes Bolinska (University of Toronto)

Abstract. When little or nothing is known about a phenomenon, scientists may learn about it by consulting extant theory or gathering empirical evidence. But there are many ways in which they may do this, and some may be more successful than others. In this paper, I argue that the order in which evidence is considered affects the efficiency of a reasoning process and suggest a measure for determining efficiency. I use as examples the determination of protein and DNA structure and conclude by showing how the construction of molecular models further contributed to this efficiency.

In the cases of protein and DNA, the determination of molecular structure was primarily informed by two sorts of evidence, which I refer to as data: x-ray diffraction photographs, produced when x-rays shone at a molecule are scattered and captured on a photographic plate, and stereochemical rules dictating permissible molecular configurations, given a molecule's atomic composition. Because x-rays are reflected but not subsequently refracted to produce a diffraction photograph, interpretation is required to determine structure from such photographs. Interpretation is also required to apply stereochemical rules to molecules.

I characterize the process of determining molecular structure as one of eliminating structural candidates through the successive interpretation of pieces of data. I argue that data serve as constraining affordances for molecular structure: the interpretation of such data yields information about structure by warranting both the elimination of certain structural possibilities and the retention of others for further consideration. Interpretations of data vary with respect to how many structural candidates they eliminate. They also vary with respect to how certain scientists could be that only incorrect structures are eliminated upon interpretation. I introduce the notion of informational entropy to show that an efficient strategy for reasoning about structure is one that, on average, maximizes the number of possibilities eliminated with each interpretation and the likelihood that those possibilities will be correctly eliminated.

I apply this notion to the cases of protein and DNA structure determination to argue that the strategy of considering stereochemical rules before x-ray diffraction photographs was more efficient than one in which this order is reversed. Then, I show that the construction of molecular models further increased the effectiveness this strategy in two ways: by serving as a concrete means of prioritizing the stereochemical rules in scientists' reasoning and functioning as a cognitive aid, enabling scientists to consider many more such rules at once.

Parallel Session 5E

Thursday, 25 June 2015 at 15:30–17:30 in G4

Session chair: Leah McClimans (University of South Carolina)

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Knowledge and Its Limitations in Otolaryngology

– Anaïs Rameau (Department of Otolaryngology, Stanford University)

Abstract. Knowledge in surgical specialties is powerful, in that it underlies decision-making regarding invasive procedures that are costly and irreversible. The goal of this paper is to review methods of knowledge production in otolaryngology and some of their limitations. Otolaryngology is the discipline of medicine focused on pathologies affecting the head and neck. Though considered a surgical specialty, otolaryngology also involves non-surgical management of disease, provided in clinic. This project is a critical analysis of the current state of knowledge in otolaryngology, and an exhortation towards greater quality of research and increased integration of various ways of knowing in this surgical subspecialty. I hope that this project will help otolaryngologists be more conscientious about knowledge generation and better identify potential sources of bias and error in their knowledge base and research methods.

First, I will look at sources of knowledge in otolaryngology, focusing on issues arising with the ever-expanding character of our sources of knowledge. I shall demonstrate that the knowledge base in otolaryngology, best represented by current published literature, is broad, in constant flux, often uncertain, difficult to navigate and not representative of the burden of head and neck disease.

Second, I will describe two longstanding paradigms in medical epistemology: rationalism, which focuses on mechanical and pathophysiological reasoning, and empiricism, which relies on clinical observations and epidemiology, and show that both are in a tug of war in otolaryngology. I will examine some limitations of each paradigm.

Third, I will look at the state of evidence-based medicine (EBM) in otolaryngology. I will demonstrate that otolaryngology, like other surgical specialties, is lagging behind in its adoption of EBM. The slow uptake of EBM methods has consequences for the purported quality and the funding of research in otolaryngology, and for health policy decisions impacting surgical practice.

Fourth, I examine the practical, epistemological and sociological reasons otolaryngologists have demonstrated a slow adoption of EBM. Practical challenges with performing surgical trials can be divided into three categories: challenges with the design and reporting of surgical RCTs, challenges related to the complex nature of surgical interventions, and challenges related to external factors, such as financial constraints and a lacking regulatory environment for surgical innovations. I will also argue that the nature of surgical craft promotes rationalism through mechanistic reasoning, over EBM's empiricism. Surgical work involves manifold contingencies that are difficult to assimilate in the standardized and technical framework of EBM, leading to "occupational resistance" against the standardization thrust of EBM.

Finally, I will argue that otolaryngologists need to be aware of the background assumptions underlying their ways of knowing, in order to critically engage with their knowledge base. I will draw from Helen Longino's analysis of the social dimension of scientific inquiry. I will suggest meta-research – a new field of medical investigation concerned with the aim of applying research methods to study how research is done and how to promote the use of best scientific practices – could be a tool in achieving critical awareness of background assumptions in otolaryngology.

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Neglected Tropical Diseases: A Case for Epistemic Pluralism

– Erman Sozudogru (UCL)

Abstract. In this paper, I argue that drug discovery involves multiple systems of practices. My aim in doing this is to articulate a normative account of epistemic pluralism, which we might define as a philosophical thesis that claims that no single system of practice can explore and explain all aspects of some phenomena of interest.

I use the case of neglected tropical diseases (NTDs), which are a group of infections that are under-researched by the pharmaceutical industry due to their low profit potential. More specifically, I concentrate on Human African Trypanosomiasis (HAT) research. HAT is a parasitic infection that is prevalent in sub-Saharan Africa affecting extreme poor in rural areas. Like other NTDs, HAT patients' lack of ability to pay for market-financed therapeutics is the cause of the low profit potential and the lack of research in the field.

HAT research takes place in public-private-partnership (PPP), which are global networks of academia, industry, governmental and nongovernmental stakeholders. These PPP networks are an exemplary case study of the interactions between systems of practices: where they interact in order to investigate different aspects of the phenomenon – for instance, medicinal chemists' work is informed by the work of structural biologists which is informed by the work of molecular biologists. Plurality in practices in this case is essential since none of these systems are capable of finding a desired cure for HAT alone. Moreover, HAT research allows us to further the normative aspect of pluralism by allowing to demonstrate benefit of pluralism based on the aim of research. The aim is to find an adequate cure to eradicate HAT, which is shaped by epistemic values (linked to furthering knowledge, understanding and explaining the phenomena) and non-epistemic values (linked to the broader social and historical context). PPPs undertaking HAT research determines the overall normative values that guide the process allowing us to underline how non epistemic values (whether linked to socio-economic conditions in disease endemic regions or values linked to economic interest) play a significant role in shaping the overarching values and therefore influences the scientific practice.

Here, each system of practice contributes towards aims that are defined by both epistemic and non-epistemic values. Moreover, the multiplicity of systems of practices in this kind of scientific inquiry is non-eliminable and it is beneficial to the aims of research.

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Kinds and Degrees of Scientific Understanding in Medicine

– Leen De Vreese (Ghent University)

Abstract. Recently, a renewed interest in the topic of scientific understanding has arisen within philosophy of science. The topic is often approached from a very general point of view and specific philosophical theories of scientific understanding are thereby often defended on the basis of single case-studies that are supposed to be representative for science in general. My interest, to the contrary, lies in the opposite approach: trying to get a grip on scientific understanding by looking at what scientific understanding comes down to within specific domains of science (as has also been done by different contributors in the book of de Regt, Leonelli and Eigner (2009)).

My specific interest for this paper lies in the domain of the medical sciences – a domain which has, as far as I know, not yet been tackled within the literature on scientific understanding. In my talk, I will briefly present some classes and cases of diseases and their (possible) "explanations" or "interpretative frameworks" (cf. Boon 2009) – in terms of (pathological) mechanisms, proximate causes, risk factors, explanatory models or classifications. These cases will be compared and used as a basis for discerning and discussing different ways in which diseases can be said to be "scientifically understood". I will further argue that it is useful to think about scientific understanding as a context-dependent matter: the question whether or not a certain explanation, theory or framework provides

adequate scientific understanding of a phenomenon will need to be related to a certain underlying epistemic interest of the researcher, the research community, the society, the patients,… that are involved.

The cases further show that it is useful to think about different extents to which diseases can be scientifically understood. In other words: scientific understanding in medicine seems to be a gradual matter. I will focus on this graduality of scientific understanding in the remainder of my talk. The following questions will be tackled. Is there a way to pinpoint the extent to which scientific understanding is achieved in medicine? What makes partial explanations partial, and full explanations full? What makes partial explanations useful? Does something like full scientific understanding in medicine actually exist? How should we define it? And is it always useful to strive for it? Is full scientific understanding a central goal of medicine, or rather an illusion or even a useless aim?

Finally, I will briefly comment on some further consequences of my findings. Are my claims generalizable to other domains of science, or do they rather point at some peculiarities of scientific understanding in medicine? And what does all this imply for the existing theories of scientific understanding?

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❧

Biological Organization, Diseases and Normativity in Medicine

- M. Arantzazu Etxeberria (University of the Basque Country)

Abstract. Naturalism in medicine aims to identify diseases as wrong conditions of the biological organization, understood as a complex abstraction in which of constitutive, interactive and experiential aspects need to coexist. The main naturalist approaches (functional, mechanist, systemic) fail to produce fully comprehensive descriptions of the “right” biological organization, or of the broken versions constituting diseases. As a consequence, a major philosophical issue is how medicine can rely on knowledge about biological organization to identify diseases and to propose how to cure or treat them.

To answer that question my strategy is to look at the practice of medicine in search of some basic presuppositions:

- Biological organization is the object of biology; the entities and processes involved are complex in their constitutive, interactive and experiential dimensions.
- As a normative discipline, Medicine evaluates when something goes wrong in a given living being and identifies diseases. Scientific (biological, ecological, etc.) descriptions guide the identification, but they are not normative in the same way as medicine is.
- For medicine diseases are objective and real: they are bad conditions of the biological organization of a living organism. The objectivity and reality of diseases must be assumed, otherwise those conditions would be falsely identified as diseases.
- Naturalism about concepts of health and disease suggests that medicine always relies on theories that are well established in biology and that according to them, it is possible to demarcate what is wrong in a living organism. According to this position the normativity of medicine is wholly based in science, but epistemically it is too optimistic about how the descriptive knowledge of biological organization motivates normative judgements.
- Strong normativism about concepts of health and disease suggests that medicine does not rely on biological theories to normatively identify diseases, but wholly depends on social, cultural,

or economic factors. Therefore all diseases are somehow subjective or socially constructed. Epistemically, this position holds a too pessimistic view about how the descriptive knowledge of biological organization motivates the normative judgements of medicine.

To conclude, the paper will argue in favour of a weak normativism compatible with methodological naturalism, according to which the normativity of medicine is grounded in different knowledge sources, as not always the same sorts of evidences are invoked, and diseases are ontologically characterised by a multiplicity of ways of being. Thus the knowledge of medicine is not fully conclusive: it can change in time, especially when conditions previously considered to be diseases are shown not to be so (because they are not objective) and, conversely, we might discover that something previously not considered to be a disease really is such (because there are arguments and evidences for objectivity). This position avoids both the excessive optimism and pessimism present in naturalism and strong normativism.

Parallel Session 6A

Friday, 26 June 2015 at 09:00–11:00 in Aud F

Session chair: Adam Toon (University of Exeter)

Organized by: Chiara Ambrosio (UCL)

Symposium: Aesthetics in Scientific Practice

Synopsis. Recent developments in philosophy of science have seen a progressive convergence with debates in the field of aesthetics. The aim of this symposium is to explore this convergence with a close eye to practice, and tackle some key questions that explicitly invite an investigation of the crossovers between aesthetics and epistemology. What drives scientists' preference toward the beauty or elegance of particular theories? How do these notions relate to truth, epistemic success, accuracy and predictive power? In attempting to answer these questions we look specifically at how practitioners articulate their aesthetic commitments, and how (and whether!) aesthetic judgments inform, infuse, and ultimately provide a constitutive basis for, a range of practices in science.

We begin with a practice-oriented account of two quintessentially theoretical constructs: truth and beauty. A widespread view in philosophy of science relates the beauty of theories to their objective features, their truth, and their epistemic success. What our panel discloses, through a careful investigation of scientific practice in its historical development, is a more subtle and nuanced picture: it is often the epistemic success of theories that determines scientists' aesthetic preference for the relation between truth and beauty.

Considered in eminently practical contexts, aesthetics can also serve as a powerful challenge – both internal and external – to the authority of science. This is another angle of the convergence between aesthetics and epistemology, and of the constitutive relation between aesthetics and science more broadly, which our symposium plans to address in close connection with practice. We explore instances in the parallel histories of science and art in which aesthetics served the critical role of disclosing novel phenomena worthy of investigation in their own right, thus contributing to challenge assumptions that scientists tended to accept unquestioningly.

An important development in the dialogue between aesthetics and philosophy of science relates to the current debate around scientific representations and the practice of modelling. It is here that philosophers of science have engaged with debates in aesthetics, and even art practice, in the most forceful way. Thus, the normative connection between aesthetics and science, investigated in the first part of our symposium with reference to the relationship between aesthetic judgments and epistemic success, is here mapped on the particular case of the construction and use of models and representations in science. What is the role played by aesthetic judgments in the recognition and evaluation of the epistemic fruitfulness and accuracy of models and representations? Would aesthetics complement epistemology in the quest for the constituents of scientific representation, and if so how? Our symposium addresses a particular aspect of the debate around scientific representation, one which, incidentally, intersects debates on truth, realism and the aims of science more broadly: the controversial status of similarity and/or resemblance in representation. While we tend to agree that resemblance does not exhaust representation by providing a single necessary and sufficient condition, our contributions offer different – but complementary – views on the role that this concept can play in science, as well as an assessment of the shortcomings and misconstructions of this notion when examined in disconnection from practice.

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Why Do Scientists Find Beautiful Theories Aesthetically Attractive?

– James W. McAllister (Institute of Philosophy, University of Leiden)

Abstract. This question seems to admit an obvious answer—"Because beautiful theories naturally are aesthetically attractive!"—but things are, of course, more complicated. Some scientists and philosophers have linked the beauty of theories to their objective, epistemic and empirical merits, such as being true or being valid. Speaking more strictly, these writers have claimed that some structural and formal properties of theories, perceivable by scientists, promote, accompany or are indicators of epistemic or empirical attainments of theories. On the other hand, scientists have certain tastes as to the structural and formal properties of theories: they find theories that display some properties of these kinds aesthetically attractive, and others not. The question is, why are scientists predisposed to find aesthetically attractive the structural and formal properties of theories that are linked to epistemic and empirical attainments? In other words, why, if some aesthetic properties of theories are connected with truth, are scientists' aesthetic tastes tuned to precisely those properties?

There are three possible strategies to explain this coincidence. First, one may posit an identity of truth, beauty, and aesthetic attraction: it is metaphysically or conceptually necessary both that beauty is linked with truth and that we find beauty aesthetically attractive. Second, one may argue that epistemic and empirical success is, at root, an aesthetic attainment: our finding theories aesthetically attractive is in some way constitutive of their having epistemic or empirical success. Third, one may argue that the epistemic or empirical success of theories is partly responsible for our finding those theories, and the structural and formal properties that they display, aesthetically attractive.

In this paper, I will assess these three strategies. I will conclude that the first meets insuperable difficulties, especially in the light of historical evidence that scientists' aesthetic preferences change in time. The second strategy has difficulty in explaining cases in which scientists concede that a theory is empirically successful but reject it as aesthetically unattractive. I shall argue that the third strategy does most justice to evidence from the history of science.

In the final part of the paper, I shall discuss the implications of saying that the epistemic or empirical success of theories partly determines scientists' aesthetic tastes for the link between beauty and truth, for scientists' behaviour in theory choice, and for the nature of scientific revolutions.

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Who is Afraid of Mimesis?

– Chiara Ambrosio (Department of Science and Technology Studies, UCL)

Abstract. "All epistemology begins in fear", claim Daston and Galison (2007: 372) in the final chapter of their history of objectivity. Their account shows that our relationship with objectivity coincides with the story of the scientific self, and of the epistemic virtues that communities cultivate explicitly to "discipline" their practitioners. In this paper, I aim to show that the notion of representation in philosophy of science, and in particular that of mimesis, followed a fate very similar to that of objectivity. Specifically, I claim that the somewhat tormented relationship philosophers of science have developed with mimetic accounts of representation marks just another chapter in the history of epistemic fear.

I begin by offering an alternative angle to McAllister's claim (in this symposium) that the empirical success of theories guides scientists' aesthetic preference for the link between beauty and truth. I

explore a slightly different aspect of the relation between aesthetics and epistemology: instances in which aesthetics acts as a trigger not so much for scientific revolutions, but for (apparently minor) changes in ways of seeing. I claim that these cases contribute to the refinement of the formal properties of theories, ultimately playing a role in their empirical success. Drawing on Jacques Rancière's (2013) view of the critical role of aesthetics as inducing a "redistribution of the sensible", I investigate episodes in which aesthetic decisions (interestingly coming from artistic practice) productively contributed to challenge established assumptions in science.

In the second part I claim that, historically and practically, mimesis is a crucial catalyst to maintain such critical role for aesthetics. A widespread criticism of mimetic accounts of representation is that they are merely a "common sense" view, built on the assumption that representation can be exhausted simply by postulating a mirror-like, dyadic relation between a representational source and its target. But as Halliwell (2002) argued, this kind of criticism was far more nuanced even in Plato, credited as one of the earliest and most adamant critics of mimetic accounts of art (and knowledge more broadly). Following recent accounts of mimesis as a form of Peircean iconicity, I argue that important shifts in systems of representation and classification in science were possible precisely because of scientists' flexible attitude towards resemblances between properties or states of affairs considered relevant for particular purposes. I thus suggest that it is more productive to investigate resemblances in the plural: not a single necessary and sufficient condition, but a set of criteria that can change (within constraints) on the basis of representative goals and practices.

I conclude by arguing that devaluing resemblance is a way of devaluing, among other things, the critical role of aesthetics more broadly. The wilful rejection of resemblance, well exemplified by avant-garde experiments in the visual arts, is itself based on a recognition of it as a relevant representative relation. I claim that the same holds in the case of scientific representations. Philosophical accounts that reject mimetic accounts neglect that the critical core of representative practices consists in coming to terms with such a foundational notion, either to embrace it or to depart from it: mimesis is productive precisely because it contains in itself the seed for its rejection.

Who is afraid of mimesis, then? Surely neither artists, nor scientists: their daily practices primarily consist of discovering, negotiating, challenging resemblances. The upshot may be that philosophers are far more concerned about it than needs be, and in doing so they are depriving philosophy of a construct that has a great deal to offer, conceptually and in real life.

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❧

Resemblance and Its Discontents in Art and Science

- Mauricio Suárez (Institute of Philosophy, London University & Complutense University, Madrid)

Abstract. I have in the past claimed that model-building is infused with aesthetic as well as epistemic goals. In particular I believe that there is a norm of acceptance for models that involves aesthetic judgements of 'elegance', and that 'elegance' is best understood as advancing inferential expediency. Hence there is a deep constitutive or normative link between aesthetics and science. In this talk I argue that there are correspondingly formal analogies between some well-known discussions regarding the nature of representation in art and science. In particular I focus on the implications for resemblance theories of representation on which, roughly, some formal similarity of apparent features ("resemblance") is required between a representational source and its target.

Charles Peirce (1931, Ch.3) carefully distinguished icons from symbols and indexes, and reserved the application of resemblance to the former type of iconic representations. Indeed the most perspicuous application of the resemblance theory is to the plastic and fine arts, and in particular painting.

Portraits are archetypal icons, which often resemble their targets. Yet, even with respect to icons the attempt to analytically reduce representation to resemblance fails. The essential problem was characterised precisely in logical terms by Nelson Goodman (1976). On Goodman's alternative account all representation is instead symbolic and reliant on denotation.

I first review my past argument that a consideration of representational practices, both in the sciences and the arts, invites a distinction between two questions, namely a prior question regarding the constitution of representation, and a secondary question regarding the degree to which a representation qua representation is faithful or accurate. The latter question can only be posed once the former has been addressed and answered positively. In other words, we are in practice only ever in a position to address the accuracy of a representational source, qua representation of some target, if we already accept that it is indeed a representation.

I then argue by inspecting a number or examples in art that the resemblance theory may to some extent successfully address the question of faithfulness but it is unable to address the constitutional question. This seems true also for sophisticated versions of resemblance, such as Tversky-similarity (Weisberg, 2012). By contrast, I suggest that a functional version of denotation addresses the constitutional question, but it fails to address the faithfulness question altogether. I end by suggesting that representation in both science and art is in general a hybrid notion, including both symbolic aspects related to denotative function and iconic aspects related to resemblance – yet it is exhausted by neither denotation nor resemblance.

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❧

'Creative Similarity' in the Understanding of Science and Art

- Julia Sánchez Dorado (Department of Science and Technology Studies, UCL)

Abstract. In the past years, numerous philosophers of science have discussed the role played by similarity –between scientific models and the objects of the world they refer to– in the obtaining of fruitful scientific representations.

In this paper, I will try to defend that, against some of the strictures formulated on its value, it is epistemically advantageous to conserve the idea of similarity to explain how scientific representations advance understanding about the world. But to succeed in the attempt, it will be indispensable to develop a specific approach to similarity that goes beyond the constrained explanations of it often proposed in the field. 'Similarity' is a many-sided term, and accordingly diverse and usually conflicting accounts of it have been lately endorsed –namely, accounts of isomorphism, homomorphism or similarity as resemblance.

Here, I will defend a more integrating approach that takes these and other varieties of similarity as compatible in principle to each other. The key of my account will be the characterization of similarity as inseparable –but compatible with– distortion of different kinds, the two of them interlaced in the same creative practice of representing and leading to a particular goal.

To be able to reach the former proposal, the strategy of enquiry I shall develop is the establishment of a dialogue with the field of aesthetics, in which there is a much longer tradition discussing the problems of representation and similarity. Considering how in modern aesthetics questions about similarity were raised, and possible answers to them were offered, can help us look with new eyes recent debates in philosophy of science, especially when the object of analysis are scientific and artistic practices. In the present paper, I will mainly refer to debates that took place in the avant-gardes period at the beginning of the twentieth century. At first sight it can seem that allusions

to similarity were radically rejected to explain the nature of artworks at that time. But quite the opposite, very interesting reflections on how to reinterpret and reconsider similarity can be found in writings on depiction and artistic practices of that period. Perhaps, artworks could not be explained exclusively in terms of "similarity of appearance" since then anymore. But far from disappearing, other kinds of similarity (perceived similarity, structural similarity, conceptual similarity) still have a presence in the theorizing of modern art.

A stimulating case study I would like to examine more in depth is Kandinsky's theoretical-applied treatise Concerning the Spiritual in Art (1910). Here, genuine representations are characterized by the presence of singular kinds of similarity that go hand in hand with distortion and with changes of the features of the object represented. Following his argumentation and connecting it to recent discussions on scientific practices, similarity should not be understood as a set of fixed features of the objects of the representation (vehicle and target). Quite the opposite, similarity should be taken as a characteristic of the process of representing, which is first and foremost a creative practice designed to fulfil a goal in mind –epistemic, aesthetic or both.

Parallel Session 6B

Friday, 26 June 2015 at 09:00–11:00 in G1

Session chair: Inmacuala de Melo-Martin (Weill Cornell Medical College)

Organized by: Evelyn Brister (Rochester Institute of Technology)

Symposium: Interdisciplinarity, Sustainability Science, and Philosophy of Science Beyond the Disciplines: Commentary on Robert Frodeman's Sustainable Knowledge: A Theory of Interdisciplinarity

Synopsis. Robert Frodeman's 2014 Sustainable Knowledge: A Theory of Interdisciplinarity (Palgrave/Macmillan) asserts that the modern university system, created in the late 19th century and developed through the 20th century, was built upon the notion of distinct disciplines which extend knowledge through subject matter specialization. Today, Frodeman argues, the social, epistemological, and technological conditions that supported the disciplinary pursuit of knowledge are coming to an end. Knowledge production has itself become unsustainable: we are drowning in knowledge even as new PhDs cannot find work. Sustainable Knowledge explores these questions through the idiom of sustainability, using examples from environmental inquiry and problem-solving, and offering a new account of what is at stake in talk about 'interdisciplinarity'. The book develops two positive themes. First, it offers an account of contemporary knowledge production in terms of the concepts of sustainability, disciplinarity, and interdisciplinarity. Second, it reconceives the role of philosophy and the humanities both within the academy and across society. It argues that philosophy and the humanities must reinvent themselves, taking on the Socratic task of providing a historical and philosophical critique of society.

This author-meets-critics panel engages Frodeman's ideas concerning interdisciplinary research with an eye toward addressing the challenges and opportunities for philosophers of science. In Sustainable Knowledge, Frodeman describes his own work with the US Geological Survey and on science and environmental policy, and he champions ways for philosophers to engage with issues of science policy inside and outside the university. He raises questions about disciplines, professional institutions, socially relevant science, and the function of philosophy in our technoscientific, hyperconnected contemporary world.

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Interdisciplinarity, Sustainability Science and the Philosophy of Science: Robert Frodeman's Sustainable Knowledge

- Paul B. Thompson (Resource Economics and of Community Sustainability, Michigan State University)
- Danielle Lake (Liberal Studies Department, Grand Valley State University (Presenter))

Abstract. The value and need for coordinated multi-disciplinary, cross- and inter-disciplinary research is increasingly recognized. Philosophers working in epistemology and in the philosophy of science have argued that key barriers to successful interdisciplinary science reside in a failure to recognize the way that epistemic values, methodological traditions, and both metaphysical and meta-ethical commitments tend to be both shared within disciplinary traditions, while divergence is observed when different disciplines are compared. As such, interdisciplinary work is often assisted by explicit identification and discussion of these philosophical commitments, even if strict agreement on such commitments may not always be required.

In a related vein, the U.S. National Academy of Science created a new section for sustainability science in its journal PNAS in 2007. Sustainability science was conceptualized as neither applied nor basic curiosity driven research, but as a domain of problem or use inspired research questions that would require significant breakthroughs and advances in understanding to resolve. Sustainability science has also been characterized as science undertaken in response to "wicked problems": challenges with large social and economic stakes, irreversible consequences, multiple stakeholders, high levels of uncertainty, low tolerance for error and little agreement about the fundamental problem definition. Sustainability science was not necessarily characterized as interdisciplinary in this literature, though contributions to PNAS's sustainability science section often do have authors from more than one discipline.

Robert Frodeman's 2014 book Sustainable Knowledge: A Theory of Interdisciplinarity links these two themes through an examination of the institutional setting for science in contemporary research universities, and an examination of how humanities disciplines (and philosophy in particular) must take on the task of challenging the barriers that current institutions pose to genuinely sustainable knowledge production. Frodeman suggests that the role philosophers play in facilitating interdisciplinary conversations must be augmented by critique of the organizational and incentive structures currently being perpetuated in universities and disciplinary organizations. This paper reviews Frodeman's key claims and links them to the recent literature on sustainability science. It sets the stage for other papers in the panel that engage Frodeman's work at a critical level.

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Interdisciplinarity, Rigor, and Deaccelerating the Growth of Knowledge

- Evelyn Brister (Rochester Institute of Technology)

Abstract. In his 2014 book Sustainable Knowledge: A theory of interdisciplinarity, Robert Frodeman says that there are two predominant attitudes toward interdisciplinarity: the booster and the skeptic. He is primarily a booster while noting that there are different ways of working between and beyond traditional disciplines. For example, typical multidisciplinary scientific research draws on disciplinary experts to contribute their skills to a larger project without the need for much communication or transformation of their disciplinary orientation. This form of interdisciplinary work is an efficient form of dividing intellectual labor which is best applied to already-familiar types of research problems, and it is not really the object of his interest. Frodeman is much more interested in what some call trans-disciplinary research: research directed at solving some unique and complex real-world problem which requires the forging of brand new research tools and perspectives. He is a booster of this form of interdisciplinarity and examines how philosophers can better engage in it. He is a skeptic, however, about there being a science or a uniform logic of this more complex

form of interdisciplinary inquiry. He also points out the irony of how some interdisciplinarians have effectively mimicked the institutional forms of disciplinarity by creating university departments, academic journals, and professional societies.

I, too, am both a booster and a skeptic when it comes to interdisciplinary inquiry. Like Frodeman, I think we are forced into being boosters because some problems require creative input from numerous disciplinary forms of expertise. Frodeman uses sustainability science as a paradigmatic case of these kinds of problems. There are few environmental problems which don't require the attention of both natural and social scientists—whether related to climate change, species preservation, pollution control, or energy production.

I also consider two specific forms of skepticism and will invite Frodeman to respond to how they fit his proposed framework for sustainable knowledge production. First, I defend disciplines as a site of necessary argumentation concerning rigor. Rigor, according to Frodeman, is too expensive. The challenges we face are urgent and disciplinary rigor costs time and research money to develop. Because it distracts us from the quickest path to solving acute problems, developing disciplinary rigor is a trade-off with fostering interdisciplinary breadth. In contrast, I analyze rigor in terms of standards of evidence, and I argue that developing context-appropriate standards of evidence is a difficult but worthwhile job.

Second, I call attention to the pressure to accelerate the rate of scientific knowledge production. Academic database, search, and bibliometric tools are increasingly automated, and there is a desire to reduce the time that scientific researchers must spend combing through the recent research literature in order to stay current. I argue that there are reasons to think that some friction in the process of research may improve the quality of scientific research at the acceptable cost of slowing it down.

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Why Has Applied Philosophy Run Out of Steam

– David Budtz Pedersen (Humanomics Research Centre, University of Copenhagen, davidp@hum.ku.dk)

Abstract. In his 2014 book, *Sustainable Knowledge: A Theory of Interdisciplinarity* Robert Frodeman goes to great length to show how the philosophy discipline has lost contact with society. Numerous societal problems call for philosophical analysis but what society gets from philosophy are mostly abstract models that simulate highly idealised intuitions, behaviours and situations – to the effect that philosophy has become almost irrelevant. In response to the lack of applicability, the philosophy discipline has branched out into a subfield called applied philosophy. But according to Frodeman, the same tragedy happened once again. The applied philosophy literature is full of insights about practical problems. But there are very few accounts of how a philosopher is supposed to ensure that these insights have an impact on real societal problems and practices. This, according to Frodeman, is deeply rooted in the disciplinary ethos of philosophy: "one has exhausted one's intellectual task and professional obligation when one deposits a peer-reviewed publication in a reservoir of knowledge" (Frodeman & Briggle 2015). Absent in the academic community is any reflection about how to actually get involved with real stakeholders in particular policy-makers, and how to effectively interject insights into real life situations and conversations. Frodeman sees "the discipline" as the main obstacle. Philosophy, he argues, should never have developed disciplinary features but should instead have kept mobile crossing boundaries between the other sciences and facilitating interdisciplinary dialogue by not belonging to any disciplinary hierarchy itself. In this commentary to Frodeman's book, I revisit the strategy of dedisciplining philosophy and critically examine what philosophy can do promote ideal theory in a non-ideal world, and how policy-makers are increasingly calling for philosophy – and the humanities in general – to become part of the interdisciplinary conversation.

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Sustainable Knowledge: Philosophy of Science in the Field

– Robert Frodeman (Dept. of Philosophy and Religion Studies, Center for the Study of Interdisciplinarity, University of North Texas)

Abstract. I will respond to points raised by Paul Thompson and Danielle Lake, Evelyn Brister, and David Budtz Pedersen, discussing how their criticisms and insights engage with my proposed theory of interdisciplinarity. Philosophers of science who focus on practice should consider how they can be field philosophers, working directly on socially relevant science and public policy.

Parallel Session 6C

Friday, 26 June 2015 at 09:00–11:00 in G2

Session chair: Joseph Rouse (Wesleyan University)

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Mechanistic Explanations of Physical Laws: How Do They Provide Understanding?

– Erik Weber (Universiteit Gent)
– Joachim Frans (Vrije Universiteit Brussel)

Abstract. In the literature on scientific explanation, there is a classical distinction between explanations of facts and explanations of laws. This paper is about explanations of laws. As our title suggests, our main question will be: how do mechanistic explanations of physical laws provide understanding?.

Hempel's views on explanation are known as the covering law model. Applied to laws, the idea is that laws have to be explained by subsuming them under other laws. We have to show that the explanandum law could have been expected given the laws in the explanans. Lower-order laws (laws with a relatively narrow scope) are explained by showing that they follow from higher-order laws (more encompassing laws). In many cases, the covering law model represents the explanations physicists give for laws. We explain, for example, the law of refraction in geometrical optics by deriving it as a consequence of Maxwell's equations in electromagnetic theory. The conviction that laws form such a hierarchy (and the possibility of a unique foundations at the basis of all physical phenomena) has been one of the most powerful driving forces in the research practice of physics.

However, physicists also often explain laws by means of micro-reduction. In that case a physical law, which describes a systemic behaviour at the macro-level, is explained in terms of the behaviour of the constituents of the system at the micro-level. Explanation by subsumption does not require decomposition of systems into lower-level parts, micro-reductive explanation always requires this (by definition). It is not hard to find examples of such explanations in physics. Boyle's law can be explained in terms of the kinetic theory of gases; the laws of circular movement of rigid bodies is explained by means of the kinetic theory of matter; and Ohm's law is explained in terms of moving electrons. Our paper deals with explanations of this type.

The mechanistic model of explanation, as it has been developed in the first years of the 21st century, can shed light on the structure of micro-explanations in physics. That will be clarified by means of two examples: the periodic table of elements and Boyle's law.

These examples show that there are mechanistic explanations in physics. The next step (and main aim of our paper) is to investigate how these mechanistic explanations provide understanding. We will argue that mechanistic explanations of physical laws can work in at least three different ways:

(i) by showing that the higher level behaviour is to be expected given the specific constitution of the system and certain physical laws.

(ii) by answering what-if-things-had-been-different questions about the explanandum in the way Jim Woodward (Making Things Happen, 2003) proposes (i.e. by telling us how the explanandum would differ).

(iii) by answering what-if-things-had-been-different questions about the explanandum in the opposite way (i.e. by telling us that nothing would change in the explanandum).

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Mechanisms vs. Difference-Making

– Lena Kästner (Humboldt Universität zu Berlin)
– Lise Marie Andersen (Aarhus University)

Abstract. What makes for a scientific explanation? Among philosophers of science, two answers are vibrantly discussed at the moment. The first is that explanations describe mechanisms underlying, producing, or implementing the phenomenon to be explained (e.g. Craver 2007, Craver & Darden 2014). According to this *mechanistic view*, if scientists want to explain a phenomenon they must discover its mechanism. The other prominent suggestion is that scientific explanations are essentially causal explanations that tell us what would have happened if things had been different (e.g. Woodward 2003). To find these explanations, proponents of the *interventionist view* suggest, we carry out (under suitable conditions) systematic manipulations of some factor X to observe its effects of another factor Y. Interestingly, there is a connection between mechanism and interventions: proponents of both views argue that scientists need to employ interventionist methodology to discover mechanisms and thus to come up with mechanistic explanation. But how does this square with the apparent contrast between mechanistic explanations and interventionist causal explanations?

In a recent paper, Jon Williamson (2013) usefully highlights that mechanistic and interventionist explanations are quite different in character. An interventionist explanation requires knowledge about which factors can potentially make a difference to a given outcome (the explanandum phenomenon). Often we can have this knowledge without knowing how or why, say, ingesting an antibiotic drug will lead to recovery from streptococcal infection. By contrast, a mechanistic explanation requires mechanistic knowledge about the precise entities and activities at work. For instance, to explain recovery from streptococcal infection mechanistically, we would have to know something about bacterial metabolism and how antibiotics interfere with it. In this context Williamson makes another interesting observation: at times we are more willing to accept a mechanistic explanation as explanatory than one that is based merely on difference-making knowledge. And indeed if we look at scientific practice this seems correct: explanations supported by detailed knowledge about the mechanisms at work seem “more convincing” or “more progressive” than those just based on statistical regularity. This is evident across scientific disciplines. In biology, for instance, explanations of traits making reference to specific genes and how they may be modified are considered superior to those just referring to dominant or recessive chromosomal or autosomal inheritance. Likewise, we can look at cognitive science. Here, well-established psychological findings are replicated in droves with modern neuroimaging techniques, cellular recordings, etc. to support existing psychological theories with evidence about the underlying neural mechanisms. But does this mean mechanistic knowledge is generally superior to knowledge about difference-making relations? And how could this be if we cannot have mechanistic explanations without interventionist explanations? What is the relation between these two kinds of explanation and the two kinds of knowledge they seem to require, after all?

In this paper, we will argue that both the mechanistic and the interventionist view reflect important aspects of scientific practice. Looking at examples from empirical research in neuroscience, we will investigate the relations between interventionist methodology and mechanistic discovery and the explanations they yield.

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Reaction Mechanisms in Chemistry: A Comparison Case for Accounts of Scientific Explanation

– Andrea Woody (University of Washington)

Abstract. Philosophy of science offers a rich lineage of analysis concerning the nature of scientific explanation. In recent years, considerable attention has been directed toward the notion of mechanistic explanation, especially in the biological sciences (see, for example the writings of Bechtel and Craver). Much of this work aims to characterize mechanisms or determine how mechanisms need to be described in order to be explanatory. Some of it aims to make explicit contact with contemporary analyses of causation, especially those of Salmon (1984, 1998) or Woodward (2005). This paper examines another scientific context in which appeal to mechanisms is arguably as widespread and central as it is in biological contexts but which has received much less attention: explanatory patterns involving reaction mechanisms in organic chemistry. There are two fundamental aims: (1) to develop a characterization of mechanisms in chemistry as a comparison case for existing analyses of mechanism in the biological sciences, and (2) to use this comparison to highlight certain aspects of explanatory practice across the sciences.

Drawing on recent work by Goodwin (2011, 2012), the paper begins with a general characterization of reaction mechanisms and their role in explanations in organic chemistry. From this characterization, I will argue that mechanistic explanations in chemistry seem different in important respects from their counterparts in biology. Mechanistic explanations in chemistry typically focus on information often lacking in biological cases, specifically information concerning the rate of operation of (some, but only some) various processes that compose a given mechanism, and at the same time, typically omit or suppress information included in biological mechanistic explanations. The next step of the analysis turns more specifically to practice. I argue that the types of information included in chemical mechanisms, as well as the way in which this information is represented in relation to potential energy diagrams, serves to support the largely synthetic aims of organic chemistry. I will suggest that general differences in the aims of given scientific communities may influence what sort of description or information is required for a mechanism to judged explanatory. Finally, I will return to broad issues concerning scientific explanation, arguing that an account of explanation sufficiently oriented toward explanatory practice will be best suited to make sense of the sorts of differences we observe in comparing chemical and biological mechanisms taken to be explanatory by their respective communities. Such an account of explanation stresses the methodological role of explanatory discourse in ways I have discussed elsewhere and will summarize briefly to conclude.

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Explanation, Inferences, and Chemical Reactions: A Mechanistic View for Scientific Practices

– Juan Bautista Bengoetxea (University of the Balearic Islands)
– Oliver Todt (University of the Balearic Islands)
– José Luis Luján (University of the Balearic Islands)

Abstract. Writings on explanation by philosophers of science such as Hempel and Oppenheim (1948), Nagel (1961), and Hempel (1965) gave way to a rich and deep literature on a topic that from the Nineties has become highly lively in many respects. During the last 20 years, several accounts (Kitcher 1989, Lipton 2004, Psillos 2002, Salmon 1984) have been given aimed to get a better articulated concept of explanation, being this closer to real practices in scientific disciplines. Some of those accounts, especially new experimentalism and philosophy of scientific practices, have helped analyze the notion in terms of their own image of science, usually one complementary to traditional studies used to focus on logical-linguistic structure of scientific theories that conceived science as merely propositional knowledge.

Our goal is to make use of a particular view called 'mechanistic explanation' (Machamer, Darden, Craver 2000) in order to consider the development of explanatory and inferential practices in experimental laboratory science. It is a proposal stemmed from two features of chemistry: one is chemistry's dependency upon both laboratory work (experimental) and many kinds of technologies, beyond its theoretical aspects insofar as it is also a science of modeling, simulation and computer calculation. The second feature has to do with the failure of the nomological-deductive account for correctly modeling those processes of explanation happening in activities of chemistry. Among the several explanatory accounts that have been lately proposed in the literature, we propose an approach to organic chemistry from the experimental practices view in order to develop a notion of explanation more adequate to the activity of generating chemical reactions. We place our proposal in the context of the mechanistic explanation view and address it to the analysis of some organic chemical reactions. In particular, we want it to be a basis for a new version of the 'inference to the best (mechanistic) explanation', which may serve us to establish a first step in a process that may capture satisfactorily the form and content of explanatory-inferential practices in both general science and organic chemistry.

Our proposal is organized in four parts. In the first one, we situate the study of scientific practices under a dynamic perspective where approaches that require laws to characterize scientific work are replaced by a non-nomological-deductive view of explanation in chemistry. The second part presents the most important current view about the mechanistic explanation. This provides us with the basic idea of how a mechanism is understood in science, as well as with several complementary notions of model. The third part constitutes the core of our proposal. We examine the elements that make some mechanisms to be a potential ground for an adequate chemical explanation, particularly in the case of organic chemical reactions. Finally, in the fourth part we describe a way to choose which among those potential items may be a real mechanism that could permit us to pick up a final real explanation for the organic reaction under analysis.

Parallel Session 6D

Friday, 26 June 2015 at 09:00–11:00 in G3

Session chair: Holly VandeWall (Boston College)

❧

The University Museum: A Microcosm for Studying Transdisciplinary Challenges

– Line Breian (University of Gothenburg)
– Johannes Persson (Lund University)

Abstract. It is easy to conceive of university museums as places where transdisciplinary processes should be the rule rather than the exception. University museums include both natural and cultural departments and can ideally cooperate with stakeholders both within and outside of the university. Transdisciplinarity, almost irrespective of how it is defined, could be expected to thrive here. Prima facie the museum could even be looked upon as a microcosm where developments in the larger world are magnified – or as in Star and Griesmer (1989, 414) "the museum is in a sense a model of information processing". To better understand the potential and challenges facing transdisciplinary communication and collaboration, we conducted a series of interviews with diverse actors within the museum sphere in Norway, Sweden, Denmark, and Britain. We wanted to examine the conditions that need to be met if transdisciplinary models and tools are to be of use. Production of transdisciplinary knowledge and increased participation, we found, remains a challenge. Fulfilling basic requirements of transdisciplinarity (variety of experts and inclusion of the public) does not always involve moving beyond rhetoric. Transdisciplinary efforts do not, in fact, entail a conviction that true participation and genuine inclusion of different types of knowledge is necessary. Nor is there necessarily time to fully enable transdisciplinary dialogue. Our research suggests that if certain requirements for transdisciplinary work are lacking, the chances of actually succeeding are significantly lowered. These requirements were i) suspension of previous decisions to believe in scientific propositions, ii) suspension of authority and iii) willingness to communicate uncertainty. The time needed for building capacity for transdisciplinary work must be taken into consideration. In addition, the emphasis on transdisciplinary work actualizes the role of the interactional expert – and who the interactional expert is. Is he or she a member of the core group or a facilitator? The core group alternative would seem to appeal to those who see authority as based on a process of demarcation of valid and invalid knowledge claims. If, on the other hand, we conceive of authority as a process of co-construction (as suggested by Nowotny et al in their notion of socially robust knowledge) or as seen in the example of the Danish species database Fugle og Natur, the facilitator perspective is more suited. Furthermore, technological advances appear to challenge the notion of both interactional and contributory expertise and of transdisciplinary work. This was clearly seen in the museums where citizen science from massive to modest would seem to imply a redefinition of participation and of knowledge production. We showed that an emphasis on transdisciplinary processes increased, rather than decreased the complexity of the knowledge-generating processes and thereby also implicitly promoted a change in how the museums related to knowledge production at large. Such novel ways of relating to research and to institutional structures would not be as easy to spot if purely scientific contexts were examined, and potentially explain our observations of a deep-seated reluctance of many players in the museum world to involve themselves in transdisciplinary processes.

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Scientists as Experts: Understanding Trustworthiness Across Communities

– Heidi Grasswick (Middlebury College)

Abstract. Scientists are often confused and surprised when their work is met with distrust from members of the public. Though many instances of distrust lack warrant, failures in the trustworthiness of scientific communities can justify such distrust. In the first part of this paper, I examine the conditions for scientists' trustworthiness as expert knowers, emphasizing scientists' dual role as both generators and communicators of knowledge and revealing some of the complications and challenges of being a trustworthy communicator. Importantly, trustworthy communicators must not only be (morally) sincere and (epistemically) competent, as testimony theorists have argued, but they also must understand the needs and expectations of their audience. Importantly, scientists are not simply communicating facts to the public, but rather are making judgements about the state of research and its relevance as these pertain to the concerns of their public audience.

In the second part of this paper, I turn my attention to the distrust of scientific communities sometimes evident in socially marginalized groups. Theorists such as Naomi Scheman (2001) have provided interesting arguments to the effect that a legacy of poor interactions with scientific communities can provide good reasons for members of marginalized groups to distrust scientific communities and their claims. Examples include historical relations between geneticists and indigenous communities, medical researchers and African-American communities, environmental scientists and Inuit communities, and sex-difference researchers and women. I focus on two aspects of such a position that require further development. First, the mechanisms of marginalization play out differently for various social groups, and hence the reasons such groups have for distrust will vary accordingly. One does not simply get from social marginalization to a singular set of reasons for scientific distrust, and it is important to analyze how certain features of social marginalization play out in providing various reasons for distrust. Second, the target of reasonable distrust needs to be more carefully specified than simply "science" or "scientific communities". There may be strong reasons for distrusting specific communities of science or particular research areas without a blanket scientific distrust being justified. Distrust does at times "travel", expanding to a broader range of scientific communities than its original target, and it can do so in a reasonable way. Scientific communities need to attend to such travelling distrust (regardless of whether or not it is reasonable) if they want to be successful communicators. But it is also important to recognize when such travels of distrust are reasonable. Only a strong scientism that overemphasizes the similarities across different scientific fields and their histories could support a broad-based reasonable distrust of scientific communities, and such scientism is unwarranted. I conclude with some suggestions concerning how scientific communities might improve their trustworthiness across a variety of social groups.

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Expert Witnesses in a Trial Against Experts: Of Causal Links and Scientific Responsibility in the L'Aquila Case

– Federico Brandmayr (Université Paris-Sorbonne)

Abstract. According to Nature, the sentence against seven members of the Italian National Commission for the Forecast and Prevention of Major Risks was one of the most striking events of 2012. The seven commissioners, all university professors in seismology, volcanology and engineering, had been convicted to six years of prison for manslaughter. The charge is that of having misled the public about the tremors that had been striking the town of L'Aquila in the first months of 2009, tremors that eventually culminated in a deadly magnitude-6.3 earthquake on 6 April. The fact that on 10 November 2014 an appeal court cleared six of the seven accused does not reduce the importance of the case, especially as the trial will continue in the Court of Cassation, Italy's highest court of appeal.

The case has longly occupied the Italian public debate but has received little attention from scholars in the academia. Particularly, the few scholars who analysed the case assumed highly polemical

tones and a prescriptive register (directed either at the sentence or at the accused). Only very rarely scholars have investigated the case from the perspective of the philosophy and sociology of science, thus leaving its implications largely unexplored.

The paper will focus on a specific aspect of the case, namely the expert witnesses that were summoned by the two sides (the public prosecutors and the lawyers of the defendants) during the first degree of the trial. These witnesses had to discuss one of the decisive points of the quarrel, the existence of a causal link between the declarations of the seven during the meeting of the commission on 31 March 2009 and a decrease in the citizens' risk perception. It is indeed argued that the latter played an important role in inducing the population of L'Aquila to stay in their houses – contrary to their habits – after the two tremors that preceded the earthquake on the night of 6 April.

The prosecution summoned a cultural anthropologist, lecturer at the university of L'Aquila, whereas the defense summoned some researchers in the fields of neuroscience and media studies. The various expert witnesses proposed wildly different theoretical frameworks and empirical evidence in order to refute or support the hypothesis of the causal link mentioned above. From a detailed analysis of their depositions, two diametrically opposed conceptions of science and of scientific responsibility emerge. Moreover, they have conceived their role and engagement in heterogeneous ways (who as neutral and professional specialists, who as intellectuals with a moral mission).

The paper will furthermore proceed illustrating the norms, motivations and constraints that were at stake in this situation of expertise, and will formulate some hypotheses on the difference between knowledge produced in the academia and knowledge produced during judicial expertises. It will discuss these questions in the light of classical and recent works in the field. In order to develop the argument, the paper will make use of a considerable amount of documents: the nine hundreds pages motivations to the first sentence, the written advices consigned by the expert witnesses, interviews with the expert witnesses and media releases.

❧

Why Scientists Cannot and Should Not Be Sincere

– Stephen John (University of Cambridge)

Abstract. Many writers claim that effective and ethical scientific communication should be "sincere": that is to say, scientists should, insofar as possible, report what they believe. For example, in a recent paper, Keohane, Lane and Oppenheimer (2014) describe honesty (which incorporates sincerity) as "intrinsic to science: the sine qua non for this form of human activity"; Nordmann (2011) has argued that sincerity is essential if science is to achieve its Enlightenment ideals. Such suggestions relate to a broader sense that "sincerity" is, in Bernard Williams' phrase, one of the key virtues of truthfulness (Williams, 2002).

This paper argues that sincerity cannot be a norm for ethical scientific communication, on the grounds that it is an unobtainable ideal, of questionable ethical significance.

Three arguments are presented for this position. The "argument from collaboration" states that given the collaborations necessary to produce scientific papers, reports and so on, it is hopeless to expect these paradigm acts of scientific communication accurately to reflect the views of each contributor. This point is developed by appeal both to recent work on collective deliberation and empirical case studies of authors' attitudes to "their" papers.

The "argument from standards" states that, given the practical difficulties of ensuring trust in scientific communities, there are often good reasons for scientists to use standardised tools and techniques to generate and report their results. Using these tools and techniques may sometimes lead them to report claims which they do not, themselves, believe. However, I argue that in these cases, the importance of enabling communication over-rides the value of sincerity.

The final "argument from non-ideal conditions" states that even if there is a sense in which scientists can and should be "sincere", the force of this demand rests on an assumption that they are communicating in "epistemically ideal" circumstances, where other, non-experts are engaged in an honest attempt to distinguish true from merely putative experts. However, in many real-life cases, scientists are communicating in "epistemically hostile" environments, where they know that others

might maliciously twist their words to distort their meaning. In these contexts, I argue, the ethical arguments in favour of sincerity have little weight.

That sincerity is not a norm of ethical scientific communication may seem rather surprising. In the conclusion, then, I note a tricky problem for my arguments: even if there are arguments against sincerity as an ethical or epistemic virtue in science communication, the widespread belief that sincerity is a virtue may give scientists prudential reasons to be sincere (insofar as possible). I suggest that these risks of being "caught out", although important, do not suffice to justify sincerity as a basic norm for scientific communication.

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Parallel Session 6E

Friday, 26 June 2015 at 09:00–11:00 in G4

Session chair: Maria Serban (University of Pittsburgh)

❧

Design Explanation and Idealization

- Dingmar van Eck (Ghent University)

Abstract. In this paper I assess the explanatory role of idealizations in 'design explanations', a type of functional explanation used in biology. In design explanations, idealized, non-existent organisms (organisms with non-existent characteristics) are invoked to make salient which traits of extant organisms make a difference to organismal fitness, understood in terms of higher life chances. For instance, by comparing gulls with white underparts with hypothetical, idealized ones with black underparts, the trait of 'having white underparts' is shown to be advantageous; it offers increasing hunting success (Götmark 1987). Similarly, the giant eyes of giant deep see squid are shown to make a difference to fitness by comparing giant squid with giant eyes with hypothetical, idealized ones having smaller eyes; large eyes enable the detection of predators (sperm whales) which would not be possible or less efficient if the squid were to have smaller eyes (cf. Nilsson et al. 2012). Such advantages offered by traits are explained in terms of 'functional dependence relations', which relate advantages offered by a trait to contextual conditions (Wouters 2007, p. 76). In the case of gulls with white underparts, for example, the advantages offered by this trait are importantly related to external conditions, such as the brightness of the sky, and internal conditions like 'not being nocturnal'. If gulls were to hunt at night, any advantage – hunting success – offered by their white underparts probably vanishes.

Given the reliance on comparisons with idealized, non-existent organisms, I argue that in design explanations, idealizations highlight which factors – traits of extant organisms – make a difference to organismal fitness.

I take 'idealization' to refer to the intentional distortion or misrepresentation of facts, i.e., the assertion of falsehoods, thus distinguishing it from 'abstraction', understood as the omission of veridical details, without misrepresentation (cf. Jones 2005).

This result negates the view that idealizations serve only pragmatic benefits (cf. McMullin 1985), complements the view that idealizations function to highlight factors that do not make a difference

(cf. Strevens 2007, 2008), and in particular strengthens the perspective that idealizations are means to highlight difference making factors (cf. Weisberg 2007; Batterman 2009; Rice 2013). In some explanatory contexts, idealizations are in-eliminable.

Of course, not every counterfactual scenario or relation is an idealization. Most counterfactuals, it seems, do not function to intentionally distort aspects of the world. In most counterfactuals there is 'nothing counter to the facts', for if certain conditions obtain, counterfactual relations become actual or true (cf. Ylikoski & Kuorikoski 2010).

However, descriptions of counterfactual organisms in design explanations are 'counter to the facts' and do intentionally distort aspects of the world. Features of extant organisms, i.e., specific traits, are purposefully misrepresented in order to make salient how specific traits of extant organisms make a difference to organismal fitness. Design explanations thus do more than highlight the explanatory role of contrasts with counterfactuals. In design explanations, distortions of facts are invoked to assess the biological advantage(s) of traits of extant organisms (cf. Wimsatt 2006).

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❧

Unified and Disunified Strategies for Explaining Parameter Robustness

- Nicholaos Jones (University of Alabama in Huntsville)

Abstract. Biologists are increasingly aware of the importance of robustness for understanding living systems (Kitano 2004; Stelling et al. 2004). Philosophers, too, increasingly recognize that there is more to robustness analysis than determining whether model predictions persist through changing assumptions about causal details of target systems. In particular, philosophers are beginning to recognize a form of robustness analysis directed toward determining whether model predictions persist though changing parameter values (which typically track details external to target systems). Call this kind of robustness parameter robustness, and say that a biological system is parameter-robust whenever it retains some particular feature across a wide range of parameter values. Biologists and philosophers are only recently starting to explore strategies for explaining why biological systems are parameter-robust. These strategies differ from each other with respect to their degree of causal

specificity, and this difference tracks explanatory power. Specifically, strategies that depend least upon causal specifics also explain more about parameter-robustness. Or so I shall argue.

Support for my argument comes from a series of research papers about bacterial chemotaxis, and in particular a series of papers devoted to explaining why bacterial adaptation to changing chemical gradients is parameter robust. But while I rely primarily upon specific explanatory strategies for a specific explanatory interest, research on bacterial chemotaxis embodies a developing contrast between general explanatory strategies among molecular and cell biologists. I gesture toward this larger contrast as part of my concluding remarks. The argument, too, engages with some prima-facie unrelated philosophical literature, and in particular with Sober's (1999) argument regarding "disunified" explanatory strategies—strategies that appeal to different details for different systems. Sober aims to show that certain "unified" explanations enjoy no explanatory superiority over their "disunified" counterparts. I argue that Sober overlooks how explanatory aims constrain explanatory quality.

I take this to be more than a quibble among philosophers about how to talk about what scientists are doing. Sober's argument has been taken to support renewed reductionist ambitions (see Schaffner 2013). Biologists skeptical of the "unified" explanatory strategy often also take themselves to be reductionists of some sort; advocates take themselves to be resisting some kind of reductionism (see see Sorger 2005; Van Regenmortal 2004; Gatherer 2010). So there is philosophical value in better understanding how the unified strategy relates to its disunified cousins.

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❧

Not Null Enough: Causal Null Hypotheses in Community Ecology and Comparative Psychology

- William Bausman (University of Minnesota, Twin Cities)
- Marta Halina (Cambridge University)

Abstract. A central goal in science is to determine the best explanation for a given phenomenon. There are many strategies for doing this, including evaluating the empirical adequacy of the available hypotheses and assessing their relative epistemic virtues. In this talk, we examine one strategy for choosing between competing hypotheses employed in the fields of community ecology and comparative psychology. A central feature of this strategy is the use of what we call a "causal null hypothesis." This is a hypothesis that practitioners in the field identify and treat in the same way that one would treat a statistical null hypothesis, but which lacks the features of a true statistical null. We present the ways in which causal and statistical null hypotheses differ and argue that an important consequence of these differences is that the strategy used for evaluating hypotheses in our cases studies is unjustified.

We begin by first showing how the appeal to null hypotheses is used in our case studies. It is used in community ecology to defend the neutral theory as the best explanation for species

abundance distributions and in comparative psychology to defend the behavioral-rules account of social behavior in chimpanzees and other nonhuman animals. Both the neutral theory and behavioral-rules hypothesis are identified as "nulls" on the grounds that they are simpler than the alternatives available. These nulls are then privileged over the alternatives in that they are rendered both easier to accept and harder to reject. We show how this form of argument is superficially similar to the statistical method of Neyman-Pearson testing, thus giving it rhetorical force. However, we go on to argue that it is disanalogous in precisely those respects that are required for justification. Whereas the null hypotheses used in Neyman-Pearson testing are hypotheses of "no effect," this is not the case for the null hypotheses in our case studies, which are both presented as positive, causal hypotheses by their proponents. Given this, we argue, the causal nulls identified in our cases studies should not be privileged over the alternatives, but treated on a par, and the rhetoric of testing null hypotheses should be dropped. In the end, we hope our analysis will stimulate critical discussions on the role that causal null hypothesis testing plays in other cases in science.

One caveat about the scope and purpose of our presentation: we are interested only in critiquing the causal null hypothesis strategy, not the specific hypotheses this strategy has been used to support. There may be better arguments or alternative lines of evidence that could be used to support the neutral theory and behavioral-rules hypothesis; we do not wish to deny that this is the case. Instead, our aim is to critique the argumentative strategy that depends on casting these hypotheses as nulls outside of the context of statistical hypothesis testing.

❧

Essentialism, Evolutionary Theory and Human Rights

– Edit Talpsepp (University of Tartu)

Abstract. My paper aims to describe the relationship between the questions of philosophy of biology and the understanding/application of human rights. The discussion about this relationship affects how we understand what human rights are, what is their source of origin, and how we should apply them.

Evolutionary theory is usually assumed to be inconsistent with essentialist thinking. (According to essentialist thinking, certain categories, such as biological categories or human groups, have an underlying essential property that all and only the members of these categories possess.) This is because essentialist thinking stresses something that is common to all species members, whereas understanding natural selection as the main force underlying evolutionary change assumes accepting the variability between category members. However, paradoxically the explanations that appeal to evolutionary or biological factors often refer to something like functional universals that have evolved in the process of human evolution, presumably possessed by most members of a certain human group and being part of the characterization of that group. In this way evolutionary explanations themselves might become part of something like essentialist thinking about certain human groups (even human species as a whole), whereas naturally selected 'functional universals' are seen as a characteristic part of the essence of a group.

In my talk I will focus on the aspect of the relationship between biology and human rights that consists of the attempts of appealing to biological differences, group differences and human evolution when justifying the application of certain human rights. On the one hand, biological differences between individuals and groups have been seen as a counterargument against what is called the 'egalitarian fallacy' by some authors. According to these authors, equal rights are applied to many other things than what they meant in the Universal Declaration of Human Rights. Now, it is claimed, equal rights are coming to be considered 'equal entitlements'. The basic reason for why this egalitarianism is unworkable, is a biological one, as each person differs genetically, experientally and in individual purposes and goals in life.

These seem like obviously anti-essentialist claims to make. However, appealing to biological and evolutionary factors when justifying the application of certain human rights might easily lead to making essentialist assumptions and inferences about certain human groups. In public discussions even in societies with egalitarian legislation we often hear arguments about how men and women,

since they are biologically different and possess different naturally selected features, also have different interests and goals, which makes gender equality something like an artificial nonsense. Not only anti-egalitarians, but sometimes feminists themselves make somewhat essentialist assumptions about women – protecting the rights of certain groups often goes hand in hand with group essentialism. Another quite telling example is the arguments against gay marriages as something 'unnatural' – based on the assumption that gays cannot fulfil the normal evolutionary function of human species to reproduce with each other. In my presentation I will demonstrate how approaching these issues from the perspective of philosophy of biology can affect actual socio-political attitudes and applications concerning human rights.

Parallel Session 6F

Friday, 26 June 2015 at 09:00–11:00 in Koll G

Session chair: Hasok Chang (University of Cambridge)

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The Epistemological Role of Systematic Discrepancies

– Teru Miyake (Nanyang Technological University)

Abstract. Recent work by philosophers of science with a strong concern for scientific practice has emphasized the epistemological role played by systematic discrepancies between calculations and observations (see, e.g., the work of George Smith, Hasok Chang, and Eran Tal). George Smith (2014), in particular, argues that the epistemological justification for gravitational theory comes, not primarily from agreement between calculation and observation, but through the discovery of physical sources for each systematic discrepancy between calculation and observation.

This spotlight on systematic discrepancies can be seen, in some respects, as a revival of a nineteenth century tradition in British philosophy of science that emphasized "residual phenomena", one that had a deep influence on prominent scientists of the time. In the Treatise on Natural Philosophy, William Thomson and Peter Guthrie Tait credit John Herschel with noticing the epistemological role played by residual phenomena, and add that "it is here, perhaps, that in the present state of science we may most reasonably look for extensions of our knowledge; at all events we are warranted by the recent history of Natural Philosophy in so doing." The notion of residual phenomena, and the procedure by which they are used to acquire knowledge of nature, is introduced by Herschel in his Preliminary Discourse on the Study of Natural Philosophy. William Whewell later terms this procedure the "method of residues" and discusses it at length in his Philosophy of the Inductive Sciences. This term is probably best known to contemporary philosophers through John Stuart Mill's discussion of it in his System of Logic. There are, however, subtle but important differences between the views of these philosophers with regard to the epistemological role of residual phenomena, the most significant being the degree of emphasis on a quantitative, as opposed to a qualitative, characterization of the phenomena.

The method of residues is traditionally associated with scientific discovery, which may account for its being largely overlooked by philosophers of science in the twentieth century, with their emphasis on justification over discovery. Smith (2014), however, views systematic discrepancies as being essential to the justification of gravity theory. Herschel's characterization of residual phenomena, which emphasizes the quantitative characterization of the phenomena, the role of systematic error, and the importance of residual phenomena in verification, bears a strong resemblance to Smith's view—perhaps unsurprisingly, given that the views of both Smith and Herschel are arrived at through an investigation of the development of astronomy after Newton. This paper will examine Herschel's view of residual phenomena, trace out its relation to the later views of Whewell, Mill, and Thomson and Tait, and finally compare it to the contemporary view of Smith. I will focus, in particular, on exactly how residual phenomena are supposed to play a role in justification, not merely in discovery.

I will also consider possible reasons for the demise of this tradition in the late nineteenth century, including the issue of whether the view will generalize to fields other than gravity theory, particularly microphysics.

❧

(Re-) Discovering Elementary Particles at Cern by Diagnostic Causal Inferences

– Adrian Wüthrich (Technical University Berlin)

Abstract. Basing myself on the publications by the UA1 and ATLAS collaborations at CERN (1983, 2012) and on some unpublished documents from the ATLAS collaboration's internal communication (2010) I argue that the detection or discovery of elementary particles such as the W or the Higgs boson is best interpreted as the application of diagnostic causal inferences. Diagnostic causal inferences reach the conclusion that a particular type of cause was instantiated in a given situation from the observation that some particular type of effect was instantiated. Such an inference rests on the validity of the cause-effect relationships that have to be presupposed and on the exclusion of alternative causes. I will give an account of how the ATLAS and UA1 collaborations were able to perform reasonably well justified diagnostic causal inferences and in what sense this amounted to a (re-)discovery of the W boson in 1983 and 2010 and of the Higgs boson in 2012.

My account of the cases shows how causal reasoning can be employed even in situations where the existence of the entities involved in a causal relationship has yet to be established. Causal reasoning is usually only concerned with establishing causal relationships between already known entities or factors, the open question being whether one of them causes the other. Moreover, the methods for answering these questions presuppose, rather than infer, the existence of the involved entities. For instance, to establish the causal relevance of a factor A for a factor B by John Stuart Mill's or similar methods of difference one has to know of a situation in which A is instantiated and of a situation in which it is not. It is hard to see how such knowledge could possibly be available without even knowing of the existence of the objects involved in the instantiation of factor A.

Peter Lipton (1991, 2004) saw such problems as a decisive reason for the need to supplement causal reasoning with explanatory considerations. He argued that when it came to establishing the existence of entities, inferences to the best explanation was indispensable. By contrast, I take my reconstruction of the W and Higgs cases to show that the scope of causal reasoning includes the establishment of existence claims to a substantial extent. Along the way, I hope, my reconstruction elucidates the function of data selection, highlights the importance of the principle of causality in recent and current high energy physics, and shows how the CERN researchers can deal with the problem of unconceived alternatives.

❧

What Would Be a Cultural Logic of Conceptual Discovery?

– Jouni-Matti Kuukkanen (University of Oulu, Philosophy)

Abstract. Study of conceptual change has been an object of increased and great interest in recent decades. It is not difficult to name several traditions that have investigated the problem of conceptual change from their mutually incompatible perspectives. Starting from the oldest, Lovejoy's writings of unit ideas and a long tradition of the history of philosophy provide two answers to what concepts are and how they transform in history. More recently, post-Kuhnian philosophers of science debated intensively in the 1970s and 1980s whether meanings of terms (concepts) and conceptual schemes change in the history of science. The most recent and most interesting approach is so called cognitive history and philosophy of science that explicitly applies models of cognitive science in the context of the history of science. Finally, one should not forget the German tradition of Begriffsgescihte either that emphasises social aspects of conceptual changes.

All these traditions provide different answers to what the concept is that changes, what a change of that concept is and what kind of historical examples can be given of conceptual stability, change

and replacement. These questions form the core of what might called Philosophy of Conceptual Change.

It has become evident that answers given to these fundamental questions determine in part the image of science provided and the kind of narrative of science written in practice. I outlined my initial view in my paper Making Sense of Conceptual Change a few years ago (History and Theory 47, 351–372). In my talk in Aarhus I continue investigating the nature of conceptual change focusing on what might be termed as the 'cultural logic of conceptual creation and discovery.'

Discovery and creation have typically been understood as phenomena that are mystical and that therefore defy rational explanations. This attitude is epitomised, for example, in the distinction between the logic of discovery and the logic of justification and in Popper's philosophy. Even the early Kuhn understood conceptual change as a sudden gestalt switch. Cognitive historians and philosophers of science have provided some explanations for conceptual creation, for example, in the forms of mental modelling of consecutive conceptual schemes (e.g. Hanne Andersen, Nancy Nersessian) and of reasoning that gives birth to new ideas and concepts (e.g. Nancy Nersessian, Paul Thagard). However, their focus is usually still on individual psychological phenomena. Begriffsgescihte studies cultural phenomena behind conceptual changes but unfortunately their theories of concept and conceptual change remain implicit.

In my talk I outline the central problematique and challenges of the Philosophy of Conceptual Change as outlined above. More important, I attempt to schematise cultural conditions that precede an emergence of a new concept. The main hypothesis is that this process implies continuity with respect to previous traditions. My view is that conceptual birth is a dynamic and creative process but that it can be rationally understood and explained. R. G. Collingwood formulated the idea follows: "Any process involving an historical change from P1 to P2 leaves an unconverted residues of P1 encapsulated within an historical state of things which superficially is altogether P2" (An Autobiography, 2002, 141). Another more specific lead is given by Imre Lakatos in Proofs and Refutations (1976) in which dialogue itself is understood a form of conceptual innovation at the end of which a new concept is born. In other words, a radically new concept may emerge through a complex cultural process of argumentation and criticism.

❧

Theoretical Bias of the Standard Research Practice in Social Psychology

– Taku Iwatsuki (the University of Pittsburgh, History and Philosophy of Science Department)

Abstract. In this paper, I argue that the standard research design in social psychology is biased toward the confirmation of simple group-level effects that do not necessarily reflect our psychological reality. I also describe alternative research designs that less likely to suffer from this bias. I support my points with actual social-psychological studies.

One of the main goals of empirical social-psychological research is to test the hypothesized causal relations among environmental, psychological, and behavioral variables. To this end, social psychologists often conduct randomized controlled experiments and analyze data by means of analysis of variance (ANOVA). In a typical social-psychological experiment, there are 2 to 4 independent variables each of which takes 2 to 4 values and one dependent variable. The typical unit of analysis is individual persons and one experimental group typically consists of 20 to 30 people.

This standard design has at least two kinds of bias. The first is that this design tends to confirm simple effects. Social psychologists typically use independent variables that take 2 to 4 values even when it is possible to devise experimental treatments that corresponds to finer-grained values because the larger the number of values is, the larger the number of participants is necessary but the number of available participants is limited. Moreover, by using analysis of variance that treats independent variables as categorical variables, social psychologists lose information about the order of or intervals between values of the original variables. Therefore, hypotheses that can be tested with the typical experimental design are limited to those that are relatively simple and less informative. However, there is no a priori reason to assume that the causal effects social psychologists study are simple.

The second bias is that the standard design is more suitable to finding group-level effects rather than individual-level effects because what random assignment in principle establishes is not the equivalence of individual participants under different treatments but the equivalence of experimental groups in the distribution of the values of variables. Therefore, random assignment does not make possible the comparison of individual participants but the comparison of experimental groups. Social psychologists have to make some assumption about the causal homogeneity of individual participants in order to infer individual-level effects from group-level effects. Such assumption, however, is unlikely to hold in the domain of social psychology where there are many individual difference variables, e.g., demographic or personality traits, that would interact with independent variables.

Next, I describe two possible directions social psychologists can pursue. First, they may increase the number of participants used in a single experiment. This would allow them to use independent variables that takes more values and to investigate their effects on a dependent variable in more informative and finer-grained manner. Second, social psychologists can use case-study designs rather than group-comparison designs, which makes it possible to acquire detailed individual-level data. My suggestion is not that social psychologists abandon the standard design, but that they enrich their toolbox, admitting these designs, or other possible designs, as well as the standard design.

Plenary talk

Friday, 26 June 2015 at 11:20–12:30 in Aud F

Session chair: Sabina Leonelli (University of Exeter)

On Materiality and Scientific Objects

Hans-Jörg Rheinberger (Max Planck Institute for the History of Science)

Abstract. At the beginning, I will explain my interest in the curious existence of epistemic things as hybrids of materiality and conceptuality. Then, I propose an outline for a typology of the different forms that scientific objects can take in the life sciences. First, I discuss preparations, a form of scientific objects that accompanied the development of modern biology in different guises from the seventeenth century to the present: as anatomical-morphological specimens, as microscopic cuts, and as biochemical preparations. Second, I discuss the characteristics of models in biology. A few remarks on the role of simulations – characterizing the life sciences at the turn from the twentieth to the twenty-first century – will conclude my reflections.

Parallel Session 7B

Friday, 26 June 2015 at 14:00–15:30 in G1

Session chair: Marcel Boumans (University of Amsterdam and Erasmus University Rotterdam)

Organized by: Leah McClimans (University of South Carolina)

Symposium: Nomothetic and Idiographic Approaches to Quality of Life Measurement

Synopsis. Interest in the philosophy of measurement has seen a resurgence of interest over the last decade and this interest is marked by its attention to measurement practices (Tal 2013). While much of this interest has been within the physical sciences, e.g. (Chang 2004; Van Fraassen 2010; Tal 2011), increasingly philosophers within the social and medical sciences have begun to enter this discussion (Alexandrova 2012; Angner 2009; McClimans 2010). In this symposium we address two questions that animate the discussion of measurement across the sciences: 1) can current measures of quality of life or well being live up to the epistemic standards of measurement in the physical sciences? And 2) is there an account of measurement that can unify the sciences?

In our first paper, A Lay of the Land: Nomothetic and Idiographic Approaches to Quality of Life Measurement John Browne (Professor of Epidemiology and Public Health, University College Cork) foregrounds the two main conceptual approaches to quality of life measurement and discusses the methodological and practical difficulties they face. In our second paper, Epistemic and Ethical Problems with Nomothetic and Idiographic Quality of Life Measures Leah McClimans (Associate Professor of Philosophy, University of South Carolina) evaluates these two approaches to quality of life measurement and finds them wanting. She argues firstly that quality of life measures are not as different from measures in the physical sciences as some social scientists have suggested. Nonetheless both nomothetic and idiographic measures are epistemically (and ethically) unsound and likely to remain so. Finally Laura Cupples (graduate student, University of South Carolina) examines one possible unifying account of the epistemology of measurement: Eran Tal's model-based account. She argues that this account does not extend unproblematically to cover nomothetic quality of life measures. Tal's models epistemically support measures primarily through theory articulation, but, as McClimans has argued, quality of life measures lack a solid theoretical ground. Cupples suggests that this limitation means that there is no theory to articulate. Thus, if models epistemically support quality of life measures, it must be in some other fashion.

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A Lay of the Land: Nomothetic and Idiographic Approaches to Quality of Life Measurement

– John Browne (Epidemiology and Public Health, University College Cork)

Abstract. There are two main conceptual approaches to quality of life assessment: the nomothetic, where the individual's perception of his or her quality of life is filtered through the lens of a standardised model of 'the good life'; and the idiographic, where quality of life is constructed from individual evaluations of personally salient aspects of life. The dominant method is nomothetic. This is criticised for a number of reasons. First, many of the original quality of life tools such as the EuroQol or SF-36 were designed with little input from patients. Second, individual definitions of quality of life are posited to be highly heterogeneous and idiosyncratic, meaning that very few patients fit the 'average' definition. Third, the experience of completing and interpreting nomothetic measures has been described as artificial and lacking face validity by many patients and clinicians. Fourth, the most popular quality of life measures in current use were developed using classical test theory methods and can only be applied at the group level. The idiographic tradition in the social sciences assumes that for many important phenomena, including quality of life, individuals cannot be described using general rules because of the complexity of each life history. These methods were applied most intensively by psychologists in the 1960s working within the phenomenological tradition (e.g. George Kelly, Carl Rogers). The idiographic approach was adopted by quality of life researchers in the early 1990s and led to the development of a number of individualised measures such as the Schedule for the Evaluation of Individualised Quality of Life (SEIQoL). These measures allow each respondent to individually define the domains and weights to be assessed within the questionnaire. The track record of individualised measures will be reviewed in this presentation. The evidence to date suggests that although individualised measures have a strong surface appeal they are generally not useful within the confines of research paradigms that are nomothetic in nature (e.g. comparative effectiveness research). A more useful role of individualised measures lies within research contexts which are defined as idiographic at the outset, such as the formulation and monitoring of individualised care plans.

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Epistemic and Ethical Problems with Nomothetic and Idiographic Quality of Life Measures

– Leah McClimans (Philosophy, University of South Carolina)

Abstract. Quality of life measures within in the nomothetic tradition are popular with health policy makers in large part because of their ability to function as quantitative measuring instruments while also providing the patients' point of view. From a development perspective this attraction requires that these measures are epistemically and ethically sound. This double burden has proven difficult to achieve and these instruments have received significant criticism, mostly from those who develop and work with them. For instance, in 1995 the *Lancet* ran an editorial cautioning the use of these measures as end points in clinical trials, in 1997 Sonia Hunt's editorial in *Quality of Life Research* argued that they are misleading and probably unethical; more recently in 2007 Jeremy Hobart and colleagues argued in *Lancet Neurology* that almost all current measures are invalid. In my own work I have argued that they are invalid and difficult to interpret at least in part because they do not accurately represent the patients' point of view.

In this paper I ask why nomothetic quality of life measures face these challenges. One explanation that researchers commonly invoke is that quality of life measures lack a 'gold standard' and are thus more difficult to measure than physical properties such as blood pressure. In what follows I examine and reject this explanation and offer a different one: the problems that quality of life measures encounter arise because quality of life lacks a theory that provides a representation of the measurement interaction, i.e. the relationship between the quality of life construct and its instruments. I further argue that the development of such a theory is in principle problematic given the idiosyncratic way that individuals find quality in their lives, particularly during times of significant change, e.g. an unexpected diagnosis, sudden loss of physical functioning. To the extent that nomothetic measures seek to quantify quality of life in these contexts they fail to be epistemically and ethically sound.

If nomothetic quality of life measures fail in this way, what of measures within the idiographic tradition? Individualized measures such as the SEIQoL are the epistemic equivalent to bioethics' emphasis on individual autonomy. Both are problematic primarily because we can be wrong in our assessments of ourselves, i.e. we can be wrong about what we think we want and we can be wrong about the quality of our life. Epistemically and ethically sound measures cannot take individual assessments at face value. I thus conclude that neither the nomothetic nor the idiographic tradition supply us with quality of life measures that meet our demands

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Applying Tal's Model-Based Account of Measurement to Nomothetic Quality of Life Measures

– Laura Cupples (Philosophy, University of South Carolina)

Abstract. Eran Tal has developed a model-based account of the epistemology of measurement. While Tal's work focuses on the measurement of time, he has suggested that this account might also apply to other measures as well, providing a unifying account of measurement across the physical and social sciences. I argue in this paper that his account does not extend unproblematically to measures in the social or medical sciences. As a case study, I examine nomothetic quality of life measures. Does the epistemic support models offer for these measures mirror that of Tal's physical measures, or are models playing a different role in these measures?

According to Tal's model-based account, "a necessary precondition for the possibility of measuring is the specification of an *abstract and idealized model of the measurement process*" (Tal 2012). That is, our claims about measure validity and our judgments about measurement accuracy only become meaningful in reference to some model of the measurement process under consideration. Similarly, we can only meaningfully compare measurement outcomes when we have a model to contextualize those outcomes (Tal 2012).

Tal explains that he takes models to be abstract representations of local phenomena that are constructed based on theoretical, statistical, and pragmatic assumptions about those phenomena. He argues that models can function as mediators between abstract theory and concrete phenomena. They can also serve as instruments that help predict and explain the behavior of target systems (Tal 2012). This account suggests that, for Tal, models provide epistemic support for measurement primarily through theory articulation, i.e. taking scientific theory in concert with statistical and pragmatic assumptions and applying that theory locally.

However, many philosophers as well as thoughtful researchers and clinicians have complained that quality of life measures lack solid theoretical grounding. There is no generally agreed upon account of what well-being entails or how quality of life varies with life circumstances or our adaptation to those circumstances. There is also little theoretical grounding for our assumptions about how respondents understand and interact with these survey instruments, i.e., how these measures tap into the phenomenon in question. Leah McClimans has argued that because respondents have varying conceptions of quality of life, they often interpret the questions posed by these survey instruments in unexpected and inconsistent ways (McClimans 2010).

It is clear that if models provide epistemic support for quality of life measures, it is not because they are mediating between abstract theory and concrete phenomena as Tal argues they do in physical measures. There is no well-developed or widely agreed theory of quality of life to serve as a target for mediation. Given this state of affairs, we should ask what it might mean to give a model-based account of quality of life measures and if it is still possible for models to provide epistemic support for claims about the validity, accuracy, and comparability of these measures.

Parallel Session 7C

Friday, 26 June 2015 at 14:00–15:30 in G2

Session chair: Justin Biddle (Georgia Institute of Technology)

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Connecting Feminist Standpoint Empiricism to Cognitive Neuroscience

– Vanessa Bentley (University of Cincinnati)

Abstract. According to Subramaniam (2009), feminist critiques of science have had minimal impact on science, due in part, she claims, to the fact that feminist science studies remains moored in the mode of science criticism rather than moving forward to offer positive recommendations for solving the problems it uncovers. My interest is to further the practical project of feminist epistemology: to effect feminist change in the practice of science. My view is that in order for feminist philosophy of science to be effective in changing the practice of science, it must be closely tied to the specifics of a particular science, since different disciplines of science differ in terms of background assumptions, standards, techniques, instruments, analyses, models, theories, histories and language. My suggestion to address the gap between science and feminist theory is to tailor a feminist philosophy of science to the specifics of a discipline of science to make it more relevant and useful to practitioners of science. Given my focus on the practice of science, I endorse feminist standpoint empiricism (Intemann 2010) because it is applicable to practicing scientists rather than aimed at changing the scientific community.

I develop a feminist philosophy of cognitive neuroscience from the feminist standpoint by using the particulars of neuroimaging practice in two case studies of sex or gender differences. Through close consideration of the standards, procedures, measurements, analyses, assumptions, theories, and language of the neuroimaging articles, I propose an alternate framework based on the feminist standpoint that overcomes the reductionist, sexist, and androcentric problems of current neuroimaging practice. By thinking from women's lives and experiences, I demonstrate problems with current practice. The problems emerge all along the research process – from the research question, to the

background assumptions, to the methodology, to the reporting and analysis of data, to the interpretation of results, and to the language used. This new philosophy of cognitive neuroscience does not assume sex essentialism; takes social influences seriously; includes a more diverse, representative sample; and embraces multiple possible brain patterns rather than assuming a single, most efficient pattern. In addition to these general changes to practice, I find problems specific to the two case studies, such as not correcting for multiple comparisons; unexplored possible differences in behavioral and activation effect sizes; ignoring one's own data to support a sexist assumption; and using sexist stereotypes to explain data.

Considering that problems arise all along the research process, effecting feminist change in science is not going to be as simple as "add women and stir." It will require rethinking many aspects of standard practice. Connecting feminist standpoint empiricism to the specifics of practice has the potential to be more useful and relevant to practitioners of science than a feminist philosophy of science that is non-specific. The benefits of a philosophy of cognitive neuroscience from the feminist standpoint are that it allows for more epistemically sound science because it is not founded upon sex essentialism and that it contributes to science that is not oppressive to women.

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The Epistemic Significance of Scientific/Intellectual Movements

– Kristina Rolin (University of Helsinki)

Abstract. Sociological studies of science have introduced a theory of scientific/intellectual movements (SIMs) in order to understand the dynamics of science (see e.g., Frickel and Gross 2005; Frickel and Moore 2006). While the so called new political sociology of science has made a convincing case for the role of SIMs in the actual practice of science, there is hardly any uptake of this research in philosophy of science (for an exception, see Leonelli 2014). My aim is to rectify this situation by arguing that feminist standpoint theory is a social epistemology of SIMs. While feminist standpoint theory aims to understand the epistemic significance of feminist movements in science and academia, it offers a model which can be applied in theorizing other SIMs.

My presentation has three sections. In Section 1 I introduce Frickel's and Gross's theory of SIMs. While Frickel and Gross acknowledge Kuhn's (1962) groundbreaking work in philosophy of science, the epistemic aspects of SIMs are not their major concern. I conclude that it is up to philosophers of science to examine the epistemic significance of SIMs.

In Section 2 I introduce three theses associated with feminist standpoint theory: (1) the situated knowledge thesis, both generic and systemic (see e.g., Wylie 2012); (2) the thesis of epistemic advantage (see e.g., Wylie 2004); and (3) the achievement thesis (see e.g., Crasnow 2013). While I agree with Wylie that the thesis of epistemic advantage is best understood as an empirical hypothesis suggesting that "contingently, with respect to particular epistemic projects, some social locations and standpoints confer epistemic advantage" (2004, 346), I propose a novel interpretation of it. I argue that insofar as there is an epistemic advantage associated with some social positions, the advantage accrues to a SIM. SIMs can play an epistemically productive role in two ways. First, they enable social scientists and scholars to generate evidence under conditions where relations of power tend to suppress or distort evidence. Second, they provide social scientists and scholars with an epistemic community where they can receive fruitful criticism for research which may be ignored in the larger scientific community.

In Section 3 I situate feminist standpoint theory in the field known as the social epistemology of scientific knowledge. Much of the literature in the social epistemology of scientific knowledge focuses either on scientific communities or on research groups thereby ignoring SIMs. For example, some social epistemologists propose norms which characterize ideal scientific communities (see e.g., Longino 1990, 2002; Zollman 2007). Some others are concerned with an ideal distribution of research efforts in scientific communities (see e.g., De Langhe 2010, 2014; Kitcher 1990, 1993; Solomon 2001; Weisberg 2013; Weisberg and Muldoon 2009; Zollman 2010). Some social epistemologists suggest that scientific knowledge produced by research groups involves collective beliefs or acceptances (Andersen 2010; Bouvier 2004; Cheon 2013; Gilbert 2000; Rolin 2010; Staley 2007; Wray 2006,

2007). Some others suggest that the epistemic structure of scientific collaboration is based on relations of trust and interactions among scientists (Andersen and Wagenknecht 2013; Fagan 2011, 2012; Frost-Arnold 2013; Hardwig 1991; Kusch 2002; de Ridder 2013; Thagard 2010; Wagenknecht 2013, 2014). Clearly, the term “social” in the social epistemology of scientific knowledge means that philosophers are concerned either with scientific communities or with research groups. After explaining how SIMs differ from scientific communities and research groups, I conclude that there is a need for a more systematic inquiry into the epistemic significance of SIMs.

Parallel Session 7D

Friday, 26 June 2015 at 14:00–15:30 in G3

Session chair: Sabina Leonelli (University of Exeter)

Upper Level Ontologies, Metaphysical Commitments, and the Production of Questions

– Brandon Boesch (University of South Carolin)

Abstract. A recent trend within many scientific domains is the organization of information through the use of ‘ontologies.’ Ontologies allow scientific data and theoretical information to be expressed with the use of first-order logical systems, allowing for the creation of categories of objects and properties and the identification of relationships which hold between those categories. By organizing the information in this way, scientists are able to more effectively use the complex, overlapping web of information in wider scale projects, such as linking genetic information with relationships that hold between the levels of organization within a particular phenotype. This is undoubtedly helpful and a worthwhile project to be engaged in. Recently, there has been an increasing trend to try to get all ontologies to fit under the scope of a broader “upper level” ontology. These ontologies, such as the “Basic Fundamental Ontology” developed by Barry Smith and colleagues, attempt to identify the basic foundational categories and relationships of the world. The idea is then for the users of a wide range of domain-level ontologies to be able to nest each of the domain-level categories underneath one of the members of the upper level ontology, using one of the relationships of the upper ontology. The aim would be to ultimately have a large amount of scientific data and theoretical knowledge from the whole gamut of scientific domains to be nested under the same upper level ontology, allowing for the potential for interesting insights that might otherwise be missed. While I admire the pursuit of interdisciplinary thought that this attempt is at least partially founded upon, I think the use of upper level ontologies should be abandoned. Of primary concern is the problematic way in which the use of upper level ontologies with unknown philosophical commitments might create ways of understanding scientific information which precludes the full semantics of a theory (or a data model) to be fully expressed. The trouble could be identified in the attempt to reduce this information to a matter of first-order logic (even with the inclusion of temporalized logics, which have created their own problems). If we ignore this problem, there is another issue insofar as the basic relationships used by upper ontologies, although described in great detail in handbooks, are still vague enough that different scientists could use them in different ways. Even if these relationships were more solidly defined, there would still be trouble insofar as the relevant relationships of consideration in one domain (e.g. physics) might be non-starters in a different domain (e.g. biology). Another problem with the use of upper level ontologies is the way in which they are not created with the domain-specific knowledge in mind, and the way in which this could be problematic in the development of theories and the way in which questions will arise within the work of a field. Ultimately, the use of upper level ontologies requires a metaphysical commitment which has the potential to create problems within the practice of scientific investigation.

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What Are Biological Mechanisms? A View From Scientific Practice

– Daniel Nicholson (University of Exeter)

Abstract. One of the most conspicuous developments in the philosophy of science over the past fifteen years has been an increasingly central concern with elucidating the role that mechanisms play in science, especially in the biological sciences. Much of the philosophical attention has focused on developing general accounts of mechanism that do justice to the way the term is used in scientific explanation. Although there is little agreement over how best to define this concept—Machamer et al. (2000), Glennan (2002), and Bechtel and Abrahamsen (2005) are the three most influential accounts—there is close to universal agreement regarding their metaphysical status. Whatever else they may be, one thing everyone appears to agree on is that mechanisms are "real systems in nature" (Bechtel 2006); that is, that they are "real and local", as the title of a recent paper makes explicit (Illari and Williamson 2011). The reason for this consensus has to do with the way we tend to think about paradigmatic mechanisms of our everyday experience like a clock or a fridge. These are clearly "real and local," and are of course "real systems in nature". But does this realist understanding remain appropriate when 'mechanism-talk' is applied to biological phenomena? The history of the usage of the concept of mechanism in biology reveals that term has gradually come to be used to designate an extremely wide range of processes (such as natural selection, inheritance, or the immune response), and in doing so, it has lost its original machine connotations, becoming a dead metaphor. Unlike other scientific terms like microtubule, mitosis, or metabolism, 'mechanism' is not a technical concept; it does not appear in the glossaries of biology textbooks, nor is it listed in its indexes. Instead, it is a term that simply 'comes up' in scientific practice, and its meaning is inferred from the explanatory context in which it is invoked. Most philosophers have assumed that one thing that has remained attached to the mechanism metaphor as it has been imported into biology is that it still refers to 'real systems in nature' (like machines such as clocks and fridges). I challenge this conviction by taking seriously two implications that follow from the realist conception of mechanisms. If biological mechanisms are 'real and local', we should be able to answer two key questions: (a) how many mechanisms make up an organism? and (b) when is a description of a biological mechanism complete? By showing the impossibility of providing principled, unambiguous answers to these questions I will show that the best way to understand biological mechanisms is not as ontological building blocks of the living world, but as abstract and idealized spatiotemporal cross-sections of biological processes that heuristically pick out certain causal relations involved in the production of the phenomena that biologists are interested in explaining.

Parallel Session 7E

Friday, 26 June 2015 at 14:00–15:30 in G4

Session chair: Maria Serban (University of Pittsburgh)

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Material and Social Conditions for the Development of Mathematics

- Morten Misfeldt (Aalborg University)
- Mikkel Willum Johansen (University of Copenhagen)

Abstract. Mathematical knowledge has traditionally been taken to be absolutely objective, i.e. completely independent of contingent facts about the agents who discover the results. Today, this absolutistic view of mathematics has been challenged by a number of different theories. Most noticeably, social constructivists such as David Bloor (1981, 2011) and Donald MacKenzie (1979) have stress the influence social factors have had on the development of mathematics, and Bloor simply describes mathematics as a social institution. Other theorists such as Rafael Núñez and George Lakoff (2000) have claimed mathematics to be embodied and fundamentally shaped by the practitioners' sensory-motor experience. In this paper will report from a qualitative study of the practice of working mathematicians (Johansen and Misfeldt, 2014). The study shows that the production of mathematical knowledge is conditioned both by social factors and by our experience of the material world. Thus, the study confirms some of the basic ideas of the two approaches mentioned above. However, the study also shows that mathematicians actively use and shape the material world as part of their work process, and thus the material world conditions mathematics not only through sensory-motor experiences but also thorough the affordances is offers especially concerning the creation and manipulation of representations. Furthermore, our study gives reason to questions the reductionism inherent in both the social constructivist and the embodiment approach. Mathematics cannot be reduced either to the social or to the material. On the contrary we will show how the interplay between these two types of conditions is clearly visible and shapes the development of mathematics.

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Generating Certainty in Mathematical Practice: A Case Study in an Ethnography of Current Research Mathematics

– Stav Kaufman (Tel Aviv University)

Abstract. How do the practices of doing research level mathematics generate knowledge? In this talk we look at one specific case of current research in pure mathematics, and analyze its minute details in order to gain some insight into the kinds of resources used to put in place a new piece of mathematical knowledge. The case we look at is a result in Field Theory and Algebraic Geometry. The result was discussed, proved, written as a paper, and published between 2012 and 2014 by a group of three mathematicians working in German and Israeli universities. The talk is based on observations and data (mainly drafts and email correspondence) collected in real time, and on multiple interviews with the participants.

The talk takes as a starting point the assumption that mathematical knowledge creation and use are human collective practices, and should be described and analyzed as such. It traces some minute details of the process of this mathematical research, and uses this story to point out some of the practices in research level mathematics which make possible the creation of new mathematical knowledge. Compared to the knowledge produced in other disciplines, mathematical knowledge is commonly granted a distinctive type of certainty. This certainty, as an empirical phenomenon, is usually associated with consensus and lack of even a possibility of disagreement. The talk therefore pays extra attention to the many types of disagreement that emerged during this production of new knowledge, and, more importantly – to the diverse ways of solving (and at many occasions- dissolving) those disagreements. The different resources used to deal with such problems are considered. These include textual resources, personal and social resources, and material resources. We focus on a few specific exchanges (from e-mail correspondences of the researchers) and see how the separation of "internal" issues of proof ("pure" mathematical technical context) from "external" issues (applications of the theorems, presentation of the ideas, format and wording, expected audiences of the paper, etc.) collapses when looking at mathematical research practices. I will claim that mathematical certainty (in this case) is achieved precisely through such a contingent assemblage of practices, some of which are later removed from the published product.

Finally, the case study will be used as a basis for a general consideration of the differences between thinking of mathematical certainty as a norm and abstract ideal, and thinking of it as a practical achievement.

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Mathematization in Practice

– Davide Rizza (University of East Anglia)

Abstract. Model theory is often related to philosophy of science in the context of the reductionist project promoted by Patrick Suppes, which, roughly speaking, revolved around the identification of models in the sense of the working scientist with mathematical models definable by a set-theoretical predicate. The main theme of this talk is that the rejection of this identification as implausible (as suggested more or less explicitly in the literature) should not come with a rejection of all interaction between model-theory and scientific modelling as irrelevant. A local use of model-theoretical notions or techniques can shed significant light on scientific practice (at times, hardly to be had in alternative ways), offer a sophisticated analysis of its relation to mathematics and afford subtle ways of understanding the conceptual dynamics of mathematical modeling itself. In support of this claim I discuss two examples involving a very small amount of model-theory, in essence only the concept of satisfiability, both summarized below.

Example 1: the nature of numerical measures

Measurement models in empirical research rely on the idea that numbers measure certain empirical attributes. The foundational question concerning the nature of measuring numbers is also a practical question concerning the meaningfulness of numerical practices wherever it is sought to introduce them as instruments of investigation (psychology is a notable example). Numerical measures can be seen as model-theoretical objects and this point of view literally allows one to conceptualize the construction of measuring numbers from experimental practice. For measurement with a unit u (and an absolute zero), a complete list of experimental records $M(x, u)$ (these are formulae) on an environment E determines the measure of an object a relative to u as the subset of $M(x, u)$ satisfied by a in E. This makes it apparent that numbers codify experimental interactions and can be seen as compressions of experimental information whose structure is directly induced by experimental operations. Whenever this account can be given for a type of experimental practice, measurement is meaningful for it.

Example 2: impossibilities in social science.

Social scientists (especially economists) often confront descriptions of types of design that admit of no solution (an example is provided by Arrovian aggregation rules). These descriptions are linguistic but depend on set-theoretical parameters and they can be written as formulae with one free-variable in a sufficiently rich first-order language. Thus, an impossibility theorem amounts to the fact that, in a modelling universe U, a certain formula $F(x(1), \ldots, x(n), P(1), \ldots, P(n))$ with parameters $P(i)$ is not satisfiable. It is often thought that the only possible way of avoiding an impossibility consists in replacing F with G, which expresses a weaker description of the original design (in the sense that it is strictly entailed by the original description). The model-theoretical formulation of the problem, however, shows very clearly that impossibilities may also be removed by a change of parameters, i.e., by replacing $F(x(1), \ldots, x(n), P(1), \ldots, P(n))$ with $F(x(1), \ldots, x(n), Q(1), \ldots, Q(n))$ (for at least one i $P(i)$ is different from $Q(i)$). In particular, one may remove an impossibility while adopting a description strictly stronger than F. Several examples of occur in aggregation theory and utility theory.

I briefly conclude by pointing to areas in which only a little work has been done but significant progress is likely to come from the adoption of a model-theoretical framework (notably, the reconstruction of aggregation procedures as amalgamations of structures and the use of model-theoretical mixtures to build and study probabilistic models).