

SPSP 2011

**The third biennial conference of the
Society for the Philosophy of Science in Practice**

**June 22—24 2011
University of Exeter, UK**

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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, etc., 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 here:

(1) 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.

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

(3) 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.

(4) 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.

Organising Committees

Permanent organisational committee

Rachel A. Ankeny	The University of Adelaide	rachel.ankeney@adelaide.edu.au
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We gratefully acknowledge the secretarial and managerial support of Egenis staff Laura Dobb, Claire Packman and Cheryl Sutton, whose help was crucial to setting up this conference.

Local information

The historic city of Exeter is in the county of Devon which boasts fabulous coastlines, beautiful countryside and extensive moorlands. Here is a taste of some of things the area has to offer.

Exeter City

Exeter was originally settled by the Romans and is a city brimming with history from many ages. More recent are the remains of Rougemont Castle, now a city park and also the spot of the last documented witch trial in the UK. Certainly worth the time to visit is the cathedral, which is the longest of continuous gothic vaulting in the world, and is decorated with over 60 'green men' typical of West Country churches. Following the city wall, you can stop for a pint at the pleasant quayside or walk downstream along the Exe and cross back over the footbridges.

Exe Estuary and the Jurassic Coast

Exeter is located on the estuary of the river Exe and rail services run along both sides the estuary down to the coast. The estuary is recognised as being of international importance for wildlife, with extensive reed beds, salt marshes, mud flats and sandbanks. There is a nature reserve where the estuary meets the sea at Dawlish Warren — also a good spot for a swim and to relax on the beach. On the opposite side of the estuary is the town of Exmouth from where you can take a boat trip along the Jurassic coast. The Jurassic Coast is a World Heritage Site covering 95 miles of truly stunning coastline from East Devon to Dorset which clearly depicts geological periods spanning the Triassic, Jurassic and Cretaceous periods. If you prefer something more active there are a wide range of coastal activities available such as fishing, sailing, windsurfing and jet skiing.

Dartmoor National Park

Covering an area of 368 square miles, the landscape of Dartmoor comprises heather-covered moorland, tumbling rocky rivers and craggy granite tors. A paradise for lovers of the great outdoors, many activities can be enjoyed here including walking, riding and rock climbing. To make the most of your visit you will need an Ordnance Survey map and also useful is the Pathfinder Guide to Dartmoor which includes more than 20 circular walks. Both are available at bookshops in Exeter or online.

Walking in and around Exeter

The *Exeter Green Circle Walk* consists of five interconnecting walks surrounding Exeter. One of the walks passes near the Streatham Campus: The *Hooper Valley Walk* (2.3mile/4km) winds close to the City Wall and through the centre of St David's revealing the now dry valley under the Iron Bridge. Elsewhere it follows parts of the picturesque Hooper Valley through the University of Exeter's Streatham Estate with its arboretum. Near the high point of the route, there are views to the sea in one direction and Dartmoor in another over Duryard Valley Park. Countryside walks surrounding Exeter include: Exeter ship canal walks • The Alphinbrook & Hambeer Lane • Topsham & Bowling Green Marshes • Countess Wear & Double Locks.

Weather

June is generally a warm and pleasant month to visit Exeter. Temperatures range from highs around 19°C to lows around 11°C, with temperatures getting higher near the end of the month. Average rainfall is 47 mm (2 inch) for June, with rain on average on 11 days.

Detailed information

To find out about cultural and natural attractions, along with activities and food & drink, visit <http://www.discoverdevon.com> and to download guides for walking in and around Exeter, go to <http://exeter.gov.uk/index.aspx?articleid=1502>.

Conference lunch

A finger buffet lunch each day is included in the delegate fee and baked goods will be served during morning and afternoon breaks throughout the conference. As Exeter city centre is at least a 15 minute walk from the conference location, it will be impossible to get lunch downtown without missing a session.

Off-campus dining

For dining in central Exeter there are several good quality and value restaurant chains: **Café Rouge**, **Carluccio's**, **Nandos**, **Strada**, and **Wagamama** (all close to the cathedral).

The following lists some independent restaurants between the campus and central Exeter:

Harry's Restaurant (Traditional food near campus)

86 Longbrook street, EX4 6AP Exeter

P: 01392 202 234.

Opening hours: Mon–Sun: lunch 12–2pm, dinner 6–11pm.

The Rusty Bike (Bistro-Pub near campus)

67 Howell Road, EX4 4LZ Exeter

P: 01392 214 440.

Dinosaur Cafe (Budget Mediterranean café near campus)

5 New North Road, EX4 4HH Exeter

P: 01392 490 951.

The Exeter Phoenix Café (Inside arts centre near Rougemont Castle)

Bradninch Place, Gandy Street, EX4 3LS Exeter

P: 01392 667051.

Opening hours: Mon–Sat: 10am–11pm, Sunday: 11.30am–5pm.

Herbies (Vegetarian restaurant near cathedral)

15 North St, EX4 3QS Exeter

P: 01392 258473.

Opening hours:

Lunch: Mon–Fri: 11am–2.30pm and Sat: 10.30am–4pm; dinner: Tues–Sat: 6–9.30pm.

The Eggplant café-deli (Vegetarian bistro next to cathedral)

1 Cathedral Yard, EX1 1HJ Exeter

P: 01392 428144.

Opening hours: Mon–Sat 9am to 6pm, except Fri: 9am–9pm, Sunday: 10.30am–5.30pm.

Tea on the Green (Full meals and traditional cream teas next to cathedral)

2 Cathedral Close, EX1 1EZ Exeter

P: 0844 980 2144.

Opening hours: Mon–Sat: 8am–6pm, Sunday: 9am–5pm.

Michael Caines at ABode Exeter (High end critically acclaimed restaurant next to cathedral)

Cathedral Yard, EX1 1HD Exeter

P: 01392 223638.

Waterfront

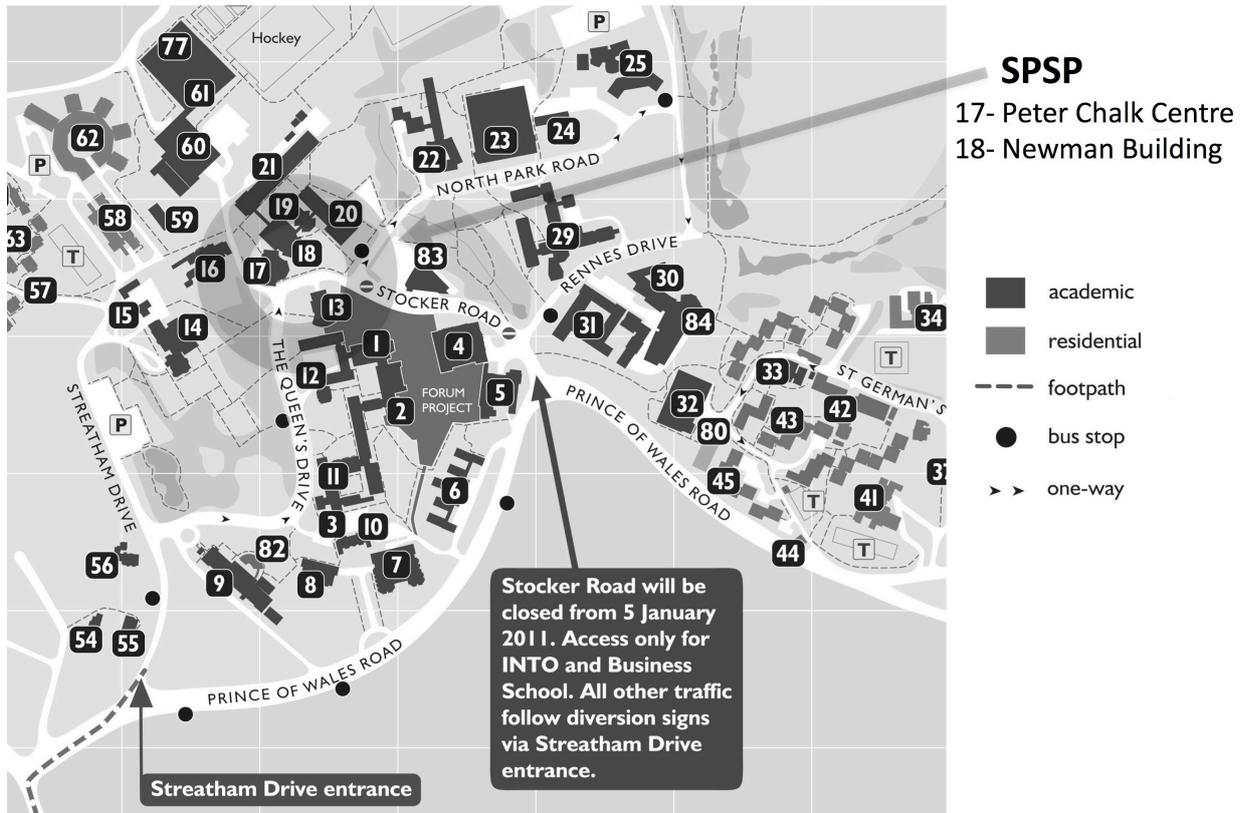
4-9 The Quay, EX2 4AP Exeter

P: 01392 210 590.

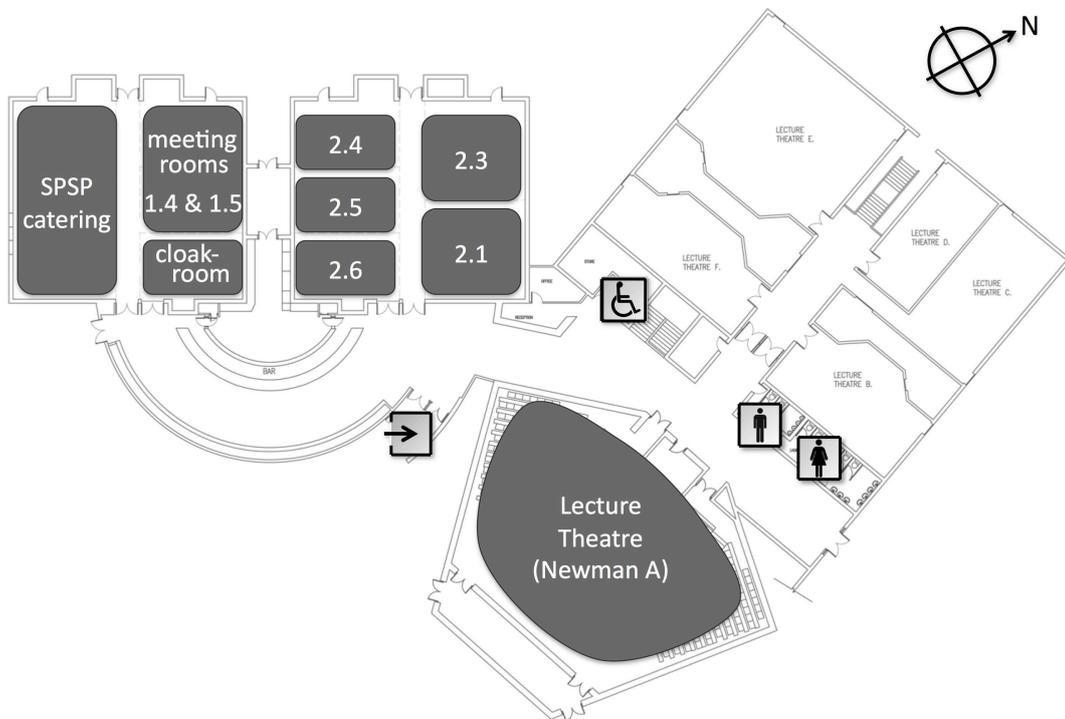
Opening hours: Mon–Sun: 11.30am–11pm.

Conference Location and Programme

The conference will take place on the Streatham campus of Exeter University:



The Peter Chalk Centre will have the following layout for the SPSP conference, with sessions taking place in the rooms as indicated on the programme:



[Please contact Laura Dobb (L.C.Dobb@exeter.ac.uk) if you wish to book a meeting room.]

Wednesday, June 22 (AM)

09:00-09:30	Welcome and opening remarks by Rachel Ankeny, Sabina Leonelli, and John Dupré [LT– Lecture Theatre]					
09:30-11:00	Philip Kitcher: Plenary session chaired by John Dupré Can we save democracy and the planet, too?					
11:00-11:30	Coffee break					
11:30-13:00	<i>Cognitive sciences I [2.6]</i> Chair: Alison Wylie	<i>Economics [2.5]</i> Chair: Michiru Nagatsu	<i>S1. Philosophical dimensions of response shift [2.4]</i> Chair: Rebecca Kukla	<i>Experimental practices I [2.3]</i> Chair: Karen Kastenhofer	<i>S2. Interdisciplinary exchanges as the object of philosophical inquiry [2.1]</i> Chair: Marcel Boumans	<i>S3. Operationalism in the life sciences [LT]</i> Chair & commentator: Uljana Feest
	Robyn Bluhm: “Neural context” and the localization of cognitive functions	François Claveau: From general claims to specific policies: Disambiguating causal claims for unemployment policy	David Wasserman & Jerome Bickenbach: Different approaches to subjective well-being and the response shift	Astrid Schwarz: Experimenting in the field: Probing the notion of “real world simulation”	Uskali Mäki: Biological markets and interdisciplinarity	Staffan Müller-Wille: The gene — A concept in flux
	Elizabeth Potter: Collaborative experimental practices	Andrej Svorencik & Harro Maas: Seneca to witness: Experiences with a witness seminar on experimental economics	Marjan Westerman: The struggle behind “I’m alright” — Where health science meets philosophy: The measurement of quality of life and response shift	Stéphanie Ruphy: Do computer simulations constitute a new style of scientific reasoning?	Caterina Marchionni: Playing with networks: How economists explain	Mathias Grote: Operationalizing reactivity — The “purple membrane” from cellular structure to nanotechnology
	Mazviita Chirimuuta & Mark Paterson: Extending, changing and explaining the brain. Neuroplasticity and the case of tactile-visual sensory substitution	Davide Rizza: Interpretation and modelling strategies in economics: The case of Sen’s theorem	Leah McClimans: A philosophical explanation of response shift	Miguel Garcia-Sancho: What does the circulation of protein sequencing tell us about the history and philosophy of contemporary biomedicine?	Till Grüne-Yanoff: Intertemporal discounting in psychology and economics	Pierre-Olivier Méthot: Virulence: An operational concept? Robert Meunier: Operationalizing phenotypes in developmental genetics and contrastive explanations
13:00-14:00	Lunch					

Wednesday, June 22 (PM)

14:00-15:30	<i>Cognitive sciences II [2.6]</i> Chair: Aleksandra Sojic	<i>Ethical and legal issues in biomedicine [2.5]</i> Chair: Susan Hawthorne	<i>Interdisciplinarity [2.4]</i> Chair: Wybo Houkes	<i>Experimental practices II [2.3]</i> Chair: Jo Donaghy	<i>Scientific integrity [2.1]</i> Chair: Henk de Regt	<i>S4. Methodological pluralism [LT]</i> Chair: John Dupré
	Hajo Greif: Engineering minds. The tools and uses of artificial intelligence	Cecilia Guastadisegni & Flavio D'Abramo: Cancer molecular biomarkers: Epistemological and ethical controversies	Sophia Efstathiou: Bridging science: Ensuring success in cross-disciplinary science	Léna Soler <i>et al.</i> : Calibration in daily scientific practices: A conceptual framework	Milena Ivanova: The inconclusiveness of theoretical virtues	Hasok Chang: Chemical atomism: Progress through pluralism Miriam Solomon: Evidence-based medicine and mechanistic reasoning in the case of cystic fibrosis
	Michiru Nagatsu: Towards practical pluralism: A case of neuroeconomics	Kirstin Borgerson: Useless, repetitive, and secretive? Assessing the scientific validity of clinical trials	Henrik Thorén & Johannes Persson: Three kinds of interdisciplinary relations	Mieke Boon: 'Same conditions—same effects' as a regulative principle in experimental practices	Janet A Kourany: Freedom of research and the public good	Alison Wylie: A plurality of pluralisms: Collaborative practice in archaeology
	Cyrille Imbert: Computational science and human understanding: The role and logic of scientific sketches	Delene Engelbrecht: The patenting of biological artefacts. Naturalness and artificiality in practice	Raoul Gervais: Making sense of interdisciplinary explanations: A pragmatic approach	Jutta Schickore: The nature and roles of methods accounts in experimental reports	Jeff Kochan: The limits of scientific integrity in practice	
15:30-16:00	<i>Coffee break</i>					
16:00-17:00	<i>Plenary panel discussion chaired by Rachel Ankeny</i>		John Dupré, with comments from Hasok Chang and Alison Wylie: How do we study science in practice?			
17:00-18:00	<i>SPSP business meeting (all welcome)</i>					

Thursday, June 23 (AM)

09:30-11:00	<i>Bias in medicine [2.6]</i> Chair: <i>Inmaculada de Melo-Martín</i>	<i>Experts and publics [2.5]</i> Chair: <i>Janet Stemwedel</i>	<i>Evolution [2.6]</i> Chair: <i>Till Grüne-Yanoff</i>	<i>Representation [2.3]</i> Chair: <i>Adam Toon</i>	<i>Philosophy of science in practice [2.1]</i> Chair: <i>Ian Kidd</i>	<i>S5. The Social organization of research and the flow of scientific information [L.T]</i> Chair: <i>Annamaria Carusi</i>
	David Teira Serrano: Debiasing rules in medical experiments	Holly VandeWall: Expertise and the disunity of science: The epistemic difficulties of providing expert advice for policy	Jean-Sébastien Bolduc: Adaptationism, beyond controversies and into the fabric of scientific reasoning	Demetris Portides: Scientific representation, denotation, and explanatory power	Haris Shekeris: Of communities and individuals as regards scientific knowledge	Justin Biddle: Intellectual property and the sharing of scientific information Bryce Huebner & Rebecca Kukla: Making an author: Epistemic accountability and distributed responsibility
	Jan De Winter: How to make the research agenda in the health sciences less distorted	Susann Wagenknecht: Joint group knowledge — Justification in esoteric and exoteric contexts	Tudor M Baetu: How contingent is evolution? Mechanistic constraints on evolutionary outcomes	Nicola Mößner: The relevance of scientific visualizations — or why words are not enough	Leen De Vreese, Jeroen Van Bouwel & Erik Weber: A pragmatic turn for general philosophy of science	Torsten Wilholt: Methodological standards, research communities, and the social diffusion of trustworthiness
			Armin Schulz: Selection, drift, and independent contrasts: Defending the conceptual and methodological foundations of the method of independent contrasts	Laura Nuño de la Rosa: 3D modeling, organicism and mechanical explanation in contemporary developmental biology and evo-devo	Dana Tulodziecki: Towards an epistemology of scientific practice	
11:00-11:30	<i>Coffee Break</i>					
11:30-13:00	<i>Plenary session chaired by Hasok Chang</i> Bernadette Bensaude-Vincent: The disciplinary models of synthetic biology					
13:00-14:00	<i>Lunch</i>					

Thursday, June 23 (PM)

14:00-15:30	<i>Agriculture, food and climate</i> [2.6] <i>Chair: Justin Biddle</i>	<i>Objectivity and consensus</i> [2.5] <i>Chair: Jonathan Y Tsou</i>	<i>Collaboration and laboratory practice</i> [2.4] <i>Chair: Delene Engelbrecht</i>	<i>Explanation</i> [2.3] <i>Chair: Armin Schulz</i>	<i>S6. Representative practices</i> [2.1] <i>Chair: Harro Maas</i>	<i>S7. Forms of iterativity in scientific practice</i> [LT] <i>Chair & commentator: Hasok Chang</i>
	Prajit K Basu & C Shambu Prasad: Delineation of a controversy in technoscience: The case of the system of rice intensification as practiced in india	Laszlo Kosolovsky: Aspiring consensus in scientific practice: Grasping consensus driven motivations by introducing a continuum ranging from consensus conferences to meta-analysis	Rachel A Ankeny, Kathryn Maxson & Robert M Cook-Deegan: Examining the history and implications of the 'Bermuda Principles' for data sharing	Andrea Woody: A functional account of scientific explanation	Isabelle Charmantier: A naturalist's visualizations: Carl Linnaeus (1707-1778) and his representative practices Silvia De Bianchi: The aims of representative practices: Symmetry as a case-study	Kevin C Elliott: Epistemic and methodological iterativity in nanoscale science and technology Maureen A O'Malley: The dynamics of scientific practice: Integration and iterativity in molecular life sciences
	Keith Hyams: Climate change and carbon rationing	Inmaculada de Melo-Martín & Kristen Intemann: Scientific dissent, objectivity, and public policy	Erwan Lamy: Science under constraints: The example of the "good laboratory practices" within the french biotechnology SMEs	Julia Bursten: Why chemical explanations are unlike biological or physical explanations	Annamaria Carusi: Representation and validation in cardiac modelling	Orkun S Soyer: Hunter-gatherers in scientific practice
		Henk W de Regt: Objectivity and scientific understanding	Marco Liverani: Structure, flows, and practices in EU-funded stem cell networks	Łukasz Lamża: Identity of indiscernibles in the real world: Between the quantum and the classical	Chiara Ambrosio: From "representations" to "representative practices": Lessons from visual history	
15:30-16:00	<i>Coffee break</i>					
16:00-17:30	<i>Mathematical practices</i> [2.6] <i>Chair: Julia Bursten</i>	<i>Technoscience in the workplace</i> [2.5] <i>Chair: Thorsten Wilholt</i>	<i>Modelling</i> [2.4] <i>Chair: Wolfgang Pietsch</i>	<i>Grouping practices</i> [2.3] <i>Chair: Emma Tobin</i>	<i>Exploratory experimentation</i> [2.1] <i>Chair: Joseph Rouse</i>	<i>S8. Philosophical and social perspectives on synthetic biology</i> [LT] <i>Chair: Bernadette Bensaude-Vincent</i>
	Andrew Aberdein: Explanation and argument in mathematical practice	Karen Kastenhofer: The interdisciplinarity culture of the new technosciences	Roberto Belisário Diniz: Scientific modelling and limited observational data: A case of conflict in modern cosmology	Christopher DiTeresi: how 'normal development' tames variation: Types as reference standards for comparative work	Alfred Nordmann: Ian Hacking as philosopher of scientific practice: <i>Representing and intervening</i> revisited	Axel Gelfert: Synthetic biology as thing knowledge Gabriele Gramelsberger: The simulation approach
	Babu Thaliath: The nature of spatial intuitions in early modern mechanics and optics	Endla Lõhkivi, Katrin Velbaum & Jaana Eigi: Scientific styles, identities and workplace cultures: A case study on the cultures of physics and humanities	Alan C Love: Modeling experimental evidence from the practices of developmental biology	Thomas AC Reydon: Bridging a theory-practice gap: What can kind essentialism contribute to understanding classificatory practices in biology?	Stephan Güttinger: The nature of exploratory experimentation in the life sciences	Kathrin Friedrich: 'Pixel by pixel': Visual programming languages in synthetic biology
	Helen De Cruz & Johan De Smedt: Cognitive limitations, distributed cognition, and mathematical practice: The case of Chinese algebra	Farzad Mahootian: A systems approach to self-reflexive science: preliminary findings from laboratory engagement studies	Christian Hennig: Some thoughts on statistical models and reality	Miles MacLeod: Kinds, natural kinds, and grouping practices in research contexts	Sara Green: Experiments as question-generating machines — Epistemology of mathematical modelling in biology	Tarja Knuutila & Andrea Loettgers: Synthetic biology and the functional meaning of noise
19:00-	<i>Conference dinner at the Imperial pub</i>					

Friday, June 24 (AM)

09:30-11:00		<i>Building science [2.5]</i> <i>Chair: Axel Gelfert</i>	<i>Mechanisms [2.4]</i> <i>Chair: Nathalie Gontier</i>	<i>Models and fictions [2.3]</i> <i>Chair: Alan C Love</i>	<i>Ontology and practice [2.1]</i> <i>Chair: Sabina Leonelli</i>	<i>S9. Investigating practical impacts of descriptive categories [LT]</i> <i>Chair: Hasok Chang</i>
		Vanessa Gorley: Putting together the pieces: Building science from local labs	Gry Oftedal: The roles of difference-making and causal mechanisms in biology: Examples from classical genetics, molecular biology and systems biology	Adam Toon: Playing with molecules	Isaac Record: Technological possibility as a condition for epistemic possibility	Brendan Clarke: Classifications of melanoma Katie Kendig: Classification and complementary science
		Stephen D John: Is medical research more like building a house or more like hitting a nail?	Juan B Bengoetxea: Scientific explanation, mechanisms and pluralist realism	Greg Lusk: Counterfactual dependence, justification, and model explanation	James A Overton: Informatics, philosophy, and ontology	Emma Tobin: Biomolecular classification
		Susan CC Hawthorne: Good* research on gender differences in mental health	Veli-Pekka Parkkinen: Genetic causation and mechanism explanation	Ann-Sophie Barwich: Science and fiction: On the reference of models	Joseph Rouse: Laws and nomological necessity in scientific practice	
11:00-11:30	<i>Coffee break</i>					
11:30-13:00	<i>Evidence and decision-making [2.6]</i> <i>Chair: François Claveau</i>	<i>Consciousness [2.5]</i> <i>Chair: Giovanna Colombetti</i>	<i>Genes, cells and individuals [2.4]</i> <i>Chair: Mathias Grote</i>	<i>Analogical reasoning [2.3]</i> <i>Chair: Andrew Aberdein</i>	<i>Applied philosophy of science [2.1]</i> <i>Chair: Astrid Schwartz</i>	<i>S10. Philosophy of psychiatry in practice: Steps towards an adequate theory of psychiatric classification [LT]</i> <i>Chair: Brendan Clarke</i>
	Heather Douglas: Weight of evidence analysis in practice	Michelle Maiese: Scientific inquiry and essentially embodied cognition	Jo Donaghy: Learning from microbiology: Are all individuals evolving?	Shaul Katzir: Reasoning by concrete imagined cases in science and its relation to thought experiments	Michael D Dahnke & H Michael Dreher: Teaching philosophy of science to doctor of nursing practice students	Jonathan Y Tsou: Intervention, causal reasoning, and the reality of entities in psychiatry: Pharmacological drugs as experimental instruments in neurobiological research on mental disorders
	Sharon Crasnow: Informed policy and purposive case selection	Elizabeth Irvine: The role of scientific practise in eliminativist claims: A case study of consciousness	Jordan Bartol: Personalized genomics as a testing ground for theorizing about genes	Andrea Sullivan-Clarke: Analogical reasoning in scientific practice: The problem of ingrained analogy	Janet D Stemwedel: Breaking the codes: How philosophers might best help scientists with responsible conduct of research	Lara K Kutschenko: ICD vs. DSM: Two classifications in practice, two perspectives on psychiatry
			Melinda B Fagan: Waddington redux: Models of cell reprogramming	Wolfgang Pietsch: The neglect of analogy	Michael O'Rourke & Justin Horn: Better science through philosophy: The story of the Toolbox Project	Kathryn Tabb: Psychiatry and the natural kinds debate: Let's get practical

Friday, June 24 (PM)

13:00-14:00	<i>Lunch</i>					
14:00-15:30	<i>Social sciences [2.6]</i> <i>Chair: Pierre-Olivier Méthot</i>	<i>Cultural evolution [2.5]</i> <i>Chair: Laura Nuño de la Rosa</i>	<i>Scientific discipline [2.4]</i> <i>Chair: Andrea Woody</i>	<i>Semantics and pragmatics [2.3]</i> <i>Chair: Hajo Greif</i>	<i>Progress [2.1]</i> <i>Chair: Staffan Müller-Wille</i>	<i>S11. Computers in scientific practice [LT]</i> <i>Chair: Sabina Leonelli</i>
	Alexandre Guay: The non-impact of sociophysics	Wybo Houkes: Extending Darwinism to culture: Population thinking or natural selection? Towards a practice(s)-based view	Ian J Kidd: A role for epistemic virtues in archaeological practice? The case of 'epistemic beneficence'	Boaz Miller: Knowledge and values: The argument from necessity for sensitive invariantism	Amy L McLaughlin: Pragmatic significance, demarcation, and scientific progress	Anouk Barberousse & Marion Vorms: About the empirical warrants of computer-based scientific knowledge
	Monika Wulz: The working class is cooking: The invention of a novel body of practical knowledge during the 19th century	Nathalie Gontier: Towards an overarching methodology to study the evolution of language: Identifying the units, levels and mechanisms of language evolution	Dunja Šešelja & Erik Weber: Continental drift debate and the context of pursuit	Hyundeuk Cheon: The three-dimensional metainformation theory of scientific concepts	David B Pedersen: The philosophy of research assessment exercises	Julie Jebeile: One computer simulation, two conceptual universes Vincent Israel-Jost: Simulated data and empiricism
	María Jiménez-Buedo: Experiments in the social sciences: Rethinking the Hawthorne effect			Lucía Lewowicz: The term phlogiston and the notion of failure to refer — Towards a pragmatic theory of reference for scientific terms.	Moti Mizrahi: The scientific practice of assessing progress	
15:30-16:00	<i>Coffee break</i>					
16:00-17:30	<i>Plenary session chaired by Andrea Woody</i> Sandra Mitchell: Integrative pluralism: The case of protein folding					
17:30-17:45	<i>Closing remarks by Hasok Chang</i>					

Plenary Abstracts

P1. Can we save democracy and the planet, too?

Philip Kitcher
Columbia University, USA

After briefly examining the evidence that leads climate scientists to think there is an urgent global problem, I'll explore the reasons why democratic decision-making seems so at odds with scientific recommendations. I'll argue that there is a general problem of integrating expertise with apparent democratic ideals, and will suggest an improved picture of democracy that would allow an ideal solution. I'll conclude with some thoughts about how we might proceed in a decidedly non-ideal world.

P2. How do we study science in practice?

John Dupré
Exeter University, UK
Commentaries by Hasok Chang (University of Cambridge, UK) and
Alison Wylie (University of Washington, USA)

The name of this society records a conviction that it is insufficient for philosophy of science to approach science solely through its formally published outputs; we need also to look behind these at how they are produced, what their producers do in the daily business of 'doing science', and what they really believe about their practice and their findings. But there is a range of possible methods for extending our understanding of science beyond the published text. The most traditional is HPS, the supplementation of philosophy of science with historical investigation of how past scientists reached the conclusions they did about nature. Of very contemporary interest is the emergence of experimental philosophy, which seeks to investigate directly the empirical questions that arise in the course of philosophical work. Can this provide a way forward for philosophy of science? A third possibility for exploring contemporary science is collaboration with social scientists with a long tradition of empirical investigation of the practices of science, or direct collaboration with scientists themselves.

The brief introductory talk by John Dupré will suggest some serious limitations to the value of experimental philosophy for philosophy of science and illustrate the value of more interdisciplinary collaboration with reference to some recent work at Egenis. A series of brief responses is intended to lead into a general discussion of the strengths and weaknesses of various ways in which philosophers may try to come to terms with science in practice.

P3. The disciplinary models of synthetic biology

Bernadette Bensaude-Vincent
Université Paris-X Nanterre, France

Despite the transdisciplinary dimension of the researches conducted under the umbrella synthetic biology, the founders of this new research area adopted a disciplinary profile. In so doing they took inspiration from two different already established disciplines. The analogy with synthetic chemistry suggested by the term 'synthetic biology' is not the unique model. Information technology is clearly another source of inspiration. In describing the debates surrounding the coinage of synthetic biology, the paper discusses the question: how do the two models that shape this emerging field co-exist in the same community? Do they chart two divergent futures for synthetic biology?

P4. Integrative pluralism: The case of protein folding

Sandra Mitchell
University of Pittsburgh, USA

Integrative pluralism defends the view that there is scientific value in fostering different levels of description and explanation, and integration, rather than reduction, will produce the most informative accounts of the behavior of complex structures. I will explore this hypothesis by considering the case of protein folding behavior in biological systems. I will consider this behavior from three perspectives; first, the physics of the material constituents and their behavior, second, the chemistry of the molecular structure of proteins, how they acquire the structures they have and third, the biological function of proteins, especially the dependence of function on particular structures.

Symposium abstracts

S1. Philosophical dimensions of response shift

Patient-reported outcomes (PROs) are meant to provide information about how different medical interventions affect patients' quality of life or health status, i.e. well-being, from their collective point of view. These outcomes are particularly important in those areas of health care that focus on the amelioration of chronic disease and palliation of terminal illness, i.e. interventions that aim to improve quality of life. Caring for the chronically and terminally ill increasingly constitute significant percentages of health care budgets and PROs are increasingly used to assess the effectiveness of this care. In this session we present three papers that address a phenomenon that often occurs when researchers collect PROs, namely, response shift.

Information on PROs is typically collected via measures (Patient-Reported Outcome Measures (PROMs); also sometimes called 'Quality of Life' (QoL) measures) that ask patients general and specific questions about their quality of life or health status. In order to use PROMs to assess effectiveness these measures must be interpretable, i.e. we need to understand the clinical significance of changes in PROs over time. One obstacle to interpretability is response shift. Response shift refers to a change in the meaning of one's self-evaluation of quality of life or health status as a result of a) a change in one's internal standards of measurement, b) a change in one's values or c) a change in one's definition of quality of life or health status (Schwartz and Sprangers, 1999). This phenomenon can lead to counterintuitive outcomes. For instance, patients whose health appears to be deteriorating may nonetheless report having a good quality of life, indeed sometimes patients report having a better quality of life as their health declines than they did when their health was objectively better. Thus, PROs may improve or stay the same over time despite the effects of, for instance, toxic therapy and a terminal prognosis.

Interpreting PROs requires that we better understand response shift. But what is response shift? Why does it occur? Should we attempt to dampen its affects or include it as a legitimate aspect of respondent answers over time? This session attempts to answer some of these questions. In David Wasserman and Jerome Bickenbach's paper 'Different Approaches to Subjective Well-Being and the Response Shift Phenomenon' the authors draw on the data from a National Institutes of Health (NIH) funded project to discuss how research into response shift might inform our understanding of different species of PROMs and also how current philosophical discussions of well-being might in turn inform research into response shift. In Marjan Westerman's paper the author draws on her own qualitative research (funded by the Dutch Cancer Society) to question the causes of her counter-intuitive findings and whether the measurement of quality of life is possible. Finally, Leah McClimans attempts to explain response shift by arguing that it is a form of what Charles Taylor (1985) calls 'strong evaluation'. She goes on to discuss the implications that this explanation has for the measurement of quality of life and perceived health status.

Different approaches to subjective well-being and the response shift phenomenon

David Wasserman¹ and Jerome Bickenbach²

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The phenomenon of response shift offers a useful context in which to tease apart two distinct understandings and measures of subjective well-being: experiential, with well-being assessed

on the basis of regular experience-reports; and evaluative, with the subject herself assessing how well her life is going overall. Both measures are subjective, since they are based on the experience and judgment of the subject about her own life. But the former understands well-being as a function of units of experience, sampled by the measurement procedure. The latter, in contrast, understands well-being in terms of the judgments made by the subject about how her life is going, over a longer or shorter period of time, in comparison to some baseline or comparison class. Both approaches need to be distinguished from objective approaches, which assess well-being in terms of the subject's achievement or performance of independently or inherently valuable achievements or activities.

We would expect long-term evaluations to be more susceptible than either experience reports, on the one hand, or objective check-lists, on the other, to the three processes thought to underlie response shift, the widespread, surprising phenomenon of higher reported well-being by individuals with worsening health conditions. Those three processes are 1) recalibration: changes in the standards or comparison class with reference to which the subject assesses how she is doing; 2) reprioritization, changes in the preferences, values, or goals she takes into account in making that assessment or the weight she assigns to them; and 3) reconceptualization, changes in the subject's understanding of what it means to live well. We also expect long-term evaluations to be more susceptible than experiential assessments to the self-presentational effects associated with response-shift.

Using data from an NIH/- (USA) funded project on well-being and health, we will explore the contributions response-shift research can make to our understanding of subjective and objective well-being, and how the philosophical analysis of well-being can inform response-shift research. For instance, part of this project explores the relationship between subjective well-being and health outcomes. Subjective well-being is often characterized as an effect of health status, i.e. as health declines the decline of subjective well-being follows. But we also know that response shifts do occur, e.g. as a person ages and their health declines individuals tend to adjust to these changes and subjective well-being tends to improve even though health does not improve. Moreover, some preliminary findings suggest that after controlling for initial health conditions happiness can extend life expectancy and decrease mortality and the negative impact of chronic illness. Does response shift offer an instance in which subjective well-being is a cause of health as opposed, or in addition, to an effect? If this is the case, then how might this information alter how and when we measure subjective well-being? If evaluative measures better track response shift, then under what circumstances should we use these measures instead of experiential measures? Furthermore, if response shift does act as a cause of health, are all such shifts legitimate? Are there cases in which we are justified in thinking that such shifts are illegitimate, for instance, a result of 'false consciousness'?

The struggle behind “I’m alright” — Where health science meets philosophy: the measurement of quality of life and response shift

Marjan Westerman
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Response shift has gained increasing attention in the measurement of health-related quality of life (QoL) as it may explain counter-intuitive findings. Response shift refers to a change in internal standards, values and conceptualization of QoL and is recognized as an important mediator in adaptation to changing health. Response shift theory has much to offer in explaining why patients' own evaluations may differ considerably from those made by clinicians and significant others. This is especially so because appraisal processes (response behavior in QoL measurement) has been recently taken into account as a legitimate phenomenon. Although response shift theory seems promising and specific statistics have been developed to explore

and “measure” response shifts, it could be questioned whether we can really capture what is behind patients’ evaluation of good QoL in the context of deteriorating health.

In this paper I will present results from a qualitative study in which we investigated the perspective of terminally ill small-cell lung cancer patients. I describe the aspects which contribute to the fact that these patient patients are able to say “I’m doing all right” despite deteriorating health. I argue that the positive image which small-cell lung cancer patients might present in the QoL measurement is neither just the result of their own evaluation of health, nor just the result of successful adaptation (i.e. response shift). Patients seem to be continuously busy maintaining control by focusing on treatment, by anchoring themselves to a positive attitude, and by continuing their commitment to their family. Living with a terminal illness sometimes requires a daily need for willpower from the patients, and presumably also from their loved ones.

While acknowledging that a positive self-report is a product of the struggle for emotional survival I discuss whether the measurement of QoL is possible at all. Why do we measure how limited cancer patients are, e.g. in their daily activities, with the presumption that this might affect QoL while these patients frequently use this questions to show how well they are controlling and dealing with their situation (Westerman et al, 2007 en 2008). Randall & Downie (2006) argue in *The Philosophy of Palliative Care* that QoL is logically impossible to define, and that the patient’s overall QoL is outside the control of the health care team. Our results confirm their statement that important factors are not determined by health and are outside the remit of health care. If QoL is impossible to define, then what about defining and measuring response shift? I end with a critical discussion regarding idea that teaching response shift is a good thing. Since Charles Taylor (1989) states that human beings constitute their identities by an ongoing (re)evaluation of “what is good” a sharp demarcation between “response shift adaptation” and “life adaptation” seems inappropriate.

A philosophical explanation of response shift

Leah McClimans
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Patient-Reported Outcome Measures (PROMs) are meant to assess patients’ collective perspective on many facets of health care including quality of life, side effects of treatment, symptoms, health care needs, quality of care and the evaluation of health care options (Rapkin & Schwartz, 2004). Indeed, these measures are increasingly used as evidence for effectiveness and drug labeling claims (Darzi, 2008; FDA, 2009). But findings from studies that use these measures raise questions about their ability to meet these expectations and thus the validity of these measures (Westerman et al., 2008). For instance, patients frequently rate their quality of life as stable in spite of deteriorating health and it is well known that patients often rate their quality of life as being better than their physicians or caregivers anticipate (Janse et al., 2004). Response shift, i.e. a change in one’s interpretation of quality of life or good health, is often blamed for these discrepancies. As a result there is much interest in how to ameliorate its effect.

In this paper, however, I propose that the different and changing interpretations that are at the heart of response shift are natural. To explain this phenomenon I turn to examine the *ethical* as opposed to psychological components of patients’ understandings of quality of life. One way to think about these ethical components is in terms of Charles Taylor’s distinction between weak and strong evaluations. Weak evaluations deem that something is good just insofar as it is desired; strong evaluations determine that something is good insofar as the desire itself is worthy. For the latter type of evaluation our choices are deemed worthy in terms of the quality of life they express relative to the life we want to lead (Taylor, 1985). I argue that patient responses in quality of life measures are often taken to be weak evaluations. Nevertheless, studies that listen to respondents as they fill out PROMs suggest that their answers are better

understood as strong evaluations.

This analysis of respondent answers has consequences for how we should use and understand PROMs. For instance, we should no longer understand them as a series of questions and answers that have only one correct meaning. Quality of life measurements are not determinate assessments of quality of life, but ought to be used as tools for enhancing communication about it. This suggestion has some similarities and some differences with current and leading work in response shift. I end my discussion with an exploration of some of synergies and departures of my proposal with other research.

S2. Interdisciplinary exchanges as the object of philosophical inquiry

Till Grüne-Yanoff (Organiser)
Collegium of Advanced Studies, Finland

Scientific disciplines exhibit varying practices or “styles” of modelling. However, it is often difficult to ascertain their differences, as the respective disciplines vary both in style and in substance. As a consequence, divergence between scientific disciplines is often explicated solely as variation in the substantial theoretical assumptions made. For example, the notion of ‘economic imperialism’ commonly focuses on substantial parts of economic theory, like rationality and efficiency, but neglects the particular style of economic modelling as a separate influence on other disciplines. This is where the study of interdisciplinary exchange acquires an important role. During such exchanges, the substantial aspects of different disciplines converge: scientists pursue a shared interest in theory or phenomena. But this shared interest is often impeded and shaped by differences in theorising practice. In order to facilitate interdisciplinary exchange, scientists have to both reflect on their own practices and objectives, as well as to negotiate the meaning and purpose of their practices with members of other disciplines. Thus, during exchange episodes, disciplinary practices become more observable than usual. Further, even when sharing an interest in substantial issues, scientists from disciplines often come away from interdisciplinary exchanges with different results. That is, the differences in theorising practices often significantly shape the exchange results. Thus, in the diverging consequences of exchange episodes, disciplinary practices become more observable than usual. Both the way in which facilitation takes place, as well as the results of these interdisciplinary exchanges reveal important insights into the disciplines’ respective theorising practices. This Colloquium will offer conceptual and methodological reflections on different interdisciplinary exchange episodes. The case studies investigate how scientists interrelate, what they import from other disciplines, how they incorporate it into their own disciplines, and how this affects their own practices. The objective of these case-based reflections is to highlight the differences and similarities between disciplinary practices of modelling, as revealed in the context of interdisciplinary exchange episodes.

Because this is a new field of philosophical research, it requires the development of novel conceptual and methodological tools. In particular, clarification is needed with respect to the dimensions along which interdisciplinary exchange takes place, and with regard to the notions of style and practice. This symposium will give an account how interdisciplinary exchange can be fruitfully studied, and it will establish the study of such exchanges as an important contribution to philosophy of science.

Biological markets and interdisciplinarity

Uskali Mäki
University of Helsinki, Finland

This is an exercise that has two goals, the general one of examining issues of interdisciplinarity, and the specific one of examining the uses of the notion of market in two disciplines, economics and biology. In their attempts to model and explain phenomena of the animal world, biologists have employed the concept of the market and associated notions such as supply and demand, trading, commodity, price, competition, investment, and so on. The very notion of ‘biological markets’ suggests that there must be some interesting relationship between the disciplines of biology and economics. The task is to explore the specifics of this relationship. It seems obvious it is not a matter of multi-disciplinarity or trans-disciplinarity, so the focus will be on varieties of inter-disciplinarity and cross-disciplinarity. In comparative terms, conceptual affinities between the two disciplines in their treatment of the market will be considered at different levels of

abstraction. Unsurprisingly, the similarities are greater at higher levels of abstraction. In terms of actual interaction between the two disciplines, the options include varieties of economics imperialism. This has to meet challenges such as that of accommodating the lack of interaction between the literatures on costly signalling in biology and economics.

Playing with networks: How economists explain

Caterina Marchionni
University of Helsinki, Finland

Network theory is applied across the natural and social sciences to model phenomena as diverse as the spread of contagious diseases, patterns of teenage pregnancy, and job search. Underlying the study of networks is the mathematical theory of graphs. Graphs are collections of nodes that are linked with each other via a discrete set of edges. Whether a graph represents a network of cells, friends or firms, it displays features that depend on the mathematical properties of the graph. A large body of empirical studies shows that real-world networks in different domains have common properties that can be captured by graph theory. On the other hand, the fields don't deal with networks in the same manner and such differences have more to do with the fields' characteristic modelling conventions than with their specific subject matters. This paper compares the way in which network theory is applied in economics with the way in which is applied in sociology and physics to bring out the modelling and explanatory conventions that distinctively characterize the economics variant of network theory.

Intertemporal discounting in psychology and economics

Till Grüne-Yanoff
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One of the most celebrated concepts of current behavioural economics is the notion of “time preferences”. Its fundamental idea is that economic agents pose a premium on enjoyment nearer in time over more remote enjoyment, and that this premium is analytically separable from their other evaluative dimensions. Time preferences are mathematically captured in a discount function. Discounting, of course, has a long history in neoclassical economics, but as an idealizing rationality assumption, not as a substantial assumption about people's psychological characteristics. Behavioural economics changed this, deriving explanations of people's behaviour, as well as prescriptions how to influence such behaviour, from specific forms of the discounting functions, specifically its “hyperbolic” form. This amounts to a fundamentally different interpretation of discounting — an interpretation that is derived not from economics, but from psychology. This paper examines how this interpretation was developed in psychology, tracing its roots to Mischel's Marshmallow Experiments, Herrnstein's Matching Law and Ainslie's research on impulse control. It then investigates how this interpretation was transferred into economics, and how formal models were adapted to match them to economic modelling practices. The result is an example how models were shaped through the conditions of interdisciplinary transfer.

S3. Operationalism in the life sciences

Pierre-Olivier Méthot (organiser)
Exeter University, UK

Commentary by Uljana Feest (Technische Universität Berlin, Germany)

Philosophers of science have traditionally been concerned with clarifying scientific concepts and to stripping them of their ambiguity. Nowadays, the tide has turned to some extent and while philosophers continue to value clarity they have also come to the conclusion that “concepts in flux” do not necessarily impede the development of the sciences but rather make it possible. This shift flows in part from the “practical turn” in philosophy and history of science. Famously, this change emphasized the need to analyse the experimental side of science, not only ideas or theories, to understand scientific activity. In this context, the concept of “operationalism” as proposed earlier by the physicist P.W. Bridgman in *The Logic of Modern Physics* (1927) emerges as a relevant approach to analyse scientific practice further, after it had lost currency among philosophers of science, partly because of its association to the work of the Logical Positivists and to the theory of verificationism (see Chang 2009).

An extreme view of operationalism in the life sciences is that all biological concepts should be defined in terms of actual biological operations, to paraphrase R. B. Lindsay (1937), an early critique of Bridgman. A more moderate version of operationalism acknowledges, though, that the formation of biological concepts is connected to experimentation and the development of experimental systems. As a consequence, instrumentation and measurements provide a privileged epistemic access to understanding concepts “in action”.

In this session, we will use the concept of operation as a guide for a historically-informed philosophy of science, following the suggestion by Hasok Chang (2009). Focussing on “doings” and “happenings” instead of “objects” and “entities” to use Bridgman’s own words, we argue that the concept of operation provides a more dynamic perspective on scientific practice. Importantly, our goal is not to propose a new philosophy of operationalism but to highlight that operational analysis broadly construed captures significant facets of scientific activity including explanation, measurements, and research programmes. Operational analysis was traditionally developed in physics and in psychology. In this session, we apply this approach to several areas of the life sciences by bringing together four papers with a focus on genetics, biochemistry, pathology, and developmental biology. The talks will be followed by a short commentary from Uljana Feest who has recently contributed in reviving “operationalism” (2010). The notion of “operationalism”, once stripped of its problematic theory of meaning, holds promises to explore several aspects of scientific practices.

The gene — A concept in flux

Staffan Müller-Wille
Exeter University, UK

There is a widespread view that operationalisation forms the last step in the evolution of a concept, settling disputes about a concepts meaning by providing a set of operations that allow to determine its reference. In my paper, I want to demonstrate that at least some concepts display a dynamic pattern of historical evolution that differs from this. Especially in the life sciences, operationalization of concepts like species or gene often leads to the detection of conflicting phenomena that can only be resolved by moving on to an alternative definition of the concept, which once operationalized in turn may again give rise to conflicting evidence. Borrowing a term coined by Yehuda Elkana, I will call such concepts “concepts in flux” and illustrate their curious productivity by looking at the history of the classical gene concept.

Operationalizing reactivity — The “purple membrane” from cellular structure to nanotechnology

Mathias Grote
Exeter University, UK

In this paper, I will analyze how experimentation with heterogeneous and complex materials has shaped scientific objects, such as macromolecules. These latter, but also the bulk natural product, were conceptualized operationally through their reactivity, that is, as “stuff doing something”. I will develop my argument along research on the “purple membrane”, a lipid-protein mixture that was isolated from the cell membranes of microbes around 1970. The term “purple membrane” soon referred both to the biochemical fraction and to a corresponding morphological structure of the microbes. The purple membrane was not only identified by its chemical composition, but especially by its reactivity, *i.e.* its ability to change colour upon illumination or to catalyze physicochemical processes in experimental assays. Research on the purple membrane shows how highly complex scientific concepts such as “photoreactive proteins” or a “molecular mechanisms” are tied to perceivable effects in the experimentation with reactive materials.

This notion of reactive matter lies also at the heart of projected nanotechnological uses of the purple membrane, which range from solar energy generation to optical data storage. In these technological projects, the “purple membrane” is not so much defined as a mixture of chemical compounds, but as a material undergoing specific transformations, which are used technologically. My account of purple membrane research from ca. 1970 to 2000 is not meant to discuss the validity of strict operationalism as a philosophical position. Rather, I will show that an operationalist perspective on science appears well suited to describe historical developments in the life sciences, chemical research and biotechnologies. An operationalist view appears to be fruitful to understand the emergence and transformation of concepts in connection with experimental work, but also for novel and changing uses of materials and translational processes resulting from such developments. Moreover, the case of the purple membrane allows sketching an operationalized understanding of science not only for an individual measurement, but for an entire research programme comprising materials, instruments and technological products.

Virulence: An operational concept?

Pierre-Olivier Méthot
Exeter University, UK

In my paper I will analyse the concept of “virulence” which is central to medical microbiology, evolutionary ecology, plant pathology, and medicine. Recently, historian of medicine Andrew Mendelsohn noted that the development of bacteriology was based not on a well articulated germ theory of disease but rather on the “intellectually empty, almost purely operational concept” of virulence (2002). Nowadays, scientists regularly call for the development of a unified nomenclature to clarify the concept of virulence and related terms like pathogenicity in order to facilitate communication and to avoid conflicting definitions across disciplines. Virulence and pathogenicity, however, are neither entities nor material objects in the conventional sense of the word; their “reality” cannot be directly assessed before some standards of comparing and measuring different levels of these characters are provided. Broadly speaking, the concept of virulence is often defined operationally as an indicator or an observable measure of disease severity. These measurements vary and include, for example, the assessment of host mortality, reduction in host fitness, tissue damage, and so on. Thus, success in providing a standard terminology for these concepts is hindered due to the fact that virulence and pathogenicity are operationalized in different and sometimes incompatible ways. Virulence remains an almost purely operational concept, but this does not mean that it is “intellectually empty”. In my talk, I do not aim for a “better” or “unifying” definition of virulence; on the contrary, I argue that

virulence is best understood as an elusive phenomenon whose meaning does not derive from a precise definition but rather through a series of concrete operationalizations. This “surplus”, to use Rheinberger’s terminology, characterizes the productivity of an “epistemic thing” such as virulence.

Operationalizing phenotypes in developmental genetics and contrastive explanation

Robert Meunier
University of Milan, Italy

The term “phenotype” was coined by Wilhelm Johannsen by derivation from “phenomenon”, which in philosophical terminology refers to the appearance of things, perceived through the senses. The phenotype is the observable parts or properties of an organism. But this observability does not imply that phenotypes are salient or in any sense obvious. Johannsen characterizes phenotypes as measurable, mainly because he needed to quantify them in order to arrive at average values (the “type”). This would require an operation of measurement, which is articulated precisely in order to guarantee reproducibility of, and comparison between experiments. The size of a seed can, for instance, be measured in many ways. But even if the phenotype is observed in qualitative terms, like Mendel’s wrinkled and smooth seeds, this observation is an operation. It has at its core an act of comparison between different specimen. It also often includes the application of various anatomical schemes, codified in anatomic atlases or models, to locate differences. Sometimes it also involves various instruments. Some are measuring devices, others are apparatuses that trigger a response in the organism, that cannot be described independently as, for instance, in behavioural studies.

In my presentation, I will focus on the practice of mutagenesis screens as performed in developmental genetics. In such a screen the genome of an experimental organism is mutagenized and phenotypic effects of single mutations are detected. Usually such a screen starts out with an interest in a particular anatomical structure or physiological function, say early embryonic patterning, the anatomy and function of the neuronal system, or behaviour. This interest predetermines the screening method. At which developmental stage will the animal be studied, what region of the body will be analysed and by what means? Phenotypes are made visible through measurement, staining or description with reference to a particular anatomical nomenclature. This often involves various imaging techniques. In this way the phenotype becomes operationally delineated.

I will show how categorization, in this case of parts or properties of organisms, is embedded in a context of presupposed categories as well as activities of delineation. This will provide a case study for a more general discussion of observation in science. At the core of each delineation is a contrast, and scientific activity can often be described as constructing such contrasts. Contrastive operations, so I argue, occupy a central place in scientific activity, because the explanations in which the categories figure can be analysed as having a contrastive structure. Contrasts are established on the basis of presupposed categories that are derived from other delineating practices (in this case anatomy). My analysis will help on the one hand to root explications of scientific explanation in the material practice of science, and on the other hand show how the operations of science can be analysed as supporting contrastive explanation.

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S4. Methodological pluralism

Alison Wylie (Organiser)
University of Washington, USA

In traditional philosophy of science the success of scientific research is presumed to depend on the existence of, and conformity to, a general scientific method applicable in all scientific domains. Many philosophers (e.g., Feyerabend, Kuhn, Laudan, Longino, Bechtel, contributors to the recent Minnesota collection, *Scientific Pluralism*), make a compelling case that research methodologies vary considerably between scientific fields and scientific communities. Indeed, they suggest that the presupposition that there must be a univocal method may not only be mistaken but also counterproductive, philosophically and methodologically. The contributors to this panel present three cases, drawn from the physical, biomedical, and social/historical sciences, in which progress in scientific inquiry depends upon on a robust methodological pluralism.

Chemical atomism: Progress through pluralism

Hasok Chang
University of Cambridge, UK

For 50 years after Dalton's publication of the atomic theory, there was no consensus on atomic weights and molecular formulas. For example, Dalton himself took water as HO, and Avogadro's formula H₂O failed to command wide agreement. The uncertainty came from an inherent circularity between atomic weights and molecular formulas, each being ascertainable only with the help of the other. This is an instance of the classic problem of the underdetermination of theory by evidence, but the traditional philosophical framework is inadequate for its full understanding

Early atomic chemistry had not merely a set of competing theories, but competing systems of practice based on different operationalizations of "atom". I identify five systems. (1) The *weight-only system* (Wollaston, Thomson, Liebig, etc.) focused on deriving atomic weights from macroscopic combining weights, employing a pared-down ontology of atoms possessing only weights. This system mostly served the needs of analytical chemistry rather than explanations of chemical phenomena. (2) The *electrochemical dualistic system* (Davy, Berzelius, etc.), grounded in the practices of electrolysis using the Voltaic battery, understood chemical reactions as consequences of the attractions and repulsions of electrostatically charged atoms. (3) The *physical volume-weight system* (Avogadro, etc.) took both weights and volumes as measurable atomic properties, with a focus on finding out the real properties of atoms and molecules. Many in this system took it for granted that equal volumes of all kinds of gases contained an equal number of molecules; Avogadro defend this assumption, whatever the consequences (including the H₂O formula for water). (4) The *substitution-type system* took classification as its main aim and activity. Dumas led the way with his "type theory", which laid down the research program of classifying organic molecules into "types" defined by the structural templates of simple inorganic substances (e.g., water, ammonia). Type-reasoning had an impressive operational basis; each branch of a type formula could be replaced by another group of atoms, yielding a different yet related substance. (5) The *geometric-structural system*, strongly inspired by the crystallographic tradition, attempted to get directly at real molecular structures, through various means including the study of isomers and the optical properties of molecules. This was in contrast to early type-theory's denial that structural formulas represented the actual geometry of molecules.

In principle there were indefinitely many systems consistent with known observations, but in practice not even one of the actually available systems was perfect when other desiderata were considered. In this situation, progress was made by maintaining a plurality of systems, each

“zooming in” on what it could handle particularly well; different operationalizations allowed the probing of different aspects of phenomena, and different concepts highlighted different kinds of connections between phenomena. After much development, it was possible to “zoom out” to a synthesis of competing systems; by the 1860s consensus was reached on atomic weights and molecular formulas on the basis of the new concept of valency, pulling together systems (3), (4) and (5) above. However, this was only possible through the renunciation of certain aims. For example, explaining the mechanism of chemical bonding, which system (2) did best, was neglected in the new synthesis; it was maintained in the new field of physical chemistry (and only adequately addressed much later, in quantum chemistry). Thus the new consensus only opened a new pluralistic era in a different configuration. Overall, this case gives a powerful illustration of how pluralism can aid the progress of science in the face of underdetermination.

Evidence-based medicine and mechanistic reasoning in the case of cystic fibrosis

Miriam Solomon
Temple University, USA

Evidence-based medicine and mechanistic reasoning are two distinct and powerful research methodologies. Enthusiasts of one sometimes take a dim view of the other. For example, evidence-based medicine is founded on skepticism about “pathophysiological rationale” (a broad category that includes mechanistic reasoning) and those who are knowledgeable about basic mechanisms are often unimpressed by the “empiric” results of evidence-based medicine. In this paper I plan to illustrate the importance of *both* methodologies and argue that they are not in competition with one another most of the time. I use cystic fibrosis as a case study.

In the 1950s, children born with cystic fibrosis (CF) rarely survived long enough to enter first grade. Today the mean life expectancy is almost forty years old. The increased lifespan has come incrementally, as antibiotics, airway clearance techniques, pancreatic enzymes, bronchodilators, ibuprofen and mucus thinners (Pulmozyme and hypertonic saline) were added as standards of care. The Cystic Fibrosis Foundation (begun 1955) has funded and coordinated many of the clinical studies, and uses the results to produce evidence-based guidelines for care in the 100 or so Cystic Fibrosis Care Centers. The steady progress and general consensus on proper care is impressive. In a 2004 article Atul Gawande writes that “CF care works the way we want all of medicine to work,” and especially praised it for being “system based.” This high praise is for interventions that are not technologically or intellectually sophisticated. It is an epistemic irony in CF research that our most precise evidence is about our crudest interventions.

The gene for cystic fibrosis was discovered in 1989, and for a period during the 1990s researchers thought that they were on the cusp of producing effective gene therapy for cystic fibrosis (Lindee forthcoming). Although a mouse model of CF was cured with gene therapy, clinical trials failed. Over the last ten years, we have learned much more about the mechanisms underlying CF. They turn out to be much more complex than anticipated, as well as more variable from person to person. Understanding of the role of the CFTR protein has led to attempts to fix the misfolding of the protein that is coded in the CF gene. The NEJM recently reported successful stage 2 clinical trials for a substance, VX770, that can correct one type of misfolding. Stage 3 trials are in progress. So, for the first time, we have RCTs for more sophisticated interventions, making use of our knowledge of CF genetics and genomics.

Mechanistic reasoning is highly fallible, perhaps because it generally provides a simplified or partial model of the world. Yet mechanistic reasoning is indispensable — we would have few ideas about how to design RCTs without mechanistic hypotheses about how to intervene in the disease process. Evidence-based medicine and mechanistic reasoning do not in general compete in the case of cystic fibrosis. Rather, they operate at different stages of the research

process, with mechanistic reasoning in the earlier stages of discovery and evidence-based medicine typically in the later stages of developing interventional success.

A plurality of pluralisms: Collaborative practice in archaeology

Alison Wylie
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In his attack on relativism and constructionism, *Fear of Knowledge*, Boghossian (2006) takes as his point of departure a 1996 *New York Times* news story about a conflict between Native Americans and archaeologists in which indigenous beliefs about tribal origins, rooted in oral history, are pitted against conclusions drawn from scientific investigation of the archaeological record. The archaeologists in the story are described as capitulating to a misguided postmodern relativism; they are in the grip of what Boghossian calls the “doctrine of equal validity.” Boghossian’s mission is to counter this doctrine in all its forms and, in the process, he rejects epistemic pluralism as irredeemably incoherent.

My interest is not so much in Boghossian’s position, but in what it obscures. It is a particularly stark example of the “anxious nightmare,” as Richardson describes it, by which any divergence of epistemic norms, any weakening of commitment to epistemic foundationalism, is presumed to entail radical incommensurability, the threat of mutual incomprehensibility (2006: 9). On Richardson’s analysis this anxiety is largely self-inflicted, an artifact of the terms in which philosophers have theorized knowledge. If we take as our starting point the practices by which divergent norms and belief systems are negotiated in the context of an active research program, the epistemic picture that emerges is not only more nuanced and complicated but also less liable to the threat of relativism that is the stuff of philosophical nightmares. I focus here on examples of collaboration between archaeologists and descendant communities that are epistemically productive in ways that are systematically obscured by the sharply drawn conflicts retailed in headline news and in *Fear of Knowledge*.

What impels archaeologists to take the interests and perspectives of Native Americans seriously are, in the first instance, moral and political demands for respect, reciprocity, consultation. But increasingly these give rise to collaborations that are also robustly epistemic; descendant communities and archaeologists jointly define the research agenda and pursue programs of historical, archaeological inquiry together, sometimes bringing radically divergent methodologies to bear on questions of common concern. While critics decry the epistemic compromises they believe such partnerships entail, the archaeologists engaged in them often make the case that their research is substantially improved by them. I focus here on cases in which discerning uses of oral history are juxtaposed with the material science methodologies typical of scientific archaeology, oral traditions being the very paradigm, for many, of an unstable, situated, non- or even anti-scientific mode of understanding. Time and again oral histories prove to embody collective knowledge and expertise that corroborates, extends, and sometimes critically challenges the settled conclusions of conventional archaeological inquiry. I have two aims here: first, to illustrate how diverse forms of expertise can enhance the epistemic integrity of systematic empirical research; and, second, to delineate a spectrum of forms of epistemic pluralism bounded at the extremes by philosophical ideals and nightmares.

S5. The social organization of research and the flow of scientific information

Justin Biddle¹ and Rebecca Kukla² (Organisers)

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

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

The first paper, by Justin Biddle, examines the way in which intellectual property rights affect the flow of scientific information. One of the primary justifications of granting intellectual property rights to items of upstream research (such as patents on genes and gene fragments) is that it purportedly incentivizes research that will lead to innovations downstream. Biddle argues, however, that current intellectual property policies are actually inhibiting the flow of information from upstream to downstream and hence are discouraging downstream innovation. The second paper, by Rebecca Kukla and Bryce Huebner, examines the implications of different models of collaborative research for authorial accountability. They emphasize that the epistemic labor in much contemporary research is highly distributed and decentralized, and they argue that often, particularly in multisite clinical trials, research is organized in such a way that there is no individual or group that is accountable for the results. They examine an alternative model, instantiated by the European Organization for Nuclear Research (CERN), that is decentralized yet nonetheless arguably yields a genuine collective knower. In the third paper, Torsten Wilholt argues that methodological standards — including standards that balance the inductive risks of false positives and false negatives, and the aims of power and reliability — are conventional and hence irreducibly social. Yet the ways in which scientific data are recorded, interpreted, and then synthesized into a communicable result are determined by these methodological standards. Hence researchers' employment of and departures from these social conventions shapes the results of research. Our social practices and policies governing intellectual property, collaboration, and methodological standards thus affect the flow and coordination of information within and between research projects, in ways that have epistemological consequences that cannot be reduced to the promotion or obstruction of transparency.

Intellectual property and the sharing of scientific information

Justin Biddle

Georgia Institute of Technology, USA

One of the primary justifications for granting intellectual property rights to the results of upstream scientific research is that it purportedly incentivizes innovative research. In a much-discussed essay in the journal *Science*, however, Michael Heller and Rebecca Eisenberg argue

that the proliferation of patenting and licensing in biomedical research is having the opposite effect; it is both epistemically and socially detrimental because it inhibits the sharing of scientific information (Heller and Eisenberg 1998). More specifically, they argue that as the number of patents and licenses on upstream (or more fundamental) research increases, people who wish to turn this upstream research into products downstream face growing obstacles, especially in the form of higher transaction costs, to the point that they will face lengthy delays or, in the most extreme cases, abandon their projects altogether. The result, they argue, is a “tragedy of the anticommons,” and they suggest that information would flow more efficiently from upstream to downstream if the results of biomedical research were to remain in the public domain.

One of the main arguments provided by Heller and Eisenberg concerns the patenting of concurrent gene fragments. The United States Patent and Trademark Office (PTO) allows patents not only on genes but also on gene fragments. As a result, those who wish to develop, for example, a diagnostic test for a genetic disease that would test for a constellation of gene fragments often find it excessively complicated and/or prohibitively expensive to acquire the rights to do so.

The tragedy of the anticommons paper has generated much discussion, and there are many who argue that the worries expressed by Heller and Eisenberg are highly exaggerated. This paper examines this debate. There are two main conclusions of the paper. First, I examine the studies that purport to falsify the anticommons thesis, and I argue that they do not falsify the thesis because they do not really test it. More specifically, while these studies do test the hypothesis that the proliferation of patenting and licensing inhibits the sharing of information upstream, they do not test the actual anticommons hypothesis — which is, again, that the proliferation of patenting and licensing upstream inhibits the development of products downstream. Secondly, I argue that there are strong grounds for believing that the anticommons hypothesis is true.

References:

Heller, Michael A. and Rebecca S. Eisenberg (1998), “Can Patents Deter Innovation? The Anticommons in Biomedical Research,” *Science* 280: 698-701.

Making an author: Epistemic accountability and distributed responsibility

Bryce Huebner and Rebecca Kukla
Georgetown University, USA

While it is widely acknowledged that the social organization of research critically influences what and how we know, questions at the interface between the social organization of research and the conditions for the possibility of authorial accountability have yet to receive the critical scrutiny that they deserve. Contemporary scientific research is often radically collaborative: studies are often carried out at multiple sites, with multiple forms of disciplinary expertise, and epistemic labor is often widely distributed and delegated. In many fields, publications routinely have dozens or even hundreds of ‘authors’. To yield genuine scientific *knowledge*, such radical collaborations must satisfy at least two conditions: 1) the *flow* of information from distributed sources must facilitate the *coordination* of epistemic resources to yield an integrated representation of what has been shown; and, 2) someone (or perhaps someones) must be *epistemically accountable* for reported results. These conditions must be met in spite of the fact that individual researchers do not just generate and share neutral data, but are interested decision-makers who necessarily exercise contextually situated and normatively inflected judgments.

Scientific research has long involved multiple parties who collect and process information as *isolated individuals*; and collaborators have typically been seen as *sources* of information to

which other collaborators would not otherwise have access. In such traditional collaborations, it is typically clear which party is responsible for which range of information. In the simplest case, collaboration is hierarchically structured, and a single individual is responsible for orchestrating diverse sources of information to produce a single authoritative representation (e.g. in the case of the knowledge that led to the discovery of Uranium-235 at the Oak Ridge National Laboratory). But much contemporary research *distributes* rather than *centralizes* authorial responsibility. As collaborative research becomes increasingly decentralized, relying on more investigators with diverse kinds of expertise, it has become increasingly unclear how the organization and coordination of information can enable a collaboration to function as a unified locus of knowledge. This poses a deep difficulty for our understanding of what, or whom, can count as an authoritative knower. We examine several models of collaborative organization and coordination. We argue that many multisite clinical trials rely upon an organizational structure that fails to produce a collective knower with authoritative knowledge. However, we also suggest that there are alternative forms of distributed organization that are more likely to produce collaborative research groups that can be held epistemically accountable for their claims to knowledge. Specifically, we suggest that the production, consumption, integration, and reporting of research projects as it is carried out by the CERN (the European Organization for Nuclear Research) provides a promising model for understanding what it would take for a decentralized and radical form of collaboration to yield a genuine collective knower.

Methodological standards, research communities, and the social diffusion of trustworthiness

Torsten Wilholt
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When we invest epistemic trust in results of scientific research, our trust is often directed at a collective body rather than at a single researcher. I focus on a very large unit of the social organization of scientific research and suggest that our epistemic trust is at least in part also directed at entire research communities that are defined by shared methodological standards. Examples of such standards include certain standards for data interpretation and for experimental design, but also established practices of communication and publication of results. As long as we regard methodological standards only as means of codifying and putting on record the procedures that are most suitable for arriving at reliable results, this sort of trust in research communities might appear to be merely a practical contingency. After all, collaborative groups or even individual researchers might be regarded as “in principle” individually responsible for finding out which methods are the most reliable ones by themselves. However, I argue that important methodological standards are in many cases solutions to problems of coordination rather than optimization. They are thus conventional and irreducibly social in character. Concrete examples of conventional standards in different areas of biomedical research serve to illustrate this point.

Ultimately, the reason for the conventional character of the methodological standards at issue is that the aim of arriving at reliable results underdetermines most kinds of methodological choices. In many cases, high reliability would be almost trivial to achieve in the absence of constraints on the *power* of the respective method of inquiry, i.e., the rate at which it churns out definitive results. Methodological decisions thus typically involve a trade-off between the aims of power and reliability. On top of this, a particular methodological choice also often implies a particular compromise between the risk of arriving at a false positive result and the risk of getting a false negative one. If every researcher determined for herself what the right balance between the power of the investigation, the reliability of positive results and the reliability of negative results should be, epistemic trust *within* a research community would be extremely difficult to maintain. Some methodological standards therefore serve to constrain these kinds of trade-off and thereby to conventionally establish what counts as the right balance within the

respective research community. Conventional standards shape the flow of information all the way from data collection to published research papers.

In placing our trust in the results of science, we are therefore also relying on entire research communities to employ conventions that reflect an appropriate balance between the aims of reliability and power as well as between the different kinds of error probabilities. This raises important questions about the often haphazard ways by which conventional standards come about — from the proliferation of certain long-entrenched practices by means of professional training to explicit recommendations of peer review panels to unilateral decisions by editors of influential journals to the prevalence of particular statistics software packages.

S6. Representative practices

Chiara Ambrosio (Organiser)
University College London, UK

In this symposium we will explore various ways in which historical and pragmatic considerations should inform philosophical accounts of representations. In doing so, we argue for the necessity of a reassessment of the issue of representation in light of the contemporary revival of philosophical interest in scientific practice. The immediate connection between our four contributions is a common emphasis on how representations function first and foremost as experimental practices and as guidelines to practical judgements. At the same time, however, the panel's scope has been left intentionally broad, to account for the diverse functions of representative practices considered as key components of scientific inquiry. Drawing on historical and contemporary case-studies, our four contributions will present a range of arguments that will place representative practices at the centre of current epistemological and historiographical debates on the dynamics of scientific observation and visualization, the quest for objectivity and the evaluative practices that implicitly inform the construction and use of representations.

A naturalist's visualizations: Carl Linnaeus (1707–1778) and his representative practices

Isabelle Charmantier
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The role of illustrations in the work of the Swedish naturalist Carl Linnaeus (1707-1778) have been discussed at length by historians: Linnaeus is said to have shunned the process of illustrating and to have believed in the primacy of a type specimen and the literary description of a plant. In the preface to *Genera Plantarum* (1737), he wrote: 'I absolutely reject [drawings], although I confess that they are of great importance to boys and those who have more brainpan than brain.' Botanists, art historians and historians have long debated Linnaeus's capacities as a draughtsman; while some of his detailed sketches of plants or insects reveal a capacity for two-dimensional drawing and a sure hand, his more general drawings of landscape and people seem childish. The overwhelming consensus, based mostly on his Lapland diary (1732), is that Linnaeus could not draw and that his botany is centered around text because of his bad drawing skills. Little has been said, however, on the role of drawing in Linnaeus's daily work as seen in his other numerous manuscripts. These, kept at the Linnean Society of London, provide an excellent opportunity to reinvestigate the matter. His interest in classification was defined by the relationship of groups of plants and animals, a context in which visual representation is unnecessary. Yet Linnaeus's manuscripts are peppered with little sketches, maps, diagrams, tables and lists which all help to represent succinctly the information on the page. Linnaeus's drawings are just one aspect of his broader representative practices, all of which highly depend on their visual qualities. This paper aims to reassess Linnaeus's representative practices in his working method, and to examine how they in turn influenced his scientific practices and ideas. Both Linnaeus's sexual and natural systems were dependent on and derived from these practices. Such a concrete historical case, which focuses on a single but highly influential naturalist of the eighteenth century, is extremely relevant to the broader philosophy of scientific practice.

The aims of representative practices: Symmetry as a case-study

Silvia De Bianchi
University College London, UK

This paper explores the role played by the conception of symmetry in representative practices from a philosophical and epistemological perspective. The main aspect on which the paper focuses on is the method that Weyl advanced in his masterpiece, *Symmetry*. He proposed to refer geometrical symmetry (bilateral and rotational symmetries) to certain operations that can be detected in both scientific and artistic representative practices: “Symmetry, as wide or as narrow as you may define its meaning, is one idea by which man through the ages has tried to comprehend and create order, beauty and perfection” (Weyl, *Symmetry* 1952, p. 5). By illustrating the history of this concept, it is possible to investigate a wide range of representative practices in different sciences. In dealing with the assessment of Weyl’s approach, it is possible to specify the purposes and the aims underlying the choice of representing organic and inorganic processes by means of certain symmetries.

The paper proceeds as follows. In the first part I introduce the reasons why our conception of representative practices should consider the aims and the objectives towards which it directs its interest. Secondly, I use Weyl’s conception of symmetry as an interesting case study, in order to show that philosophy can find fruitful pathways of interaction with sciences, if it deals with the practical implications of the employment of symmetry in representing phenomena. I will deal in the third part of the paper with two examples related to the use of symmetry in representative practices in biology and crystallography. I shall conclude with some remarks on the implications that this approach involves in epistemology, especially on our conception of objectivity.

Representation and validation in cardiac modelling

Annamaria Carusi
University of Oxford, UK

Biomedical modelling and simulation of physiological systems has rapidly developed over the last decades. It is now a mature discipline and, as stated in the Virtual Physiological Human website, it is expected that “it will improve our ability to predict, diagnose and treat disease, and have a dramatic impact on the future of healthcare, the pharmaceutical and medical device industries”. Mathematical and anatomical models and tools (such as simulation software) have been produced, stored in repositories and used in simulation studies to help understanding structure and function of heart, lung, liver, brain, pancreas, bone and kidney, amongst other subsystems. However, the advanced state of development of the VPH research programme raises important questions regarding what the models represent, how to validate them and what they are useful for.

This paper reports on research conducted by myself as embedded philosopher in a cardiac modelling team. As such, it is as much a reflection on that process itself, as well as reporting on and analysing the results of that process. Cardiac modelling is an important sub-domain of the VPH enterprise, with one of the longest histories dating back to 1960, following from the Hodgkin-Huxley model of reaction potentials in neurons in 1952. Focusing on the issue of validation, the paper argues that what validation might consist in is closely related to the conception of the nature of models. While there is a prevailing explicit definition of models as ‘simplified representations of reality’ in the domain, the meaning of ‘representation’ is ambiguous, shifts across different members of the group, or is contested by other members. In addition, in a domain which is saturated with visual artefacts, ranging over graphs, charts, diagrams, images produced by a variety of biological imaging techniques, and the computational visualisations that are used to render the output of simulations, the term ‘representation’ can have several layers of meaning. The interpretation of these visual artefacts depends on the scientists’ understanding of how they are related to the content depicted, and importantly, on what counts as a ‘good’, ‘successful’ or ‘accurate’ image. However, the understanding of a mathematical model as a representation is — or, on the surface, should be — discontinuous with the understanding of visual artefacts as being representational at least in the sense that it does not rely on depictive or visual success. Yet, so far in the development of

this research programme, it is not clear that in practice it is substantially different, since the visualisations — in their qualitative visual aspect — play a crucial role in the mediation between simulation and experiment, for example, in producing comparability between them. What, then, are the possible routes for the development of criteria for the validation of these models? The paper proposes that one possibility is to bring out the implicitly evaluative dimension of the conceptualisation of models as representations, and to exploit this for an understanding of validation in the domain.

From “representations” to “representative practices”: Lessons from visual history

Chiara Ambrosio
University College London, UK

One of the most disappointing — and at times frustrating — drawbacks of the recent philosophical debate on the nature of scientific representations is its lack of engagement with visual histories of science and their connections with the visual arts. Yet, even the most cursory glance at scientific atlases or scientists’ notebooks suggests that scientific experimentation hinges on a variety of visual practices, which often challenge the boundaries between scientific and artistic visualization. This historical material discloses entirely new possibilities to reassess the role of representations in scientific inquiry. In this paper I explicitly invite philosophers to engage with the experimental character of visual practices and reconsider representations as dynamic constituents of scientific inquiry. Rather than a normative quest for the formal constituents of representations (a quest that seems to occupy a central place in the philosopher’s agenda nowadays), I propose a pragmatic evaluation of the means and strategies through which practitioners devise useful and perspicuous ways of exploring natural phenomena and intervening upon them. With this aim in mind, I suggest to shift the focus of philosophical inquiry from a concept of “representation” to a historically grounded, pragmatic view of “representative practices”. My argument draws on Charles S. Peirce’s pragmatist account of representations, and in particular on an interpretation of his controversial formulation of iconic signs.

The structure of my paper will reflect the necessity of reconciling philosophy, visual history and actual practice toward an integrated account of representative practices. In the first part, I will use the early history of photography across scientific and artistic experimentation as a case study to explore how “correct” or “accurate” ways of representing within particular communities result from complex interactions and negotiations that exceed the boundaries of science itself. In the second part of the paper, I will present a more general argument in support of a view of representations as experimental practices, which will serve as a commentary drawing explicit connections between the four contributions to this symposium. I will conclude with some suggestions on how the concept of representative practices emerging from this symposium is complementary to the emphasis on “epistemic activities” that characterises Hasok Chang’s formulation of “systems of practice” (Chang, forthcoming, *Is Water H₂O? Evidence, Pluralism and Realism*. Boston Studies in the Philosophy of Science, Dordrecht: Springer), and on how it could more generally further the aims and goals of a philosophical inquiry into scientific practice.

S7. Forms of iterativity in scientific practice

Maureen A. O'Malley¹, Kevin C. Elliott² and Orkun S. Soyer³ (Organisers)

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2) University of South Carolina, USA

3) University of Exeter, UK

Commentary by Hasok Chang (University of Cambridge, UK)

A number of philosophers of science have called attention to iteration in scientific practice in order to explain how knowledge advances are made even when they are initiated by false models (e.g., Bill Wimsatt, Thomas Nickles, Hasok Chang). In this session, we will address the notion of iterativity via three discussions and a commentary.

Kevin Elliott will set out the differences and similarities between what Hasok Chang has called epistemic iterativity and what many life scientists have called methodological iterativity. Using examples from the field of nanotoxicology, Kevin will argue that the relationship between the two is of considerable importance for both understanding scientific practice and making advances in knowledge.

Maureen O'Malley will focus on methodological iterativity as it occurs in contemporary systems and synthetic biology, specifically the new field of noise biology. Her examination of how the integration of different research strategies has produced entirely new understandings of biological phenomena will suggest that integration and its dynamics need to be understood rather than iterativity per se.

Orkun Soyer will take a broad view of the topic. Using the analogies of hunters and gatherers to describe exploratory and hypothesis-driven scientific practice, Orkun will outline how the hunter-gatherer modes of practice work, what iterativity and integration mean within each mode, and how iterativity and integration might function across and between the two modes. His examples will be drawn from systems biology, some of it his own research.

Hasok Chang, whose discussions of epistemic iteration are central to this session, will comment on these presentations in relation to his formulation of iterativity. He will conclude with suggestions for how philosophers might examine it further in a range of research programmes.

Epistemic and methodological iterativity in nanoscale science and technology

Kevin C. Elliott

University of South Carolina, USA

Numerous historians and philosophers have noted that science progresses in an iterative fashion. This paper emphasizes that there are at least two aspects to this iterativity —epistemic and methodological. Using a case study associated with contemporary research in nanoscale science and technology, the paper explores the relationships between these forms of iterativity in scientific practice.

The first section clarifies what the paper means when it refers to epistemic and methodological iterativity. Epistemic iterativity refers to a self-corrective process by which tentative scientific models or theories provide both a starting point and a source of guidance for developing improved models or theories. According to Hasok Chang, 'we throw very imperfect ingredients together and manufacture something just a bit less imperfect.' This process frequently occurs in response to errors, anomalies, or other surprising phenomena that spur scientists to improve their claims in response to these unexpected results. Methodological iterativity can contribute to this sort of self-corrective epistemic process, but it need not always do so; it refers to the process of moving back and forth between various research modalities in the course of scientific investigations. For example, Dick Burian, Kevin Elliott, Chris Haufe, and Maureen O'Malley

have recently argued for paying more attention to the ways in which researchers move between hypothesis-driven investigations and more exploratory, question-driven, or technology oriented forms of practice.

The paper's second section shows how recent research on the toxicity of carbon nanotubes illustrates both forms of iterativity. Epistemic iterativity is present in this case, insofar as scientists encountered several forms of unexpected results that spurred them to revise and improve their initial claims. In particular, they faced two major problems: (1) nanoparticles appeared to interfere with at least some of the assays used for measuring toxicity, thereby invalidating at least some previous toxicity studies; and (2) some evidence suggested that nanoparticles could exert toxicity through novel, indirect pathways. Methodological iterativity is also present, insofar as the scientists involved in this case could not easily resolve these difficulties just by proposing and testing specific hypotheses. They also had to engage in more 'exploratory' forms of research that involved varying numerous experimental parameters in order to better characterize the phenomena that they were studying and to zero in on promising hypotheses.

The paper's final section attempts to develop some lessons about the relationships between epistemic and methodological iterativity based on this case study. It suggests that both forms of iterativity are likely to be observed in areas of innovative research. Methodological iterativity provides a richness of experimental practice that enables scientists to identify and characterize novel and unexpected phenomena in a way that moves the research enterprise forward. Hans-Jorg Rheinberger describes this process eloquently: 'the productivity of a complex research endeavor depends on its capacity for orchestrating a polyphonic texture of experimental operations within which the contingent, the unthought-of, the unprecedented can take on meaning.'

The dynamics of scientific practice: Integration and iterativity in molecular life sciences

Maureen A. O'Malley
University of Sydney, Australia

Two aspects of everyday practice that are only now gaining explicit attention in scientific and philosophical literatures are integration and iterativity. Each term has been used in rather different ways in contemporary molecular life science and philosophy of science. I will first tease apart some of these differences and then, using examples from systems-biological research, will show how integration and iterativity work separately and together, and why more insight into each will be useful for philosophy of science and scientific practice in general.

Many sorts of activities can be integrated in scientific practice. Even in traditional experimental science, integration occurs when one line of evidence is used to back up a hypothesis already supported by another line of evidence. But in this guise, integration is only sometimes seen as a problem that needs a solution — usually in the form of consilience or triangulation. This is what has changed in contemporary molecular biology, which is blessed (or cursed) with a superabundance of data and no easy way in which to make sense of it. In such situations, integration means the functional combination of different methods and/or datasets, such that these combinations produce insights that could not be produced by single methods or datasets. Philosophers nowadays continue to discuss integration rather differently, as Sandra Mitchell's book, *Unsimple Truths* (2009), demonstrates. She outlines 'integrative pluralism' as the means by which complex biological processes can be understood. This view of integration is an explanatory one, however, that does not cast light on how a range of approaches might be integrated. I will attempt to expand this discussion with examples of how systems biologists are trying to achieve integration, and what sorts of explanatory results these attempts have.

However, even with practice-focused insights into integration, it is still necessary to understand

what *drives* integration to combine approaches or different explanations of phenomena. This is where an understanding of iterativity might be useful. Hasok Chang, in *Inventing Temperature* (2004), introduced 'epistemic iteration', by which he means the way in which new understandings are produced at each stage of inquiry. This understanding of iterativity needs elaboration, however, because the relationship between new knowledge and the means of producing it is not addressed explicitly. It is not clear how 'self-correction' is achieved, and why it often isn't. In systems biology, iterativity is usually discussed methodologically, as the cyclic application of methods aimed at generating functional understanding from large-scale datasets. I will suggest that both methodological iterativity and epistemic iteration can be understood within a framework that emphasizes integration while paying attention to its dynamics.

To gain a better understand of these dynamics, I will explore the new field of noise biology. Through a combination of approaches and new technologies, noise biologists are making much more obvious the deep biological importance of stochastic fluctuations in molecular events in cells, development and evolution. Because these revelations have been produced only at the intersections of very different practices, they provide an excellent platform for the analysis of integration and its dynamics.

Hunter-gatherers in scientific practice

Orkun S. Soyer
University of Exeter, UK

Scientific practice is a complex human activity aimed at generating new knowledge about and understanding of the natural world. Here, I argue that a broad categorization could be a useful first step in describing this complex process. I use analogies to the hunters and gatherers of stone-age and other societies to introduce such a two-category classification.

Gatherers in stone-age societies are thought to have operated in a common environment, collecting food from natural resources that were well-known to them. This was not a static task and gatherers constantly had to extend their territory, sample new sources of food and optimize their yield. This process probably was at the origin of farming practices, which were one of the biggest developments in the history of humankind. Like gatherers, scientists in well-established research areas usually absorb a 'well-defined' picture of their field during their education. This picture represents only a small fraction of the field's unexplained phenomena and how they can be addressed to improve the knowledge in the field. While this closely bounded process can lead to breakthroughs that might reveal totally new sides of the picture, progress is usually incremental.

Hunters, however, operated in a larger area and spent a good fraction of their time in exploring in pursuit of big prey. They faced uncertain environmental conditions and had to improvise in unexpected situations, while overcoming the challenges of hunting. The individual and social skills required probably played a key role in the development of many human innovations. Like hunters, some scientists engage in exploratory practices that are not bound to the 'current picture'. These exploratory practices usually hinge on an unusual question, a new methodology, or simply random and even playful experimentation. This type of science has a high failure rate but sometimes leads to novel findings, or to insights that completely change well defined fields. Scientists involved in this mode of scientific inquiry are like the hunters and need similar skills and mindsets, such as openness to failure and false decisions, the will to explore, and the ability to improvise and take risks.

In this talk, I will give examples of hunters and gatherers from some groundbreaking research in systems biology, including the analysis of diverse networks and the subsequent discovery of network motifs and the analysis of bacterial chemotaxis. Although iterativity and integration are employed to differing degrees within such specific projects, I will highlight the different ways these processes are employed by hunters and gatherers, although seldom in bridging the two

modes. This might be because hunter and gatherer modes are linked to the educational backgrounds and personalities, which form additional boundaries to integration and iterativity occurring between or across modes. In summary, I will argue that both modes are indispensable for scientific progress and that better communication between the two would be beneficial.

S8. Philosophical and social perspectives on synthetic biology

Gabriele Gramelsberger¹ and Tarja Knuuttila²

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2) University of Helsinki, Finland

Synthetic biology is a relatively novel and highly interdisciplinary field, which combines in an intriguing way a basic science approach focusing on the exploration of the basic “design principles” of biological systems underlying biological functions such as temporal organization, and an application oriented approach seeking for a purposeful design of novel biological parts or systems. It is located at the interface between engineering, physics, biology, chemistry and mathematics, and its research practice combines methods, concepts, tools, models and theories from all of these disciplines. The impact of engineering is most visible in synthetic biology. It makes use of the ‘design approach’ characteristic of engineering referring to ‘bio bricks’, ‘minimal cell concepts’ and ‘chassis’ as standardized components in ‘real world modelling’ of new entities. In its aim to engineer novel biological systems, which do not exist in such a form in nature, synthetic biology goes beyond genetic engineering. The reprogramming of cells (via recombinant DNA) to produce certain products like insulin has already been successful since the 1970s. In the case of Jay Keasling’s anti-malaria drug artemisinin whole parts of the metabolic circuit are replaced/changed. The ultimate goal is to have a platform which can be changed by attaching different chassis so that one day medical drugs can be produced and the next day biofuels.

Apart from modularity, engineering notions such as robustness, noise and redundancy are central in both the exploration of biological design principles as well as in the design of novel biological systems. Interestingly, the basic science oriented branch of synthetic biology explores also the suitability of these notions to biology. Synthetic models provide a central tool for this task. They are engineered genetic circuits that are built from biological material and implemented on natural cell environment. As such they are located somewhere in-between mathematical models and model organisms. On the one hand, they are less complex than any model organism, on the other hand, mathematical models usually function as blueprints for the design of synthetic models. In the modeling practice of synthetic biology synthetic models are typically combined with other types of models: mathematical models and simulations, and experiments on model organisms. This combinatorial practice makes synthetic biology an especially interesting place to study contemporary modeling methods and strategies.

The symposium will explore philosophically and socially interesting aspects of both application oriented and basic science oriented branches of synthetic biology.

Synthetic biology as thing knowledge

Axel Gelfert

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Synthetic biology has developed as a (prospective) new biological discipline over the last dozen years or so. While there exist historical precursors in the work of those biologists who have studied systems at the threshold between organic chemistry and primitive life forms, it is only against the backdrop of the recent successes of molecular biology, DNA sequencing, bioinformatics, and genomics that a truly ‘synthetic’ biology has come to be regarded as within reach. By emphasizing ‘engineerability’ over the evolved character of living systems, synthetic biology places itself in opposition to ‘traditional’ biology. Nowhere is this contrast clearer than in attempts to develop homogeneous building blocks — or ‘biobricks’ — that conform to clear manufacturing protocols, and exhibit predictable behaviour in accordance with technical

specifications, which may be documented in advance in the form of ‘data-sheets’. Whereas traditional biology emphasizes the evolvability, variability, and heterogeneity of living organisms, synthetic biology envisions a future technoscience of homogeneous, artificially designed systems that may be combined in modular fashion. In the present paper, I argue that this development amounts to more than a shift in emphasis: it is revisionist of the character of (the dominant mode of) theoretical knowledge in biology itself. Other authors have suggested that the success of various synthetic biology projects — such as the construction of artificial life forms from standardized modules — is conditional on the hypothesis that *actual* life forms, too, are best explained in terms of their fundamentally modular nature, with natural selection being the main driver in the recombination and ‘fine-tuning’ of these different modules. However, rather than portraying synthetic biology as a theory-driven enterprise that depends for its success on the veracity of theoretical hypotheses about the modular nature of biological systems, I shall argue that synthetic biology represents a shift away from theoretical concerns with explanation, towards the instrumental value of manipulability. In making this case, I take my lead from recent discussions of ‘thing knowledge’ (Baird 2004) in the philosophy of scientific instrumentation.

‘Thing knowledge’ here refers to a form of ‘working’ knowledge (as demonstrated by the empirical success of various kinds of effective action), which need not be primarily representational in character (though it may well be, as in the case of material models such as mechanical orreries representing planetary motion) and which may be thought of as enshrined in the material features of the object itself. While it is easy to see how technical artifacts — such as measurement instruments — may be credited with such ‘thing knowledge’, living organisms would traditionally not have been viewed as potential repositories of knowledge (except, of course, in the case of higher mammals with complex cognitive apparatuses of their own). With the increasingly likely creation of synthetic life forms (such as *Mycoplasma laboratorium*), however, the contrast between technical artifacts and living systems is likely to be eroded, thus allowing for an extension of the notion of ‘thing knowledge’ to synthetic life forms. While it may be too early to tell whether this will bring about a wholesale revision of our understanding of biological knowledge, it seems fair to say that it poses a challenge to the traditional focus on representation and explanation in our scientific accounts of how biological systems work.

The simulation approach

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The basic concepts of synthetic biology draw on the idea of engineering based on simulation. The use of simulation refers on the one hand to design, on the other hand to synthesizing epistemic diverse elements. Therefore, each element has to be scaled down into parameterized processes, which can be translated into operational pieces. These pieces can then be combined and tied together. The possibility of synthetization that characterizes the epistemology of computer-based simulation results in the ability of building constantly growing models. The paper explores the role of simulation in synthetic biology in terms of synthesizing epistemic diversity.

‘Pixel by pixel’: Visual programming languages in synthetic biology

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Computer-aided design (CAD) tools offer a human-readable surface to visually program synthetic biological systems. Based on standardized diagrammatic languages these programs

should guide the work of biologists and provide a visual and operational ‘breach’ into digital data. The *engineering paradigm* as well as the *design paradigm* in synthetic biology demand effective visual and computational techniques to manage the complexity of biological systems and therefore the large amount of data which are connected to their construction and reconfiguration. The management of information to understand biological systems and to construct new ones is so far primarily achieved by *in silicio* design. *In vivo* or even *in vitro* applications of synthetic systems — not only components — are not yet conceivable. On an experimental level scientists are faced with problems of cell death, cellular noise, crosstalk and mutations when transferring cell structures from *in vitro* to *in vivo* environments, because “our biological knowledge and design capabilities are not yet at the level of sophistication needed for *a priori* design and production of a prototype with a fair shot at success.”

By visual programming languages it becomes conceptually possible to build up synthetic entities ‘pixel by pixel’ and to test their behavior. Hence, synthetic biologists need to design and test their constructions within an *in silicio* setting of computational models, software design tools and graphical notations. In order to standardize and generalize the design process and the (visual) communication among the scientific community adequate software solutions as well as diagrammatic notations are based on principles of modularity, abstraction and consistency.

The paper examines a community wide effort to introduce such a standardized visual programming language, namely the Systems Biology Graphical Notation (SBGN). This visual programming language offers a set of iconic components and connectors which should help researchers from different disciplines to describe, design and program pathways entities and activities within a cell or cellular systems. To convey and at the same time construct ‘invisible’ but machine-interpretable mathematical models into a human-readable form and to provide their legibility among a broad scientific community an interdisciplinary research team tries to develop this assumed “systematic and unambiguous graphical notation”.

Furthermore CAD tools like CellDesigner will be explored according to their epistemic functions in the research and design process. At the interface of computational modelling and the graphical diagrams of SBGN computer-aided design programs help the user to interact with data on a visual basis. This graphical user interface provides the conceptual framework to apply standardized graphical languages like SBGN to, for example, the design of metabolic pathways. By looking at the CAD interfaces and visual programming languages (e.g. SBGN) in synthetic biology, the paper wants to show how intertwined and complex these computational tools and visual languages are and how much they themselves are products of design processes and scientific negotiations.

Synthetic biology and the functional meaning of noise

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In synthetic biology the use of engineering metaphors to describe biological organisms and their behavior has become a common practice. A host of engineering notions and metaphors have both served as basic theoretical concepts of the field as well as vehicles for public understanding of synthetic biology. The concept of noise provides one of the most compelling examples of such transfer. But the notion of noise is also confusing: While in engineering noise is a destructive force perturbing artificial systems, in synthetic biology it has acquired a functional meaning. It has been found out that biological systems make use of noise in driving processes such as gene regulation, development, and evolution. What is the epistemic rationale of using the notion of noise in both of these opposite meanings? One philosophical answer to this question is provided by the idea of negative analogy. According to it not just the similarities but also the differences found out in analogical comparison between two fields can further

theoretical inquiry (e.g. Hesse 1966, Morgan 1997, Bailer-Jones 2009). But this is only part of the story. We will show how the notion of noise in the field of synthetic biology actually subsumes more heterogeneous interdisciplinary relations: Despite the engineering connotations of the concept of noise, the new functional meaning of it had already emerged in physics from where synthetic biologists have adopted the majority of their modeling methods.

Synthetic biologists often use the term noise to refer to stochastic fluctuations in gene expression (or other processes) caused by the low number of molecules in the cell. Such inherently random processes have been extensively studied in statistical mechanics, both conceptually and experimentally. The functional role for noise was already evident in the more general field of the study of complex systems, and in the analysis of neural networks, for example. Thus, studies on stochastic fluctuations in biological systems overlap with statistical mechanics and investigations of general complex systems. Methods and techniques developed in the aforementioned fields have been transferred to and used in synthetic biology. However, the application of these tools to biological systems is not unproblematic. The sources of the fluctuations in biological organisms are largely unknown in all but a few cases, as is their exact impact on the dynamics of the system. We will argue that one reason for the use of the notion of noise is exactly this uncertainty: noise functions both as an umbrella term and as a place holder for the emerging research on different forms of fluctuations, their sources and consequences for the dynamics of the biological systems. The case of the concept of noise also shows, we suggest, that concepts are often accompanied by specific modeling methods and formalisms. Yet they can undergo semantic transformations and subsume new kinds of research agendas employing novel modeling tools.

S9. Investigating practical impacts of descriptive categories

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Classifications of melanoma

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Finding a satisfactory way of classifying instances of malignant melanoma has become something of a sore point in current medical practice. A full taxonomy of classification-related difficulties is beyond this abstract, but by means of illustration, I suggest the following six aspects of the problem are of significant clinical importance: first, no single scheme of classification presents a really satisfactory means of classifying all cases for all purposes. Second, this difficulty has given rise to the development of a great many distinct schemes of classification. Third, the actual properties used to effect meaningful divisions are often themselves troublingly vague. Fourth, the evidence base for just how well these classifications function is sorely lacking — a critical failure given the rise and rise of evidence-based medicine (EBM). Fifth, the multiplicity of extant classifications has produced tangible terminological confusion in melanoma description. This has made the production of standard EBM-type descriptions of evidence, such as meta-analyses, increasingly problematic. Finally, deciding which classification should be employed tends to produce rather acrimonious and unhelpful arguments. In short, the classification of melanoma is a mess.

Given the conflict and confusion that has resulted from the use of so many different schemes, I begin this paper by making a rather counterintuitive suggestion. Rather than, say, outlining methods of standardising melanoma classification, I instead suggest that the resolution of this debate would be most efficiently achieved by adopting a pluralistic stance about melanoma classifications. In this paper, I'll attempt to defend this stance in a way that is primarily empirical. I'll make the case in the following way. I'll begin by outlining what I regard as the current state of melanoma classification. I will then give a brief statement of the problems outlined above. I'll then move on to examine a sub-class of melanoma classifications where a certain degree of pluralism is already practiced. These are the specialist schemes of classification - staging and grading — which seek to draw rather different boundaries between types of melanoma from those drawn in the classificatory main-stream. The existence — and fruitful employment - of such alternative schemes of classification seems to open a pluralist chink in the armour of the monistic mainstream for melanoma. As these (different) types of classification are already willingly accepted for the different light their use can shed on an instance of melanoma, does this not suggest too that a pluralist approach to classifying melanoma *tout court* is at least a viable and valuable alternative to the current monistic approach?

Classification and complementary science

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Initially described and illustrated in 1669 by Jan Swammerdam (Birge 1918), the freshwater crustaceans of the genus *Daphnia* have been and continue to be extensively studied by scientists, students, and naturalists alike. Commonly known as the water-flea, the ecological polyphenisms of *Daphnia* are frequently cited (Laland, Odling-Smee and Gilbert 2008, Piersma and Van Gils 2010). When hypoxic they turn red (due to an increase in hemoglobin), and in response to the chemical kairomone of their predator they develop a defensive helmet (in *D.*

magna), a “crown of thorns” (*D. atkinsoni*), and neck teeth (in *D. pulex*) (Hunter & Pyle 2004). Its sensitivity and responses to toxicity and environmental pollutants means that it is widely used in hydrobiology and limnology studies (Cerbin et al. 2010). Extensive data has been collected since the late 1700s (Birge 1918). And a draft genome of *D. pulex* has been available since 2005 (Colbourne et al. 2005). Despite this wealth of information, the genetic mechanisms involved in these polyphenisms remain a mystery and its classification and phylogeny remain unresolved (Edwards 1980, Schwenk, Ender, Streit 1995, Kotov & Taylor 2010).

Philosophers have long discussed and criticized the classificatory systems of biologists. In these, Ernst Mayr’s biological species concept continues to be a frequent target in discussions about the concept and category identified by the basal unit of classification, as is his argument that clonal/asexual lineages are not species at all (Mayr 1982: 283). While acknowledging the value of these ongoing debates, I pursue a different path of investigation.

I instead take seriously Hasok Chang’s suggestion that philosophy of science can provide a complementary function in its ability to generate scientific knowledge in the form of critical philosophical scrutiny through the testing of experiments recovered from history (Chang 2004, 2009). As a highly polyphenic asexually reproducing clone that is fairly easy to culture, *D. pulex* is an obvious choice for this kind of investigation. Exploring the role of complementary scientist with the kind help of colleagues in the biology and chemistry department, we are now in the process of developing a project to investigate how this approach will be used in recovering and extending some of the classic research on *Daphnia pulex*. We will explore new ways of manipulating the environment using new chemicals, (some of which will be structurally similar to kairomones), and maintaining different temperatures. We will then record any changes in morphology and behavior and use these to interrogate past classifications based on a variety of different polyphenisms.

The focus will be on neckteeth and other defensive mechanisms of *D. pulex* developed in response to chemicals structurally similar to kairomone using combined methods of investigation in biology, biochemistry and Changian complementary science. Concentrating on these ecological polyphenisms we will go on to compare them to phenotypic and genetic homologues (in *D. magna*). This study may provide new information on the evolution of the mechanisms that make these polyphenisms possible thereby serving as additional means for classification.

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Biomolecular classification

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This paper will examine the relationship between the structure and subsequent function in an assortment of biomolecules including proteins, polysaccharides and smaller molecules such as metabolites. I will argue that the microstructural malleability of biomolecules confers a functional advantage in the organism allowing different biomolecules to assemble in response to different selection pressures. Thus, constitutional and structural disorder confers functional promiscuity. I will address the question of whether these biomolecules ought to be individuated in terms of functional role, rather than microstructural constitution. The first part of this paper will examine the nature of this relationship and its implications for biomolecular classification.

The second part of this paper will examine the role of modelling in biomolecular classification. Recently, philosophers of science have acknowledged the use of models in the semantic view of theories. One of the most famous examples of a scientific model is the Gaussian chain model of a polymer. However, biomolecules are increasingly being classified by computer simulations that model their structures and functions (an example is quantum mechanical/molecular mechanical (QM/MM) methods used to study protein conformational changes, dynamics and binding). The role of modelling in biomolecular classification will be addressed with particular reference to the representations of structure and function in molecular modelling.

S10. Philosophy of psychiatry in practice: Steps towards an adequate theory of psychiatric classification

Psychiatric nosology provides a particularly fruitful subject for analysis from the perspective of philosophy of science in practice, insofar as the methods and aims of the classification of mental disorders are fundamentally constrained by the pragmatics of medical care. As an art and a science, wherein the line dividing pure and applied contexts is not well defined, psychiatry raises important philosophical questions concerning how to balance epistemic with practical aims. Accordingly for the philosopher, the normative aspects of psychiatry necessitate that formal methods from the philosophy of science be re-evaluated: To what extent is the traditional conception of 'value-free science' useful, even as an ideal, in psychiatry? How can insights from philosophy of medicine be used to help make philosophy of science more useful and engaged with scientific practices? How should nosological practice influence philosophical accounts of psychiatric kinds? To what extent should psychiatric projects (e.g., classification) be guided and constrained by practical goals (e.g., facilitating the treatment of patients)?

The intimate and dynamic relationship between pure and applied contexts of classification also raises interesting questions concerning the generation of psychiatric knowledge. What kinds of activities and practices are distinctive and crucial to the formulation of psychiatric categories? What is the significance of the various instruments and human artifacts (e.g., pharmacological drugs, diagnostic tests) that psychiatrists employ to gain access to and study various mental phenomena?

Each paper in this symposium seeks to give a philosophical account of the way psychiatric nosology functions in practice, and in so doing to revise traditional accounts of what constitutes a 'good' classification. Tsou's paper analyzes the manner in which pharmacological research contributes to our understanding of mental disorders, arguing that only through intervention can we develop the causal accounts necessary for the justification of diagnostic categories. Kutschenko argues that researchers and practitioners make different, even competing, demands on classification, illustrating how the project of developing a good classification of mental disorders is deeply intertwined with epistemic assumptions about the relationship between research and medical practice in psychiatry. Finally, Tabb offers a new philosophical account of the psychiatric kinds that nosologists seek to classify, which abandons some of the ambitions of traditional natural kind accounts in favor of the ability to capture the complex negotiations through which psychiatric diagnoses are in fact developed, modified, and abandoned. Tabb and Tsou are philosophers of science; Kutschenko is a bio-medical researcher and philosopher.

Intervention, causal reasoning, and the reality of entities in psychiatry: Pharmacological drugs as experimental instruments in neurobiological research on mental disorders

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Pharmacological research has played a central role in the development of neurobiological theories of psychopathology. Successful pharmacological interventions with mental disorders have often been discovered through fortuitous experimental results (e.g., the first antidepressant drug was discovered accidentally during attempts to find a treatment for tuberculosis), which subsequently shed important insights about the neurobiological basis of mental disorders, which in turn have allowed for refinements in pharmacological treatments. This paper examines the ways that experimental practices in pharmacology contribute to

neurobiological knowledge about mental disorders, the role of pharmacological drugs as artifacts in psychiatric knowledge generation, and how this experimental research contributes to our evidence for the reality of mental disorders.

The main argument of this paper is that intervention with psychiatric patients with pharmacological drugs provides us with evidence for the reality of mental disorders by giving us knowledge about the neurobiological causes of mental disorders. In supporting this argument, I draw upon Ian Hacking's (*Representing and Intervening*, 1983) discussion of experimental realism (or 'entity realism'), which maintains that intervening with and manipulating theoretical entities provides us with evidence for their real existence. Hacking's analysis emphasizes how scientists use knowledge about the causal powers of known entities (e.g., electrons) to create new technologies for experimenting with other more speculative entities (e.g., quarks). This informs Hacking's well-known motto that electrons are real because we can 'spray' them with electron guns. While mental disorders can clearly not be manipulated (or 'sprayed') in the same way as electrons, I suggest that pharmacological interventions with psychiatric patients approaches Hacking's ideal of experimental realism insofar knowledge about human physiology and neurotransmitters allow us to develop pharmacological drugs to gain knowledge about the causes of mental disorders. In this process, pharmacological drugs can be regarded as technologies or instruments that play a crucial role in both the discovery and justification of neurobiological theories of mental disorders. In articulating this view, I discuss the evolution of neurobiological theories of schizophrenia and depression. These two cases highlight the ways in which pharmacological research functions to reveal important causal regularities and properties of mental disorders. It is in this sense that I argue that causal reasoning provides us with cogent evidence for the reality of mental disorders. More generally, these cases illustrate the dynamic relationship between applied and pure contexts in psychiatry, which both contribute to the generation of psychiatric knowledge.

ICD vs. DSM: Two classifications in practice, two perspectives on psychiatry

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How should a good classification of mental disorders be? This paper argues that this question requires a use-dependent answer. Use-dependence refers to the respective aim of classifying (e.g., developing or applying treatments) as much as to the specificities of the context (e.g., of empirical research and of clinical practice). As a consequence, the evaluation of medical classification systems will be relative to epistemological assumptions about the nature of the unit of classification, namely mental disorders, and about the way in which research and practice are linked to each other. The two most important classification systems in psychiatry seem to involve quite different perspectives on these issues. By comparing the *International Statistical Classification of Diseases and Health-Related Problems (ICD)*, published by the World Health Organization, and the *Diagnostic and Statistical Manual of Mental Disorders (DSM)*, of the American Psychiatric Association, I will analyse the theoretical underpinnings of their different approaches to classifying mental disorders.

For example, the *ICD* offers separate manuals for clinical use and for research use. The latter aims to define more precise and sophisticated diagnostic criteria than the former. This dual approach acknowledges the different demands of practitioners, who require a classification based on easily identifiable surface phenomena, and of researchers, who need patient populations that are as homogeneous as possible. The current revision project of the *ICD* even envisages the introduction of a third manual in order to distinguish between primary care, speciality settings and research. Notably, while the different manuals refer to each other by using the same index, they explicitly allow for inconsistencies or gaps. The *DSM*, on the other

hand, pursues a unified approach to classification. Since the introduction of its third edition in 1980, detailed operationalised criteria have been used to enhance the reliability of psychiatric diagnoses. These are used across very different settings, including biomedical and clinical research. The current revision process will maintain the unity of the manual. It will introduce, however, a gradual shift from the classification of symptoms to that of biological correlates, such as biomarkers. The proposal to include early stage forms of disorders that require elaborate diagnostics in the *DSM-5* is a case in point.

Of course, the *DSM* and the *ICD* have a lot in common. By making their differences explicit, however, I hope to shine some light on the reasons for substantial disagreements within both revision projects. What is more, I will argue that the insistence on unity within the *DSM* has resulted in an unwarranted conceptual restriction that has endangered (the funding of) research that looks off the beaten track. The *ICD*'s diversified approach, on the other hand, might facilitate the interaction of different explanatory models without reducing all clinical observation to specific biological correlates. Yet, it might complicate the transmission of research results into practice. This is a crucial issue of philosophy of medicine. By drawing the attention to the differences between research contexts and medical practice, I will conclude that *ICD* is more appropriate than *DSM* with respect to how psychiatry works.

Psychiatry and the natural kinds debate: Let's get practical

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Recently psychiatrists as well as philosophers of psychiatry have concerned themselves with the question of whether or not mental diseases are natural kinds. I argue that the adoption of this term of art ushers into the philosophy of psychiatry certain expectations traditionally applied to scientific objects, which sidelines the status of psychiatric kinds as *medical* objects. I introduce a rather deflated kind concept that can better facilitate the philosophical project of describing the kinds as they are actually referred to by practitioners and researchers. The shift away from a traditional natural kind concept towards a more accurate concept of psychiatric kinds in practice is essential for a philosophical analysis of nosology.

I will begin by briefly demonstrating how psychiatric kinds fall short of being traditional natural kinds, arguing that, like many kinds in the life sciences, they do not display necessary and sufficient membership conditions. More problematic for the traditional account, however, is that the decision to recognize a certain cluster of properties as pathological is fundamentally normative. Finally, there is a fuzziness of intension as well as extension in the use of psychiatric kind terms — different psychiatric professionals, such as social workers, researchers, and clinicians, may use different descriptions to identify members of the same kind.

I argue that another kind of kind, a property-cluster kind, can best capture these complexities. Under this account psychiatric kinds are seen as clusters of symptoms that are deemed to be pathological and demanding intervention. However, I suggest that to actually capture the way psychiatric kinds are established and maintained, philosophers must view them as more than the criteria lists offered by diagnostic manuals. Using Boyd's homeostatic property cluster (HPC) kind concept, I argue that psychiatric kinds have underlying causal pathways that can explain their co-occurrence. Insofar as the methods used in the laboratory and the clinic to investigate these pathways are often heterogeneous, contested, and complex, few psychiatric kinds have well-established etiologies. Rather, their utility is the result of negotiations that integrate those property clusters (that is, syndromes) that demand medical attention with the homeostatic mechanisms (that is, causal pathways) theorized by researchers.

I use Major Depressive Disorder (MDD) as a case study, and focus in particular on its ambiguous relationship with General Anxiety Disorder. I demonstrate how the HPC kind account can be used to describe the struggles of researchers and clinicians to establish MDD

as valid and robust object of inquiry, rather than to justify claims about the naturalness or “reality” of the diagnostic category post hoc (as traditional natural kind accounts might). I conclude that there is no easy — and certainly no categorical — answer as to whether psychiatric kind terms pick out discrete divisions in the world. Rather, a philosophical account of psychiatric kinds should recognize that operationalized symptom clusters can be useful in practice; even as it distinguishes such kinds from those that, through scientific methods, have been demonstrated to reflect causal structures in the world.

S11. Computers in scientific practice

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Computers have become central to scientific practice as they dramatically enhance our cognitive performances in many ways (they extend both our perceptual and computational abilities). Among these, we see in particular that scientists can now use computers to solve equations that were previously completely intractable, and this in turn permits them to make use of quite complicated mathematical models to represent phenomena. Computers are also used to generate visual representations of data, whether these are recorded by some instrument or entirely computer-generated. The novelty of these practices has to be accounted for with new epistemological tools, as the traditional categories used by philosophers do not straightforwardly translate to how computer practices and the "motley methodology" (Winsberg, 1999) that characterizes them yield scientific knowledge.

Hence, in this session, our goal is to survey a number of issues that arise from these new practices through the following talks. In "About the empirical warrants of computer-based scientific knowledge," Anouk Barberousse and Marion Vorms will use epistemological tools to clarify the nature of the warrants of computer-based knowledge. Julie Jebeile will discuss the way different actors can collaborate to implement the different stages of computer simulations in an efficient way in "One computer simulation, two conceptual universes". Finally, in "simulated data and empiricism", Vincent Israel-Jost will discuss the nature of simulated data and the way simulations can shed some light on observation and empiricism.

About the empirical warrants of computer-based scientific knowledge

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In this paper, we tackle the issue of how computer-based knowledge in the empirical sciences is justified. Our central question is: To what extent the computer simulations' (CSs) outputs and the knowledge that is based on these outputs are warranted, and on what grounds?

As Winsberg (1999) shows, the question of the reliability of the results of a simulation process is not restricted to concerns about the reliability of the calculation itself. Indeed, as he puts it, CSs involve a "complex chain of inferences that serve to transform theoretical structures into specific concrete knowledge of physical systems". The validity of these various inferential steps is questionable for at least to kinds of reasons. First, the purely mathematical content of the original model's equations is not preserved through the computational process, because the computation is not strictly deductive (the equations have to be discretized, round offs errors are generated, etc.). Second, in order to build up the algorithm allowing the computer to calculate the (substitutes of) the original model's equations, a number of idealizations and additional hypotheses have to be introduced.

Whereas Winsberg focuses on the "motley methodology" of CSs, we address the question of where to put CSs on the map between theory and experiment from an epistemological point of view. In epistemological terms, it is unclear where the warrant-providing elements of the results of computer simulations come from: the knowledge elaborated therefrom is neither purely a *priori* (like pure mathematical knowledge), nor purely empirical. In many simulations, information about natural phenomena is obtained through mathematical exploration and / or experimentation. A study of the means by which practitioners sanction belief in the outcomes of CSs is thus needed.

In order to analyze the nature of the knowledge obtained through CSs, we try to find out what

kinds of warrants scientists have access to and what type of information they are entitled to retrieve when using CSs. We are less concerned with the validity of computer models in general than with the epistemic attitudes scientists are entitled to entertain when facing their computer's outputs. Our investigation into the scientists' epistemic attitudes is not psychological in nature for it dwells within the realm of reasons and rational warrants. It is therefore a project within epistemology, and our method will consist in extending Burge's (1993,1998) analyses of *a priori* justification and content preservation, and their application to the study of computer-based mathematical proofs, toward CSs in the empirical sciences.

First, we shall draw epistemological tools from Burge's analyses of situations of knowledge acquisition in which the *a priori* or empirical character of the warrant is unclear and discussed, like memory- and testimony-based knowledge. This will help us clarify the respective contributions of *a priori* justification (through mathematical exploration) and of empirical knowledge in CSs. Drawing on this analysis, we shall then present and discuss various accounts of the reasons why the outputs of CSs can be said to be empirically warranted (in particular the so-called "materiality thesis", advocated by Parker, 2009). This will lead us to seeing within the semantic specificities (vs. physical realization) of the computational process the reason why the simulation's outputs can be interpreted as being about the investigated phenomena.

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One computer simulation, two conceptual universes

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In most research teams, in science or in engineering, for which computer simulations (CSs) are indispensable, one can find the same management of research again, namely a division of work between developers and users of CSs. While developers mainly focus on the implementation and the verification of CSs, the work of users is generally merely dedicated to running CSs on concrete cases. Although unavoidable, this management is fiercely criticized by scientists themselves. For them, this management prevents users from developing knowledge and skills similar to those of developers on how CSs work. And irretrievably, it makes users unable to properly use CSs.

In this paper, I provide epistemological reasons why, in practice, this management of research nonetheless does not prevent users from making progress in their activities. In order to do that, I examine the concepts and arguments users employ for explaining the phenomena they investigate with CSs. In doing so, I show that, generally, as long as CSs are deemed reliable, users actually do not need to use in their explanations the theoretical concepts and arguments that are required for the writing of the computer programs. In other words, I try to justify in a new way why users can manipulate CSs as black boxes from where results come out (Dowling, 1999).

For supporting my claim about research management, I present a case study: the CSs investigating red blood cells dynamics. Red blood cells show distinctive physical features, such as their deformability, which allows them to pass through very narrow capillaries. In studying their physical features with CSs, biologists want to understand the physical mechanisms that

govern the path of red cells throughout the bloodstream. In this way, they hope to prevent some diseases like cancer metastasis or heart attack. In their activities, do biologists need to have a deep understanding on how the CSs work? It seems that, as long as the CSs they use are deemed reliable, they don't, as I shall show.

First of all, developers and users of CSs of red blood cells do not process the same formats of representation (Vorms, 2009). While developers deal with the theoretical models underlying computer programs, users manipulate non-linguistic output representations (graphs or pictures) that CSs typically provide. Thus, for example, while users visualize a red elliptical form moving on screen as a red blood cell in a blood vessel, developers consider a bilayer system containing an incompressible viscous fluid and a viscous membrane immersed in another fluid of lower viscosity. Besides, while users look at providing a mechanistic account of the different modes of motion of red cells in their explanations (e.g. tumbling and tank-treading motions), developers deal with the resolution of the Navier-Stokes equations applied to the bilayer system and the surrounding fluid at different velocities. The identification of the objects of interest (e.g. red blood cells) and the form of the explanation (mechanistic for users, application of the Navier-Stokes equations for developers) are not the same in the two cases. In this sense, developers and users work in different conceptual universes.

As long as CSs are deemed as reliable, users do not need to leave their own conceptual universe for entering into the developers' one. This is the reason why, for me, users can manipulate CSs without knowing how they work in detail. Actually, this paper extends to CSs the Hacking's idea that experimenters can manipulate instruments without knowing how they work (Hacking, 1983).

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Simulated data and empiricism

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At first sight, computer-simulated data seem to be archetypical of what should be rejected by any empiricist to count as observational data serving as an indubitable basis for pieces of knowledge. Among the elements that go against the idea that computer-simulated data have the same epistemic status as observational data, we find in particular:

- i. That they can be completely disconnected from reality since no physical interaction with the target system (the investigated object) is required to produce the data. The target system does not even need to be instantiated. For example, one can run a simulation aiming to study the growth of an imaginary population on an imaginary planet.
- ii. That computer-simulated data are the result of a highly indirect process. In the course of a simulation, models are created that implement a set of underlying hypotheses. Computer-simulated data are then produced by applying one such model to some initial conditions and see how the current solution evolves. For example, we would have an initial distribution of individuals of our imaginary population and a set of hypotheses regarding properties of this population (reproduction rate, known predators, etc.) Thus, the result of a simulation reflects these hypotheses very intimately and cannot be thought of as 'theory-neutral' or 'objective' as is generally supposed to be the result of an observation.

Yet, I will argue that establishing a clear-cut demarcation between simulated and observational

data is not so easily done. Concerning the first idea (i), it is important to notice that there is no requirement against there being physical interaction with the target system in the course of producing computer-simulated data. Typically, physical interaction occurs if the experimenter chooses to use real data as initial conditions in the simulation. So, just like the initial distribution of a population can be imaginary, it can equally correspond to data that express the actual distribution of an actual population. The conclusion is that (i) is indecisive to rule out simulated data from the empirical basis.

If we then turn to (ii), there is no way to deny that any simulated data would reflect theoretical hypotheses. But just like simulated data, observational data reflect a number of hypotheses, under the form of background beliefs. Indeed, enough has been said during the past fifty years on the 'theory-ladenness' of observation to have most philosophers of science agree that observation is always inferential to some extent. So instead of pointing out an important difference between simulated and observational data, reflecting on (ii) makes us see that observation is not as empiricism needs it to be (neutral and theory-free) and therefore, that empiricism cannot be sustained.

Several philosophers (Shapere, Hacking, Vollmer and others) have proceeded differently. Instead of rejecting empiricism, they have accepted that background beliefs play a role in observation, while trying to defend a demarcation between observation and non-observation as well as the epistemic authority that empiricism attributes to observation reports. They do so by claiming that some (possibly theoretical) hypotheses are consistent with observation neutrality and reliability and they give criteria that permit scientists to identify those hypotheses. My claim here is that this way to articulate a reformed empiricism can be improved by analyzing computer-simulated data, since computer simulations make use of many different types of hypotheses and these hypotheses are explicitly implemented and therefore entirely accessible. I will especially focus on why some simulated data in medical imaging are considered observational by scientists and what this tells about hypotheses that are compatible with observational practices.

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Contributed abstracts

Explanation and argument in mathematical practice

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A motivation behind much mathematical practice is explanation. Mathematicians seek not merely to prove results, but to find proofs that are explanatory. Surprising mathematical facts are widely understood as standing in need of explanation. However, mathematical explanation has been far more resistant to philosophical analysis than explanation in natural science. In this paper I argue that more careful attention to the details of mathematical practice than has been customary in much philosophy of mathematics may provide a solution.

The covering law account of explanation, despite its resilience in general philosophy of science, is of little use in philosophy of mathematics, since it requires the explanandum to be contingent. The most influential alternatives are [4] and [3]. However, both accounts have serious problems: Johannes Hafner and Paolo Mancosu have argued convincingly for the inadequacy of each theory in [1] and [2], respectively. Philip Kitcher's thesis is that explanation should be understood as theoretical unification — an explanation succeeds to the degree that it minimizes the number of 'argument patterns' required. However, Hafner and Mancosu demonstrate that Kitcher's reliance on a formal characterization of 'argument pattern' and a purely quantitative comparison between the argument patterns of different candidate explanations makes his account vulnerable to gerrymandered 'explanations' contrived to minimize argument patterns without regard to understanding.

Hafner and Mancosu concede that Kitcher's account might be rehabilitated by appeal to a qualitative comparison between explanations, but observe that Kitcher gives no guidance as to how this might be accomplished. Clearly, something more sophisticated than argument patterns will be required. A promising candidate may be found in recent work in argumentation theory [5]. 'Argumentation schemes' are stereotypical patterns of plausible but not necessarily deductively valid reasoning, characteristically accompanied by series of 'critical questions', which a respondent may use to challenge the cogency of an instance of a scheme. I develop an account of argumentation in mathematical practice which remedies the defects in Kitcher's account by substituting richer, practice oriented argumentation schemes for his purely syntactic argument patterns.

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Examining the history and implications of the 'Bermuda Principles' for data sharing

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This paper will explore the sociocultural and scientific history of the 'Bermuda principles' for genomic data sharing, considered by many to be a gold standard within science, which require researchers to post their data publicly within 24 hours for unconditional use by others. Although the Bermuda principles are often cited as critical to the ethos of contemporary research work in human genetic sequencing, much less attention has been paid to the rationales underlying their drafting, which include not only communitarian motivations but also pragmatic considerations relating to coordination of a globalized set of scientific practices. Using historical research techniques, we will explore the values underlying the principles as well as their effects on the culture and epistemology of not only genetic research practices but also other areas of scientific practice, including whether they have resulted in the promotion of more 'public science.'

How contingent is evolution? Mechanistic constraints on evolutionary outcomes

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Presumably, one of the motivations for developing the Evolutionary Contingency Thesis (ECT) is to provide a theoretical justification for the observation that there seem to be no timeless, exceptionless and/or necessary generalizations in biology. Unfortunately, the strong contingency component of ECT also implies that evolution is an entirely accidental, unpredictable and unreproducible phenomenon, to the point that nothing useful can be said about phenomena like convergent/parallel evolution, patterns in ecological adaptation and speciation, or conserved body plans and molecular mechanisms. In this paper, I propose that the notion of 'law' in biological sciences could be replaced by that of 'mechanistic constraint'. While it is true that evolution of life on Earth is dependent on contingent events like environmental changes and spontaneous mutations, empirical evidence suggests that many evolutionary outcomes are in fact 'weakly' to 'highly' constrained by biological mechanisms. In light of this evidence, I argue that evolution is subjected to constraints by biological mechanisms resulting in the elimination of one or more possible evolutionary outcomes (stringent constraint) or in an inequality in the degree of probability of possible evolutionary outcomes available at any given time (weak constraint). Thus, mechanistic constraints provide the basis for the prediction and explanation of certain evolutionary outcomes and tendencies despite the fact that mechanistic constraints are not necessary (they could have been different) and universal (many constraints are actually different in different organisms). This may pacify the tension between the generally accepted intuition that there are no strict laws in biology and empirical findings showing that there is lawfulness in biology (e.g., given the same species subjected to the same environmental conditions, the same evolutionary outcome repeatedly obtains via mutations targeting the same components of the same mechanisms).

Personalized genomics as a testing ground for theorizing about genes

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The importance of the gene to contemporary biology is by now quite clear. Recent scholarship about the gene is but the latest in a long history of debates. Very little attention has been paid, however, to the impact of new technology, practices, and techniques on new ways in which the gene is being put to work. One noteworthy example is the newly emerging Personalized Genomics (PG) industry. This is an especially important example, as these Internet-based genetic tests have an immediate and direct effect on the public, and are not mediated by the medical establishment.

I view PG as an excellent case study for current and future theorizing about genes, genetics, and society. Though this rapidly expanding industry has aroused a great deal of interest, it is also shrouded in scientific controversy. This controversy centres principally on the poor significance of the gene-trait correlations upon which the industry relies (Hunter & Khoury, 2008). This has triggered a number of attempts to locate the source of the error in testing procedures, quality control standards, translational research, and analytic tools. As of yet, however, no attempts have been made to apply any of the pertinent philosophical literature about genes to this problem. Following a brief introduction to the scientific controversy, I seek to test two prominent philosophical and historical analyses of gene concepts using PG and the controversy in which it is entangled.

I examine Griffiths and Stotz' (2006) treatment of genetic determinism, and Moss' (2003) Gene-D/Gene-P distinction. Each of these analyses contributes a unique insight into the problems faced by PG. The former forces reflection about the limitations of molecular genetics, while the latter provides a framework within which to examine two types of practical application of the gene. Yet neither of these analyses is in itself sufficient to address the problem. In light of these insights I propose a new gene concept, which falls roughly within the instrumental gene tradition (Falk, 1986), and Moss' Gene-P category. I contend that this new concept, dubbed the 'systems gene', can best diagnose the nature of the obstacles faced by PG and guide research in new directions.

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Science and fiction: On the reference of models

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When we try to describe nature we systematize our knowledge of it using models, classification systems, and other forms of representations that work as public devices of depiction. However, there are models that are meant to refer to 'real entities' and others that represent 'fictional entities' (without, since they don't exist, properly *referring* to them), the latter forming a class of empty subjects (phlogiston). Moreover, there are also 'mixed cases' where some elements seem to refer and others do not (Kepler's *Somnium*), and 'hypothetical entities' to which we have no direct access (electrons). Therefore, dealing with models raises the question how we determine whether a model refers or not.

The difficulty of determining reference results from the fact that we cannot account for reference by the model's own structure. According to Goodman, neither structure nor resemblance is sufficient to establish a representational relation in general or a referential relation in particular. That representations refer is a result of their use. However, what exactly does it mean to *use* a model as non-fictional or fictional?

I will provide an argument that renders the distinction between non-fictional and fictional uses of models visible. I will assume the status of models is a macroscopic feature: the interpretation is based on its representational entirety and not on its individual components.

This assumption concerns conditions that are required for the interpretation of what is represented: all models and their elements are complete regarding their respective representational entirety, but underdetermined regarding the external world. Thus, when we interpret a model we complete vague or inexplicit descriptions by consulting our knowledge of the external world. However, the nature of this consultation is different in fictional and in non-fictional uses.

To define use I will argue that fictional uses of models mean that the interpretation of its represented entity can only be accessed in virtue of a particular representation whereas non-fictional uses allow for the interpretation of the represented entity by various sources. Fictional entities are existentially dependent on their representation (no Hamlet before Shakespeare) whereas non-fictional entities are not dependent on any particular representation. They can be represented within various and also independent frameworks (light can be accessed as a wave or a particle). Thus, I will argue that a plurality of models is actually not a problem for realism, but an indicator for it.

The result is a difference in the use of knowledge: If a representation is used as fictional then external information for the interpretation of the represented entities can be excluded on the basis that it is not relevant and in principle not answerable by a particular representation (we cannot answer how many children Lady Macbeth had), whereas it *cannot* be excluded on that basis when we are dealing with non-fictional uses. The reason for this is that with non-fictional representations we can raise questions that can potentially be answered and portrayed by different models whereas with fictional representations we encounter questions that are meaningless, because we cannot answer them on the basis of the model alone and we don't have other sources to elaborate on them.

Delineation of a controversy in techno-science: The case of the system of rice intensification as practiced in india

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This paper is an attempt to closely follow a debate regarding the efficacy of an alternative way of cultivating rice, a staple diet of a large part of India, in various geographical regions of India. The method known as System of Rice Intensification (SRI), an agricultural practice first started in Madagascar in the 1980s, entered the Indian agricultural arena in the late 1990s and early part of the first decade of the twenty-first century. The evolution of SRI practice(s) in India is shown to be quite diverse depending upon the variety of actors playing a diverse set of roles at different times and places.

However, one feature that had remained unchanged is the non-acceptance of SRI as a legitimate agriculture practice by the community of Agricultural Scientists in India. Although this is a reflection of the skeptical attitudes of the International community of Agricultural Scientists arguing that SRI does not merit serious attention, one aspect of the debate is played out in terms of legitimate field trials and experiments. The arguments are offered in terms of agriculture science — both methodologically and ontologically — and the SRI practices are allegedly shown to be at odds with the received view and hence not worthy of consideration.

Two sets of responses have been offered from the SRI practitioners or supporters as a way of mitigating the skeptical arguments of the received view especially that promoted by the International Rice Research Institute, Manila. First, some of the agricultural scientists and practitioners have attempted to develop an explanation of the SRI practice within the framework of understanding a complex system thereby questioning the methodological and to that extent the ontological basis of the received view. Second, the role(s) of the non-institutionalized knowledge producers had been equally important in taking up field trials and developing a shareable body of knowledge, among themselves, that seem to question the orthodox wisdom of both the agriculture scientists and the traditional peasants.

While the notions of workability or success are part of the arguments for the practitioners of SRI, the notion of sustainability of practice also provides an important basis for the argument for them. Several other sets of actors which have responded to the challenge made by the received orthodoxy had been the agricultural extension officers, the district administrators and the political representatives of people. This paper attempts to show that the knowledge produced by these various actors were meaningful (at least to them) and yet of diverse kinds and played a significant role in crystallizing the challenge to the received view. This opens up the possibility in recognizing the political, social and historical subtexts of a controversy in techno-science.

The Indian scenario signals that, sometimes in a techno-scientific controversy, the debates work with varied notions of observation and thereby posing questions about the replicability of data within field trials. The messiness in transferring processes from the laboratory to the field trial to the actual field is well recognized. What has stumped the agricultural scientists, among other things, is the inability to replicate the data from the farm land to the field trials and finally to the laboratory. What seems to be not messy has turned out to be indeed messy and where there is a mess, there is a controversy, and hopefully a creative tension.

Scientific modelling and limited observational data: A case of conflict in modern cosmology

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The present hegemonic scientific model for the origin and evolution of the Universe as a whole is the *inflationary theory*, also called *standard cosmological model*, proposed in the early 80's. It can be seen as an adaptation of the original Big Bang model. Although its quantitative predictions are very consistent with much of the available measured data, today most of the cosmologists agree that it has theoretical and observational problems and that it should be modified or replaced. However, there is still no consensus as to what should be done in this respect. Some reviews have identified more than one hundred different alternative models (Novello 2008).

We have analysed about 40 of such models, in a spectrum as wide and representative as possible. In this work we report some epistemological characteristics of cosmological research which appeared in our analysis and seem not to appear in other fields of physics. Pre-existing surveys (e. g. Novello 2008) and publication databases (basically in arXiv.org) were helpful to scan and select the different models.

Our investigation suggests that model-making cosmologists are exposed to some specific conditions which have interesting effects on cosmological research on an epistemological level. In modern cosmology, there are few possibilities of obtaining measurable data, as compared to other fields in natural sciences. Present data are not enough to decide among the various alternative models and this may still be so for many years to come. Thus, a degree of conflict with ever-increasing demands for advancement in science is created. This seems to be related to two general features of today's research in cosmology. First, the large number of competing models. Second, a weakening in the traditional scientific canons for writing papers in physics, which can be seen in the way scientists justify their models. According to our analysis, the main justifications are not always observational, as the prevailing canon states, but in many cases purely theoretical, aesthetic (based on symmetries that Nature "should have"), or even subjectively based on world views (e.g., the Universe "should not" have a beginning; else we would not be able to explain rationally the world where we live).

In fact, such a conflict is also observed in particle physics. However, in that case very few alternative paradigms were produced — one of them, string theory, being by far the most investigated (by paradigm, we mean a set of models with similar general assumptions). In cosmology, on the other hand, we found more than 15 different paradigms, that split into more than a hundred competing models. Interestingly enough, this situation resembles some elements of Thomas Kuhn's description of the moments that precede paradigm shifts in scientific revolutions (Kuhn 1962), although this does not necessarily suggest that a scientific revolution in cosmology is under way.

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Scientific explanation, mechanisms, and pluralist realism

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Among the several ways to argue for realism, the current literature in philosophy of science has mainly focused on the epistemological way in order to answer quite radically to antirealist and relativist stances, as can be seen in the case of the *causal theory of reference*. In this paper I propose a moderate alternative, the pluralist realism, which helps me show that mechanisms and explanations are necessary in science. In particular, I focus on both chemistry and the notion of water, whose analysis allows me to claim a *systemic* philosophical approach to science.

Particularly, the structure of my proposal is the following: first, I review Goodman's relativist thesis (*pluralist irrealism*) (Goodman 1978), which I characterize as too weak because of its epistemological incapacity to give a minimal account of objectivity. Second, it is briefly showed the scientific realism (Kripke 1972) alternative by focusing on two problems that press it: the incommensurability and the opacity of reference. In the third part, I claim that the particular realist case of the *causal theory of reference* is problematic as well, and so, in the fourth part, propose a moderate version of realism, namely the *pluralist realism*. The fifth part consists of the main point of my proposal: a defense of the aforementioned realism based upon the concept of explanation connected to mechanisms (Craver 2007). This view will be applied in the next part (sixth) to the water case. Finally, the last part is dedicated to show some basic conclusions.

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“Neural context” and the localization of cognitive functions

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It is generally accepted, among both cognitive neuroscientists and philosophers, that a major goal of functional neuroimaging research is to localize cognitive processes to specific areas of the brain. Yet the concept of localization, in both the neuroimaging and the philosophical literature, is complex, and there is disagreement about the kind of relationship that will be found between cognition and brain activity. Early neuroimaging researchers were explicit about their expectation that complex cognitive processes (e.g. memory, problem solving) could be broken down into elementary cognitive processes, and that these elementary processes would turn out to be “strictly” localized. That is, a specific area (or areas) of the brain would be found to be responsible for implementing each elementary process. Ultimately, then, neuroimaging research would reveal a one-to-one mapping of elementary cognitive processes onto brain areas. (Or, if a single elementary process occurred in more than one brain area, there might be a one-to-many mapping, though this outcome is seen as less likely than a one-to-one mapping.)

In contrast with this view, however, other researchers have argued that what a specific area of the brain is doing at a given time is likely to depend on the details of its functional relationship with other brain areas during the performance of a task, which McIntosh (1999) has dubbed that area’s “neural context.” In arguing that neural context is important, but has been generally overlooked by neuroimaging researchers, McIntosh describes studies that show that the same part of the right prefrontal cortex appears to be active in both memory search and successful retrieval, but that the pattern of covariance of the activity in the area with that of other parts of the brain is different in the two types of cognitive operation. Thus, understanding the function of that area of interest requires examining how activity in that area is related to activity in other parts of the brain. If neural context is indeed important in identifying the process carried out by an area of the brain at a particular time, then the details of its functional connections with other brain areas must be known in order to determine what process it is engaged in and, therefore, neither a one-to-one nor a one-to-many view of localization is adequate.

In this paper, I examine two philosophical discussions of the localization of cognitive processes in the brain, specifically those of Mundale (2002) and Bechtel (most recently in Bechtel, 2008). Both of these theories acknowledge that cognitive processes are performed by networks of brain regions. I argue, however, that, despite the sophistication of these theories, they ultimately presuppose the kind of “strict” localization described above. I then briefly outline several statistical techniques that are used by neuroimaging researchers to examine patterns of connectivity among brain regions and show that these techniques can provide the basis for an amended view of localization that can accommodate the functional importance of context-specific patterns of connectivity among areas of the brain.

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Adaptationism, beyond controversies and into the fabric of scientific reasoning

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More than thirty years after Gould and Lewontin's Spandrels, there is still a feeling of uneasiness surrounding the question of adaptationism in some circles of philosophers. The fact adaptationism has been described, characterised, and commented over and over again certainly bears witness to this. Unfortunately, one has the feeling the wealth of works on this topic has progressively fog the issue. Attempts at distinguishing different commitments to adaptationism (Godfrey-Smith 2001), or different types of adaptationism (Lewens 2009), have probably been the most harmful of all. Under the guise of typologies, they in fact draw under the same appellation statements of various epistemic statuses. Thus, 1° some biologists' assumptions on the role of natural selection in evolutionary history, 2° the biologists' special interest for design in nature, and 3° the pre-eminence of design in some of their hypotheses construction, to list a few, all fall under the umbrella tag of adaptationism.

In this paper I want to argue for a radically different understanding of adaptationism. Indeed, I draw an account of adaptationism that is not based on biologists' explicit or implicit assumptions. Rather, I focus on some of the empirical and theoretical work that is used to investigate the design of traits (or 'adaptations' for some). More precisely, I first proceed to analyse the inferences characteristically drawn from a trait's design. They come in two varieties (Griffiths 1996). Then, using three historically relevant examples, I show how these two varieties of inferences are systematically articulated together. I will last argue that this articulation defines one of the reasoning modes used by biologists in their practice: adaptationism. This understanding of adaptationism is intent to draw a clear line between scientists' actual work and some of their personal assumptions, and between scientists' use of traits' design and their fascination for it.

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‘Same conditions — same effects’ as a regulative principle in experimental practices

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How do scientific practices, in particular those that work in the context of application, produce reliable and useful knowledge? In this paper, I will argue that the notion ‘same conditions-same effects’ provides the key to an epistemology of knowledge-production-by-inductive-inference in the natural sciences. This epistemology accounts for inductive inference to conditional rule-like knowledge of the form, “If A then B, provided C_{device} , and unless other known and/or unknown causally relevant conditions (K and/or X, respectively)”, thus providing an alternative to epistemologies which aim at justifying laws of nature, whether true, *ceteris paribus*, or probable. Additionally, it accounts for epistemological aspects of *employing* empirical knowledge, for instance in scientific modelling of more complex systems (i.e., physical systems that may ‘contain’ or ‘bring about’ a mixture of ‘conditions and effects’). Related to the requirement of reliability and usefulness, ‘same conditions-same effects’ directs to a methodology in which experimental practices firstly aim at broadening the span of empirical knowledge relevant to practical purposes such as experimental or technological applications, procedures and devices.

In my approach, ‘same conditions-same effects’ functions as a regulative principle in the Kantian (transcendental-pragmatic) sense, not as a metaphysical truth. According to this principle, deviations between ‘conditional rule-like knowledge’ and empirical outcomes must be explained by yet unknown causally relevant conditions, (C_{device} and/or X), and not by the falsity (or diminished probability) of the law, “If A then B”. This epistemology of inductive inference accounts for Hume’s fundamental insight that by observation and/or measurements we cannot attain any ‘deeper’ knowledge of how A and B are related — every empirical possibility requires experimental tests while nothing can be known in advance. At the same time, it adopts a manipulationist account of causality (c.f. Woodward, 2003).

Same conditions-same effects as a regulative principle justifies methodological criteria for producing and accepting conditional rule-like knowledge, such as ‘repetition’, ‘reproducibility’, and ‘variation’ (or ‘multiple-determination’). Instead of focus on methodologies that prove (or falsify) the laws of nature (or their probability, as in Bayesian epistemology), these methodological criteria guide in widening the span of empirical knowledge, which is more adequate about how a great deal of modern experimental practices produce, improve, refine and use conditional rule-like knowledge, not only about the natural world, but also about the functioning of technological devices, apparatus and instruments.

The turn proposed here is that inference to conditional rule-like knowledge is accounted for in a different manner: ultimately this principle ‘regulates’ our *reasoning* about observations, measurements and interventions with ‘the world’, instead of being a logical, probabilistic, or metaphysical principle for the justification of the (conditional or probable) truth of the results of inductive inferences. This implies that ‘same conditions-same effects’ as a regulative principle presents us with a more productive epistemology than the *ceteris paribus* clause or Bayesian probabilistic accounts of inductive inference.

Useless, repetitive, and secretive? Assessing the scientific validity of clinical trials

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Clinical research ought to be scientifically valid.¹ This ethical requirement is widely accepted and works its way into most contemporary ethical guidelines. It would seem, then, that it would be a good idea to have some account of what we mean by scientific validity (at least in the particular context of clinical research, if not more generally) as well as some way of distinguishing better from worse standards of scientific validity. We might also like to know who is qualified, or best-positioned, to assess the scientific validity of clinical trials and how broad or context-specific the scope of scientific validity is. I do not believe that philosophers of science or bioethicists have adequate answers to these questions. As a result, clinical researchers and members of research ethics committees (RECs) alike share in a general confusion about the particular demands of scientific validity and the strength of those demands relative to the other ethical requirements of clinical research.²

In this paper I draw on research by clinical epidemiologists in order to identify and critique two particular assumptions underlying current conceptions of scientific validity.³ The first assumption, creatively identified as *isolation type one*, is that the appropriate level of analysis when assessing scientific validity is the isolated individual clinical trial: a trial is either valid or not on its own merits and without any particular regard to past, concurrent, or future trials. The second, *isolation type two*, is the assumption that scientific validity should be assessed independently of the other ethical requirements of research. When instantiated as a division between ethical and scientific review committees, this makes trade-offs among the ethical requirements (for instance, between scientific validity and social value or informed consent) effectively impossible. This, in turn, leads to situations in which the 'rigor' of clinical trials may be aggressively pursued regardless of the cost to, for instance, the social value of that research.⁴

¹ Emanuel, Wendler, and Grady identify seven requirements that must be met in order for clinical research on human subjects to be ethical. Scientific validity is the second requirement on this list. See: E.J. Emanuel, D. Wendler, C. Grady. What Makes Clinical Research Ethical? *JAMA* 2000; 283: 2704. The requirement that research be scientifically valid was originally defended in the Belmont Report commissioned by the American government in 1978. For details, see: The National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research. *The Belmont Report: Appendix*. Vol 1. Washington, DC: US Government Printing Office; 1978: Chapter 9. Available at: <http://ohsr.od.nih.gov/guidelines/belmont.html> [Accessed August 16, 2010].

² Research Ethics Committee (REC) is a neutral term which is meant to refer broadly to research ethics boards (REBs) in Canada, Institutional Review Boards (IRBs) in the United States and other similar boards internationally.

³ I identify these assumptions on the basis of close attention to clinical research has been carried out. The general idea is that we can learn something about the standards of scientific validity used in practice if we look at sorts of trials that made it through (scientific and) ethical review and have been conducted.

⁴ There is empirical evidence to suggest that researchers seem to be pursuing trials with, for instance, strict exclusion criteria, even when this makes translation to the clinical setting very difficult. I would characterize this as a situation in which a particular conception of scientific validity is permitted to 'trump' concerns with social value. Examples of strict exclusion criteria are easy to find: one such example comes from a recent trial on depression (the "STAR*D")

I argue that both of these assumptions are problematic. General confusion over scientific validity in the context of clinical research may be contributing to a situation in which researchers fail to conduct a comprehensive review of the literature before launching into a trial, fail to report the results of their research, and prioritize rigorous methods over important social goals. These are serious problems, or so I will suggest. Moreover, each of these assumptions stems from a more general failure to appreciate the ways in which science is a social practice. A robust understanding of scientific validity requires that we attend to the ways in which research trials, like researchers, are not isolated and independent. In the final section of the paper, I discuss some of the ways in which social epistemologists might contribute to our understanding of scientific validity in the context of clinical research.

study), which qualified fewer than one in four (22.2%) of the patients with depression considered for the trial. Researchers did an independent assessment of how well patients did in the study and determined that those who met the inclusion criteria were more likely to have better outcomes across a variety of measures. This can lead to “more optimistic outcomes than may exist for real-world patients.” University of Pittsburgh Schools of the Health Sciences Media Relations. 2009. Are we cherry picking participants for studies of antidepressants? Available at: <http://www.upmc.com/MediaRelations/NewsReleases/2009/Pages/STARD-trial.aspx> [Accessed August 20, 2010].

The philosophy of research assessment exercises

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Research evaluation practices such as bibliometric and scientometric accounting in universities and scientific institutions are highly invested with epistemological assumptions that have the potential to override and alter the epistemic values and norms associated with established philosophical doctrines of knowledge. The implications of these evaluation instruments for science are far-reaching. They are transforming the cognitive division of labor in science, setting up incentives for a specific organizational behavior, and are documented to affect scientific problem-choice, research design, and selection and processing of scientific data.

Often introduced as a tool for enhancing accountability and transparency in science, new clusters of research evaluations and assessment exercises are implemented in universities and knowledge institutions worldwide to render the scientific knowledge production observable and measurable within a limited number of indicators (e.g., publications, citations, patents, etc.). In establishing ever more pervasive evidence-based indicators, contemporary science policy assumes that only what can be measured can be managed, while the long-term impact of scientific results are likely to be neglected — both economically and scientifically. Basic scientific contributions such as standardization, nomenclature, hypotheses, explanations, interpretations, and similar cognitive abstracta are deprived from epistemic and social value, since their outcome can only be assessed using a wide range of justificatory and epistemic criteria. On this account, the politics of scientific knowledge in modern societies rest on a model of science that is highly limited in its scope. Against this sneaking reductionism, the paper argues that, on most accounts, scientific knowledge is irreducible to its first order observable and material qualities — i.e. texts, citations, patents, products, technologies — and that we need to advice an alternative model of research evaluation.

In the face of the present tendency to identity science by its products, we need to insist on the social epistemic qualities of scientific knowledge, and the institutional settings producing and enhancing these qualities (as recently argued by Alvin Goldman, Philip Kitcher, Aant Elzinga and Ronald Giere). The significance of the epistemic and social value of science cannot be exhausted by descriptions of its products only — whether it are publications or contributions to business and society. In conclusion, the paper opts for a smarter economics of scientific knowledge that operates with long-term evaluation, multiple causation, and positive externalities. Assessing scientific knowledge needs to take into account the public good character of science and adjust the accountability regime to longer time frames and institutional spill-over effects.

Why chemical explanations are unlike biological or physical explanations

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In the *Metaphysics*, Aristotle articulates three kinds of scientific practice: theoretical, practical, and productive. Productive science is concerned with knowledge for the sake of the production of substances — in other words, synthesis. Much of contemporary chemical practice aims toward synthesis. This makes chemistry a productive science, to use Aristotle's helpful nomenclature, and so separates it from theoretical sciences such as physics and biology. Because chemical practice is chiefly productive, rather than theoretical, it is accompanied by a set of methodologies that are distinct from those found in the theoretical sciences. Among the methodological distinctions between chemistry and theoretical sciences is the role explanation plays in chemical practice, as well as the criteria for what counts as a chemical explanation. In this talk, I aim to answer the question of what counts as a chemical explanation through a discussion of the productive aims of chemical practice.

The productive nature of what is now known as chemical practices has been explored already in recent as well as ancient times. Roald Hoffmann has notably argued that chemistry's relative absence from the contemporary philosophy-of-science literature is in large part due to its aims at synthesis over analysis. For instance, while a physicist may aim to classify the world in terms of ever-simpler, more fundamental particles, and a biologist may aim to explain metabolic processes by breaking it down into transformations of enzymes along increasingly subdivided metabolic pathways, chemists frequently aim to create new molecules or substances. The former two practices are clearly analytic, breaking down bits of the world or the theory into smaller bits, while the latter is synthetic, constructing a novel stuff from pre-existing components.

Hoffmann uses the distinction to discuss common issues in philosophy of science from a chemist's perspective, addressing reductionism, the realism debate, and Kuhnian values. But few have put the distinction to further philosophical use. This is perhaps surprising, because the distinction offers, among other things, a natural way in to talk about the difference between chemical explanation and explanation in other sciences. By pointing out that chemistry has different aims from the theoretical sciences, it immediately suggests that the kinds of things chemists want to explain will be different than the kinds of things physicists or biologists want to explain. Chemists want to explain why a molecule exists, or why a substance has the macroscopic features that it does, and often answers to these why questions incorporate discussions of laboratory procedure as well as analogies molecules whose components are related, via placement on the periodic table, to the molecule under scrutiny. I argue that, while no current account of explanation provides an adequate framework to describe how these why-questions are answered, Robert Batterman's account, in which the similar behavior of similar systems is explained by pointing to the irrelevance of sets of differences between the systems, provides the best foundation for a theory of what counts as an explanation in productive chemical practice.

The three-dimensional metainformation theory of scientific concepts

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Philosophers of science have long treated scientific concepts as theoretical terms used in scientific theories. I call this tradition the linguistic approach to scientific concepts, according to which we can fully understand particular concepts by revealing the syntactic and the semantic properties they have. Alternatively, in this paper, I propose an account of scientific concepts from a cognitive approach, which takes the concept possessor's cognitive processes and mechanisms as an integral part of it. I begin with a general characterization of scientific concept:

(SC) A scientific concept of x is a body of information about x that members of a relevant scientific community have in common, and that is used by default in the processes underlying scientific practices dealing with x .

To say that a scientist has a scientific concept of x means that she has the ability to use it to do her scientific practices concerning x . Note that this is not a theory of scientific concepts but a general formulation upon which specific theories can be built. The theories will vary depending on the nature of information stored in scientific concepts. I argue that in order to understand it, we have to pay attention to three kinds of metainformation of scientific concepts: the function that a concept is supposed to do, the ontological category that it is used to represent, and the specific types of information that it includes. I take these components as three dimensions of metainformation, not simply information, of scientific concepts. Not only metainformation represents some aspects of information stored in concepts, but it constrains which information can be stored in a certain concept, and how they are organized and structured in the concept.

I explore the general roles of three dimensions of metainformation, followed by adducing the reason three metainformation are so important that they are constitutive of the core part of our theory of scientific concepts. First, the function of concepts emphasizes the fact that scientific concepts are invented as intellectual tools for scientific practices in concrete contexts, and it supports the rationality of conceptual change by providing a criterion, upon which changes can be evaluated. The ontological category of a concept puts constraints which sorts of information are stored in the concept. A concept can store different kinds of information according to whether it is used to represent entities, processes, or mechanisms. Finally, scientific concepts may include the prototypical information of properties that members of a category typically have, the information of individual members, the theoretical (causal, functional, or nomological) information about the members of the category, or some mixtures of them. For example, the concept NATURAL SELECTION is supposed to account for the diversity of biosphere and the omnipresence of adaptation, is used to represent mechanism as an ontological category, and includes prototypical information (i.e. finch's beak) and theoretical information.

In conclusion, I claim that with the three dimensions of metainformation in scientific concepts we are in a better position to understand scientific practices. I would mention some of philosophical implication of it.

Extending, changing and explaining the brain. Neuroplasticity and the case of tactile–visual sensory substitution

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The science-technology relationship is of particular interest in brain research. Basic neuroscience yields hundreds of thousands of publications annually, exploiting an impressive range of techniques from genetic engineering to functional neuroimaging. Yet the discipline lacks an overarching theory of brain function to unify the massive amount of data collected, and neuroscientists focussing on single levels of investigation (e.g. cellular, molecular or sensory), share little common ground. At the same time, certain findings in basic neuroscience have fostered practical applications, including neural technologies with significant therapeutic and commercial potential. For example optogenetics uses genetic insertion of photosensitivity in brain cells to enable fine control of neural circuits with impulses of light (Zhang et al 2010). Much neural technology aims simply to control the operation of neurons, especially in cases of psychiatric and neurological disease where function is pathological. Other technologies aim to *extend* neural function, and this is the focus of our paper. In these cases, the technology is made possible because of the brain's lifelong capacity for plasticity, the alteration of brain anatomy and connectivity in response to trauma, demands of learning, or interaction with new objects in the environment. We ask how such technologies can contribute to basic neuroscience. In other words, does *changing* the brain rule out *explaining* the brain?

In order to answer this we look to examples of Brain Computer Interface (BCI) technologies which link the brain to a computerised environment. The purpose of BCI is twofold. Firstly to augment or compensate for sensory information that is currently lacking, for example in blind and vision impaired subjects; secondly as a parallel and supplemental channel of information to the brain, to counter sensory overload (e.g. Danilav & Tyler 2005; Bains 2007). At its most basic, a BCI like Emotiv straightforwardly uses non-invasive electroencephalography (EEG) data as inputs into a computer system. A more sophisticated use of BCI is the Brainport, utilised in a tactile-visual sensory substitution device (TVSS) (Bach-y-Rita et al. 2001). We examine how technologies of sensory augmentation extend neural function in the blind, and the role of brain plasticity in the successful application of this technology.

Such interventions lead to an enhanced two-way interchange across sensory systems, potentially restoring mechanisms of sight to the blind. Skepticism is justified that we are left in the dark about the nature of sensory systems in their untampered state. We argue that the findings of brain-extending technologies can be of benefit to basic neuroscience, but with certain caveats. It is unlikely that the physiology of the extended systems will mirror that of the natural system, yet understanding how the brain adapts itself in order to decode new types of sensory information will give us insight into normal function, especially in development. Even if there is no unified theory of brain function, work on plasticity may become a unifying thread in cellular and sensory neuroscience.

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From general claims to specific policies: Disambiguating causal claims for unemployment policy

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After extensive research in the last decades, a consensus has emerged among academic labor economists regarding the qualitative causal effect of some labor market institutions on the aggregate unemployment rate. Let me focus on two institutions. First, it is believed that the generosity of unemployment benefits has a positive effect on the unemployment rate. Second, the strictness of employment protection is deemed to have no net effect on the unemployment rate (see Blanchard 2006, 2007; Boeri and van Ours 2008, ch.10-11).

How useful are these discoveries for concrete policy issues — assuming for the time being that the beliefs of labor economists turn out to be correct? This contribution will argue that, to assess usefulness, one needs to clarify the actual meaning of these causal claims. Economists leave it too ambiguous.

First, some causal relations break down when they are mobilized for policy purposes. Are we in presence of such fragile causal relations here? Since the causes, in the present case, are directly controllable by policy makers — they are the ones indeed setting the generosity of unemployment benefits and the strictness of employment protection — fragility should not be our more pressing worry. In other words, the causal claims do not simply lend themselves to an interpretation in term of possible interventions *à la* Woodward (2003), we have, in fact, come to formulate them by studying actual interventions.

A second precision is required. Do labor economists mean, for instance, that all policy reforms decreasing generosity of benefits, *at any point in time and in any jurisdiction*, will lead to a lower unemployment rate than otherwise? It will be argued that a more plausible interpretation of their causal claims is that they are assertions about average causal effects for a restricted population: for a certain set of countries — say, the Western developed economies — and for a certain period of time — say, from the 1980s to the near future — interventions on unemployment benefits or on employment protection will have, on average, the causal effects as claimed. Such an ‘average’ interpretation leaves open the possibility that, due to the heterogeneity of the causal effect across units, the actual causal effect for a given country at a given time might be different than asserted.

Such average causal claims can be useful for policy only in some circumstances. This limited usefulness will be illustrated by the recent policy suggestions made by the OECD (2010) to tackle the high unemployment rates due to the economic crisis. The striking thing with the OECD's recent report is that the implied policy suggestions are the exact reverse of what one would expect given the consensual causal claims presented above: countries are told to weaken their employment protection — even though it has a null causal effect according to the consensus view — and to give generous benefits to job seekers — even though the consensus is that generous benefits pushes the unemployment rate up. The reason for this divergence is simply that *context matters*. The average causal claims are not believed to give much guidance to unemployment policy in the present context.

This result supports my main point: usefulness depends on the precise meaning of the causal claims. It can also be reformulated as a warning: acting on an established causal claim might lead to disastrous results, not because the causal claim is wrong but because it has been misinterpreted.

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Informed policy and purposive case selection

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Recent literature in the methodology of political science has re-examined the evidential role of case studies. The purposive selection of cases, a frequent practice in political science research, has been one site of debate. The truism that cases should not be selected on the dependent variable has been cited as an argument against such purposive selection (Geddes 1990, 2003). However, the many different uses of case studies — heuristic, hypothesis testing, process tracing, etc. — give some reason to think that this prohibition stems from too narrow an understanding of scientific method — one that does not fully recognize the multiplicity of goals of political science research (Bennett and Elman 2006, Collier 2010, Seawright and Gerring 2008). I argue that the methodological rule “never select on the dependent variable” is embedded in a methodological monism which in turn is driven by a conception of science as a search for laws, or, at least, or lawlike claims. This methodological monism assumes that researchers should separate the *scientific* value of the research from the question of its use; this view rests in a traditional conception of scientific objectivity that does not distinguish knowledge *simpliciter* from relevant knowledge. A more pluralistic understanding of the methodology and the goals of science makes it clearer why purposive selection of cases would be appropriate under some circumstances and relative to some goals.

For example, case study researchers in political science sometimes distinguish between cross-case and within-case analyses. Cross-case analyses are more closely associated with the statistical methods for which selection on the dependent variable is prohibited. Within-case analyses, those more traditionally associated with the comparative and historical tradition in political science, are geared towards different goals, such as the identification of causal mechanisms and the investigation of causal complexity. I examine this claim against a backdrop of recent philosophical discussions of causality (e.g. Woodward 2003, Cartwright 2007) and consider how the close examination of cases can provide evidence for causal claims and what sorts of evidence it might provide. The way causal mechanisms function in particular cases, as well as why they may not operate in others, could motivate purposeful selection of cases. One way that this might happen would be that researchers would choose cases that vary from the “average” case in order to determine the limits of generalizations. Such an approach would be particularly useful for informing policy decisions, given that such research would reveal when and where mechanisms operate and what sorts of factors could be relevant for causal efficacy.

Teaching philosophy of science to doctor of nursing practice students

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The Doctor of Nursing Practice Degree (DNP/DrNP) is a relatively new degree in the U.S., but already there are more DNP programs and graduates than the PhD which has been around since 1933. As opposed to an academic degree like the nursing PhD the focus of which is research, the nursing practice doctorate is intended to be an advanced degree focusing on the *practice* of nursing. Being a new degree, introduced in 2001 but not becoming a significant force in nursing education until 2005, the doctor of nursing practice is still in a process of development and evolution. Its essence, its purpose and its proper curriculum is as yet a matter of negotiation working itself out. Being a practice-oriented rather than an entirely research-oriented degree, the amount and depth of theoretical knowledge necessary is one of those points still under discussion. Within this realm of theoretical knowledge is the possible inclusion of the teaching of the philosophy of science. Nursing's status as science itself is of an uncertain nature (or is it better described as an applied science?), making the relevance of philosophy of science on the one hand uncertain but on the other hand possibly especially intriguing in potentially facing a form of the demarcation question. Philosophy of science courses are very common if not universal in nursing PhD curricula, as scientific and theoretical research is the prime focus of these programs. In the doctor of nursing practice curricula such courses are far less common and more controversial. Their relevance depends not only on the natures of nursing and of science but the nature of this particular degree and the roles to be filled by graduates of these programs. Part of this nature and these roles include questions of the role of knowledge for advanced practice nurses. Are they to be merely implementers and at most interpreters of knowledge or generators of knowledge (as PhD nurses are conceived to be) in their own right or perhaps generators of *practice knowledge*? We would contend the deeper their commitment to producing practice knowledge for the nursing discipline, the deeper the justification seems to be for including the philosophy of science in their academic study.

In our DrNP program at Drexel University we have included a course in the philosophy of science since its inception in 2005. We believe the inclusion of this course reflects our commitment to our understanding of what nursing is, what this particular degree is, and the roles these graduates are expected to fill, as well as a commitment to rigor in advanced nursing education. This constitutes our affirmation of the place of philosophy of science in a practice discipline. In this presentation we plan to defend this view with an overview of our course and our experience teaching it and with an exploration of the nature of nursing, the nature of this degree, and the relevance of philosophy of science to the relationship advanced practice nurses will have toward knowledge and practice knowledge development.

Cognitive limitations, distributed cognition, and mathematical practice: The case of Chinese algebra

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Experimental studies indicate that human cognitive capacities are limited by heuristics, biases and memory constraints. Scientific and mathematical practice seem to be relatively unhindered by these limitations. How can people produce scientific knowledge beyond the scope and limitations of their cognitive capacities? Some philosophers of mind, philosophers of science, and social epistemologists (e.g., Clark, 2003; Giere, 2004) have argued that scientists are able to overcome their natural cognitive limitations by three types of external resources: other minds, artefacts (such as measuring devices or books), and (artificial and natural) language. However, it remains unclear precisely how these external resources extend our cognitive capacities, how they interact with each other, and whether they are all equally crucial or important for scientific and mathematical practice.

This paper outlines a conceptual and analytical framework in which these three types of distributed cognition are incorporated. We extend a game theoretical model of distributed cognition that we developed earlier (De Cruz & De Smedt, in press), and apply it to mathematical practice, in particular the development of algebra. We provide a brief overview of the cognitive psychological and neuroscientific literature on human mathematical skills. Mathematical practice depends crucially on a set of evolved numerical skills, which we share with other vertebrates (De Cruz & De Smedt, 2010). Empirical studies suggest that these evolved numerical skills are severely limited in their scope and precision. We will indicate how the interaction with other minds, artefacts and artificial language (i.e., mathematical notation) can extend this evolved numerical cognition beyond what is seen in other species. We apply our analytical model to the development of Chinese algebra from the Han to the Qing dynasty (206 BC-1912). In this case study, it becomes clear that the development of Chinese algebraic concepts was crucially dependent on the use of artefacts such as counting rods and abacuses. Also, we show that the absence of symbolic notations to indicate variables placed severe limitations on the kinds of algebraic problems that could be tackled. Moreover, we demonstrate that the size of the community of practicing mathematicians correlates closely with progress or decline in Chinese mathematical knowledge and concept formation. The case study of historical Chinese mathematical practice strongly suggests that artefacts, artificial language, and other minds are all essential the development of mathematical concepts.

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Scientific dissent, objectivity, and public policy

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Many have argued that allowing and encouraging public avenues for dissent and critical evaluation of scientific research is a necessary condition for promoting the objectivity of scientific communities (Longino 1990; Solomon 2001; Longino 2002). A community that ensures members are accorded the opportunity to raise criticisms, offer alternative models and explanations, and identify unjustified background assumptions, and have those objections taken seriously is thought to be more objective. Such mechanisms are necessary for limiting the influence of problematic biases of individual researchers as well as ensuring that a full range of research projects, hypotheses, models and explanations receive adequate attention.

In spite of the importance conceded to dissent, recent attention to the ways in which those with certain commercial and political interests have manufactured dissent in research on climate change science, smoking, and environmental health problems (Michaels 2008, Oreskes and Conway 2010) have brought attention to what appears to be a seriously problematic role of dissent. Scientific consensus is often taken to be a benchmark for scientific knowledge and it is often thought to be necessary for legitimately grounding policy options. Hence, dissent can be used to discredit the science, spread confusion among the public, and promote doubt about the truth of particular scientific claims. This is thought, by both sides of the debate, to undermine the adoption of particular public policies.

Given the potential consequences that dissent can have on the adoption of necessary public policy, many scientists have become reluctant to engage in, or be supportive of, even what is considered legitimate dissent. Indeed, some scientific communities in a variety of research areas have engaged in practices that attempt to mask or quell dissent (Beatty 2006; Waltz 2009).

We argue that denunciation of scientific dissent is both misplaced and dangerous. First, it is misplaced because it is often based on the mistaken assumption that there is a unequivocal correspondence between particular scientific claims and particular policy outcomes (Kennedy 2007). Second, denunciation of dissent is dangerous because it is likely to stifle legitimate dissent, which will serve neither science nor public policy. Moreover discouraging and masking disagreement provides ammunition to those who would argue against trusting scientific expertise. Third, criticism of scientific dissent can increase illegitimate instances of creation of doubt. This is so because it can reinforce the incorrect assumption that particular scientific knowledge is unequivocally related to particular policy options. Hence, if people believe that the only way to undermine a policy outcome is to challenge the science, they will feel compelled to manufacture dissent. In order to combat public misperceptions and confusion, a more promising strategy is to educate the public, scientists, and policy makers about the complex relationships between scientific knowledge and public policy.

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Objectivity and scientific understanding

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The notion of understanding has long been ignored by philosophers of science because of its allegedly subjective nature, which would make it irrelevant to a philosophical analysis of science. In my paper I challenge this view. First, I will argue that in scientific practice understanding is crucial for achieving the epistemic aims of science. Second, I will show that while scientific understanding is inherently pragmatic, it can still be objective.

The idea that understanding is at odds with objectivity goes back to Carl Hempel. He argued that a philosophical theory of scientific explanation should avoid notions such as understanding and intelligibility, because these are pragmatic, relative, and subjective. According to Hempel, whether or not a proposed explanation is intelligible and provides understanding may vary from person to person and has no implications for its objective validity. By contrast, philosophy of science should focus on features that make scientific inquiry and its results “objective in the sense of being independent of idiosyncratic beliefs and attitudes on the part of the scientific investigators.” Today, the Hempelian view that understanding should be banned from philosophical discourse is defended by J.D. Trout, who endorses a similarly objectivist approach to scientific explanation.

I will challenge these views by arguing that understanding is essential for achieving the epistemic aims of science. Study of the practice of science shows that in order to construct and evaluate scientific explanations merely possessing knowledge is not enough: in addition scientists need particular skills to use and apply this knowledge. Achieving the epistemic aim of explanation unavoidably has a pragmatic dimension in which skills and judgment play crucial roles. Successful use of a theory requires pragmatic understanding of it. This can be rephrased as the condition that scientific theories should be intelligible, where intelligibility is the value that scientists attribute to the cluster of qualities of a theory that facilitate use of the theory. Intelligibility is a context-dependent value, and which theories are deemed intelligible can vary through time, across disciplines, or even within a particular discipline.

One might object that my thesis that pragmatic understanding and the (contextual) value of intelligibility are inextricable elements of science threatens the objectivity of science. However, this complaint is based upon a misguided conception of objectivity. On the traditional view, objectivity is interpreted in the sense of exclusion of any kind of values from the reasoning process. As Heather Douglas argues in her recent book *Science, Policy, and the Value-Free Ideal* (2009) argues, this idea of ‘value-free objectivity’ is unattainable and undesirable, even as an ideal. Rejecting value-free objectivity does not mean giving up the idea of objectivity altogether: science can be objective in other ways, so that relativism and subjectivism are evaded. I will apply Douglas’ account of objectivity to the issue of scientific understanding and show that its pragmatic and value-laden nature does not bar the possibility of objective understanding.

A pragmatic turn for general philosophy of science

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In recent years, the growing attention for scientific practice in philosophy of science has primarily resulted in important developments within philosophy of the special sciences. While we applaud this evolution, we are convinced that it should be accompanied by a similar change in the mode of thinking within general philosophy of science. We believe that general philosophy of science still focuses too much on the generality and universality of its theories rather than on developing philosophical tools adequate to grasp real scientific practices. If general philosophy of science aims to be primarily concerned with science as it is, our theorizing in general philosophy of science will have to recognize, and start reasoning from, the diversity in the use of concepts and related methods within science. We will argue for the need and usefulness of such a pragmatic turn, and demonstrate that pluralism is an inevitable result.

We use causation and explanation as test cases, and give examples that demonstrate how these concepts get different interpretations within different (scientific) contexts. We argue that a pragmatic approach enables us to account for these differences, resulting in a pluralist view on causation and explanation. The analyses offer useful knowledge about the scientific practices involved, which also matters to the scientists themselves. Now, we find it the principal task of philosophers of science to make a contribution that really matters to scientists and their practice. Therefore, we think that the importance of the pragmatic, pluralist approach to causation and explanation is obvious. We further argue that, from a scientific practice point of view, it is uninteresting and useless to aim for a unifying, general theory of causation/explanation that can replace all alternatives. We conclude that it is better to regard different notions of causation and explanation - as explicated in competing philosophical theories - as different tools in a toolbox, which are all potentially valuable for use in different cases.

Generalizing from our test cases, we argue that an important, future task for general philosophy of science is to further develop and refine such toolboxes, and additionally, to write the manual that comes with them, specifying why certain notions are accurate and adequate for use in certain contexts. Additionally, since scientific knowledge, interests and methods change and evolve, general philosophy of science will have to follow up and reflect the dynamics in the ongoing development of the tools and the manual. We show how general philosophy of science, in this format, will form a necessary complement to philosophy of the special sciences, offering a general and comparative view on scientific practice on the whole, which cannot be gained from the fragmented work within philosophy of the special sciences alone.

To conclude, we refute two possible objections. First, we explain that a pragmatic turn in general philosophy of science does not imply that all models and analyses resulting from traditional general philosophy of science are obsolete. And second, we argue that it does not follow that general philosophy of science will have to give up her normative task with respect to science.

How to make the research agenda in the health sciences less distorted

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A well-known problem in the health sciences is the distorted research agenda. This problem can be divided into at least three sub-problems: (1) research money is spent on research in which methods are used to exaggerate a product's effectiveness and/or underestimate its side effects (e.g., by testing a new product on patients who are younger and healthier than the target population), (2) too little research money is spent on research that is tailored to the health problems of the poor, and (3) too little research money is spent on non-profitable solutions to health problems (e.g., change of lifestyle). In the paper, these three sub-problems are analyzed in more detail.

Next, I discuss for each sub-problem different proposals for a solution. One possible solution to the first sub-problem, which is suggested by Julian Reiss and Philip Kitcher, is to leave the running of clinical trials to an independent body committed to neutral hypothesis testing and overlooked by a board whose members represent different stakeholders. Implementing this solution requires, however, a lot of effort, since it implies a radical departure from the existing system (in which pharmaceutical companies run clinical trials, and in which the results are assessed by independent agencies such as the U.S. Food and Drug Administration and the European Medicines Agency). Some less drastic measures to deal with the first sub-problem are proposed.

Several strategies to tackle the second sub-problem have been proposed in philosophical and other literature. These strategies can be divided into two categories: pull mechanisms and push funding. The central idea of pull mechanisms to stimulate research that is tailored to the health problems of the poor is to offer prize money to anyone who has developed a solution for such a problem. Push funding usually takes the form of research grants allocated by a central granting agency. The idea is then that more research grants are devoted to research that is tailored to the health problems of the poor. I show that both pull mechanisms and research grants allocated by a central granting agency have disadvantages. My proposal is that governments of advanced countries create and support (through push funding) government-owned corporations that aim at tackling poor people's health problems. As further empirical research is needed to check whether this policy outperforms pull mechanisms and a system that is based on research grants allocated by a central granting agency, I only offer some speculative arguments in favor of my proposal.

The third sub-problem can be solved analogously: we can offer prize money to anyone who has developed a non-profitable solution to a health problem, we can devote more research grants to research that supports the development of non-profitable solutions, or we can create government-owned corporations that aim at the development of non-profitable solutions. I offer some speculative arguments for the claim that the latter solution will probably be more cost-effective than pull mechanisms and research grants allocated by a central granting agency. I also describe the kind of further empirical research that is needed to test this claim.

How 'normal development' tames variation: Types as reference standards for comparative work

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Normal development is a foundational component of the conceptual framework of developmental biology, and it is embedded in the practices and material culture of developmental biology in various ways. Textbooks offer extended descriptions of normal development for the most widely used experimental organisms. Stage series of normal development, used at the lab bench to stage embryos, are essential tools of experimental practice. And in the case of canonical model organisms, standardized wild type lines and husbandry practices serve in part to stabilize normal development as a reliable and regular experimental phenomenon. Yet despite its centrality, the very notion of normal development has been criticized for well over a century on the grounds that it ignores or eliminates developmental variation. The usual response to such criticism has been to say that normal development is not a theoretical commitment, but rather is merely pragmatic. In this paper, I attempt to explicate and evaluate this usual response by offering a pragmatic analysis of normal development. I argue that normal development should be understood as a conventional reference standard that solves the practical problem of organizing the substantial descriptive-comparative work required to perform intelligible experimental investigations of development. As a reference standard, normal development enables researchers to efficiently describe and to communicate their descriptions of developmental variation, and it secures comparability between variants. Far from ignoring or eliminating variation, normal development is actually the key component of a strategy for taming variation by transforming populational variation into organized typological variation.

One potential objection to this analysis of normal development is that, insofar as it affirms that normal development is merely pragmatic or merely conventional, it undercuts - or at least loses track of - the philosophical task of articulating the theoretical commitments of developmental biology. In response to this potential objection, I contend that it is misleading to say that normal development is *merely* pragmatic. We can locate theoretical commitments in and around normal development, but we must do so indirectly, on the understanding that such commitments are mediated by pragmatic success. If we think of normal development as a conceptual tool, then we can ask why that tool works when and where it does, as well as why it doesn't work when and where it doesn't. To illustrate this, I consider the zebrafish stage series as an example. By articulating the criteria of success for a stage series, I show: 1) how producing a pragmatically successful stage series is itself an achievement, and 2) how, given that staging embryos using this stage series works, the details of this achievement have theoretical implications for what development must be like.

Learning from microbiology: Are all individuals evolving?

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The individual organism has been considered the most fundamental unit of biological organisation. They are considered by many to constitute both functionally autonomous living systems and units of evolutionary selection. Both evolutionary theory and work on biological self-organisation and emergence, and the relationship between these two processes, have centred upon this conceptualisation of the individual organism and its historical appearance. In contrast to this, recent research in microbiology has led to the notion of the metaorganism as the most fundamental type of biological organisation. It is proposed that communities of individuals, both purely microbiological and microbiological and multicellular, constitute functionally autonomous living systems. Metaorganisms are open and dynamic systems whose flexible composition is linked to their persistence. However, this feature, amongst others, is associated to them not having a reproductive inheritance system, and not forming a clear generational series of evolving individuals.

I argue that the standard concept of the individual organism and that of the metaorganism are significantly different from each other, and potentially incompatible. Both concepts describe a system which is in some sense a functionally autonomous whole. However, I claim that, unlike the standard concept of the individual organism, the metaorganism does not constitute a unit of evolutionary selection. Significantly this implies an increased significance for emergent accounts of biological organisation.

In this paper, I then explore the consequences of adopting this position with respect to (1) Bouchard's view of metaorganisms as 'emergent evolutionary individuals' (2010), and (2) the ways in which the component parts of a metaorganism, i.e. individual organisms, might be conceptualised.

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Weight of evidence analysis in practice

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How should we weigh complex sets of evidence? In making decisions, we often need to consider science from multiple disciplines, each with its own methodological conventions and problems, and the science rarely cleanly converges on a single clear picture or answer. We need to assess sets of divergent evidence, to determine “where the weight of evidence lies,” so that we can assess which position has the most epistemic support at any given time. Doing this in practice has most often meant relying on individual subjective expert judgment, although alternative approaches using expert elicitation, meta-analysis, evidence ranking, and Bayesian nets have been proposed. Part of the problem with these alternatives is methodological; they are not sufficiently broad to be applicable to many cases (e.g., meta-analysis and evidence ranking) or they suffer from epistemic inadequacies (expert elicitation). Additional problems can arise from a lack of transparency, particularly to non-experts, of an approach (e.g. Bayesian nets). This talk will discuss a qualitative alternative to conducting weight of evidence analysis, an approach that strives for both rigor and transparency. The approach centers on explanatory accounts of the evidence, and presumes the presence of competing explanatory accounts for most real-world weight of evidence problems. The qualitative approach faces challenges of its own, including 1) how to delineate the set of evidence to be weighed, 2) how to generate and clarify the competing explanatory accounts, 3) how to impose epistemic rigor on the explanatory accounts, and 4) how to determine which account best captures the “weight of evidence.” In this paper, I will argue that the first challenge for the explanatory approach is a problem for any weight of evidence analysis, but at least the explanatory approach can help delineate what should count (using explanatory relevance). I will argue that the second challenge is also a problem for any Bayesian or causal network approach, as the competing causal stories must be all on the table for those methods to be applicable. Thus, the explanatory approach is minimally needed in order to develop a Bayesian net. I will also suggest that expert elicitation techniques are best employed to assist with generating explanatory accounts, rather than solving the weight of evidence problem itself. The third challenge I will argue is best addressed by demanding consistency in explanations and utilizing the predictive capacities of the explanations offered, but that coherence considerations do not provide the epistemic checks we need. The fourth challenge is the one that in practice may be most amenable to quantitative aid, e.g. Bayesian nets or simple weighting. For easy cases, where there are clear winners, it is obvious where the weight of evidence lies, and quantitative aids are unnecessary. For harder cases, where there are still multiple viable candidates after consistency and predictive checks, quantitative aids may be useful, but still must be made transparent. Thus, weight of evidence analysis has implications for how quantitative and qualitative approaches in philosophy of science can be usefully combined.

Bridging science: Ensuring success in cross-disciplinary science

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The UK Research Councils identify several 'grand challenges' for future research, including hunger, poverty, climate change and sustainable development. Meeting these requires scientists to step out of discipline-specific problem-solving, to combine skills and collate knowledge, to include non-academic stakeholders and to rush science to the service of society.

None of this is straightforward! Scholars report various obstacles to genuine cross-disciplinary scientific production: lacking a shared language with other disciplines, working with policy-makers' and lay publics' agendas or lacking guides for assessing the quality of often inherently novel research. These are significant concerns; they indicate that a better understanding and further support of cross-disciplinary scientific endeavours is key to meeting these grand challenges.

I examine how science studies can support interdisciplinary work, based on my empirical study of a Southampton University research project recently funded to study ageing. RCUK efforts to address grand challenges include the EPSRC's call for Complexity Science in the Real World. Complexity science is a new field that uses computer simulations to study and understand systems that are made up of parts that are dynamically changing and interacting, such as social systems. It is thus potentially very useful for studying human social systems. One of the projects to successfully respond to the call was The Care Life Cycle: Responding to the Health and Social Care Needs of an Ageing Society (CLC), based at Southampton University. Teams of researchers with extensive expertise in their respective fields but no prior record of collaboration are now coming together at Southampton University to engage in a novel, ambitious and important cross-disciplinary effort: CLC aims to understand the impact of ageing and migration on the UK's needs and resources in health and social care. How? By combining expertise in the social sciences with cutting edge complexity science tools, and getting policy stakeholders involved in the development of these tools.

My paper isolates three problems that interdisciplinary research on a socially relevant topic is expected to face (section 1): 1. Working across disciplines, specifically 1.1 sharing theories and concepts, 1.2 utilizing diverse work routines, and 1.3 using the built environment; 2. Transferring knowledge to 2.1 policy stakeholders and 2.2 policy makers; 3. Building science in a political context, specifically 3.1 managing contingency and 3.2 managing dissent. Over the past two decades science studies has produced a corpus of localized investigations of the nature of these problems in specific settings, of how these problems are handled and what kinds of successes and failures result. Despite large amounts of detailed work there is still no general blueprint to teach scientists how to negotiate these problems. The paper organises and evaluates some positive solutions proposed in the science studies literature for these problems (section 2), considers the workings of CLC to see whether current guides can be or are already implicitly being used in practice (section 3) and traces a frame for a working relationship between science studies and science researchers, in situ, and in real time for helping make science 'boundary breaking' and 'socially responsible'.

The patenting of biological artefacts. Naturalness and artificiality in practice

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An invention must meet various criteria if it is to qualify for patent protection, such as being novel, inventive and industrially applicable. First and foremost, however, the object or process for which patent protection is sought must be an invention, meaning it must constitute patentable subject matter. In the field of biotechnology, patent law regards products of nature as natural objects and therefore unpatentable whereas unnatural objects are considered to be patentable subject matter. In light of this distinction, the fact that countless patents have been granted for objects many would consider natural, such as human proteins or genes causes confusion and raises the question of how exactly the naturalness (or unnaturalness) of objects is ascertained in patent law. An analyses of two recent patent disputes in which the naturalness of isolated and purified human genes and the associated proteins was at issue reveals that the US and Europe have very different approaches when it comes to assessing the naturalness of biological objects. More specifically, in the United States a properties-based view to naturalness is applied, the so-called 'marked difference' doctrine. In Europe, on the other hand, the naturalness of an object is determined by way of the 'technical character' doctrine which in turn is best characterized as a history-based view to naturalness. In the 'marked difference' doctrine, the naturalness of an object is determined by comparing its properties to those of its naturally occurring counterpart. In the 'technical character' doctrine an invention constitutes patentable subject matter if it is 'a technical solution to a technical problem'. One might question the wisdom of framing the distinction between inventions and non-inventions as one between natural and unnatural objects. For one thing, as the analysis of the two patent doctrines shows, naturalness can be interpreted in numerous ways, and therefore, what is considered natural in one account might be unnatural in another. Secondly, most approaches view naturalness not as an all-or-nothing characteristic, but as having a gradient nature which still leaves the question unanswered where on that gradient a natural object becomes unnatural. Thirdly, by couching the concept of patentable subject matter in terms of naturalness versus unnaturalness, a potentially important feature of an invention, namely that its function must in some respect be the result of intentional human action is overlooked. By regarding inventions as artifacts rather than as unnatural objects this central aspect is given its due regard. Some implications of viewing biotechnological inventions as biological artifacts for their patentability are discussed. What's more, the possibility that patent law and practice can provide philosophers with new insights concerning the nature of biological artifacts is considered.

Waddington redux: Models of cell reprogramming

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Waddington's epigenetic landscape (1957) represents embryonic development as an undulating surface of hills and valleys. This abstract, analogical model was constructed to visually explicate concepts of an integrated theory of genetics, development and evolution. More than fifty years on, Waddington's model has been appropriated by two very different groups of stem cell biologists: systems theorists and experimentalists. Both groups use visual diagrams representing cell development as a landscape, explicitly attributing the analogy to Waddington. From this common starting point, however, the role of 'Waddingtonian' landscape models diverges. This case reveals key features of systems-theoretic and experimental approaches, and illuminates the relation of theory and experiment in stem cell biology today.

Waddington's original diagram represents developmental potential as gradually restricted over time, partitioned into distinct, diverging channels that connect the initial state of early development with multiple discrete end-states. A complementary 'underside' view expresses the hypothesis that developmental pathways are determined by networks of interacting gene products. The original landscape model is thus a forerunner of contemporary developmental systems theory. However, unlike contemporary systems models, Waddington's landscape does not yield predictions. Instead, it offers a vivid, intuitive framework for explicating concepts of a unified theory of genetics, development and evolution.

Systems biologists today aim at a similar unification, and their models of development preserve the "formal features" of Waddington's epigenetic landscape: directionality in time, multiple discrete termini from a single undifferentiated start, and robust bifurcating tracks. But systems explanations in stem cell biology are in very early stages. The current aim is to show how one cell state is transformed into another in terms of changes to an underlying regulatory network. The landscape model plays a pivotal role in these emerging explanations. By correlating cell state with developmental potential, the landscape identifies the transformations that need to be explained. The model thus imposes an explanatory order on the properties of cell regulatory networks, providing a 'scaffold' for developing systems explanations. Testing predictions by experiment is vital for 'Waddingtonian' systems models.

Experimental biologists use Waddington's landscape quite differently. The model appears in speculative surveys of "cell reprogramming," a method of "inducing" stem cell capabilities in mature somatic cells (Takahashi and Yamanaka 2006). Our current techniques are inefficient, and yield unpredictable results. In efforts to move the field forward, cell reprogrammers use Waddington's model both as a constraint on acceptable hypotheses and as a unified framework for representing diverse experiments.

These different appropriations of the original landscape model illustrate the diversity of unifying roles for abstract models in biomedicine. In the stem cell case, their complementary aspects offer prospects for synergy between experimental and theoretical approaches. The surface of Waddington's landscape may soon expand its unificatory role.

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What does the circulation of protein sequencing tell us about the history and philosophy of contemporary biomedicine?

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My paper explores the circulation of protein sequencing techniques in the context of the development of the biomedical sciences in Spain between the 1970s and 90s. I, concretely, focus on the trajectory of Enrique Méndez, a pioneer in the incorporation of these and other techniques for structural analysis of proteins. Méndez started his career in the late 60s at the Centro de Investigaciones Biológicas (CIB), a biomedical centre of the Spanish National Research Council. The CIB was the first Spanish research centre in incorporating a laboratory of molecular biology. This laboratory fostered structural analyses of proteins at a time in which biochemistry in Spain rather focused on the function of different molecules in metabolism.

Méndez continued his career at New York University and the Roche Institute, where he learnt protein sequencing techniques in the context of immunological investigations of antibody structure. Upon return to Madrid in the late 70s, he established a protein sequencing facility and engaged in collaborations with other scientists at the Hospital Ramón y Cajal, one of the first in combining medical practice and biomedical research in Spain. The impact of recombinant DNA led Méndez to move to the Centro Nacional de Biotecnología shortly after its creation and to shift his investigations to the detection of gluten in food processed for celiac patients. He developed a detection kit during the late 90s and commercialised it through a spin-off company.

The trajectory of Méndez raises significant points in the philosophy and historiography of biomedical technologies during the last third of the 20th century. Studying protein sequencing in a country which was not involved in its invention shows that the circulation of knowledge and techniques is, by no means, unproblematic. Protein sequencing was invented by Fred Sanger in the context of purely academic research during the first half of the 50s. However, Méndez learnt a different strategy proposed by Swedish biochemist Pehr Edman and always applied sequencing to medical problems, often in cooperation with the pharmaceutical and food industries. Secondly, the spread of recombinant DNA during the 80s led protein sequencing and other biochemical techniques to be perceived as out of date. Méndez and other researchers had to seek new horizons in food science and the development of kits. Biotechnology was the bridge which articulated the new and promising research on DNA with protein chemistry, shortly transformed into proteomics and presented as a complement of the rising genomic projects. Thirdly, the circulation of protein sequencing can be approached as a historical case study to investigate long-standing problems in the philosophy of biology and bioinformatics, such as the structure-function debate, the reductionist tradition of molecular biology and the shift of biomedicine from test tube experiments to computer assisted data-gathering.

Making sense of interdisciplinary explanations: a pragmatic approach

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Attempts to construct a single, unified theory of scientific explanation have failed by and large. One response to this situation is to abandon philosophical analysis and instead just focus on the practice of scientific explanation. However, this is not very attractive if it amounts to nothing more than a case-by-case description of concrete examples of scientific explanation. The challenge then, is to devise an account of explanation that is conducive to scientific practice, yet at the same time sufficiently broad so as to be philosophically instructive. A promising strategy to meet this challenge is to adopt a pragmatic approach. In this paper, I will take van Fraassen's erotetic model as a starting point. Foregoing some technicalities, this model interprets explanations as answers to why-questions. Linking question and answer is the relevance relation R .

However, a cursory glance at scientific practice indicates that interdisciplinary research plays a large and important role in science. Social sciences and psychology, psychology and neuroscience, biology and chemistry, to name just a few, mutually influence each other. It is these interactions that seem to fuel progress, sometimes even spawning entirely new fields of research. Particularly salient in this respect are *mechanistic explanations* in the cognitive and biological sciences, which tie together insights from many different levels, including behavioural, neurological, cellular and molecular. Clearly, a theory of scientific explanation should account for such interdisciplinary explanations.

Alarmingly, it is precisely in this interdisciplinary context that the pragmatic approach of explanation is wanting. If explaining is interpreted as formulating why-questions and finding appropriate answers, then inter-level explanations present us with a problem. How can an answer be relevant to a question when it is couched in terms of an entirely different scientific vocabulary? This problem, first recognized by Hardcastle, has up to now received little attention. One solution might be to somehow *translate* the answer into the vocabulary of the question, but this is reminiscent of Nagel's bridge laws, and faces problems connected with meaning invariance. Another solution would be to impose further restrictions on R to guarantee relevance. The danger here is that one lapses back into the error of claiming that all instances of explanation have some formal property in common.

The solution I propose is to take into account the interests scientists have in posing explanation-seeking questions. Explanations are sought after with specific goals in mind, and consequently, the relevance of an answer can be evaluated by considering how well it serves these goals. In the case of interdisciplinary explanations, the goal is often *control* or *manipulation*. Accordingly, I will interpret relevance along the lines of Woodward's manipulationist account of causality: the relevance of an answer lies in the fact that it suggests possible interventions. Thus, if the state of affairs the question is about has been observed to change after some intervention has occurred upon some other state of affairs, then an answer referring to the latter can be viewed as relevant, regardless of any difference of level. The advantage of this solution is that it bypasses the need for corresponding vocabularies, instead relying upon the ideal interventions of scientific experiments to link explanation-seeking questions with appropriate answers. In this way, the pragmatic approach can yield valuable insights into interdisciplinary explanations. These advantages will be illustrated with a historical case study: the explanation of depression in terms of serotonin imbalance.

Towards an overarching methodology to study the evolution of language: Identifying the units, levels and mechanisms of language evolution

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Today, the evolution of language is studied from within a variety of non-linguistic disciplines such as evolutionary biology, genetics, neurology, psychology, primatology, artificial intelligence etc. These different research fields all use their own scientific methodologies, such as fMRI studies, genetic sequencing analyses, population dynamics etc, to investigate certain aspects of language evolution. Fruitful as these methodologies and techniques might be, in themselves they cannot provide us with the tools to build adequate and interdisciplinary theories on how language evolved. In this paper, a general and overarching methodology is introduced that allows us to combine the different data provided by the variety of disciplines involved in evolutionary linguistics in order to build general theories on language evolution. This methodology takes the study of life as conducted by biologists and evolutionary epistemologists as exemplar. In biology and evolutionary epistemology, the evolution of life is studied by examining the units that evolve, the levels where these units evolve and the evolutionary mechanisms by which they evolve. The Modern Synthesis for example argues that organisms evolve in the environment by means of natural selection. By analogy, we can also look for the units, levels and mechanisms involved in language evolution. A methodology will be provided that describes how we can examine the current data on language evolution for its status of being either a unit, level or mechanism of language evolution, and how such identification allows us to build a general picture of how language evolved.

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Putting together the pieces: Building science from local labs

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The paper I propose links current inquiry into the philosophy of experiment (Sullivan 2009) with an investigation of the very same issues that was published towards the end of the constructivist movement (Knorr-Cetina 1983). What I find significant is that some of the issues described during the constructivist movement are being independently rediscovered and continue to remain unsolved. In what follows I present an abstract of my paper and mention the importance of these issues in the larger question of theory choice in science.

Experimental procedure has too often been overlooked. Sullivan (2009) is concerned with experimental practice. In particular, her point is that before we can apply scientific results to general theories of philosophy of science, we first have to understand how those results are produced because particulars of the production process may affect the applicability or scope of the results produced. Investigating two laboratories that study long-term memory in mice, Sullivan points out a number of methodological differences and how those differences affect how we can combine and interpret those results across labs. However, Sullivan is not the first to point out problems relating to the specific contingencies of research in local labs. Knorr-Cetina's (1983) ethnographic work focuses on the non-rational, local, contingent, constructed nature of the scientific process. Both Sullivan and Knorr-Cetina reject the arationality assumption, which assumes that only the social activities of scientists are available for sociological analysis. In rejecting the arationality assumption, even the so-called "rational decisions" of scientists are open for sociological analysis. For both, convenience and practicality play major roles in the laboratory.

Although Knorr-Cetina does not intend it as such, her microsociological analysis hints at ways to resolve some of the problems Sullivan points out in the idiosyncrasies across labs. First, Knorr-Cetina's microscopic analysis of scientific work serves as a method to understand the idiosyncratic decisions scientists make. A beneficent side-effect is that such focus on scientific problem-solving and methods may lead to building bridges between different labs' protocols. Secondly, Knorr-Cetina discusses the social decision-making component of science, which Sullivan overlooks. However, Knorr-Cetina accepts the constructivist project, so her analysis stops at describing laboratory practice instead of solving the problem of how science seems to progress despite its situated, contingent character, which I find unsatisfying.

In addition to the above analysis, I suggest that problem of differences in experimental protocols across labs is one of the most compelling problems in regards to theory choice and the progress of science. Although most attention has been on how theories become accepted by scientists, what Knorr-Cetina's and Sullivan's research shows is that there are potentially critical idiosyncrasies in the production processes that provide the evidence for theories and that serve as evidence in scientific controversies. The problem runs much deeper than accepting one theory over another: the same sociological and practical contingencies are at work in the data production process. I will show this connection by exploring Solomon's (2001) social empiricist epistemology of science.

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Experiments as question-generating machines – Epistemology of mathematical modelling in biology

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In recent years many philosophers of science have called for the establishment of experimentation as a philosophical object of investigation in its own right. Hans-Jörg Rheinberger among others has shown that the traditional picture of experiments as activities that exclusively test hypotheses has to be revised. Studies on recent historical examples of biological experiments point to a picture of experiments as complex question-*generating* machines rather than tools exclusively for *answering* research questions. Experimentation has the potential to raise and answer questions not postulated by researchers beforehand. That does not mean, however, that letting the experiments “talk” leads to a blind empiricism. In fact, the revised view can bridge between empiricism on the one side and the philosophical tradition that gives priority to theory on the other, so arguing for the position that we neither depict reality directly nor construct it.

Scientific experimentation is a constant realization of the “possible” that being the technical, scientific as well as the metaphysical “possible”, and these need not be the same in different fields or time periods. Scientific theories, however, cannot be constructed at will without meeting empirical “resistance”. Therefore one could say that even though questions may be generated and answered without there being a substantial theory on the research object, what counts as questions and answers respectively is not answered by nature itself. Thus, focusing on experiments as open-ended question generating machines is not at odds with a pluralistic view on science.

My paper assumes the stance that both theory and experimentation play important roles while neither on its own provides a permanent basis for scientific discovery and justification. As a part of my PhD project I will draw on Rheinberger’s historical analysis to examine the status of experimentation in *contemporary* science. Since I have a background in biology, I have chosen to focus on biological disciplines, with a case study within systems biology. I am interested in how questions and hypotheses are generated in science still “in the making”, and more specifically how mathematical modelling plays a role in empirical predictions and theory development.

Systems biology is an interesting case for philosophers of science in general, since it is at the interface of many different scientific disciplines. With its aim of understanding the interactions of components of biological systems on different levels, various disciplines are implemented in quantitative experimental analyses. Most of the classical questions of the philosophy of science can be asked in relation to the work and achievements of systems biology, but in this talk I will focus on how questions and hypotheses are generated when using mathematical modelling to explain and predict biological phenomena. The aim of the talk is not to reject the importance of hypothesis-testing experiments but rather to point to another side of experimentation as *exploratory* activities, where the experimental systems have the potential to transcend the theoretical frame from which they were built.

Engineering minds. The tools and uses of artificial intelligence

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It seems plausible to argue that the transition from “Good Old-Fashioned”, symbol-based Artificial Intelligence (AI) research to biological, especially evolutionary approaches constitutes an example of a paradigm shift. That shift apparently occurred when the older research programme was confronted with explanatory difficulties that could not be resolved within the realm of its own theories, methods and models. Thus, the notion of abstract, formalised problem-solving has been replaced by the notions of physical embodiment, environmental embeddedness and evolutionary development. However, the basic purpose of AI research as such is presumed to remain by and large unaltered, namely to create representations, models and simulations of the human mind.

In this paper, I will argue that, contrary to this convenient presumption, the very purpose of AI research has as changed as much as its theories, methods and models. This purpose has changed not only in terms of how the human mind is being conceived of. The entire subject matter and the aims of the research practice have been significantly modified.

Historically speaking, the point of departure for AI was to be found in practical demonstrations of the versatility of computer systems by endowing them with a variety of highly specified, application-oriented problem solving abilities. These relied on a subset of human intellectual capacities as their models. The creation of computer models of the human mind, marked by their reversal of the direction of modelling, was a mere subspecialty of this broader programme (in Feigenbaum and Feldman, 1963, AI is the heading of the broader programme). Much of classical AI however, and virtually the entire philosophical debates around AI, focused on computer models of the human mind exclusively, being perceived as tools of cognitive psychology — while inheriting the notions of formal abstraction and disconnectedness that were both typical and appropriate for early AI’s purposes, but not for the coping with complex, changeable environments that is typical of human action.

In trying to make up for this well-known deficit, attention of contemporary, behaviour-based AI has focused on cognitive and behavioural exchanges with the environment. In doing so, the focus of research has shifted from representations, models and simulations of the human mind to representations, models and simulations of the environment and behavioural interrelations with it. While the philosophical and psychological question of what happens ‘in the mind’, from representation to consciousness, is found to be either sidelined or suspended, the target systems of the models and the explananda of the theories are now mostly to be found in the environment, which is conceived of as a set of (natural or artificially constructed) ecological niches for a variety of possible behavioural systems, humanlike or other, to adapt to. Discussing some examples from AI research, I will seek to elucidate some of the background assumptions of this approach, and their practical and epistemical implications. What is to be found there might be both more and less than “philosophy of mind using a screwdriver” (Harvey, 2000).

Cancer molecular biomarkers: Epistemological and ethical controversies

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In this paper we evaluate both epistemological and ethical issues raised by the use of epidermal growth factor receptor (EGFR) inhibitors in cancer treatment. Cancer molecular markers are one of the most promising tool of genetic medicine, in particular mutations in the KRAS gene are used in the clinical practice to predict whom patients will respond to therapy with EGFR inhibitors. Since the discovery of cell growth promotion through the EGFR signal transduction, during the mid-1960s, this receptor has been regarded as a suitable antineoplastic target. From the knowledge of EGFR signal transduction it seemed reasonable that only cancers which do not present mutations in the KRAS oncogene responded to anti-EGFR treatment. The intracellular KRAS pathway apparently justifies the use of this molecular biomarker in the decision-making of anti-EGFR treatment. Although in vitro experiments have shown evidence that KRAS mutations are the cause of resistance to anti-EGFR drugs, only a part of cancer patients with no KRAS mutations respond to these drugs and several clinical unexpected results are emerging. Contrary to what was expected from the inhibition of a receptor's signal transduction, a high degree of modulation and plasticity and therefore a high degree of uncertain clinical outcomes are found. The mutational status of several other molecular markers (BRAF, PIK3CA and PTENI) are now discovered as an integral part of dissecting anti-EGFR resistance. Prediction of respondent patients is the principal goal of personalized medicine but the use of a single biomarker in a complex and plastic network of cancer cellular pathways has limited value in identifying the right patients to be treated. The recommendation of testing KRAS mutations as a predictive biomarker for selection of patients has now limited value and considerable complexity has been added to the clinical decision making. The need to identify respondent patients is also urged by the high cost of anti-EGFR drugs, however the identification of target patients will require a great number of biomarkers and will always retain a degree of uncertainty due to the interconnections of receptors pathways and the several mutations in cancer cells. The empirical observations on the use of cancer molecular biomarkers to choose anti-EGFR treatment show critical epistemological aspects and suggest that ethical considerations, such as diversity in patients preferences and values, should impact the clinical decision making for these extremely expensive drugs. In accomplishing this task of integrating patients' choices in evaluating scientific evidence, fair and sympathetic communication with potential drug recipients is crucial in light of medical ethics. The patients should be informed by the physicians not only on the limited extension of survival, but also on the high variations of individual cancers, which are the background of the limited success of anti-EGFR treatments.

The non-impact of sociophysics

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Each year more than 60 papers on the topic listed as “social and economic systems” by PACS are published in important physics journals, like *Physical Review E* and *Physica A*. Of these about half are about economical issues and the other half discusses other social subjects, like vote dynamics, migration patterns, social network dynamics, etc. In this paper, I will concentrate on this second half, often called sociophysics. Sociophysics is a slowly but steadily growing research field in terms of number of publications and conferences. However, it seems that this research has virtually no impact on more traditional social sciences disciplines. The current paper is an exploration of possible reasons for this fact.

At first we could think that sociophysics suffers from a lack of visibility in social sciences. It is true that most sociophysics papers are currently published in physics journals. But it was not always the case, of the earliest papers published in sociophysics, before 1989, nine out of eleven appeared in social, psychological or interdisciplinary journals. At least three introductory manuals to sociophysics are available. Sociophysicists have tried to reach social scientists but did not succeed. The question of competence is not an issue either. Many social scientists are well trained in mathematics and computer simulations. They could read and understand sociophysics papers if they wanted to.

If social explanations cannot be completely put aside, we believe that philosophical reasons are potentially more convincing to explain the non impact of sociophysics. From the social scientist's point of view, sociophysics models are methodologically and ontologically bizarre. On the one hand, they do not rely on accurate models of what is happening at the micro-level (i.e. on the level of human individuals) but neither are they adopting a holistic approach at the macro-level (i.e. on the level of social entities). By avoiding to choose between an individualistic and a holistic approach, sociophysics models could be accused of lacking explanatory power. By contrast, multi-agent simulations seem to explain emerging social phenomena on the basis of accurate modeling of individual agents. Non-reductionist theories require the use of macro-sociological elements as explanations. Sociophysicists postulate the social level as governed by relatively few order parameters and their main goal to derive from the general principles of statistical mechanics dynamic equations for these order parameters. In order to do so they have to connect individual actions at the micro-level to the macro-level parameters. To build this connection, only a very simple probabilistic model of individual actions is needed. Statistical mechanics research shows that only certain broad parameters of the micro-level are important to model the macro-level. We only need to know little information about molecule movements to be able to model a gas behavior. Here is the social scientist probable puzzlement. On the one hand, sociophysics reduces the macro-level to the micro-level ontologically, but on the other hand the macro-level behavior is not explained by a careful study of micro-level features because statistical mechanics explanations rely on statistical collective effects where the individuals' details are proven non pertinent. If statistical mechanics is good enough for physics why shouldn't it be for social sciences?

The nature of exploratory experimentation in the life sciences

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In recent years, philosophers of science have shown renewed interest in the nature of experiments and the roles they play in science. In this context, Friedrich Steinle and Richard Burian have introduced the notion of “exploratory experimentation” (EE), which describes a form of experimentation that is not theory-directed and that depends on what one could call ‘flexible’ or ‘loose’ experimental setups that allow for a great variation of experimental parameters (Burian 1997; Steinle 1997).

In my talk, I want to analyse the notion of exploratory experimentation in the context of the life sciences. I will defend the view that 1) EE, at least in the life sciences, can be theory-driven and that 2) no special ‘flexible’ experimental setup is required for an experiment to be exploratory in nature. I will support these claims with a case study from the research on protein kinases.

1) EE is generally seen as being theory-*informed*, but not theory-*driven* (Waters 2007). The aim of EE is not to test specific theoretical expectations, but to discover new regularities, or to help develop and stabilize new phenomena or concepts.

Building on this initial picture of EE, Kevin Elliot developed a taxonomy of EE (Elliot 2007). Interestingly, his taxonomy suggest that EE can also be theory-driven. This is in contrast to previous views of EE and it was suggested that Elliot’s taxonomy should rather be understood as an analysis of whole research *programs*, which can contain both EE and theory-driven experiments (Waters 2007).

I will defend Elliot’s claim that EE can include theory-driven experiments using an example from molecular life sciences, namely protein kinase research. I will first discuss the experimental assays put to use in this field of research. I will then show how theory has directed both “classical” and exploratory experiments using these assays.

2) Another feature of EE that is often mentioned is its use of ‘flexible’ instrumentation. Using again the example of kinase research, I will show that the use of ‘flexible’ setups is not a necessary feature of EE; the same experimental system can be used both for testing theories and for exploratory experiments. There is no difference in the number of parameters that can be manipulated or a difference in the ‘flexibility’ of the elements of the experimental system.

More specifically, I will show that the kinase assay is used both in a “testing-a-specific-expectation” context and in an exploratory context. This analysis will demonstrate that the crucial difference does not lie in the nature of the experimental assay per se, but in the nature (and the combination) of the controls and read-out systems put to use in the respective experimental system.

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Good* research on gender differences in mental health

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Mental health research, like research in other clinical sciences, involves values at multiple stages — for example, in defining a set of traits or behaviors as a disease or disorder, in choosing ethical methodology, in setting significance levels to determine efficacy, and in decisions to promote one's results (e.g., Wakefield 2000; Sadler 2002; Wakefield 2002; Anderson 2004; Fulford and Colombo 2004; for contrasting views see, e.g., Boorse 1975; Boorse 1976; Schwartz 2007). The research is also often fraught with epistemic uncertainties, given lack of knowledge, changing circumstances and constructs, and individual variability. Frequently, then, epistemic uncertainties cloud the data *and* the various values motivating or guiding the research are in conflict. In these complex cases, research can be “good” only when it properly manages both the uncertainties and the values. When it does so, the research and its results are good*: good in a sense that includes both sets of evaluations.

The claim that “good” should be understood as good* in clinical research follows a pragmatist tradition, not demarcating values sharply into epistemic and nonepistemic or sharply dichotomizing facts and values (e.g., Putnam 2002); for a contrasting view, see, e.g., (Douglas 2009). Feminist influences are also important, emphasizing the contextual nature of evaluating research, and the need to retain a rough fact/value distinction that allows critique of facts and fact formation on the basis of values, and values and value formation on the basis of facts (e.g., Anderson 2004; Wylie and Hankinson Nelson 2007).

Study of gender differences in attention-deficit/hyperactivity disorder (ADHD) illustrates the complexities of determining whether research is good* (e.g., Arnold 1996; Hermens, Kohn et al. 2005; Bauermeister, Shrout et al. 2007; Clarke, Barry et al. 2007; Lahey, Hartung et al. 2007; Ek, Westerlund et al. 2008; McKee 2008; Mikami, Hinshaw et al. 2008; Qui, Crocetti et al. 2009; Biederman, Petty et al. 2010; Valera, Brown et al. 2010) Values conflict in this body of research: clinical values suggest that it is important to know about male/female differences in ADHD presentation or treatment responsiveness. In potential contrast, feminist values decry generalizations about mental disorders that stereotype or essentialize (ADHD diagnosis and treatment are also controversial). Gender difference/similarity in mental disorder is epistemically ambiguous in that the causal space for the role of gender is broad and difficult tease apart: gender difference/similarity might stem from “nature,” “nurture,” nature/nurture interactions, temporary developmental considerations, the effects of comorbidities, or alternatives. In addition, the fact that mental disorders are defined according to symptoms raises the conceptual issue that if women and men have different symptoms, they may have different mental disorders.

The utility of determining whether research is good* is not to prescribe or proscribe research projects. Instead, the point is to facilitate robust discussion of constructs, questions, methods, and analyses, so that clinical research can progress beyond the myth of its value neutrality to confront or support the values embedded in its approaches. Importantly, a full understanding of good*, as it includes epistemic values among others, would not typically allow value-valenced criteria to quash exploration of new hypotheses. Yet how one balances the conflicting epistemic and valued possibilities is not at all clear. As a start, I suggest that one must have a tentative stake in the ground on both sides of the fact/value divide. Empirical adequacy can represent the minimalist epistemic criterion (Longino 1990) and, to take a criterion that may be acceptable in a range of ethical theories, a respect for humanity can be the minimalist stake for ethics, at least in clinical research.

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Some thoughts on statistical models and reality

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Statistical reasoning is strongly based on probability models. Although statisticians do not believe that these models hold precisely, it is emphasised in the literature and in teaching that it should be checked whether assumptions hold at least approximately whenever using a statistical method that is based on them.

However, checking whether assumptions hold approximately is not a well-defined problem and it could be argued that if this is meant in a statistically well defined sense, it is no less illusory to believe that they can hold approximately than that they can hold precisely.

Some elements of a constructivistically inspired philosophy of statistics and data analysis will be presented that attempts to make the role of models and the meaning of "checking them" clearer.

A constructivist concept of how general mathematical models relate to reality will be outlined, incorporating the concepts of "observer-independent", "personal" and "social reality" (mathematical models are essentially a part of the latter).

I will then comment on the two most widely used interpretations of probability, namely the frequentist and the Bayesian one, and show that the problem which one to adopt is rather a question of subject-matter dependent choice than of finding the "right one", taking into account observability and identifiability of aspects of the models.

Model-based characterisation of the behaviour of statistical methodology will be treated as one aspect among others of understanding what the methodology does. This can enable the researcher to make informed, but ultimately subjective decisions, which are required in data analysis, because the data alone cannot tell us the truth.

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Extending Darwinism to culture: Population thinking or natural selection? Towards a practice(s)-based view

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A century and a half after Darwin's *Origin*, a substantial number of interconnected scientific communities are making a sustained effort at extending evolutionary theory to the human-made world of culture and technology. Recently, there appears to be some consensus among philosophers of science (Lewens 2009, 2010; Godfrey-Smith 2009) that the gist of "the best" theory of cultural evolution — namely the dual-inheritance theory of Boyd and Richerson (1985; 2005) — should be understood as extending population thinking to the human-made world. This would mean that Darwin's other revolutionary insights, most notably that of "evolution by natural selection", would play at most subsidiary roles and may even be limited to the biological realm. This undermines, and is even actively directed against, another view of extended-evolution projects, namely the "generalized Darwinism" associated with Dawkins and especially Dennett (1995), who emphasizes that evolution by natural selection may function as a "universal acid" regarding virtually any complex system.

The first part of my paper is devoted to arguing that the 'population-thinking' image of the extended-evolution programs is too reconstructive of actual research practices in these programs.

I offer two reasons. Firstly, the population-thinking image underrates the effort Boyd, Richerson and their various students and co-authors make to model the conditions under which populations of cultural traits evolve by natural selection. Rather than a subsidiary to a more general population-thinking program, evolution by natural selection is central to their explanation of what they call the "gradual accumulation of cultural cognitive capital" — which is a prime explanandum of their program. Secondly, while Boyd and Richerson's dual-inheritance theory is undoubtedly a successful and growing research program, it is hardly the only line of inquiry in extended evolutionary theories — and it is hard to find independent reasons for Lewens' contention that dual-inheritance theory is the "best" current effort to extend evolution to culture, apart from a prior interest in limiting this effort to population thinking. There are many other advocates of evolutionary approaches to culture in general (e.g., Mesoudi et al. 2004; 2006), economics (e.g., Andersen 2004; Saviotti 1996) and archaeology (e.g., O'Brien and Lyman 2002). Explanation through evolution by natural selection is also central to these approaches, both in their self-presentation, in the motivation of their research efforts and in deriving or formulating research results. In all cases, research efforts in these programs would be misspent if the gist of extending Darwin's heritage were population thinking; and self-presentations of the research programs would be misleading.

In the second, shorter part of the paper, a less reconstructive image of extended-evolution programs is presented. This image starts from and underwrites Lewens' observation that extending evolution to culture not necessarily involves applying the full neo-Darwinist conceptual framework or its wide variety of modelling techniques. Current research, in dual-inheritance theory and in other programs, may involve more than population thinking, but it does not support Dennett's "universal acid" image of Darwinism, nor Dupré's (1994) suspicion of "scientific imperialism". Rather, there appears to be a variety of tailor-made Darwinisms specific to various fields and research programs, all of which include a diverse selection of modelling techniques and an integration of evolutionary explanation with disciplinary background knowledge. The contours of these tailor-made Darwinisms are sketched for research in (evolutionary) archaeology and economics.

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Climate change and carbon rationing

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One of the aspects of climate change that has received relatively little attention from philosophers is the proposal that states reduce their greenhouse-gas emissions by issuing 'personal carbon allowances' (PCAs) — also sometimes called 'domestic tradable quotas' (DTQs), or 'tradable energy quotas' (TEQs) — to each of their citizens. According to this proposal, citizens would be required to surrender PCAs in order to engage in various greenhouse-gas emitting activities. The number of PCAs issued each year would decline, so as to ensure a year on year reduction in national greenhouse-gas emissions. The present paper argues that a system of PCAs allows policymakers to regulate greenhouse gas emissions in a manner more sensitive to issues of justice than alternative schemes. The paper asks how PCAs should be distributed among members of a state. In so doing, it distinguishes between two rationales for a concern with the distribution of PCAs: the *rationing case*, which requires that PCAs not be tradable; and the *initial distribution case*, which allows that PCAs be tradable. In considering the rationing case, various arguments for not permitting that PCAs be traded are considered and, ultimately, rejected. In addressing the initial distribution case, the paper examines the normative merits of three proposals for introducing PCAs into a market, including proposals made by advocates of PCAs and proposals based on distributional principles used in existing schemes like the EU Emissions Trading Scheme (ETS). These proposals are: that PCAs be auctioned to individuals, that more PCAs be given to those with historically high emissions (grandfathering), and that all individuals be given an equal quantity of PCAs. The paper rejects all three of these proposals, instead defending a new approach to the distribution of PCAs, according to which PCAs should be distributed so that, insofar as is possible, the distribution does not affect any agent's opportunities for welfare any more or less than other agents, as a result of circumstances beyond the agent's control. The paper concludes by discussing obstacles to putting this proposal into practice and suggests that policies based on the proposal would need to be introduced gradually over time as we learn more about the carbon consumption needs of a population.

Computational science and human understanding: The role and logic of scientific sketches

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The notion of explanatory sketch is used by Carl Hempel to describe explanations falling short of the logical standards of complete explanations; their philosophical analysis, as the one of elliptic explanations or enthymemes, is described as essentially belonging to pragmatics. They are at best seen as preliminary steps in need of elaboration and supplementation of their ideal and complete explanations; they only have vicarious virtues, borrowed from their ideal counterparts — unless one turns to question in the philosophy of education and popularization.

In this talk, it is first argued that, given the widespread presence of computational science, the use of scientific sketches is now unavoidable not only for the purposes of education but also in the very functioning of scientific research; thus, if one is to understand how science can work in practice, this notion needs more philosophical scrutiny. Indeed, it is now possible to produce computational proofs, theorems, simulations, etc. which can no longer be surveyed by human minds. At the same time, the role of the human mind is more central than ever, since humans and human understanding are still the “control unit” that tries to monitor and control computational science and to benefit from its results both for applied and theoretical purposes. In this perspective, it is crucial to understand how human minds can develop some grasp on this no longer human-sized science and regarding this aim, sketches play a central role, even for research purposes — or so I shall argue. For example a sketch of a proof may decompose it into modules and display the strategy for going through each modules, without going into the gory details; in turbulence studies, drawings may describe how some typical phenomenon arises — even if the real and reliable explanation requires to go through heavy processing. In these cases, just like a quickly drawn sketch of a person does give you a chance to recognize her, scientific sketches do provide some understanding of what needs to be otherwise studied at computational long length.

The core of this talk is devoted to the discussion of some central properties of sketches. Even if there is little doubt that a fine-grained account of scientific sketches requires taking into account various elements such as context or users, as well as distinguishing different types of sketches (e.g. visual, linguistic, etc.) my purpose is to characterize the set of minimal ingredients that should be included in a sound and meaningful, even if coarse, definition of scientific sketches. I shall discuss in particular the following points. *i.* Representations can obviously be sketched but many other scientific items can be sketched such as proofs, justifications, results, explanations, predictions, etc. *ii.* Sketches are irreducibly relative to an original item, but what these items are is not always clear. *iii.* How should the quality of a sketch be defined? How much a sketch can be grasped by a human mind is no doubt crucial. But should one not consider how much a sketch efficiently summarizes its original? And does not this (annoyingly) imply that the properties of a sketch depend on the properties of its original (such as its length, absence of redundancy, etc.). *iv.* To what extent can sketches replace their original by fulfilling the same functions? And finally, can they play roles that cannot be played by their originals?

I finally emphasize that many versions of the representations, proofs, predictions, explanations, etc. that can be found in scientific books or articles would best be analyzed as sketches.

The role of scientific practise in eliminativist claims: A case study of consciousness

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Eliminativist claims are often based on the role of a concept in scientific practise. Debates over the elimination of the concept 'species' (Brigandt, 2003, Ereshevsky, 1998) and 'emotion' (Griffiths, 1997) refer to the epistemic and heuristic role that the concept plays in the relevant science. For example, if it can be shown that that the concept cannot continue to be used to state progressive research questions then it can be eliminated. Many eliminativist claims about 'consciousness' (Dennett, 1991, Churchland, 1996, Wilkes, 1984) have so far failed to use this strategy of investigating the actual practices of consciousness science to motivate their claims. I will argue this strategy is a crucial one that can provide deeper reasons for taking eliminativism about consciousness seriously.

The central argument is that many of the methods used in consciousness science fail to fulfill their standard heuristic roles of redefining terms, providing taxonomies of phenomena, or suggesting new research questions. For example, dissociations in behaviour are typically used to identify functionally independent cognitive processes, and to suggest new research questions (Shallice, 1988). However, these methods are misused in consciousness science as dissociations in behaviour are overinterpreted, are not used to generate accurate taxonomies, and therefore fail to generate new research questions.

Secondly, integrative approaches that attempt to combine a set of measures or theories over different levels of analysis encourage inter-level refinement and are often extremely fruitful (Craver, 2007). However, attempts to integrate different behavioural and neurophysiological measures of consciousness, (as proposed by Seth et al. 2008), fail as it can be shown that different measures are either parasitic on each other, or measure entirely different phenomena, so are fundamentally incompatible. This means that integrative approaches are simply unfeasible.

Similarly, making identity claims between different levels of analysis can be used to mutually refine the concepts used at all levels, and therefore to promote new research (McCauley and Bechtel's Heuristic Identity Thesis, 2001). In consciousness science, identity claims can be found in the project to establish the neural correlates (identities) of the contents of consciousness (content NCCs). However, detailed knowledge of experimental work at lower levels of analysis is rarely used to inform the concepts at higher levels that characterise the contents of consciousness. Identity statements are rarely used to challenge concepts at either level, so fail to provide any new and truly inter-level research questions.

These points suggest that an analysis of scientific practise provides support for eliminativist claims about consciousness science. Further investigation into the implications of the proper application of scientific methods in this area likely to be very informative.

The inconclusiveness of theoretical virtues

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Theoretical virtues, such as simplicity, unification, fertility, are employed in cases of underdetermination, to justify why we privilege one out of a set of empirically equivalent theories. Scientific realists believe that these virtues have epistemic significance and treat theories that exemplify these virtues as approximately true. Anti-realists do not ascribe epistemic significance to these virtues but regard them as convenient devices for choice. However, according to both views, the theory virtues lead to a conclusive choice between underdetermined theories.

I challenge the view that theoretical virtues can conclusively decide between empirically equivalent rivals. Genuine empirically equivalent rivals exemplify important theory virtues and as a consequence we can justify choosing them over their rivals. Thus, the significance and importance of each virtue needs to be considered and weighted. How this is done, however, cannot be described algorithmically. By pointing at the virtues possessed by the theory we cannot justify the outcome of our choice to the exclusion of other choices. Even if we grant that all scientists agree on the importance of these virtues, they have the freedom to employ them differently and therefore might make different choices based on the same list of virtues.

I argue that theory virtues cannot lead to conclusive choices in cases of underdetermination because they are themselves underdetermined by the theories exemplifying them. To illustrate this, I present the problem of underdetermination in quantum mechanics. I argue that a case can be made for and against each rival by prioritising a particular theory virtue over another. As a consequence, a choice between these rivals, based only on their virtues, cannot be conclusively established. For example, adherents of Bohmian mechanics can argue that their theory is superior because it provides a deterministic solution of the measurement problem and is continuous with classical mechanics. But why should determinism and conceptual continuity with an already established framework be regarded as a superior virtue? And should it weigh more than the virtue of simplicity? Bohmian mechanics is deterministic but this comes at the price of simplicity. By introducing the 'guiding' equation, it is less mathematically elegant and simple in comparison to the Everettian formulation. The latter, however, is attacked on the grounds of not satisfying the criterion of ontological economy. Even though both Bohmian mechanics and dynamical collapse theories exemplify important virtues, they are regarded as *ad hoc* and also do not score on the virtue of unification. Since they are non-local, they are incompatible with relativity, a fact which makes it difficult to see them being unified into a future quantum field theory. Does unification then count against them and favour the Everettian interpretation? I argue that the traditional list of theory virtues is not sufficient to conclusively favour one rival or justify why we should favour it and as a consequence theoretical virtues should not be regarded as providing a solution to the problem of underdetermination.

Experiments in the Social Sciences: Rethinking the Hawthorne Effect

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In the last years, and particularly since the late 1990s, the number of social science articles vindicating the experimental method as the ideal means both for the generation of primary data and of empirical evidence in support for a given theoretical stance has increased notably, even in some social sciences in which its use was traditionally thought of as difficult or even impracticable, such as sociological theory, political science, and above all, economics (Levitt and List, 2007; Morton and Williams, 2009). This is the background motivation for this paper: the revival of experimentation in those social scientific disciplines that lack a long standing experimental tradition (in particular, economics) is yet to be met by a subsequent effort on the part of methodologists and philosophers of science in order to adapt existing theoretical accounts of experimentation to the terrain of the social sciences. Although exceptions do exist (see e.g., Guala 2005), many of the methodological discussions around experiments in economics are framed with the terms and accounts that were developed attending to the characteristics of the natural sciences and their patterns of experimentation.

In particular, the commonly held picture is that experiments in social science are best understood by reference to the well-known structure of controlled variation of the putative cause; and that the best way to capture the effect of the putative causes is to calculate the difference in means in the variable of interest of the treatment and control groups. However, and as it will be argued in this paper, many experiments in behavioural economics, partially following on a long-standing tradition in social psychology, do not follow that structure, but instead, are best defined by the experimenter's recreation of a complex scenery (i.e., an artificial environment) that is supposed to let the inner capacities and dispositions of the experimental subject emerge, in a setting that is nothing alike the idea of controlled variation of potential causal elements one by one. In this respect, the well-known family of experiments associated to the Ultimatum Game provides an example that will be elaborated upon. In this paper it is argued that although the majority of experimenters in the area of behavioural economics do follow the "controlled variation" scheme in the presentation of their experimental results, this logic does not always properly account for the design of the settings with which they present their subjects.

The paper finally focuses on a particular undertheorized phenomenon in the experimentation of the social sciences: the Hawthorne effect, generally defined as the problem in experiments whereby subjects' knowledge that they are in an experiment modifies their behaviour from what it would have been without the awareness of being an experimental subject. Although the Hawthorne effect is often mentioned in the methodological discussions of social science experimenters and in virtually all research design textbooks, the notion is often unsatisfactorily depicted, where contradictory definitions can be found in the literature. We review the common standard methodological strategies of experimenters for dealing with 'reactivity', as the Hawthorne effect is often referred to (Adair, 1984; Chiesa and Hobbs, 2008). Our argument is that the Hawthorne effect is more than a methodological annoyance, and that it needs to be addressed by an epistemology of social science experimentation that is now on the making.

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Is medical research more like building a house or more like hitting a nail?

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One important question in recent social epistemology and philosophy of science, discussed by authors such as Gilbert, Wray, Pettit and Bird, is whether groups can have beliefs or knowledge in a way which is not straightforwardly reducible to the beliefs or knowledge of members of those groups. A less well-explored, but related, question is whether we can talk of the activity of scientific research as something which is carried out by groups of researchers, where that activity is not reducible to the sum of the activities of members of that group. In this paper, I explore this latter question and some of its implications.

In the first part of the paper, I build on Pettit's work to argue that we can and should adopt a "social model" of research; that is to say, at least some claims that a group is researching some topic are not straightforwardly reducible to claims about the actions of individual members of that group. I distinguish this claim from the more familiar claim that scientific research must often rely on trust in the testimony of others and on background social norms. I also distinguish this claim from recent work on "Actor Network Theory".

In the second part of the paper, I then go on to consider cases, such as medical research, where research involves experimentation on human subjects. Building on work by Gilbert, I argue that in such cases, rather than see research as something which is carried out by the researchers on subjects, we should, instead, see research subjects as themselves parts of the "joint subject" which is doing the researching. Much medical research seems indistinguishable from medical treatment. I suggest, however, that the two practices can be distinguished because the former, but not the latter, involves a joint subject. More poetically, medical research is more like a case where you and I jointly build a house, whereas medical treatment is more like a case of hitting a nail.

In the third section, I outline three important normative implications of my arguments: for how we conceptualise the role of informed consent in research, rather than treatment, contexts; for how we should choose between different research topics in medicine; and for how we assign rewards for successful research. I suggest that all three of these topics are joined by a common thread: if we ask someone to help us in our research, then we seem under some obligation to allow them to dictate the terms of their involvement, and some obligation to reward them if we are successful in our goals.

I conclude by noting how my arguments might relate to broader strands in social studies, how emerging trends in the organisation of medical research (most notably, the "off-shoring" of research) might disturb my arguments, and how the normative implications of my arguments in the case of medical research might or might not extend to other scientific contexts.

The interdisciplinarity culture of the new technosciences

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The label technoscience refers to the realm of nano-, bio-, info- and cogno sciences and technologies. It not only marks a specific array of emerging research fields but also points at changes in the related research practices and cultures. An observed convergence of science and engineering goes hand in hand with new modes of interdisciplinarity. Collaborations between the engineering sciences, natural sciences, humanities and arts seem to be flourishing in this context. One could also speak of a specific new type of technoscientific interdisciplinarity that is oriented toward pragmatic eclecticism (cp. Weber 2010). Such interdisciplinarity seems to build upon similarities and compatibilities resulting from the use of transdisciplinary approaches, joint research practices and technologies.

'Pragmatic eclecticist' interdisciplinarity not only brings along a different relation between the sciences, it also goes hand in hand with a new relation between science and engineering. While in new research fields such as synthetic biology, biology adopts an engineering approach (focussing on control and construction rather than understanding and representation), it is also argued that the newly emerging technologies have become biology (e.g., because the application of organic matter and organisms within technological systems necessitates in-depth biological knowledge and know-how). Moreover, research and interventions addressing ethical, legal and social aspects of synthetic biology (so-called ELSA research), lead to an increasing interaction between social scientists, historians and philosophers with synthetic biologists. On this basis, another very specific kind of interdisciplinary collaboration is emerging and goes on to evolve (cp. Rabinow 2009).

For these new settings of interdisciplinarity, a detailed characterisation and a broad methodological discussion still need be accomplished. Although Schmidt (2007) put forward a typology of interdisciplinarity that is especially apt to address the new technosciences, further elaborations seem necessary to grasp the fundamental differences between technoscientific interdisciplinarity and other modes of ID.

The presentation will delineate the multidisciplinary research culture of systems biology and synthetic biology. It will draw upon interviews with scientists, visits to research laboratories and research conferences as well as on literature on epistemic cultures (e.g., Knorr-Cetina 1999). This research is part of a larger project on epistemic presumptions and socio-cultural implications of systems biology (research project "Towards a Holist Conception of Life?", funded by Austria and Germany within the transnational ELSAGEN funding initiative).

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Reasoning by concrete imagined cases in science and its relation to thought experiments

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Scientific argumentation often involves evoking imagined cases that could be materialised, in principle if not in practice. These are concrete cases as they include specific details needed for the argument, and for showing their relevance for the real world. Scientists employ such non-empirical cases in the daily processes of extending and elucidating theories and their consequences, as well as in rethinking their theories and concept. Philosophers and historians often described the latter use as “thought experiments.” In this talk I will draw on my historical research to discuss a few examples of scientists employment of imagined cases in the physical sciences of the 19th and 20th century (chosen due to time limits from thermodynamics, photo-electricity, piezoelectricity, relativity and electric technology). I will use these examples to examine the argumentative roles of these imagined cases and the strategies for generalising from their particularities. My analysis shows that in some cases (which are philosophically more interesting) the particulars of the imagined cases are essential for the argument. In other words, concrete examples provide a powerful tool for theoretical analysis. In addition, by viewing thought examples as a sub class of larger use of imagined examples in science, this analysis provides a fresh perspective on the controversy about their nature. In particular it pose a challenge to a non-argumentative interpretation of thought experiments.

A role for epistemic virtues in archaeological practice? The case of 'epistemic beneficence'

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Introduction

There is growing interest in the role of 'epistemic virtues' within the history and philosophy of science. As yet, however, there have been few 'case studies' of how specific virtues may function within scientific practice. This paper addresses this lacuna by exploring how one virtue — 'epistemic beneficence' — can be used to provide descriptive and normative accounts of archaeological practice. The paper has three parts, outlining the virtue of epistemic beneficence, its application to archaeology, and a 'case study' of the phenomenon of 'engrossing'.

Epistemic beneficence

My account uses 'virtue-responsibilism', emphasising the role of epistemic virtues in the formation of 'virtuous' epistemic agents; that is, those whose conduct is both epistemically productive and ethically praiseworthy. The application of virtue responsibilism to scientific practice, specifically archaeology, is justified by appeal to recent work by David E. Cooper (2006) and Robert C. Roberts and W. Jay Wood (2007). I provide an account of the virtue of 'epistemic beneficence' and distinguish it from the related, but distinct virtue of 'epistemic generosity'. An epistemically beneficent person ensures that epistemic goods are available to others and takes active measures to ensure this.

Archaeological artefacts as epistemic goods

Part two applies epistemic beneficence to archaeology by focusing on how artefacts — ceramic, technological, biological and so on — become 'epistemic goods'. Archaeological artefacts become epistemic goods only when three conditions — 'awareness', 'availability', and 'accessibility' — are fulfilled. Epistemic beneficence consists of a disposition to ensure that these three conditions are fulfilled such that archaeological artefacts become, and remain epistemic goods. This is illustrated with two examples — that of the 'beneficent' and 'maleficent' archaeologists — which indicate how beneficence requires the 'co-operative' activity of other, related virtues (and how maleficence, conversely, has co-operative vices).

'Engrossing'

Part three then applies my account of the virtue of epistemic beneficence in archaeology to an example of 'maleficence', namely, 'engrossing'. This refers to the wide variety of behaviours, both active and passive, whose consequence is the removal of objects of cultural and epistemic value — such as archaeological artefacts — from public and professional access. Engrossing is therefore a form of epistemic maleficence, since it undermines one or more of the three conditions under which artefacts become epistemic goods. Engrossing is an 'epistemic harm' because it impairs our capacity for learning, understanding and knowledge and indicates a failure of epistemic beneficence.

Conclusions

The virtue of epistemic beneficence applies to all those involved in the production and distribution of epistemic goods. This primarily includes scientists, but also extends, in the case of archaeology, to curators, buyers, collectors and others whose activities affects the 'conditional' status of archaeological artefacts. Since all of these persons and professions can affect the status of archaeological artefacts as epistemic goods, they are all bound to manifest the virtue of epistemic beneficence. Failure to do so, whether accidental or deliberate, generates the vice of epistemic maleficence and opens those persons to ethical criticism.

'Engrossing' is one form of the deliberate violation of epistemic beneficence. I conclude with some proposals for future studies of the role of epistemic virtues in scientific practice.

The limits of scientific integrity in practice

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A preoccupation with the integrity of the scientific enterprise brings with it specific commitments about what science is, and what it ought to be. Indeed, too strong an emphasis on scientific integrity may distract us from other important, perhaps essential, aspects of successful scientific practice. The American National Academy of the Sciences has defined 'scientific integrity' in terms of both individual and institutional adherence to honest and publicly verifiable methods. These principles have been more recently reaffirmed in the 2010 "Singapore Statement on Research Integrity." I test this definition of scientific integrity against a specific case: the decision-making process which preceded the 1986 Space Shuttle Challenger disaster. Through a discussion of this case, I seek to demonstrate two complementary points: first, that a too-strong focus on integrity could blind us to the possibility that reliable science may also depend upon methods which are honest but not publicly verifiable; and second, that some commentators have judged the integrity of science against standards which are overly idealized and inappropriate, thus promoting an image of science which fails to capture its reality as an extraordinarily complex, fallible, messy, but for all that wonderfully reliable, enterprise.

Following the Challenger disaster, a U.S. Presidential Commission found that the integrity of NASA's Shuttle program had been compromised by individual "managerial misconduct." Sociologist Diane Vaughan has convincingly challenged this conclusion, arguing that NASA managers abided by all internal NASA rules and norms for flight-readiness assessments. Yet she too concludes that the integrity of the Shuttle program was compromised, though as the result of an "institutional mistake." I argue, in contrast, that the integrity of the scientific process at NASA was compromised at neither the individual nor the institutional level. Both the U.S. Presidential Commission and Vaughan were blinkered by an overly idealized and inappropriate conception of scientific practice. Furthermore, it was because NASA officials were so concerned to maintain the integrity of their flight-readiness assessment that they ultimately chose to ignore vital evidence: expert knowledge, which was honest but not publicly verifiable, suggesting that the launch be postponed. On this basis, I argue that standard accounts of scientific integrity fail to capture an important, perhaps essential, aspect of successful scientific practice.

These considerations point to further questions about the nature of scientific expertise and its relation to the concept of scientific integrity. We need to know what expertise is, and whether or not it can always be sufficiently explicated in terms of publicly verifiable rules. This difficult question will have consequences for our understanding of, as well as the importance we place upon, the concept of scientific integrity, and also for the way we navigate over the rough waters running between successful science, on the one side, and democratic accountability, on the other.

Aspiring consensus in scientific practice: Grasping consensus driven motivations by introducing a continuum ranging from consensus conferences to meta-analysis

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In this paper, I propose a way to grapple consensus driven motivations that are apparent in many sciences - i.e. climate science, medical science and psychology - resulting in either consensus conferences, meta-analysis or something in between. My research will focus on the way in which assessment reports are produced by the IPCC (Intergovernmental Panel on Climate Change), the CAG (Canadian Association of Gastroenterology) and the Psychological Bulletin. What I propose is that their different ways of handling research results - and moreover aspiring consensus - have to be seen as part of a continuum.

In the first part, I present an account of scientific consensus, which will help us to get a satisfactory grasp of what 'consensus' entails in these diverse organisations. This notion is based on Habermas' consensus theory (1971), Fuller's essential consensus (1986) and Beatty's deliberative acceptance (2011); wary of negative influences associated with accidental consensus (Fuller, 1986), joint acceptance (Gilbert, 1987) and the unanimity requirement (Solomon, 2006).

In the second part, an analysis of the review process behind the organisations' different assessment reports will be put forward. On the one hand, my notion of continuum is grounded by the fact that these organisations all share the same benefits. This is done by referring to the notions of reliability, fecundity and practical benefit as proposed by Thagard (1999). On the other hand, the extent to which the organisations appeal to deliberative interaction between actors serves as the criterion for ascribing them a place on my continuum.

This descriptive analysis allows me - in the final part of the paper - to propose some normative suggestions on how the review-processes of (1) consensus conferences and (2) meta-analyses should be structured to increase their efficiency.

First, I propose that there are differences in the ways researchers interact with each other, differences that turn out to have an effect on the efficiency of the corresponding conferences. For example, when accounting for dissenting opinions, implementing means to allow for 'deliberation' between actors has a more promising turnout than stressing the mere need for 'aggregation' of opinions. Secondly, I propose that extending the group of consensus participants with stakeholders enhances the reliability of consensus conferences, whereas this will not be of great use to meta-analysis.

Implementing these normative suggestions in scientific practice will have a positive effect on the relation between scientists and the general public, augmenting democratic accountability of science without weakening its scientific legitimacy.

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Freedom of research and the public good

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Much is being made, these days, of intellectual freedom, academic freedom, and especially scientists' right to freedom of research, and how central these are for social and cultural progress. Much is being made, as well, of current threats to these freedoms — from politics, from religion, from commercial interests, from the military, even from the public. I shall take up the case of scientists' right to freedom of research in particular, look into its foundations, and consider the limits that those foundations impose. How shall I do this? I shall suggest that the documents (constitutions, declarations, charters, etc.) that recognize the right to freedom of research at the same time recognize other important rights that can conflict with the right to freedom of research, such as the right to human dignity and the integrity of the person, the right to environmental protection, and the right to equality between men and women and people of different racial and ethnic groups. Asking about the extent and limits of scientific freedom — asking how much freedom scientists really need or deserve — will thus involve asking how the conflict between the right to freedom of research and these other rights is to be resolved.

In order to pursue my question in a precise way, I shall focus on a specific case — the right to gender and racial equality and the kinds of scientific research that have been thought to threaten the enforcement of this right. The particular example I will take up here is research looking for gender- or race-linked differences in intelligence, particularly biologically-based differences in intelligence. This is a case that continues to command considerable attention (see, e.g., the two-month long debate in the journal *Nature* last year), and I will offer a number of competing analyses of the case. The conclusion I will suggest is that the right to freedom in group cognitive differences research does conflict with the right to equality and that no satisfactory solution has yet been offered. In the attempt to arrive at such a solution, I will explore three kinds of rights conflicts of the past that involved the right to freedom of research and that were successfully resolved — by the U.S. National Research Act of 1974, the U.S. National Institutes of Health Revitalization Act of 1993, and the Fink report and related policy directives of 2003-2004. I will end by suggesting the solution that I take to be modeled on these precedents.

Science under constraints: The example of the “good laboratory practices” within the french biotechnology SMEs

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The classical tension between the principle of scientific freedom and social and economic constraints is exacerbated within industrial or entrepreneurial context, where the practices of scientists are most often redirected to practical applications, and where they must conform to some entrepreneurial constraints such as the hierarchical structure of the enterprise.

Some authors argue nonetheless that the very idea of such a tension is a delusion, even for basic science within industrial context, for boundary between science and industry is vanishing and leave place to the technoscience and to a “new production of knowledge” (Gibbons 1994; Nowotny 2001) which substitutes heteronomy for autonomy.

Such claims rely on some epistemological assumptions, in particular on the idea that the epistemic justification of scientific freedom is flawed: if scientific freedom is not an essential precondition to an efficient process of scientific knowledge production, then it can be abandoned and the scientific production can take place in very constraints environments without any real difficulty or tension, and it's possible to manage scientist without consideration for their intellectual autonomy, as one manage other employees.

To test this idea, we meet researchers in biotechnology SMEs who are confronted to a specific kind of constraints: the “Good Laboratory Practices” (GLP) implemented within their enterprises. The GLP are OECD's norms which are defined as follow: “Good Laboratory Practice (GLP) is a quality system concerned with the organizational process and the conditions under which non-clinical health and environmental safety studies are planned, performed, monitored, recorded, archived and reported.” In France, the organism in charge of the GLP certification is the AFSSAPS, the French Health Products Safety Agency. Our investigation is based on several interviews with AFSSAPS's representatives and on a case study of a biotech SME which has implemented GLP norms.

Our aim is to understand the way entrepreneurial scientists are dealing with these norms, and how they negotiate their scientific freedom. Beyond the classical arguments in defense or against scientific freedom, we try to understand in which way it can be important for scientific practices. Philosophically speaking, this study is a way to empirically address the question of the epistemic necessity of scientific freedom for scientific practices. In which way autonomy of individual scientific practices is a necessity? If scientific freedom is also important in an entrepreneurial context, we have some good reasons to assume that it relies on some epistemic necessity.

Identity of indiscernibles in the real world: Between the quantum and the classical

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Leibniz's Principle of the Identity of Indiscernables has long been discussed in the literature, especially vividly in the context of quantum mechanics. One assumption is usually taken for granted — that there is in fact a solid, fundamental difference between classical and quantum objects that can be used as a starting point for further considerations. Granted, there seem to be very good reasons to accept that proposition; one is the stark contrast between classical and quantum statistics, where the former require an assumption of the discernibility of states under a permutation symmetry, while the latter explicitly require the permutation symmetry to yield systems that are not only indiscernable, but which should not even be counted as distinct possibilities. The discussion of relevant statistics is usually the starting point of modern expositions of the Principle.

Here I take a different approach. I begin with an examination of individual physical systems, from elementary particles to chemical and supra-chemical systems, to identify the famous border between 'macroscopic' objects that are supposed to be easily discernable by intrinsic properties (like a scratch on a coin), and 'microscopic' objects that, by virtue of having only a very limited set of properties (or degrees of freedom), can only exist in few enumerable flavors, and within a single flavor are in the strict sense indiscernable from one another, because there are no more properties to discern them by. In the course of this exploration, two main discoveries are made.

First, a question of what an 'object' is, is very slippery. Any definition of identity requires us to identify individual entities in the first place. It seems, however, that in the area where 'classical' behavior and statistics emerge, discerning individual objects in the sea of interrelated phenomena turns out to be tricky. For example, an unbroken succession of 'objects' can be traced: from clearly individuated — like covalently bound molecules in gas-phase — through 'sort-of-individuated' — like macromolecules or nanocrystals with weakly bound adsorbate layers — to 'not-really-individual-objects' — like temporary aggregates of water molecules in liquid-phase.

Second, there seems to be no crisp distinction between 'rock-hard', discrete intrinsic properties (like quantum numbers), and 'soft', non-discrete properties (like the specific shape of a molecule). For example, while a chemical formula seems to fully determine a molecule's identity (at least by the 'bundle theory' of identity), it is straightforward to show that two molecules described by the same formula may in fact be discernable from one another; and no 'primitive thisness' is needed. A dimer of stacked toluene molecules in gas phase is an excellent example.

Both of these issues have non-trivial consequences.

A discussion like this, I submit — one that begins with the real complexity of our world, not with a discussion of idealised statistical ensembles of two idealised classes — shows that the quantum-classical transition is fuzzy and metaphysically troublesome, and that its status as an unresolved problem makes any serious progress in the examination of Leibniz's Principle and related issues effectively impossible.

The term phlogiston and the notion of failure to refer. Towards a pragmatic theory of reference for scientific terms.

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Finding out which terms — scientific or otherwise — fail to refer is an extremely complex business since both felicitous reference and failure to refer must be negotiated (Eco, 1997). Causal theories of reference — even so-called hybrid theories (Enç, 1967; Nola, 1980, Kitcher, 1978-93) — posit that in order to refer to something, we need the regulative idea of an ontological reference, which operates even when we refer to *impossibilia* or inconceivable objects. Evidently this is not the case of the referent of *phlogiston*, which is neither inconceivable nor impossible, nor, alas, does it exist. In the antipodes, from a representational-physicalist point of departure (Devitt and Sterelny, 1987), a term fails to refer if it has no actual ontological grounds: *phlogiston* fails to refer because it has no physical existence. Phlogiston can even be considered to be a fictional entity, and referring to a fictional entity is not the same as a *reference in a fiction* or a *fictional reference* Salmon (1998): a fictional entity is an object in the same sense as an abstract object, and therefore we can genuinely refer to it.

The question is: who claims that phlogiston does not exist? Nowadays, everyone does, fundamentally and primarily because science has established it as a fact. The process that led to this result is extremely complex, lengthy and multi-dimensional. It involved factors of several kinds: cognitive, social, political, historical, as well as *ontological*, and this last one has been neglected. I do not claim that phlogiston (like dinosaurs) once existed and then ceased to exist — science only allows us to sneak into what exists and what, sometimes mistakenly, is *supposed* to exist. Paraphrasing Latour (1985), this inquiry is about following the journey of the referents, even when they do not end up being physical-existent or existing objects.

I believe with Bach (1999, 2004) that the notion of reference is essentially pragmatic; that the difference between alluding to something and referring or denoting something must be established; that to achieve this the semantic properties of terms are not sufficient, and finally that reference or denotation are not semantic but cognitive properties that relate thoughts and objects of any kind. My assumption is that to refer to something one must be capable of having thoughts about it and that the propositions one attempts to communicate in the course of referring to it are singular with respect to it. Being in a position to have a thought about a particular thing requires being connected to that thing, via perception, memory, communication and/ or education. Therefore, only in an exceedingly narrow realist theory of reference does *phlogiston* fail to refer.

Unless a theory of reference of scientific terms is based on the study of the actual linguistic communicative practices among scientists, it will inevitably pose serious epistemological difficulties.

Structure, flows, and practices in EU-funded stem cell networks

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In the past decades, the transnational network has become the main approach for European cooperation in science and technology. Networks and 'consortia' can harness different expertise, shared equipment, data banks, and prevent the duplication of scientific labour. Thus, they can provide a flexible and efficient framework to organise the production of scientific knowledge. Also, the promotion of transnational networks has well combined with the political vision of a Europe 'united in diversity', where local knowledge and resources are valued and fostered, but at the same time can be integrated into a collective and coherent 'European' project.

However, the realization of this technopolitical vision has not always been straightforward. While the development of IT communications - as well as the increasing digitalisation of scientific knowledge - has greatly facilitated the flow of data and information across different geopolitical regions, the speed and reach of these flows has been hampered by important technical and cultural hurdles.

On the one hand, the reconfiguration of scientific work from the traditional laboratory to the network has required the establishment of a uniform 'epistemic space'. But this ideal condition is difficult to achieve, due to the existence of dissimilar professional environments, measurement systems and laboratory protocols. As a result, the problem of *harmonisation* has become increasingly important not only in science and technology policy, but in the wider political context of European integration (Barry 2001). On the other hand, the creation of a common European platform for scientific research has been hindered by divergences between national attitudes and regulations, especially in contentious areas such as cloning and stem cell research. Thus, the European Union has had to find a difficult balance between the promotion of scientific research, the principle of 'subsidiarity', and the safeguard of common 'European values' in the protection of human rights (Jasanoff 2006).

This paper examines these issues by focusing on EU-funded stem cell networks. Results from fieldwork and interviews conducted in laboratories involved in two European consortia are presented. The case of stem cell research is particularly interesting to make sense of European science cooperation because of the fragmented regulatory landscape and the controversial cultural implications. While stem cell science has been identified as a promising sector to boost European competitiveness in science and technology, discrepancies between national attitudes have highlighted once again the long standing problem of the harmonization of research policies in Europe and, more broadly, of European unity and cultural identity (Salter 2007). In this situation, the circulation of information, biological material and scientific 'facts' has been driven and mediated not only by scientific rationality and collaborative patterns, but also by cultural and political motives.

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Scientific styles, identities and workplace cultures: a case study on the cultures of physics and humanities

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Our research group has carried out two empirical studies in different scientific fields, physics and humanities. In an interdisciplinary co-operation project with partners from five European countries we studied in 2005–2008 workplace cultures of physics with special attention to the scientists' career choices and identity formation in order to find out gendered cultural patterns of inclusion and exclusion of people, ideas and work styles (see www.upgem.dk). Second project initiated locally in Estonia in 2010 is aiming at the explanation of the cultural similarities and differences between the physical sciences and humanities. To some extent, the comparative analysis has been influenced by Stephen Fuchs' analysis of scientific styles. He applies two variables, mutual dependence and task uncertainty — in natural sciences which are more expensive and resource consuming than the social sciences and humanities, researchers are more mutually dependent, whereas in the humanities there is more space for disagreement, various interpretations and uncertainty. Respectively, the work style of the natural science is oriented to producing solid facts, it is less risk prompting, endorsing work in large groups, whereas humanities support freedom, independence and originality. However, as our analysis reveals, the picture can be more complicated, more diversities and contrasts than just between the two disciplinary styles can be identified.

Why is such a cultural analysis interesting for philosophy of science? As our point of departure, we hold the view that empirical cultural studies can contribute to the normative epistemology of science and open up new opportunities for transformative criticism. We agree with Kristina Rolin (2004) who has demonstrated that gender as social factor is relevant for epistemology of scientific inquiry with respect to assumed mutual trust among the members of a scientific community and as a factor in distribution of research effort. Nevertheless, there is more to be said about the cultural inclusion and exclusion mechanisms. Gender needs to be seen in combination with other identity factors. Therefore, we also rely on the distinction Joseph Rouse has drawn between post-positivist interpretation of scientific communities qua consensus communities and that of cultural and gender studies which regard the research communities as consisting of many culturally fragmented identity groups: "heterogeneous alignments or solidarities that do not reduce to either shared beliefs and values or tolerance for individual difference" (Rouse 1996: 111). Only the latter approach in the analysis of scientific practice enables to reveal the real diversity of identities, at the same time promoting normative criticism. In our paper, we shall discuss some examples of how scientists' identities, work styles and workplace cultures are involved in the inclusion or exclusion mechanisms, and how these relate to the epistemology of science.

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Modeling experimental evidence from the practices of developmental biology

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Many philosophical analyses of confirmation or hypothesis testing proceed on the assumption that the evidence has been gathered already. These ‘after-the-fact’ treatments miss important features of scientific evidence because determinations of relevance and the inferences licensed often depend on the circumstances of procurement. Philosophers must attend to the details of these scientific practices in order to explicate how these judgments of evidential relevance emerge out of experimental inquiry.

Some philosophers have explored the procurement of evidence under the label “data models” (Frigg and Hartmann 2009). Unfortunately, they only address statistical evidence, encompassing the process in two steps: (a) data reduction (error elimination); and, (b) curve fitting (‘clean’ presentation). This overlooks key aspects of the evidential practices observable in experimental biology, especially those that involve images. To redress this lacuna and better understand pictorial evidence, this paper explores the practice of producing images of gene expression patterns from *in situ* hybridization⁵ experiments and how they become evidence in developmental biology.

The practice of pictorial evidence production can be likened to modeling, and the resulting images can be considered models (Goodwin 2009). Therefore we can analyze this practice by looking at “modeling choices” such as idealizations — knowingly ignoring variation in properties or excluding particular values for variables (Love 2010). Because many of these choices are made long before a scientist executes a particular experiment, I introduce the notion of “serial idealization” to capture this temporally extended practice and distinguish it from other senses of idealization (Weisberg 2007). The practice of serial idealization can be characterized in terms of three phases (upstream [model system, research problem]; mid-stream [process in view]; downstream [particular experiment]) and three sources of origin (forced [unable to model unless choice is made]; entrenched [past choice established in the research community]; conventional [choice could be made otherwise but is not]). Many upstream and mid-stream choices that are entrenched or conventional can be found embedded in experimental practice. As a result, discussions of confirmation that focus on forced downstream choices systematically ignore the idealizations that illuminate how evidence comes to bear on hypotheses.

My analysis of the practice of serial idealization: (1) explains how different disciplines using the same experimental methods can disagree over standards of evidence (including artifacts) and maintain biases in data gathering; (2) enriches our understanding of data models because pictorial evidence is not akin to curve fitting where an indirect summary representation (graph or histogram) is utilized; and, (3) expands our notion of scientific modeling to include more than the “theoretical” approaches that have been a mainstay of philosophical discussion.

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¹ Hybridizing a labeled complementary DNA or RNA strand probe to localize a specific DNA or RNA sequence in a portion of tissue (*in situ*).

Counterfactual dependence, justification, and model explanation

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The widespread use of models in scientific practice, coupled with claims by scientists that these models can explain scientific phenomena, calls for an account of how models can be explanatory. Traditional views of scientific explanation, like the deductive-nomological (DN) account, leave very little conceptual room for models and computer simulations, both of which dominate current scientific practice. In order for something to count as explanatory, the DN account requires that (1) the explanandum is a logical consequence of the explanans, (2) the explanans contains a general law as well as (3) empirical content, and (4) the sentences constituting the explanans are true (Hempel and Oppenheim 1948). A model-based explanation will rarely, if ever, meet these four requirements. Perhaps the most basic difficulty is that many of our best models and simulations rest on assumptions that are known to be false, in violation of (4). The truth of the explanans is a requirement of many accounts of explanation, and thus, it models are frequently excluded from explanatory roles. This seems completely at odds with the kind of explanatory claims made by scientists in practice.

Recently, a serious attempt to provide an account of model explanation has been given by Alisa Bokulich (2009). Bokulich abstracts from three prior views of explanation in order to form a more general account. The goal of the account is to articulate what makes an explanation a model explanation, and what it is about model explanations that make them genuinely explanatory. In addition, Bokulich's account attempts to demonstrate that both idealized and fictionalized models can have explanatory power. Building on Woodward's counterfactual account of explanation, Bokulich's account has three requirements; a model must: 1) contain an idealization or fictionalization, 2) display counterfactual dependence between it and the phenomenon it purports to explain, and 3) pass a justificatory step.

I argue that, although Bokulich's account has some attractive features, it is incomplete in a number of important respects. In order to be considered complete, it must be shown that: 1) counterfactual dependence is a coherent notion when divorced from intervention, 2) fictional entities can answer what-if-things-had-been-different questions, and 3) the practice of model justification does not reduce to mere prediction. I believe that resources exist to complete the account, however, the end result will be significantly less general than originally desired. In the end, I conclude that although the account can successfully handle some models justified top-down from theory, it may not be able to mark as explanatory models built from the ground-up through empirical research.

Kinds, natural kinds, and grouping practices in research contexts

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For the most part historically kind concepts have been considered within the framework of the 'natural kind' debate, with its associated metaphysical and semantic issues, as a more or less classical philosophical issue. The endless largely irresolvable debates however over how to classify kind concepts as real or artificial has prompted Hacking (2007) recently to argue for the jettisoning of 'natural kinds' from philosophy of science. But grouping practices are a central part of science in all fields. Yet a lot less attention has been paid to the role of grouping concepts in actual research contexts, for which questions revolve around the processes of their construction and maintenance, their proliferation and redefinition, as well as how they are relied upon to secure knowledge and what the epistemic bases for this reliance are. While the roles and function of classification have been rather better considered we don't yet know what makes kind concepts tick beyond broad presumptions about their part as units of generalisation and explanation.

In this talk I want to foment such a discussion by exploring how epistemic qualities such as *projectability* and *significance* (a concept I'll introduce) can be used to demarcate kind concepts according to their practical roles and reflect something like a natural/artificial distinction in practice which traces to the role of such concepts as presumed sources of novel information in ongoing research contexts. These concepts like for instance 'mammalian masticatory system' or 'wnt pathways' have central roles in research as such sources, and can be distinguished from other concepts like 'predator' or 'cancer' in modern research which are generally labels for collections of properties, rather than indicative of any deep set of commonalities amongst their members. Such a distinction generalises over practice, and what scientists think or believe at a particular point in time, rather than any presumed unitary physical or metaphysical distinction between natural and artificial and is informative of how kinds operate in practice with distinct epistemic roles. Around this distinction one can present and understand scientific practice with respect to groupings and group concepts as a decision-making calculus over where and how to draw and describe kind boundaries (with sets of characterising properties) that provide robust projectable groupings and which *mark* information in the form of shared properties and relations of group elements relevant to the epistemic goals of researchers concerned. We study in fact the diversity of ways such decisions get made across different contexts, where beliefs about projectability aren't asserted always through knowledge of homeostatic mechanisms nor is the significance of a kind for researchers always the causal-mechanical basis that underlies a grouping.

In this regard my talk will focus on examples from the life sciences, although I believe this approach generalises more widely. I consider the topic of kinds in general as a good instance of a case where there has been too little attention to practice and too much to philosophical concerns with language and reality.

A systems approach to self-reflexive science: Preliminary findings from laboratory engagement studies

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I present some preliminary findings from the Socio-Technical Integration Research (STIR) project and use a systems approach to generalize a model of laboratory engagement. Broader impact of the model on science education and innovation is also briefly considered.

“Socio-technical integration” here refers to the integration of conceptual, ethical, and political factors operative in the actual practice of scientific research with the purpose of mitigating adverse societal consequences. The project was developed in response to EU and US mandates calling for “responsible innovation” and “responsible development” of emerging sciences and converging technologies. STIR involves “laboratory engagements” that “embed” a humanist or a social scientist in research labs internationally. The idea is “to open up the ‘black box’ of science and innovation ...[and] induce greater reflexive awareness among scientists in their specialist work worlds, with the expected result that innovation processes indirectly gain added sensitivity to human needs and aspirations, and thus greater resilience and sustainability.” (Macnaughten et al. 2005). Early findings anticipating the STIR project (Fisher, 2007) indicate the possibility that such laboratory engagements can stimulate and sustain self-reflexive thinking with positive impact on the R&D process.

STIR lab engagements focus engagement activities on R&D in the midstream, that is, in its implementation phase. Accordingly, I concentrate on the social and cognitive dynamics of midstream modulation. The research laboratory can be metaphorically described as an open system whose state of dynamic equilibrium varies through interaction with its environment. We may come to understand the features of more and less successful laboratory engagements by using selected system parameters to track interactions of the embedded humanist with lab researchers. Research labs encompass several overlapping systems, e.g., human, social and material, at the same time. The dynamics of these systems occur at variety of temporal scales, ranging from minutes to months (e.g., daily procedures, weekly lab meetings, publication and funding cycles), nevertheless interactions across temporal scales is inevitable. Insertion of an observer into the laboratory setting necessarily introduces perturbations throughout the system.

In order to navigate the shifting boundaries of this maze, I bring a second layer of metaphoric redescription into focus, namely, the explicit consideration of the often tacit dimensions of reflection, decision and action. This can be achieved by explicating metaphors that are implicit in the lab researcher’s daily activities, *including* the activity of interacting with the humanist. In some cases, we have observed that by foregrounding the tacit background of “normal lab activities,” the humanist transitions from background observer to active participant. Awareness of the transitions between alternative metaphorical narratives (e.g., background, foreground, impediment, facilitator, etc.) is essential to the process of reflexive modulation. Metaphoric redescription is often a key driver of scientific and technological innovation and discovery, a key driver of paradigm change (Kuhn, 1993; Hesse, 1988; Harré, 1982; Ricoeur, 1981) Commitment to metaphor has material impact; this is most obvious when such commitments shift. A similar process of metaphoric description and redescription is operative in the training of young researchers.

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Scientific inquiry and essentially embodied cognition

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Many feminist epistemologists have noted that because scientific inquiry always takes place against the backdrop of a particular social context, cultural location, and perspective, it cannot ever provide us with a “view from nowhere.” Similarly, recent work in philosophy of mind suggests that because human cognition is deeply rooted in facts about human embodiment and neurobiological dynamics, science cannot ever give us access to an “external reality purged of any and all subjectivity.”[1] Shaun Gallagher describes the body as the source of spatiality and maintains that “perceiving subjects move through a space that is already pragmatically organized by the construction” shape, and capacities of the body[2]. Moreover, as Marc Johnson highlights, we make use of our recurring bodily experiences of spatiality to organize our more abstract understanding of the world, and the position and functioning of our various limbs and sensory organs determine the kinds of categories we have and what their structure will be[3]. Because human cognition is *essentially embodied and enactive*, “all objects of experience, whether encountered through everyday perception or from a theoretical standpoint, have our own contribution sewn into their structure and are thus partially *made by us*.”[4]

In addition, these essentially embodied cognitive processes necessarily involve *affectivity* due to the very nature of our biological dynamics. The constant regenerative activity of metabolism endows life with a minimal “concern” to preserve itself, so that the environment becomes a place of attraction or repulsion. Living organisms themselves determine which environmental stimuli and information has “vital significance” on the basis of their bodily structure, needs, and the way they are coupled with their surroundings. Thus, we don’t ever truly encounter objects in our world as detached, theoretical entities, but instead are biased toward particular ways of conceiving the world on the basis of our bodily structure and emotional comportment. Our interests and values influence which kinds of scientific inquiry are pursued, which research methods are favored, and which hypotheses are taken seriously. Even physical and chemical phenomena in and of themselves have no particular significance, but only take on meaning in relation to our cares and concerns.

However, even though all of human cognition is unavoidably biased and subjective, we still can attain some sort of objective knowledge and move toward truth via *shared essentially embodied understanding and rationality*. Such a shared understanding is a matter of embodied structures that emerge in our bodily functioning, are recurring patterns in our dynamic experience, and allow us to *adapt* and interact successfully with our environment[3]. Although truth may not be absolute or universal, we can see the world through shared public eyes to arrive at a contextually situated, humanly universal, and humanly objective truth that inherently reflects our rational human purposes and the nature of our interactions with our surroundings. What we strive for, then, is objectivity *for us*, or *shared human perspectives*[3]. From the standpoint of science, what is crucial is that these structures are communicable and that the way in which they operate in our experience can be examined.

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Pragmatic significance, demarcation, and scientific progress

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In recent decades the so-called demarcation problem has fallen largely by the wayside, primarily due to concerns that the problem gains significance only if one crucially neglects the role of context in scientific activity. In this paper, I consider Charles Peirce's pragmatic maxim as a tool for demarcating scientifically significant (from scientifically insignificant) claims in a way that acknowledges the central role of context. I argue furthermore that the pragmatic maxim, properly understood, serves as a guide for making progress in science.

Charles Peirce intended his pragmatic maxim to serve two primary roles, one as a rule for demarcating scientifically significant claims, and the other as a prescription for formulating new hypotheses. Discussions of the demarcation role of the maxim have focused largely on its relationship to logical empiricism or positivism. The vast majority of other literature on Peirce's pragmatist principle center on its role in Peirce's system, specifically its relationship to his theory of signs. Little has been said, however, about how to understand the maxim in its purported role as a prescription for formulating new hypotheses, that is, as a means to progress. Indeed, as Christopher Hookway has observed in "The Principle of Pragmatism: Peirce's Formulations and Examples," little argument has been provided either in favor of or against any particular understanding of the pragmatic maxim. Since this principle of Peirce's is the lynchpin of his pragmatism, it is presumably central to his understanding of scientific activity. Given Peirce's success as a working scientist, one might suppose that he had some useful advice about how to effect scientific progress. I argue that appreciating how to apply the pragmatic maxim is key to understanding Peirce's views about how to make progress in science, and that we would do well to take his advice on this subject.

The paper begins with an examination of the pragmatic maxim and how to appropriately apply it. I argue that properly understanding the pragmatic maxim helps to rescue the demarcation problem from the principal complaint against it. I rely on Peirce's later formulations of the pragmatic maxim to specify what it requires for determining the significance of a claim. In particular, I show that recognizing the pragmatic significance of a scientific claim, X, requires having an alternative claim, Y, with which X can be compared. I argue further that understanding the role of comparison in determining the pragmatic significance of a hypothesis serves to provide some explicit recommendations for effecting scientific progress. Among these recommendations are: (1) intensive study of scientific history, not only for a sort of meta-scientific projection, but for hypotheses that might prove fruitful in new contexts; and (2) theory proliferation, so as to increase the comparison base from which to draw. Briefly, I also consider what Peirce's pragmatic maxim suggests for formulating new theories so that they are worthy of consideration, and how demarcation factors in this process.

Knowledge and values: The argument from necessity for sensitive invariantism

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According to recent views in epistemology, such as Interest-Sensitive Invariantism (ISI), the concept of knowledge is inherently related to practical concerns. On such views, whether a subject S knows that P depends not only on facts about whether P is true and justified, but also on facts about S's practical interests. Specifically, a subject who has high stakes with regard to P is in a worse epistemic position to know that P than a subject with low or no stakes regarding P.

In my paper, I review three lines of argument for ISI and argue that they all fail. I propose a new argument that may support such a position. The view against which I contrast ISI is invariant intellectualism, namely the view that knowledge depends only traditional epistemic factors such as truth and justification, and that these factors stay fixed in different contexts.

The first argument for ISI, which is provided by Jason Stanley, is the argument from the semantics of knowledge attribution. It relies on the claim that speakers tend to attribute knowledge to subjects in low-stake situations and deny them of it in high-stake situations. Apart of the general difficulty of arguments from knowledge attribution with supporting sweeping claims about knowledge, empirical data militate against the claim that speakers do actually attribute knowledge in this way. Moreover, the intuition underlying this claim is not decisive.

The second argument, provided by Jeremy Fantl and Matthew McGrath, is the argument from the so-called Knowledge-Action Principle, according to which knowledge is a necessary and sufficient condition for rational action. This argument is problematic for two reasons. First, there are obvious cases in which it is rational to act on belief that is less than knowledge. Second, I argue that ISI does not follow from the Knowledge-Action Principle, unless you accept as an initial premise that the argument from the semantics of knowledge attribution is correct.

The third argument, provided by Heather Douglas, is the argument from scientific practice. According to this argument, in actual scientific practice, scientists cannot avoid making value judgments about the acceptable level of inductive risk that they are willing to tolerate. As she argues, social values affect not only the context of application, but also the context of justification. Since the influence of social values on the context of justification is unavoidable, Douglas argues, it is also permissible. While I regard this as the strongest argument among the three, I argue that despite her claim to the contrary, Douglas does not manage to draw a principled distinction between cases in which the influence of values is permissible, and cases in which it is impermissible and amounts to wishful thinking.

In the last part of my paper I propose a basis for such a principled distinction. Research in experimental psychology shows that in some cases, people are prone to engage in motivated reasoning, and are able to rationalize any conclusion on the basis of the same evidence. I argue in those cases people cannot follow the directive according to which they ought only to form beliefs based on epistemically relevant factors. Hence, since "ought" implies "can", in those cases where people cannot avoid the influence of values on their belief formation, the influence of social values on it is permissible.

The scientific practice of assessing progress

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In a recent debate about the nature of scientific progress, Alexander Bird and Darrell Rowbottom have argued for two accounts of progress. Bird (2007, 2008) argues for the following epistemic account of progress:

(E) An episode constitutes scientific progress precisely when it shows the accumulation of scientific knowledge.

Rowbottom (2008, forthcoming), on the other hand, argues for the following semantic account of progress:

(S) An episode constitutes scientific progress precisely when it either (a) shows the accumulation of true scientific belief, or (b) shows increasing approximation to true scientific belief.

Both offer thought experiments and appeal to intuitions in support of their views, and it seems that the debate has reached an impasse. In an attempt to avoid this stalemate, I propose to study actual scientific practices rather than appeal to intuitions. In particular, I propose to examine the institution of the Nobel Prize, where scientists award their peers for what they consider to be important contributions to science, in an attempt to shed new light on the question of scientific progress.

I discuss two case studies that illustrate what I call the *scientific practice of assessing progress*. The first is Landsteiner's discovery of blood groups and why it was deemed worthy of the Nobel Prize. The second is Pavlov's work on the physiology of digestion and why it was deemed worthy of the Nobel Prize. These case studies show that scientists make evaluative judgments about scientific discoveries based on epistemic criteria as follows:

- (PA1) Survey the body of knowledge in field F at time t prior to discovery D .
- (PA2) Estimate what was known (the body of knowledge) in F at t .
- (PA3) Identify a lacuna, imprecision or error in the body of knowledge in F at t .
- (PA4) Spell out how D improved on the body of knowledge in F by adding new knowledge, correcting imprecision or exposing errors and correcting them.

As far as scientists are concerned, then, progress is made when scientific discoveries contribute to the increase of scientific knowledge. The scientific practice of assessing progress also shows that scientists take progress to consist in the accumulation of knowledge of the following sorts:

- (EK) *Empirical Knowledge*: Empirical knowledge usually comes in the form of experimental results, observations, instrumental readings and measurements, and other sorts of "data."
- (TK) *Theoretical Knowledge*: Theoretical knowledge usually comes in the form of explanations and well-confirmed hypotheses.
- (PK) *Practical Knowledge*: Practical knowledge usually comes in the form of both immediate and long-term practical applications.
- (MK) *Methodological Knowledge*: Methodological knowledge usually comes in the form of methods and techniques of learning about domains in nature.

I then propose that we should articulate an account of scientific progress that does justice to this scientific practice. I discuss one way of doing so, namely, by abandoning the distinction between 'knowing that' and 'knowing how' and granting that know-how counts as scientific knowledge.

The relevance of scientific visualizations — or why words are not enough

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In October 2010 a message concerning the discovery of the most remote galaxy (called “UDFy-38135539”) was published. This means having evidence for the existence of a celestial entity when the universe was just 600 million years old that helped to clear the hydrogen fog in the so called “Dark Age”. Being a fascinating topic by itself, our concerns are the images attached to the paper in two different kinds of publications:

The first one (1) is the article for the relevant scientific community — the professional audience. It appeared in “Nature” (doi: 10.1038/nature09462). The second (2) was published in “Spiegel Online” (<http://www.spiegel.de/wissenschaft/weltall/0,1518,724247,00.html>), thus being directed to the interested public, i.e., laymen in the relevant scientific area. Publication (1) was supplemented by four diagrams. The galaxy was observed by its emission in the near-infrared and the diagrams referred to the spectrographic analysis. It was said to show the significance of the discovery. In publication (2) we can also find images, but of an absolutely different kind. Three pictures are shown: the result of a computer simulation which is said to depict the universe at the time when the hydrogen fog is starting to clear up through the influence of the ultraviolet radiation of young galaxies; a collage of two photographs of the galaxy taken from the Hubble Space Telescope showing the position of the galaxy in the “Ultra Deep Field”. And the third one is a photo of the Very Large Telescope in Chile, who is source of the aforementioned analysis done by means of the SINFONI spectrograph installed there.

Here, two aspects are of importance: Firstly, scientists use different kinds of visualizations for different target groups. Second, visualizations play obviously different roles in those contexts. In publication (2) the pictures are means to attract attention. They do not really transfer any relevant information. You can grasp this when you take a closer look at the second picture. Obviously, it is meant to give the reader an impression where to look for the galaxy, but the photo is no map. There are no coordinates mentioned and, as everybody knows, the sky is wide. In publication (1) the case is different. Here it seems the diagrams are the most essential part of the text. Furthermore, being directed at a professional audience there is no need for integrating a mere eye-catcher. The diagrams must be there for different epistemic reasons — but what for?

The thesis will be that scientific visualizations, especially in the professional context, often transfer information which is not (fully) translatable into verbal text. To make this plausible I want to adapt a thought experiment well known in the philosophy of mind, namely Frank Jackson’s famous “knowledge argument” or “Mary-argument”. In its original context it is meant to show that physical information is not enough for a fully grasp of the world and for conscious experience.

Towards practical pluralism: A case of neuroeconomics

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The paper starts with a critical examination of *integrative pluralism* (Mitchell and Dietrich 2006), a prominent version of scientific pluralism. I will argue that the main problem of integrative pluralism lies in the fact that it implicitly shares with unificationism the assumption that explanation is the ultimate and most important goal of all science. Whilst integrative pluralists acknowledge the importance of other purposes such as prediction and intervention, they presume that these goals are best achieved with integrated, complete, true causal explanations (unificationists share the same assumption, *mutatis mutandis*; see Kitcher 1999). Although these assumptions appear intuitive and metaphysically harmless, they fly in the face of some of the best contemporary scientific practices in biology and economics. The divergence between explanation and other practical goals in these disciplines suggests a sense in which pluralism runs deeper than integrative pluralists assume. That is, not only are the target phenomena and their causal explanations diverse, but also the goals of scientists are plural, such that different goals require different methodologies. In light of this view, which I call *practical pluralism*, methodological implications of integrative pluralism to disciplinary interactions should be modified accordingly. First, while rejecting the isolationist stance, practical pluralism sees the integration of causal explanations as neither essential nor central for successful disciplinary interactions. Instead, practical pluralism recommends scientists to opportunistically employ/modify the cognitive and material resources, regardless of whether they were originally developed to explain the same phenomena (or different aspects thereof). For example, although the history of evolutionary game theory, a striking example of such opportunistic interactions, seems to support neither unification nor integration of biology and economics, it nonetheless illustrates fascinating and fruitful disciplinary interactions.

This observation brings us to the second methodological implication of practical pluralism, which concerns relativism, the idea that every scientific method is as good as any other. Although integrative pluralism resorts to the *ad hoc* integration of diverse explanations as a regulative ideal against relativism, this principle is not applicable in many disciplinary interactions where the *explananda* do not perfectly coincide between different disciplines, or where explanation is not their common goal in the first place. A better alternative to resorting to such an impractical ideal, I shall argue, is to accept relativism, although in its weaker form, *viz.*, anything goes as *long as it works*. That is, relative to their practical purposes (including but not limited to explanation), scientists are encouraged to employ any cognitive and material resources developed in other disciplines. The upshot is that there is no one-size-fits-all methodological strategy (such as the integration of causal mechanisms) for interdisciplinary research. Rather, methodological recommendations should be based on a deeper understanding of the practical goals of the relevant scientific communities. In this spirit, I will examine the practices of neuroeconomics and point out the diversity of goals pursued by different subgroups of neuroeconomists.

Ian Hacking as philosopher of scientific practice: *representing and intervening* revisited

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Undoubtedly, Ian Hacking's *Representing and Intervening* had a profound impact on the philosophy of science. It provided a decisive impulse for the emerging philosophy of scientific experiment, its instrumental realism at right angles to standard positions regarding scientific realism, and most provocatively the suggestion of parallel activities that are not coordinated in a pre-established methodological manner:

In nature there is just complexity, which we are remarkably able to analyse. We do so by distinguishing, in the mind, numerous different laws. We also do so, by presenting, in the laboratory, pure, isolated, phenomena. (p. 226)

And yet — just what kind or how much of a philosopher of scientific practice was Ian Hacking in *Representing and Intervening* and subsequent works? In particular, what kind of practice is he interested in when he considers the natural (as opposed to the human) sciences?

The very title of his book indicates that Hacking adopts a novel perspective on a received notion of scientific practice: i) Science is concerned with the relation of theory and reality; ii) one must not underestimate the role that is played by instrumentation, experimentation, and the presentation of phenomena in establishing this relation — though it seeks to theoretically represent the world, the practice of science is not exclusively representational but requires the presentation of phenomena and thus requires intervention.

More than 25 years later, the limits of Hacking's approach become visible with a general direction indicated, e.g., by Norton Wise's *Growing Explanations*, Hasok Chang's *Inventing Temperature*, Craver, Darden and Machamer's new mechanicism, Paul Humphrey's *Extending Ourselves*, Morrison and Morgan's *Models as Mediators*, et al. These accounts begin to describe science in technological terms — less concerned with the presentation of phenomena in order to validate theoretical accounts, more concerned with bringing a variety of conceptual and interventionist tools to bear on the control of phenomena. Hacking only inaugurated a line of thought which is leading to a confrontation of philosophy of science and philosophy of technology and which culminates in the question whether and under what conditions science should be considered as technological practice.

3D modelling, organicism and mechanical explanation in contemporary developmental biology and evo-devo

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Despite the role of models in scientific practice is increasingly being recognized, modelling in developmental biology and evo-devo has just recently started to be explored (Laubichler & Müller 2007). In particular, few attention has been paid to three-dimensional (3D) modelling strategies in embryology (but see Chadarevian & Hopwood 2004). The need for 3D representations of developing embryos is as old as embryology itself, and the modern approaches to development are no less dependent on accurate knowledge of tissues and structures (Metscher 2009). Today developmental biology is living a new technical revolution in 3D imaging and modelling of embryological form that promise to open new avenues in our understanding of the embryological and evolutionary origin of animal form.

Developmental biology and evo-devo are often reduced to developmental genetics and evolutionary developmental genetics. Few attention has been paid to the 'epigenetic school' (Müller 2008). This third way, halfway between the morphological and the genetic approach, was explored by some experimental embryologists and theoretical biologists and is being recovered and developed by new research programs that are contributing to the understanding of the biophysics (Forgacs & Newmann 2005) or morphomechanics (Belousov & Grabovsky 2006) of development and evolution. From this perspective, nor development nor evolution can be reduced to the mechanisms of gene activity, but a new mechanistic, multilevel analysis of developmental processes, and an understanding of generative mechanisms underlying the evolution of form is needed (Salazar-Ciudad & Jernvall 2004). One of the main principles of organicism is precisely that the properties at one level of complexity (e.g., cells or tissues) cannot be ascribed directly to their component parts (e.g., genes), because they emerge through the interactions among the parts at different levels of organization (Hall 2003): a cell interacts with its neighbour cells as well as with the extracellular medium, cells aggregate in germ layers and tissues, tissues interact in organogenesis, organs interact with the rest of the body, and the organism itself interacts with the surrounding environment. All these interactive processes, taking place along several spatial and temporal scales, are essential for understanding the generation of form. Thus, different (context-dependent) rules are appropriate for each level of the irreducible hierarchy of living organisation (Gilbert & Sarkar 2000).

If we want to understand how different regimes of causality operate at each scale of organisation in the developing embryo, the first methodological stage is to accurately characterise these different organisational levels. As recognized by Wilhelm His (one of the founders of mechanical embryology and the inventor of the microtome) body form is not a self-evident problem awaiting mechanical explanation, but embryologists have to make their problem, "to give body" to their views (Hopwood 1999): if animal form is to be grasped, developmental biologists must actively engage in reconstructing the embryo, reproducing the relationships they want to understand. Within this theoretical framework, I will explore how new imaging techniques for the 3D representation of developing embryos (in particular, electronic microscopy X-ray microtomography) are contributing to an organicist and mechanical explanation of the development and evolution of animal form at the tissular level.

Better science through philosophy: The story of the Toolbox Project

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Philosophers like to think of their subject as the mother of all disciplines. Typically, this is served up as a historical claim concerning disciplinary origins; however, one could also interpret it as a claim about philosophy's deep concern with the character of the various intellectual disciplines. Understood in the latter way, it should come as no surprise that philosophy has something to offer the growing number of cross-disciplinary projects that dot the research landscape. These projects confront many challenges, including linguistic differences and epistemic incommensurabilities. Underlying challenges such as these are fundamental differences in the worldviews that frame disciplinary research. Because of its connection with a wide range of disciplines, philosophy can be systematically employed as a medium through which to abstract away from specific disciplinary differences toward epistemic common ground; once attained, this common ground can support efforts by cross-disciplinary teams to build the mutual understanding necessary to meet some of the challenges that confront them.

Among the most central challenges to cross-disciplinary scientific research are those that involve communication. Effective communication is essential for the success of cross-disciplinary collaboration. The Toolbox Project (<http://www.cals.uidaho.edu/toolbox>) is built upon the premise that there exist central challenges to cross-disciplinary communication that can be met through the use of philosophy to generate mutual understanding. With support from NSF IGERT and NSF SES, we have developed an approach that uses structured dialogue to encourage collaborative teams to examine the philosophical dimensions of their scientific projects, dimensions that are otherwise rarely examined explicitly as part of collaborative efforts. After conducting more than 40 such workshops, we have gathered evidence that our approach enhances the collaborative process and provides an opportunity for understanding the unique epistemological perspectives that collaborations entail. In this talk we describe in detail the nature of our engaged philosophical work, focusing on the role that philosophy can play in improving the effectiveness and efficiency of cross-disciplinary communication.

The roles of difference-making and causal mechanisms in biology: Examples from classical genetics, molecular biology and systems biology

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There are at least two central assumptions regarding causality in experimental biology: (1) a cause makes a difference to the effect, and (2) there is a causal mechanism that links cause and effect.

The first is recognized in how experiments generally are set up. A goal is to keep all relevant factors constant except for the factor(s) we are interested in. This factor is then manipulated so as to vary among the experimental population and the control population. If the variation in the relevant factor correlates with a variation in the investigated effect, we have a strong indication that the factor is a cause of the effect. Thus, a general experimental set-up is based on the view that a cause makes a difference to the effect.

The second assumption is recognized in how much effort is put into the search for causal mechanisms. It is not scientifically satisfying just to find that the variation in a factor x causally influence the variation in a factor y . A description of *how* variation in x causes variation in y is needed in order to explain the effect and prove the causal relationship. In genetics and molecular biology such a causal mechanism is typically given as a description of a continuous and dynamic chain or network of interactions between objects on different levels, connecting the proposed cause and the relevant effect.

These two scientific approaches parallel two lines of inquiry in the philosophical causation debate. On the one hand we have difference-making/counterfactual theories of causation. On the other hand there are production/physical connection views of causation.

Although these views traditionally are seen as competing theories of causation, some harmonizing attempts are made, e.g. by arguing that counterfactuals and mechanisms work in tandem to give a better understanding of causation (Psillos 2004). Although there are some problems with current harmonizing attempts, I find the idea of harmonizing counterfactuals and mechanisms interesting based on the observation that both approaches are prominent and seem to build on each other in biological experimental approaches.

I investigate the role of difference-making and mechanisms in biology through examples from classical genetics, molecular biology and systems biology. I suggest that difference-making approaches play the roles of giving access to causation, while mechanism approaches are about *explanation* of causal relationships. Additionally, mechanisms are seen as informative of causal structure, but not of causal strength, while difference-making approaches can be informative of causal strength and causal priority, but not of causal structure. I discuss whether these roles of difference-making and mechanisms played in biological science can inform discussions in philosophy on the relation between counterfactual theories of causation and mechanism/production views of causation. I explore one harmonizing route, namely viewing different causal theories to be about different aspects of causation as indicated by scientific practice; counterfactual theories are about access to causation and mechanisms are about the structure of causation.

Informatics, philosophy, and ontology

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The rise of science informatics demands attention from philosophers. "Big science" projects such as the Human Genome Project and the Large Hadron Collider devote large portions of their budgets to information technology, and would not be possible without it. Granting agencies are starting to require that smaller research projects share data using systems such as the National Cancer Institute's cancer Biomedical Informatics Grid (caBIG). But computer systems, like social institutions, shape our practices and our thinking by making some things easy, some things difficult, and some things impossible. We usually start with software designed around a simplification of our concepts and practices, and we end up redesigning our concepts and practices around the strengths and limitations of the software.

Biomedical ontologies provide an example of the importance of philosophical engagement with informatics. The shortcomings of large-scale classification systems such as Systematized Nomenclature of Medicine - Clinical Terms (SNOMED CT, <http://www.ihtsdo.org/snomed-ct/>) can often be traced to pervasive but simple mistakes such as poorly structured definitions and the failure to make use/mention distinctions (Ceusters 2004, Ceusters 2005). The success of the Open Biomedical Ontologies Consortium (OBO, <http://www.obofoundry.org/>) is due in part the involvement of philosophers such as Barry Smith (Smith 2007), and their willingness to recognize both technological and philosophical challenges.

OBO brings together dozens of domain ontology projects, from amphibian gross anatomy to vaccines, under a set of shared best practices. Each domain ontology provides a network of terminology within a scientific domain, where each term is carefully defined and linked to other terms using well-defined relations. While each domain ontology is narrowly focused, they are designed to interoperate and form a larger network of biomedical terminology. And all OBO ontologies share a common Basic Formal Ontology which makes fundamental ontological classifications familiar to philosophers.

In this paper I explain what biomedical ontologies are, how they are built, and how they are used. They provide a case study for my argument that philosophers of science should engage with science informatics.

Genetic causation and mechanism explanation

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Explaining phenotypic properties by underlying genetic causes faces notorious difficulties. One reason is that genes play many different roles in the development and constitution of an organism. The functions of gene-activity in producing phenotypic outcomes may differ at different points in development, and a single gene can be involved in multiple developmental pathways, thus resulting in non-specific phenotypic effects. Also the fact that biological systems are robust undermines the reliability of experimental procedures, such as gene-knockout experiments, in producing evidence for genetic explanations.

This paper investigates the explanatory import of genetic causes for understanding phenotypic effects in the light of counterfactual and mechanistic theories of explanation. Interpretations of gene-knockout experiments are used as examples through which a heuristic framework for understanding genetic explanations is developed. First, the gene-knockout examples' reliance on counterfactual inferences is specified in the light of Woodward's (2003) counterfactual-interventionist theory of causal explanation. The implications of robustness in regard to making counterfactual inferences from knockout results are assessed. By employing Woodward's criteria for causal intervention and causal modularity, it is shown why the knockout results alone do not permit reliable causal-explanatory inferences about causes of phenotypic properties.

To complement the simple counterfactual heuristic for interpreting knockout results, the paper employs the central ideas of Craver's (2007) mechanistic theory of explanation to distinguish between two aspects of explanatory relevance for genetic causes in explaining phenotypic properties: the causal-developmental, and the mechanistic-constitutive. In both cases the explanatory relevance rests on counterfactuals describing in-principle manipulability relations between the explanans and the explanandum. The causal counterfactuals relate diachronically distinct events, while the constitutive counterfactuals relate synchronous properties of parts and wholes. Gene knockout examples are then reframed as bottom-up experiments that test for constitutive relevance of the knocked out genes for phenotypic properties.

A hierarchy of explanatory relevance is then outlined, where genetic causes may be invoked in answering two types of explanation-seeking questions about the properties of an organism. Genes as active parts of mechanisms that constitutively explain phenotypic properties answer questions about the causal capacities of the organism at a certain time. Genes as causes of developmental processes serve to answer questions about why the organism has a particular constitution at a certain point in time. Biological explanations describing developmental processes are mixtures of causal and constitutive explanations, as development is a series of changes in the organism's constitution, and causes of these changes are partly inherent in the operation of the mechanisms that constitute the organism's causal capacities at a certain time. The simple causal-counterfactual interpretations of knockout results are shown to be potentially misleading by way of neglecting the constitutive aspect of genetic explanation.

The neglect of analogy

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In this paper, I will first establish that reasoning by analogy is neglected in modern philosophy of science. I will then argue that the reasons that are usually given cannot justify this disregard. My conjecture is that the neglect results from an ill-founded urge to exclude from scientific method all those elements that bring out pluralistic, pragmatic and contextual aspects.

Pierre-Simon Marquis de Laplace writes at the very beginning of his celebrated 'Philosophical Essay on Probabilities': „even in the mathematical sciences themselves, the principal means of ascertaining truth — induction and analogy — are based on probabilities“[1]. Nowadays, these sentences must irritate the typical reader with a background in contemporary philosophy of science. While she will have heard much about induction, analogy is a less familiar term. What Laplace puts on a par with induction, is barely mentioned in modern textbooks on philosophy of science. A survey of subject indices corroborates this impression: while there are plenty and detailed entries on induction, analogy is often completely missing.

If analogy is mentioned in connection with scientific method, it is mostly considered an intuitive and unreliable tool that may serve to generate ideas but has no place in mature scientific method. As Carl Hempel once stated: „all references to analogies or analogical models can be dispensed with in the systematic statement of scientific explanations“[2]. Bayesians typically allocate analogical reasoning to the substantially subjective determination of prior probabilities. These are allegedly rough and inexact estimates in need of confirmation by induction.

Analogical reasoning concludes from the correspondence of some properties of different entities to the correspondence of further properties. For example, it becomes more likely that an entity y with property A also has property B , if another entity x is known to have A and B . In the 20th century, the formalization of analogous reasoning has been attempted by such diverse authors as John Maynard Keynes, Rudolf Carnap, or Mary Hesse. Some of this work is quite accessible and geared towards applications (Keynes); other work is more austere and remains detached from real-world contexts (Carnap). For our purposes, a few formulas suffice that can be derived solely from propositional logic and the axioms of probability. In particular, it can be shown that analogical reasoning can in certain situations infer probabilities close to one.

The view that analogy plays only a heuristic role in scientific reasoning is mistaken for several reasons. First, analogy and induction cannot be separated. It is easy to show that analogy turns into enumerative induction and vice versa if we exchange the roles of entities and properties on a formal level. As a consequence, Bayesians cannot divide the roles of analogy and induction in the manner stated above. Second, one can show that analogy can infer almost certainty, i.e. probabilities very close to one — for example when we identify an object by a detailed description of its properties. This stands in contrast to the claim that analogy can only provide rough estimates. Third, science is not only about universal statements, rather conclusions from one particular to another are often also crucial. Models in medicine and engineering are typical examples. In physics and other theoretical sciences, analogical reasoning provides a powerful method to develop scientific theories, for example when James Clerk Maxwell and William Thomson elaborate the analogy between electrodynamics and Fourier's theory of heat. If such analogies turn out fruitful, their function goes beyond mere generation of ideas. Maxwell spoke of *physical* analogies. Of course, analogy can never provide absolute certainty — but in this respect it is just like induction. There is a problem of analogy as a twin sister to that of induction.

Analogy leads to pluralism, when a theory is built starting from various different analogies. The situation in electrodynamics after Maxwell provides an instructive example. Analogy is also largely contextual, since it clearly depends on what knowledge is available. My conjecture is that the neglect of analogy in philosophy of science results from a widespread dislike of everything that makes science contextual, pragmatic, and pluralistic.

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[1] Laplace P-S, *A Philosophical Essay on Probabilities* (Mineola, N.Y.: Dover, 1996), p. 1.

[2] Hempel CG (1965). *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science*, p. 440.

Scientific representation, denotation, and explanatory power

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It is widely admitted that to understand how scientific theories relate to the world three intertwined concepts must be understood: model, representation, idealization. That these concepts are intertwined is evident. A model is meant to represent something, whether a physical or an ideal system. For instance, a model of a building is a representation of an actual building. Moreover, a model represents a physical system in an abstract and idealized way. That is, a model of a building is not meant as an exact replica but as an idealized and abstract representation of an actual building. In science one encounters several kinds of models, such as iconic or scale models, analogical models, and mathematical models. In this paper, my discussion is restricted to mathematical models. Representation seems to be a primary function of such models, and idealization seems to be the steering thought process by which this function is achieved. I highlight this point to suggest that a better understanding of 'scientific representation' could be achieved if we examine it in relation to 'scientific models' and to 'idealization'.

In science various means of representation are used. We say that a diagram of an electric circuit represents its target; that a graph of velocity plotted against time represents the acceleration of a body; that a material construction of double helical structure represents a DNA molecule; that a Feynman diagram represents a neutron decaying to a proton, an electron and an anti-neutrino; and that a mathematical model represents the behavior of a mass-spring system. Whether our scientific representations are diagrammatic, graphical, material, model-based, or other, they are important aspects of scientific inquiry; they enhance our understanding of the workings of physical systems and often enhance our understanding of abstract theoretical propositions. If one aims for a general theory of scientific representation then the latter must account for all these kinds of representational vehicles. I shall herein confine my analysis to how mathematical models represent physical systems.

One can categorize existing accounts of scientific representation into two types. Firstly there are accounts that attempt to reduce representation to other relations. The Semantic View of scientific theories, for instance, relies on the construal of the representation relation as mathematical mapping. The second category could be divided into two: denotative and non-denotative accounts of representation. The inferential account, advocated by Suarez 2004, is an example of a non-denotative account. Denotative accounts interpret the concept of representation as strongly linked to the function of denotation, e.g. Hughes 1997 and Elgin 2009.

In this paper I focus exclusively on denotative accounts and attempt to develop a denotative account of scientific representation that ties the representational function of scientific models to their explanatory power. In the first part of the paper I argue that existing denotative accounts are plagued with some weaknesses that prevent them from accurately capturing important elements of scientific modeling. In the second part I argue that denotative accounts must make use of the notions of 'mechanism' and 'explanatory power' if they are to overcome those weaknesses and do justice to how scientific models represent their target systems. Both arguments rely on understanding the notion of representation in relation to the notions of idealization and scientific model.

Collaborative experimental practices

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Experimental collaboration is one aspect of the current interest for philosophers of science in experimental practices. As a way of understanding collaborative experimental practice, we can take advantage of Rouse's approach to experimental situations as co-constitutive of all participants. Framing experiments as intra-actions is useful in that it affords a rich way to approach the collaborative activities among people and things that make up scientific practices. However, although it is useful for understanding the production of generalizations (including evaluative generalizations), phenomena, and of the practitioners themselves, I disagree with Rouse that this frame requires a commitment to normativity (even Rouse's more naturalized, mitigated version of it). One can be sympathetic with Rouse's claim that all meaningful boundaries are intra-actively established within the world and not accept the extra claim that the boundaries are normative (as opposed to 'merely' regularly occurring in similar situations/intra-actions). We may take up a more naturalizing approach than Rouse and, like Turner, view generalizations as regularities.

However, an important result of the view that participants in experimental practice are co-constituted is that they cannot be independent Cartesian actors; thus, we reject Turner's (1994) individualism. Joint action theories, intended to explain collaboration, vary; most philosophers, like Turner (1994), understand joint actions to be aggregates of individual intentions and actions. But for others, joint actions can be understood as more than the sum of their individual actors' parts (collectivist, e.g. Barnes', 1995, 2000; or social interactionist, e.g. Tollefsen, 2002 and social neuroscientists such as Böckler, et al, 2010 and perhaps Turner, 2010).

Different participants are differently constituted within collaborative action. Objects and scientists are both "practically constituted components of repeatable phenomena" but in different ways. The constitution of experimental (and other) scientists is a continually produced result of many practices in which they participate. But I argue that the co-constitution of the scientists in the intra-action is a consequence of their embodiment, specifically their embodied cognition (which both Rouse and Turner accept).

There is no agreed upon understanding of embodied cognition, but a naturalizing one takes into consideration, among many other sciences, neuroscience. Thus, I argue that as we cash out the co-constitution of human participants as competent practitioners we need cognitive social neuroscience, both for understanding their linguistic interactions and their perceptual motor activities. I use as an example a moment of collaboration in which two scientists, looking through a light microscope, decide that they see microglia, "Del Rio-Hortega cells." Evidence from neuroscience indicates that, to mention only one relevant interaction, A watching the actions of B as he adjusts the microscope can result in peripheral motor potentiation in A's own corresponding muscles. Thus, A's competence is constituted and reconstituted in this, and many similar ways over a few seconds of the intra-action among humans, microscope and microglia.

Technological possibility as a condition for epistemic possibility

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In this paper, I explore the relationship between technology and scientific practice. We often see technology as providing affordances that enable us to gain new knowledge about the world. But technology can also be a constraint on knowledge. When technological interventions are adopted into scientific practice, their performance becomes a duty. In such cases, technology is a condition for knowledge.

In the first half of the paper, I introduce the notion of technological possibility to describe states of affairs that can be brought about given the contingent availability of the particular material and conceptual means. These two aspects of technology, the material and the conceptual, are crucial to understanding its relation to scientific practice more generally. The possibility of spanning a river with an iron bridge turns on what the world is like: that iron is available, has certain properties that allow it to be formed into trusses, and so on. But the possibility of spanning a river with an iron bridge also turns on how our concepts fit together. If we did not know that iron has the properties it does, it would not occur to us to attempt the project. In precise terms, for a state of affairs to be technologically possible, it is necessary but insufficient that it be physically possible and conceptually possible. Furthermore, the material and conceptual means have to be available in practice, not merely in principle.

In the second half of the paper, I consider the relationship between technological and epistemic possibility. I first distinguish weak and strong notions of epistemic possibility. The weak version depends simply on what an agent can rule out on the basis of his or her current mental state. The strong version of epistemic possibility allows for the incorporation of epistemic duties such as those connected to scientific practice. Thus, a biologist may be expected to reflect critically or even perform experiments before rendering judgment. Epistemic duties are constrained by practical considerations, and

available technology is one such constraint. More perspicuously, a claim is epistemically possible for a scientist only when she has satisfied certain expectations — and that often involves some technological intervention. Technological possibility is a necessary but insufficient condition for epistemic possibility.

I conclude by suggesting that a similar analysis can be provided for other constraints on scientific practice, including ethics and economics. Moreover, these constraints are interrelated, and studying them in concert should prove illuminating

Bridging a theory–practice gap: What can kind essentialism contribute to understanding classificatory practices in biology?

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Despite their deep roots in the history of philosophy, both essentialism about individuals and essentialism about kinds continue to be heavily disputed views, in particular among philosophers of science. In the case of kind essentialism, an important reason for this is a profound theory-practice gap: on the one hand there is kind essentialism as a philosophical theory, on the other hand there are actual classificatory practices in the sciences as the phenomena that this theory is supposed to account for, and between the two there is a deep mismatch.

This is particularly clear in the philosophy of biology. At least since the 1970s there has been a strong consensus among philosophers of biology that traditional forms of essentialism about kinds of biological entities have no part to play in philosophical accounts of biological classification because of conflicts with how biological kinds feature in evolutionary accounts of biological phenomena. Consequently, essentialism about biological kinds has long been a dead issue.

In recent years, however, a number of philosophers of biology, as well as several authors working primarily in general metaphysics, have undertaken attempts to resurrect essentialism about biological kinds in some form or another (e.g., Boyd, 1999; Griffiths, 1999; Ellis, 2001; Ellis, 2002; Okasha, 2002; Walsh, 2006; Oderberg, 2007; Devitt, 2008). These attempts involve a number of different conceptions of what kind essences are, some of them quite different from traditional views of kind essences. Together, they have caused renewed interest in essentialism in biology and a debate on what kind essentialism could and should be if it is to apply to biological kinds.

In this paper I examine the feasibility of essentialism about biological kinds with particular emphasis on the question whether the practical benefits of essentialist accounts of scientific kinds outweigh the costs incurred when trying to establish such positions. Why would one want to resurrect essentialism about biological kinds in the face of the large problems that have been seen to occur when trying to reconcile essentialism with evolutionary theory? Does philosophy of biology *really* need a form of essentialism to be able to make sense of biological kinds? If so, what work would essentialism do?

I address these questions by taking recourse to the idea that in science kinds function as the hinges of investigation by simultaneously being the *explanantia* and the *explananda* that scientists work with (Reydon, 2009). That is, postulating a kind involves identifying a group of things over which generalizations can be made that can be used in explanations and predictions, as well as individuating a group of phenomena that can (and eventually must) itself stand at the focus of further investigation. I argue that this role for kinds in science does to some extent involve an essentialist understanding of kinds, albeit not one that conceives of kind essences in any metaphysical manner and thus not one that entails a commitment to a particular metaphysical position on the nature of kinds. Such an essentialism could do philosophical work for our understanding classificatory practices in biology.

Interpretation and modelling strategies in economics: The case of Sen's theorem

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In 1970 Amartya Sen proved that no collective decision procedure that respects the unanimity of preferences and individual rights can give rise to consistent (i.e. acyclic) social choices. This conclusion is reached within a remarkably simple framework and it seems to cast some serious doubt on the possibility of ethically adequate collective decisions. Whether this impression is correct depends on how Sen's formal model is interpreted.

In this talk I will discuss three major attempts to tackle Sen's result in order to show the particular form in which the problem of the interpretation of a formal model arises in economics. I will then provide an argument to select what I take to be the correct interpretation.

The interpretations of Sen's theorem I am interested in are the following:

1. Sen's theorem provides a natural normative framework for collective decision methods and exhibits an inherent limitation of such methods. The limitation may be circumvented by weakening Sen's assumptions (Salles 2008).
2. Sen's theorem presupposes a misguided characterization of individual rights. Its solution rests with the adoption of a different characterization (Nozick 1974, Gärdenfors 1981).
3. Sen's theorem provides a characterization of dysfunctional types of decision methods. The natural way to avoid its conclusion is to correct the decision rule in order to eliminate the dysfunctional phenomena it originates (Petron & Saari 2006, Li & Saari 2008).

Note that (1) and (3) are mutually inconsistent while (1) and (2) call for fundamentally different models. Although arguments in favour of each of (1) to (3) may be offered, it is possible to show that (3) is the appropriate interpretation of Sen's result. This can be done by observing that Sen's proof is essentially the construction of an example of a dysfunctional social decision framework. In addition, it can be shown that Sen's result generalises the Prisoner's Dilemma. But the Prisoner's Dilemma describes a problem that arises from the suppression of information concerning individual preferences: thus Sen's theorem describes collective decision rules in which information concerning individual preferences has been suppressed. Once the information is reintroduced, inconsistencies no longer arise. In the light of these remarks, (1) should be rejected while (2) changes the context of Sen's problem by making it a problem about the modelling of rights as opposed to a problem about the accessibility of information.

This analysis shows how difficult it may be to evaluate the significance of a formal model in economics. It also points to the main sources of this difficulty: first, it may not be easy to identify a typical empirical interpretation of the model; secondly, this can sometimes be done only by studying certain variations of the model (in the present case, its game-theoretical version and its degenerate form, in Petron and Saari 2006, Li and Saari 2008 respectively). This suggests that normative results can properly be assessed only by replicating their internal dynamics in different ways.

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Laws and nomological necessity in scientific practice

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Philosophical concern with scientific practice has often de-emphasized laws in favor of diverse models. Theoretical understanding supposedly arises from the details of models (including mutually inconsistent models), and the analogical and sometimes “property-dragging” (Wilson 2006) relations along extended chains of models. The consequent disunity of scientific understanding also seems to undermine traditional conceptions of laws as governing principles. Within such model-centered views, laws have been treated as not describing any actual physical systems (but only the abstract models, of which they are true by definition) (Giere 1988, 1999, Teller 2001); as principles that only suggestively guide and loosely unify theoretical model-building (Giere 1988); as conceptually gerrymandered claims of empirically limited scope (Cartwright 1999); or as loosely bound atlases of discontinuous “theory facades” (Wilson 2006).

The importance of laws has also been under dual attack within philosophy of biology. The evolutionary contingency of virtually every distinctively biological pattern, the complexity of biological processes that blocks invariant, simple regularities, and the context-sensitive functional normativity of biological mechanisms seems to undermine any conception of nomological necessity or Humean regularity in biology. Meanwhile, emphasis upon biological practice has shifted focus toward model organisms and other experimental systems, and toward the articulation of mechanisms described functionally rather than nomologically.

I nevertheless argue that laws play important and indispensable roles in scientific practice, including biological practice. This argument draws upon a new conception of laws and nomological necessity put forward by Marc Lange (2000, 2009), and a re-conception of the role of laws by Lange, John Roberts (2008), and John Haugeland (1999, 2007). Lange treats lawhood as the holistic invariance of domain-indexed sets of laws under any counterfactual supposition consistent with all members of the set. Lange, Roberts and Haugeland then show how laws are crucial for inductive projection and confirmation, assessing the reliability of measurement procedures, constituting autonomous disciplinary domains that circumscribe inquiry, expressing conceptual content, and for counterfactual and counter-nomological reasoning in a wide range of scientific context from experimental design to distinctive patterns of argument in cosmology or statistical mechanics. Laws of nature are thus essential to scientific practice for their contribution to multiple aspects of scientific reasoning and understanding. The collective invariance of sets of laws within a domain then express the normativity of scientific understanding within that domain.

I further argue that renewed attention to laws highlights both the importance and the finitude of conceptual articulation in scientific practice. This conception of laws actually complements earlier work on scientific practice that supposedly challenged the centrality of laws, by re-conceiving laws as expressions of conceptual norms whose actual content is worked out through the material articulation of experimental systems, models, and the infrastructure of measurement and data-analysis. Whereas laws of nature have often been taken to express a God’s-eye view of a unified nature, this re-conception of laws emphasizes their importance for expressing a finite conception of scientific understanding as situated both temporally and materially in domain-specific scientific practices.

Do computer simulations constitute a new style of scientific reasoning?

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Philosophers of science readily acknowledge today the pervasive and central role played by computer simulations in many disciplines. Less consensual is the claim that computer simulations constitute a distinctively new set of scientific practices raising new philosophical issues. Stöckler (2000) for instance contends that they do not amount to a revolution in methodology, and Frigg and Reiss (2009) argue that the problems they raise are only variants of already-discussed problems pertaining to models, experiments and thought-experiments. Humphreys (2009), on the other hand, defends the novelty of issues raised by distinctive features of computer simulations such as their epistemic opacity, and Winsberg (2001) emphasizes the specificity of the ways simulations get justified.

My aim in this paper is to develop a different perspective on this question of novelty, by investigating whether computer simulations constitute a new style of scientific reasoning, in Hacking's sense of the notion. Building on A. C. Crombie's historical analyses of the existence of several distinct styles of scientific thinking in the Western tradition, Hacking's concept combines two major aspects (too often considered separately) of scientific methodology, to wit, its heuristic aspects — how do scientists find out about the world? — and its logical or justificatory aspect — how does a scientific result get to be justified?. More specifically, to count as a style of scientific reasoning, a set of modes of scientific inquiries must accomplish three things: i/ it must introduce new types of entities (such as objects of study, propositions or explanations); ii/ it must be “self-authenticating”, that is, it must define its own criteria of validity and objectivity; iii/ it must develop its own techniques of stabilization (on these three conditions see Ruphy, forthcoming).

The focus of my inquiry will be computer simulations of complex physical systems. Asking whether they constitute an emerging, new style of scientific reasoning will necessitate i/ investigating the ontological status of the “parallel worlds” they create and how these parallel worlds articulate with the real-world systems they simulate; ii/ analyzing the conditions of possibility for truth (or falsehood) of statements about the world derived from a simulation, in order to see whether they are dependent on a specific procedure of reasoning; iii/ investigating the sources of the stability of computer simulations when new data come in.

For each of these three lines of interrogations, I will determine to what extent the answers are specific to computer simulations (in contrast in particular with models and experiments) and I will illustrate my claims with case studies in the astrophysical sciences.

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The nature and roles of methods accounts in experimental reports

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In my contribution I draw attention to a key yet neglected element of scientific writing about experiments: methods accounts. By “methods accounts” I mean scientists’ accounts of the rules one should apply in experimental practice, the justifications for these rules, and the problems one may encounter while applying them. I contend that methods accounts are an integral part of what Peter Galison has recently called “technologies of argumentation,” the concepts, tools, and procedures needed at a given time to construct an acceptable scientific argument. I characterize methods accounts in experimental reports from the late 17th to the mid-19th century and examine how they were deployed to make a case.

I consider reports of experiments with snake venom. For over 200 years, there was a strong sense of an experimentalist tradition of venom research, and investigators presented their works as contributions to an ongoing endeavor, engaging with and explicitly building on the work of their predecessors. Snake venom research is thus uniquely suitable for the study of the changing nature and role of methods accounts in writings about experiments. My focus is on two methodological tenets: “multiple determinations” and “repetitions with variations”. According to recent philosophers of experiment, experimenters today are centrally concerned with multiple determinations of experimental outcomes (e.g. Hacking 1983, Wimsatt 1981). Given the recent emphasis on the confirmatory power of multiple determinations of empirical evidence, it is surprising and remarkable to find that multiple determinations did not play a role in methods accounts prior to the 20th century. Rather, my research suggests that references to multiple repetitions and repetitions with variations bore the epistemic weight

Selection, drift, and independent contrasts: Defending the conceptual and methodological foundations of the method of independent contrasts

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Felsenstein's method of independent contrasts (FIC) is one of the most widely used approaches towards the study of correlated evolution; however, it is also quite controversial. Among the objections raised to it, there is one that stands out from the rest: firstly, it is rather philosophical in nature, and secondly, it has received only very little attention in the literature thus far. This objection concerns Sober's charge that the FIC is methodologically flawed due to its resting on the assumption that the evolution of the relevant traits follows a *random walk*. According to Sober, this assumption is problematic, as it seems to suggest that the evolution of these traits was driven by drift only — and thus, that selective hypotheses are ruled out from the start. In this paper, I try to rebut this charge.

To do this, I firstly consider a preliminary conceptual worry: the question of how it is even possible for two drift-driven traits to be evolutionarily correlated. I show that this worry can be answered by noting that such a correlation is likely to be due to the two traits having a low degree of modularity with respect to each other — i.e. due to the existence of genetic, developmental, and physical linkages between them. Importantly, the FIC gives us the means for establishing both the existence and strength of these linkages — and hence, it cannot be said to rest on a conceptual confusion.

Given this, I then show that Sober's methodological charge can be mitigated by noting that the random walk assumption behind the FIC does not in fact preclude it from investigating selective hypotheses. There are three different reasons for why this is so. Firstly, this assumption can be used as a mathematical simplification without genuine descriptive importance. Secondly, this assumption can be used to describe directional selection for a randomly changing optimum. Thirdly, this assumption can be used to describe a process that might be called 'internal selection': the adaptation of some traits of an organism to some other traits of it. Note that since these scenarios cannot describe every kind of selective hypothesis — in particular, the case of strong unidirectional external selection is not included among them — the present defence of the FIC cannot fully resolve Sober's worry. However, since these three scenarios do comprise a significant part of the landscape of hypotheses we might want to consider in this context, the present defence at least makes Sober's charge significantly less threatening.

I end by pointing out that this discussion is not just relevant for defending the conceptual foundations of the FIC, but also for developing a deeper understanding of correlated evolution in general. In particular, consideration of the differences between the conceptual and the methodological worries brings out the extent to which correlated evolution can come about through different causal routes operating at different levels. For these reasons, the issues discussed here hold an interest for anyone concerned with deepening our understanding of the way biological evolution works.

Experimenting in the field: Probing the notion of “real world simulation”

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Ecology is built of multiple research programmes that are not necessarily related to each other - concepts and theories used in the field might be incommensurable. Thus, ecology embraces a multidimensional account of knowledge and thus a plurality of conceptions and approaches, which by now is mostly acknowledged as being an important aspect of how ecology fruitfully grapples with the complexity of its objects. From a philosophy of science perspective this complexity is mainly due to the fact that ecological objects are construed according to different modes of description. In the scientific mode, these objects are described as pure objects along the traditional separation of the natural and the artificial, of representing and intervening. These categories become intertwined and interdependent in the technoscientific mode of description, which results in the description of hybrid objects. An object of contemporary ecological research will be used to discuss this descriptive pattern. A number of analytically useful and distinctive concepts will be presented that are to sharpen our understanding of what technoscientific objects are and how they can be distinguished from scientific objects. Concepts discussed in this paper will be focusing on some characteristics of the field experiment, such as individuality or resilience, but most notably on the notion of real word simulation.

The object in question is an artificial water catchment that is a constructed natural site. The “Hühnerwasser - Chicken Creek (DFG-Project SFB/Transregio 38)” serves to analyze the „structures and processes of the initial ecosystem development phase in an artificial water catchment“. Basically, it is an isolated artificial sand heap, with an altitude difference of approximately 10m and a small lake at the deepest part of the site. The object is situated on the area of an abandoned pit mine in Eastern Germany.

To bring forward the notion of the real world simulation it is of particular interest that in a certain sense the artificial water catchment simulates its own behavior in that it monitors its own performance. It is a specific kind of field experiment that abolishes the carefully maintained spatial separation between an experimental system and the natural system, which it is supposed to represent. This raises the problem of how to critically assess findings from this “real world simulation”, and thus of how to adequately characterize the vantage point of description.

This study on real world simulation and the more general aspects of developing a descriptive pattern for technoscientific objects is part of a German-French DFG/ANR research project on “The Ontology and Genesis of Technoscientific Objects”.

Continental drift debate and the context of pursuit

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According to Larry Laudan, “acceptance, rejection, pursuit and non-pursuit constitute the major cognitive stances which scientists can legitimately take towards research traditions (and their constituent theories)” ([Laudan, 1977], p. 119). The first two stances, in contrast to the latter two, have usually been considered the main subject of epistemic justification. However, before a scientific theory can be considered acceptable, it first needs to be pursued. Therefore, a number of scholars have pointed out that an assessment of promising features of a theory is indispensable to scientific practice, and is thus at least as important as the evaluation of the acceptability of theories (e.g. [Whitt, 1990], [Whitt, 1992], [Nickles, 2006], [Šešelja and Straßer, 2011]). What scientists are usually concerned with is not so much the confirmation of theories, but rather the question: How to proceed with further research? Which theories should be further investigated? Despite this significance, the evaluation in the context of pursuit has often been neglected by both philosophers of science and scientists. The aim of this paper is to explicate this problem paradigmatically in view of the recent revolution in the earth sciences.

The revolution in geology, initiated with Alfred Wegener’s theory of the continental drift (henceforth, Drift), has been the subject of many philosophical discussions aiming at resolving the problem of rationality underlying this historical episode (e.g. see [Frankel, 1979], [Frankel, 1987], [Laudan, 1987], [Le Grand, 1988], [Oreskes, 1999]). However, the question as to whether Drift was worthy of pursuit in the first half of the twentieth century, that is, in its early development, remained open or inadequately addressed. In this paper we will evaluate Drift by means of an account of theory evaluation suitable for the context of pursuit, developed in [Šešelja and Straßer, 2011]. We will argue that pursuing Drift was rational, i.e., that it was irrational to reject its pursuit as unworthy.

The significance of our research question is two-fold. On the one hand, we will argue that the idea of pursuit-worthiness is often insufficiently explicated in the literature on the Drift debate. More precisely, the question as to whether a theory is worthy of pursuit for the scientific community has been confused with the question as to whether it is worthy of pursuit for an individual scientist. Nevertheless, if a theory is worthy of pursuit in the former sense, that does not imply that each scientist should actually pursue the given theory. It may be rational to ascribe the pursuit of a theory only to a small group of scientists, while the rest of the community is to investigate other theoretical rivals. Clarifying this distinction has important consequences for the assessment of the rationality underlying epistemic stances and decisions of scientists.

On the other hand, the evaluation of Drift in the context of pursuit will allow for a better insight into the rationality of the geological community at the time. Moreover, by distinguishing the justification in the context of acceptance from the one in the context of pursuit, we can clarify certain confusions in debates among geologists in the 1920s. Since rejecting a theory in the context of acceptance and accepting it in the context of pursuit are two compatible stances, the awareness of the distinction between these two contexts may sometimes help scientists to avoid disputes on otherwise compatible ideas.

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Of communities and individuals as regards scientific knowledge

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In this paper I will be implicitly defending the following thesis:

An individual X obtains knowledge of scientific claim p in virtue of being a member of a community A that regards claim p as knowledge.

The thesis states is that a claim p only becomes scientific knowledge once it's been through a process of validation by a scientific community. This is meant to be contrasted with the claim that individuals first obtain scientific knowledge perception or inference, and then transmit it to their colleagues, without the community playing any epistemological role.

The strategy that I will follow is the following. In the first section I will consider the claim "that collaboration plays a causal role in advancing scientists' epistemic goals, and that its growing popularity is a consequence of its effectiveness in aiding communities of scientists to realize their epistemic goals" (Wray 2002). I will conclude that the claim is rather weak in the sense that it only justifies certain sections of scientific practice and does not establish that in principle scientific knowledge is produced in the manner described above.

An attempt to strengthen the thesis will be made through the presentation of evidence that all through history in what is widely recognised as scientific activity (the activity which claims as its originators the methodological writings of Bacon and Newton) the scientist is never alone, even if they are the single author of a scientific work. I will draw on certain insights from Latour's (1987) study in the making of scientific knowledge to support the thesis that the individual scientist is necessarily surrounded by allies.

This attempt will consist of two parts, the first being what the exploration of what I term the intra-laboratory aspect of scientific activity, and the second being the public forum aspect. I will conclude that the latter aspect is the aspect which supports the claim that the production of scientific knowledge is in principle social, that is that the appropriate unit of epistemological analysis of the production of scientific knowledge is the scientific community rather than the individual scientist.

Finally, I will promote the thesis that community agreement is constitutive of knowledge, presenting and arguing for the communitarian account of scientific knowledge (Kusch 2002). I will briefly argue against an individualistic conception of knowledge acquisition, based on the model of the solitary Cartesian thinker and the notion that knowing something involves being in a certain mental state, and then briefly talk about belief as the property of plural subjects before I move on to present the communitarian model of knowledge acquisition.

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Calibration in daily scientific practices: A conceptual framework

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Calibration is analyzed from the standpoint of scientific users inside the laboratory (i.e., the standpoint of those who use measuring instruments previously conceived and built, and already implanted in the scientific community). Starting from the actual practices of scientists with these instruments, the paper proposes a conceptual framework and taxonomy of the calibration process. The aim is to clarify the kind of practice this calibration is, and to improve the understanding of its internal logic. Against the widespread commitment that calibrations are straightforward and routine procedures, we stress both the complexity of the calibration practices, and their epistemological importance since they constitute a necessary ingredient of the significance and robustness of instrumental outputs.

Our strategy is to start from an uncontroversial prototypical case of calibration illustrated with a simple and familiar instrument — the scales. Next we examine more questionable candidates for calibration, and we analyze them as specified variations with respect to the prototypical case. Our analysis is fueled by two more complex examples of calibration practices: i) The calibrations of a CCD camera in astrophysical observations: ii) Calibrations in X-ray diffraction experiments.

We will explore the following lines:

1) The *target* of calibration: What kind of thing can be the *object O* of a calibration?

The target can be a measuring instrument or a part of a complex measuring device.

2) The *presuppositions P* of calibration: what is *taken for granted about O*, which delimitates what is *not* granted and has to be checked and controlled?

3) The *aim* of the calibration applied to the object *O* under the presuppositions *P*.

The aim is to master the possible shift between a token and a type: between the instrumental outputs *actually obtained* with *this* individual instrumental device *at a given time in a given context*, and the result that *should have* been obtained in *optimal* conditions *according to the theory of the type* of the instrumental device. At this level we stress: i) that a calibration procedure is always accomplished with the intention to perform subsequent targeted measurements that are the true aim of practitioners; ii) that the practice of calibration of a given instrument varies depending on the characteristics of the target-measurements at stake.

4) The *procedure* of calibration and its internal structure: the kind of logical stages through which the aim is achieved.

We will stress that two stages need to be distinguished: i/ a stage devoted to calibration *tests* (evaluation of the possible discrepancy between the actual instrumental output and the expected measurement result); ii/ a stage consisting in the application of calibration *operations* according to the conclusions of the testing phase. Two types of calibration tests will be characterized: *blank (or background)* calibration tests; and calibration tests using measurement *standards*. Correlatively, two kinds of calibrating actions will be distinguished: the *material operations* (concrete manipulations exerted on the individual instrument) and the *symbolic operations* (for example, mathematical corrections applied to the instrumental outputs actually obtained).

*This paper is the result of a collective work developed by some members of the PratiScienS research group directed by Léna Soler in Nancy (France). The six co-authors are: Catherine Allamel-Raffin, Catherine Dufour, Jean-Luc Gangloff, Léna Soler, Emiliano Trizio and Frédéric Wieber. Several co-authors will be present at the conference.

Breaking the codes: How philosophers might best help scientists with responsible conduct of research

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In her recent book *Philosophy of Science after Feminism*, Janet A. Kourany argues that philosophers of science should work with scientists to help create a socially responsible science, making special efforts to help scientists recognize the entanglement of epistemic and ethical concerns. Kourany identifies the formulation of adequate ethics codes for scientific fields as a project where collaboration between philosophers and scientists might be especially effective.

While in agreement that the epistemic project of science gives rise to a rich set of ethical demands on scientist, I am doubtful that a focus on the formulation of ethics codes will allow philosophers to make a contribution to scientific practice that is either valued by scientist or effective in addressing the scientific community's need for ethical conduct by its members.

In this paper, I consider the problems with approaching the project of responsible conduct of research through the instrument of a code of conduct. I examine the attitudes scientists voice about codes (as well as those they express about interlopers from other fields who seem to be interested in policing their ethics). I sketch out ways that the formulation and adoption of ethics codes might actually tend to undermine responsible conduct of research — and scientists' enthusiastic engagement with ethical issues relevant to their disciplinary practice — by reinforcing some of scientists' preexisting prejudices against the project of ethics.

A more productive approach than tinkering to make existing codes more adequate and more precise, I argue, would be for philosophers to help scientists step away from a code-centered approach to ethics. Instead, philosophers of science can help scientists develop approaches to responsible conduct of research that are centered on ethical methodologies. Like methodologies for scientific research, these ethical methodologies would serve as problem-solving resources, not authoritative lists of what is permitted and what is forbidden. They would not be viewed as definitively answered questions, but as starting points for serious intellectual labor, ready to adapt to new situations. They would get their value from regular use and extension in a disciplinary environment that emphasizes public reasoning, strives for objectivity, and invites the active involvement of the scientific community.

Casting ethics in terms that are more continuous with practices that scientists already embrace strikes me as a better way for philosophers to help scientists live their ethics than does the project of developing or refining more codes. Here, though I disagree with Kourany's focus on codes, I agree that philosophers of science, especially those who attend to actual scientific practice, have a contribution to make in helping to elucidate the elements of practice in a community's shared project of being ethical.

Analogical reasoning in scientific practice: The problem of ingrained analogy

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In spite of its widespread use by scientists, the status of analogy as a justificatory inference, for philosophers of science, remains tentative. Grounded on a non-literal form of language, arguments from analogy were deemed inferior by the logical-positivists, who often limited their use to cases of discovery. In the late 1960's, Mary Hesse sought to reconcile a largely positivist approach to explanation with actual scientific practice. Although Hesse's approach demonstrated the empirical nature of analogies used to develop models, her systematic account of the plausibility for their use was too narrow in key areas. In his recent book, *By Parallel Reasoning*, Paul Bartha addresses the narrowness of Hesse's account by offering normative criteria for evaluating/justifying that include more instances of the analogical inferences used by scientists. Although the focus on the plausibility of individual arguments is essential to any account of analogical reasoning, I suggest that the broader conception of scientific practice — the interaction between metaphor and arguments generated by it — must also be considered. In this paper, I address the broader conception of scientific practice by presenting the analysis of a potential difficulty for analogical reasoning, referred to hereafter as the problem of ingrained analogy.

This problem, introduced by Nancy Leys Stepan (1986), occurs when the analogy underwriting a conceptual metaphor becomes reified (or is no longer viewed as non-literal) within the community and is subsequently taken for granted when forming secondary analogical arguments. A classic example discussed by Stepan is the 19th century work on gender and race. Using Stepan's example, I will show that an ingrained analogy affects the content of the premises in the subsequent arguments and, in turn, influence determinations of relevancy as well as interpretation of data, which results in a self-reinforcing metaphor. An ingrained analogy can proceed undetected for a long period of time; resulting in not only in negative epistemic consequences, but in social and political ones as well. With these worries in mind, I suggest some strategies that challenge the cogency of the analogical arguments generated by the metaphor. By doing so, I believe the scientific community's sensitivity to the metaphor will be raised and the likelihood of the reification of a poor metaphor will be decreased.

Seneca to witness: Experiences with a witness seminar on experimental economics

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In *On the Shortness of Life* Seneca famously remarked that the ‘busy people’ of the present prefer not to be remembered of their past (Loeb translation):

They are, therefore, unwilling to direct their thoughts backward to ill-spent hours, and those whose vices become obvious if they review the past, even the vices which were disguised under some allurements of momentary pleasure, do not have the courage to revert to those hours. No one willingly turns his thought back to the past, unless all his acts have been submitted to the censorship of his conscience, which is never deceived.

Everyone engaged in the writing of contemporary history — not just contemporary history of science — is faced with Seneca’s problem and has to find a solution for it. We address this historiographical problem by presenting the considerations that led us to organize a so-called witness seminar, a format originally developed at the Contemporary History Centre in London. The seminar was held in Amsterdam on 28-29 May 2010 and was devoted to what is without any doubt one of the major developments in economics over the past five decades: the emergence of the experiment in economics and the laboratory as a site for observing. We will outline the practical problems we faced and the choices we made in the running-up to the seminar and will narrate about the actual event itself. One of the important (if not the most important) input for the seminar were 2-5 hour interviews held with all 12 invited participants by Andrej Svorencik. The witness seminar itself was organized in six sessions on four topics:

- the emergence of a community of experimental economists
- relations to funding
- the development of experimental skills and techniques
- the development of the laboratory as a site of observation in economics

At present we are working on the transcript of the seminar and drawing preliminary conclusions (7 hours audio and video-taped materials in total). We will show a short précis of the seminar event, and give our first reflections on whether the witness seminar is a viable format to face Seneca’s problem.

Debiasing rules in medical experiments

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Debiasing rules are all those methodological strategies applied in experiments in most scientific disciplines in order to eliminate errors generated by subjective biases. Instances of such strategies are blinding (experimental subjects or the experimenters) or randomization in the allocation of treatments. There are a number of a priori arguments justifying the implementation of debiasing rules, but there is very little evidence about why experimenters actually apply them. These arguments, that we owe namely to Allan Franklin and Gyora Hon, show that it is reasonable to control for subjective biases, but they acknowledge nonetheless that implementing a debiasing rule does not imply that the evidence generated in the experiment is actually free from biases. Sociologists of science have claimed that this is why the closure of experiments is negotiated on non-epistemic grounds: since we cannot be actually certain about the epistemic purity of the result, experimenters are always entitled to negotiate the results depending on their private interests.

I argue instead that in medical experiments, at least, accepting an experimental outcome depends on the fairness of the experimental procedure, even if the result is not actually free from subjective biases. My argument hinges on the analogy between medical experiments and fair distribution processes. These latter have been empirically studied throughout the last four decades by psychologists and economists, analyzing how agents react to outcomes against their interests depending on the fairness of the procedure leading to such outcomes. There is ample evidence showing that unbiased procedures generally make acceptable to the participants even outcomes that are clearly unfair for their private interests.

I contend that medical experiments are just an instance of such fair distribution processes. Medical experiments are appraised by the concerned parties (pharmaceutical producers, physicians, patients, regulators) as decision procedures over certain properties of a therapy. The decision is not neutral, it carries different costs and benefits for them all and there is an informal understanding of how the interests of the parties can bias the experimental procedure. It is therefore surprising to notice how unbiased experiments bring to an end public controversies on therapies, where accusations of partiality flow free. I argue that our preference for fair procedures provides a plausible account of this phenomenon, at least as much as any purely social explanation. In order to illustrate my claim, I will present two early medical experiments that incorporated debiasing rules already in the 18th century. They provide good instances of medical experiments as impartial decision procedures over controversial therapies.

The nature of *spatial intuitions* in early modern mechanics and optics

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This paper will examine the nature of spatial intuitions in the epistemology of early modern natural sciences, particularly of celestial mechanics and optics. The mechanical and optical cognitions, as represented in the seminal works of Kepler, Descartes, Galileo, Newton, Hooke and others, are apparently based on spatial or spatio-temporal intuitions of static and dynamic structures. The axioms of early modern sciences such as mechanics and optics were derived from spatial geometrical intuitions whose a priori visualized structures form irreducible and, as such, final spatial structures. The fundamental spatial intuitions in these sciences seem, therefore, to attain an epistemological finality, resulting in the origin of scientific axioms.

The main object of investigation is the method of Newton in his *Principia*, namely the mathematization or mathematical demonstration of the laws of celestial mechanics (principle of inertia, law of elliptical orbits, area law and the law of gravitation) that were originally proposed by Descartes, Kepler and Hooke. Newton, however, observed with disapproval the original propositions of Kepler and Hooke as mere guesses, and considered the mathematical reasoning and demonstration to be the true method in mechanics that alone can *axiomatize* its laws imparting them universality and apodicticity. Newton's claim on the primacy of his mathematical methods has been subject to discussion in several important treatises by leading historians of science in the 20th century, like I. Bernard Cohen, Richard S. Westfall, and François De Gandt.

But how could mere guesses lead directly, i.e. without mathematical reasoning, to axiomatic knowledge in the above-mentioned fields of early modern science? Can there be mere guesses in the science of mechanics that appear immediately to be true and apodictic? I would argue that these guesses (of Kepler and Hooke), that Newton observed in dispraise and, consequently, disclosed from his mathematical methodology, should have originally been *free-spatial-structural intuitions* that alone can attain an adequate *epistemological finality* and scientific-axiomatic legitimacy. This would necessitate a reexamination of the mathematical methods of Newton in order to find out whether purely mathematical premises and methods can bestow an axiomatic status on the original mechanical and optical structural intuitions, as represented in their apodicticity and universality. In short, I want to examine whether in the context of early modern mechanics and optics the immediate *free-spatial-structural intuitions* reach a deeper foundation — thus gain a deeper axiomatic finality — as compared to their mathematical and deductive reasoning.

The primacy of free-spatial-structural intuitions over geometrical deductions in the early modern science of mechanics and optics seems to lie in the fact that geometry is essentially a spatial science like mechanics and optics and as such, presupposes the free-spatial-structural intuitions which originally brought about its axioms. The most fundamental epistemological finality of free-spatial-structural intuitions can be demonstrated through a few examples from the early modern mechanics and optics. Furthermore, the epistemological finality of free-spatial-structural intuitions can be observed in the historical context of these classical sciences. The trajectory from intuitive-epistemological process to axiomatic finalities in the early modern spatial sciences (geometry, mechanics and optics) proves ultimately to be historical; i.e. it forms a *historic-epistemological process to finalities*, from which the axiomatic foundations of these sciences constantly evolve, deepen and thus develop further. Such an epistemological processuality seems to underlie the contextualization of sciences that defines their bounds and, at the same time, expands them historically.

Three kinds of interdisciplinary relations

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One potentially useful way to bring order into our views of interdisciplinarity is to sort interdisciplinary attempts or ambitions by a few typical relations they presuppose between components of disciplinary matrices. In this paper, we identify three kinds of interdisciplinary relations: problem feeding, conceptual drift, and methodological migration.

We take problem feeding to be a process by which a problem passes from a field or discipline to another. In their influential 1977 paper on interfield theories, Lindley Darden and Nancy Maull, mention a functional role interfield theories sometimes fill, namely "[t]o answer questions which, although they arise within a field, cannot be answered using the concepts and techniques of that field alone." It seems that disciplines, in the context of discovery, sometimes fill out the gaps for each other; they feed each other problems.

This happens in many different ways, some more 'integrative' than others. The process can be markedly fruitful in cases where sub-mechanisms in a larger system are identified as being within the domain of investigation of another discipline. Or, in general, when certain steps in the process of discovery requires types of investigation unavailable in the field of origin.

Conceptual drift and methodological migration both involve the passing or sharing of concepts and methods respectively. The phenomena vary from the sharing of very general concepts and methods proving only a weak interdisciplinary connection (if any) to quite substantive forms where concepts are actively developed and deployed from the standpoints of various disciplines or fields.

Drawing on examples taken from the emerging field of Sustainability Science we attempt to deploy the above basis in a critical discussion of degrees of promises and pitfalls connected to these three forms of interdisciplinary relations.

Finally, we utilise the above relations to point to the fact that whereas discussions about interdisciplinarity traditionally assume enduring or even perennial integration, many real cases involve temporary entanglement only. Following the path of scientific investigation and discovery a research programme will sometimes wind off into the domains of other disciplines. This can be a strong motivation for interdisciplinary research though the connection made will not propagate itself to dissolve disciplinary boundaries in the long run. This appears to have interesting consequences for how to train interdisciplinary researchers. Often the goal seems to be to build several disciplinary competences within each interdisciplinary individual. In some cases however it is sufficient to be able to roughly formulate and assign problems (to the correct field or discipline) rather than being able to solve them.

Playing with molecules

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Recent philosophy of science has seen a number of attempts to understand scientific models by looking to theories of fiction. Such proposals draw upon a variety of different analogies between the two. Some emphasise that both involve claims acknowledged to be false, for example, while others draw parallels between the ontology of theoretical models and fictional entities. While analogies between models and fiction are suggestive, the real test of fiction-based approaches must be whether they can provide a coherent overall account of scientific modelling. In previous work, I have defended an account of models that draws on Kendall Walton's 'make-believe' theory of fiction. According to this account, models should be understood as 'props' in games of make-believe, like dolls or hobbyhorses. I have argued that we may use the make-believe view to address a number of philosophical issues raised by scientific modelling, such as the ontology of theoretical modelling and the problem of understanding scientific representation.

In this paper, I will ask whether the make-believe view provides a convincing analysis of the practice of modelling. Does the view provide a good account of the way that models are used and the attitude that users take towards them? My assessment of the make-believe view will be based on an empirical study of the use of molecular models. In the study, I examine both hand-held physical models and a computer modelling program. One way to discover that children are engaged in a game of make-believe is to listen to what they say when they are playing the game. For example, if we see children standing astride the hobbyhorse shouting 'giddy up', we quickly guess that they are pretending that it is a horse, that standing astride it counts as riding the horse, and so on. Similarly, I assess the plausibility of the make-believe approach by examining the actions carried out by users of models, and the way that they talk about those actions.

I will argue that the make-believe view does gain some support from the way that molecular models are used. Users' interaction with molecular models does indeed suggest that they imagine the models to be molecules, in much the same way that children imagine a hobbyhorse to be a real horse. If we focus only on the models themselves, however, we miss an important part of what is going on in molecular modelling. Users of molecular models do not only imagine models to be molecules, I will argue; they also imagine themselves *viewing* and *manipulating* molecules, just as the children playing with the hobbyhorse might imagine riding the horse, or stroking it. Recognising this imaginative 'participation' in modelling, I suggest, points towards a new account of how models are used to learn about the world, through what I call *imagined experiments*. It also helps us to understand the value that scientists sometimes place on the tactile, bodily engagement allowed by physical models.

Towards an epistemology of scientific practice

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In this paper, I want to suggest that there are aspects of scientific practice that make a central contribution to the epistemic standing of our scientific theories and hypotheses, such as methodological rules and principles, experimental procedures, and our engagement with scientific instruments.

The purpose of this paper is to outline a meta-philosophical programme detailing what such a project would involve. Specifically, I will explain what is required in order to show the following four inter-related things: (i) that these different factors really do make epistemic, not just pragmatic, contributions to our theories, (ii) how it is that they make these contributions, (iii) that claims about the epistemic nature of these factors are, at least in principle, testable, and, lastly, (iv) that we can actually test for them by engaging in historical-empirical work.

In this paper, I will focus specifically on scientific methodology (with the eventual aim of developing similar accounts for other aspects of scientific practice) and outline an account of our methodological principles according to which these principles are robust both epistemically and empirically. This means putting special emphasis on principles that abound in scientific practice, and not just abstract philosophical principles that we might or might not be able to read back into specific scientific or historical episodes.

While this approach takes into account the imperfect epistemic predicament that comes with doing empirical science, it also faces the following difficulty: the diversity of scientific reasoning means that many principles will likely not be applicable across different scientific disciplines. Thus, one of the main goals of this paper is to show how, given this enormous variety of scientific practices, many of which change over time, we could ever — even in principle — provide an epistemic justification for any of these strategies.

The starting point for my own account is the debate between Laudan and Worrall from the late 1980s about the aims of science and the value of a fixed methodology. Laudan (1984, 1990, 1996) takes a historical approach, but ends up defending theses about the aims of science as changing, and a view of methodological principles solely as hypothetical imperatives. Worrall denies both of these, arguing both for fixed scientific aims and also for a 'fixed methodological core' consisting of 'formal' or 'procedural' principles in contrast to more substantive principles (see 1989: 385ff).

In this paper, I argue for a new position that incorporates elements from both Laudan's and Worrall's views. I then show that this allows us to both (a) accept and justify some fixed general and abstract methodological principles that stay constant over time (and that also preserve the notion of a more or less unchanging overall aim of science), and, (b) at the same time, give an account of how it is that less abstract and more concretely prescriptive methodological principles get their epistemic bite, even though they are the ones that change, and even though they are, in fact, sensitive to different local and changing aims of science.

Expertise and the disunity of science: The epistemic difficulties of providing expert advice for policy

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Were I accepted to the 2011 Society for Philosophy of Science in Practice conference my presentation would consider the epistemic problem of adapting expert knowledge to an established political goal. My particular focus of research is the failure of scientific and technical experts in different fields to effectively communicate across their disciplinary boundaries in order to provide coherent advice. The information needed to meet the goals of environmental policy is rarely limited to the domain of any individual scientific discipline or technical field. Each discipline has its own technical language, experimental procedures, problem solving strategies, exemplars, scale of application, factors that are included in models, factors which are considered exogenous to models, and background assumptions — all elements of what might be termed the “cognitive map” of a discipline. Because experts produce knowledge within the context of their field’s cognitive map, and these cognitive maps vary greatly between disciplines, there are significant epistemological difficulties involved in the provision of interdisciplinary expertise for policy purposes. Using the Clean Water Act as a case study, I will argue that these epistemic divisions between different disciplines are an important part of the reason why a group of technical advisors who are honest, competent, attempt to be objective, and have similar goals for the policy can still manage to fail to communicate, or even to have productive disagreements about the technical advice they provide to lawmakers.

Having laid out the problem in the case study I would then explore the extent to which a philosophy of epistemic mediation might be possible. I claim that my research provides an example of how philosophers of science might help not only to *clarify* but also to *bridge* some of the divides between disciplinary fields of expertise by serving as what H. M. Collins has termed “interactive experts.” That is, someone with sufficient experience in the relevant fields to interact interestingly with participants and to assess when and how the relevant disciplines are likely to encounter misunderstandings rooted in their different conceptual maps. Having thus identified and described possible sources of disagreement, the technical expert advisors and the interdisciplinary specialist could work together with policy makers to craft a policy based on more coherent, better-integrated advice. Is this a substitute for truly interdisciplinary research? No. Neither would it totally eliminate problems of confusion or non-compliance at the policy implementation stage. But I do believe that a policy that attempted to *integrate* advice from multiple disciplinary sources, rather than simply *collect* advice from those sources, could be more successful.

Joint group knowledge — Justification in esoteric and exoteric contexts

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Taking as my starting point that knowledge production is a collective endeavor, my aim is to develop an empirical case study of research on a group level with a specific focus on interdisciplinarity. As theoretical insights from social epistemology can provide an excellent springboard for empirical studies, I will be drawing on the 'joint belief' approach to group knowledge (Gilbert 1989, Schmitt 1994 and e.g. Rolin 2010).

But you cannot simply apply the theoretical approaches, as described in the 'joint belief' literature, on a one-to-one basis to empirical cases. To accommodate different practitioners' perspectives on their own work, it is advisable to adopt methodological individualism. It should be noted that this does not require a commitment to ontological individualism. For the purpose of this article I will not discuss questions such as whether groups are plural subjects and whether they can actually possess beliefs. Therefore I will use the more general term 'view' instead of 'belief' in the following. The aspect of justification, however, deserves elaboration. In a research context, epistemic views have to be justified in accordance with scientific standards. For that reason I will differentiate the notion of justification further.

I propose to introduce Ludwik Fleck's (1935) notion of 'audiences' to a concept of group knowledge. By suggesting a continuum between 'esoteric' and 'exoteric' audiences Fleck conveys the idea that the communication of knowledge claims works differently within a research group than between group members and peers external to the group.

My argument is that groups employ different standards of justification for the same piece of knowledge according to the relevant audience. In relatively *exoteric* communication (in publications and applications) research groups employ a strong standard of justification, i.e. every knowledge claim has to be substantiated and accounted for in great detail (less so for an application, depending on the type of application). This is guaranteed on the basis of division of labor among group members. In consequence, the group view should at best be described with a non-summative account. In other words, not every single group member has to be capable of substantiating the knowledge claims put forward. In *esoteric* communication, a summative account of group views is more adequate. The group view is the view that every — or, at least, most members or the most important members — single member holds. This is necessary for a number of scientists to come to function as a veritable research group. A summative account is possible, because a weak standard of justification is employed. Within a research group, scientists do not have to be capable of epistemically justifying their view in terms of proof and evidence. It is sufficient if they have good reasons to believe that their colleagues are able to substantiate the hypotheses in question. In short, it is enough to trust.

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A functional account of scientific explanation

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This talk discusses a functional account of scientific explanation, one that focuses on the role(s) of explanatory discourse within scientific practice, broadly construed. After introducing the basic tenets of such an account, the functional perspective is compared to accounts of scientific explanation that have dominated the philosophical literature for the past half a century (the inferential, causal, erotetic, and unificationist accounts). The functional account, however, is not situated as a direct competitor to these conceptions of scientific explanation. Rather, while standard accounts aim to reveal the logical and conceptual structure of *individual scientific explanations*, the functional account aims to reveal how the *practice of explanatory discourse* functions within scientific communities. In short, there is a shift in perspective away from explanations, as achievements, toward explaining, as a coordinated activity of communities. After making the contrast clear, I argue, first, that this alternative vantage point helps us to recognize how explanatory discourse offers significant resources for tackling particular challenges of social epistemology, thus highlighting the methodological role of explanatory activity in science. I also argue that the functional account allows us to salvage some of the central virtues of standard accounts of scientific explanation, by recasting their significance, while simultaneously embracing explanatory diversity across scientific disciplines. Furthermore, the functional account provides a platform for investigating the concept of explanatory depth in novel ways. Perhaps most importantly, I maintain that the functional account provides a satisfying rationale both for why scientists should desire theoretical frameworks with explanatory (in contrast to either descriptive or predictive) power, and why the *activity* of explaining is itself crucial to coherent and productive scientific practice. The discussion will be illustrated by concrete examples from diverse sciences, including molecular chemistry, economics, and organismal biology.

The working class is cooking: The invention of a novel body of practical knowledge during the 19th century

Monika Wulz

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The talk will deal with the condition and changing of the nutritional situation of unskilled workers and their families in the context of Vienna and the Austro-Hungarian Monarchy during the 19th century. Taking this historical perspective, the talk will present an epistemic transformation of the problem at stake: the shift from (1) the diagnosis of undernutrition and the resulting endeavors of material welfare providing food for the poor towards (2) the multilayer diagnosis of the problem that brought to appear the complex constellation of the practical conditions of the nutrition situation. Due to this epistemic shift, the nutrition situation was henceforward located in a broad economic and organizational constellation of the working and living conditions of unskilled workers and their families. As a consequence, this epistemic transformation within the diagnosis of the nutritional situation of the working class generated a novel body of practical knowledge regarding the supply and preparation of food both in the domestic realm and in the workplaces of unskilled workers. It generated new knowledge on nutrition, food preparation and combination, food economies, and housekeeping. It effected the invention of new cooking technologies (the haybox and the “cooking bell”) as well as of industrialized techniques of food preparation for staff canteens and gave rise to new scientific examinations combining questions of nutritional science with economic questions and labor physiology. Furthermore, this epistemic shift brought about an integral moral code for working class families.

Taking the example of the housekeeping handbook “Das häusliche Glück!“ [Domestic Felicity!], that first appeared in Austria in 1886 as an endeavor of the social reform movement, the talk will present the epistemic shift of the problem of undernutrition towards the complex consideration of the living conditions of working class families as an integral system that shaped their nutrition conditions: the working conditions (working hours and break times), the payment structure of their wages, the conditions of working spaces (that provided no possibilities for the preparation of hot meals), the hygienic conditions of foodstuffs and households, the lack of knowledge on the storage and preparation of food and on nutrition guidelines. This handbook imparted not only practical knowledge on the preparation of food but provided a whole body of knowledge of an all-encompassing living design for working class families with the aim of improving their physiological situation. Using the example of this domestic handbook, the talk will moreover address the pedagogical endeavor that was connected to this novel body of practical knowledge.

The fact that the working class started to cook and eat elaborate meals during the 2nd half of the 19th century was the effect of this epistemic shift towards a practice-based understanding of the nutritional problem.

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