SPSP 2013

The 4th biennial conference of the
Society for the Philosophy of Science in Practice

June 27-29, 2013
University of Toronto, Canada

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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 wilfully disregarding the world except as a product of social construction. Both approaches have their merits, but they each offer only a limited view, neglecting some essential aspects of science. We advocate a philosophy of scientific practice, based on an analytic framework that takes into consideration theory, practice and the world simultaneously.

The direction of philosophy of science we advocate is not entirely new: naturalistic philosophy of science, in concert with philosophical history of science, has often emphasized the need to study scientific practices; doctrines such as Hacking's 'experimental realism' have viewed active intervention as the surest path to the knowledge of the world; pragmatists, operationalists, and late-Wittgensteinians have attempted to ground truth and meaning in practices. Nonetheless, the concern with practice has always been somewhat outside the mainstream of English-language philosophy of science. We aim to change this situation, through a conscious and organized programme of detailed and systematic study of scientific practice that does not dispense with concerns about truth and rationality. Practice consists of organized or regulated activities aimed at the achievement of certain goals. Therefore, the epistemology of practice must elucidate what kinds of activities are required in generating knowledge. Traditional debates in epistemology (concerning truth, fact, belief, certainty, observation, explanation, justification, evidence, etc.) may be re-framed with benefit in terms of activities. In a similar vein, practice-based treatments will also shed further light on questions about models, measurement, experimentation, and so on, which have arisen with prominence in recent decades from considerations of actual scientific work.

There are some salient aspects of our general approach that are worth highlighting:
(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 artefacts, such as conceptual models and laboratory instruments, mediate between theories and the world. We seek to elucidate the role that these artefacts play in the shaping of scientific practice.
(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.

Organizing Committees

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Additional thanks

We gratefully acknowledge the administrative and managerial support of Ms. Muna Salloum whose help was crucial to setting up this conference. We also thank Shahar Avin for web support and Olin Robus for assistance with producing the abstract booklet.

And with great appreciation, we acknowledge the generous support of:
The Social Sciences and Humanities Research Council of Canada
The Institute for the History and Philosophy of Science and Technology, the Department of Philosophy, and Victoria University at the University of Toronto

Local information

One of the most cosmopolitan metropolises in the world, Toronto is a fascinating and entertaining place - considered the best-run large urban centre in the western hemisphere. It is the largest city in Canada and is connected by some of the best highways in North America and has one of the busiest airports in the world, allowing of easy inflow and outflow of visitors who, on the whole consider it 'a city which works and has much to offer'

Tourist and sports landmarks, soaring apartment buildings, sky-reaching office towers, scores of parks and forested ravines, up-to-date museums, dozens of theatres and other night spots, ethnic stores, exotic restaurants and some of the finest shopping malls in the world make it a town which caters to all tastes. Of course visitors cannot hope to see or partake of the all offered goodies. However, with a CityPass a sample of the city and its attributes is possible.

Visitors holding a Toronto CityPass will enjoy the city's five major attractions at one excellent price, doing away with the worries of what to see and do when tourists or visitors and friends travel to this city. At the top of these attractions is the **CN Tower**, Toronto's top landmark. Known as The World's Tallest Building and a Wonder of the Modern World, it provides its more than 2 million annual visitors with three observation levels and a 360-degree view of Toronto, Lake Ontario, and a horizon that stretches to the mists of Niagara Falls. Vying for visitors with the CN Tower is the **Royal Ontario Museum**, Canada's premiere museum, boasting internationally renowned collections of human culture and natural history. One can stroll among colossal dinosaurs from the Jurassic era or discover the richness of Earth's modern bio-diversity and explore the many fascinating artefacts from the ancient world. Even more important to many tourists is the **Ontario Science Centre**, notable for its commitment to tantalizing and informing minds with an eye to future accomplishment in a wide range of disciplines. Hundreds of exhibits encourage visitors to see and think about the world around them. Enjoyed by all ages, the Science Centre features mind-expanding exhibits on space, sports, the human body and the living earth. For many, especially for travellers with children, the **Toronto Zoo** has a magnetic appeal. It houses over 5,000 animals in a spectacular 710-acre zoological park. Travellers interested in history will find that visiting the castle of **Casa Loma**, a fairy-tale castle that was once a renowned palatial home, is an interesting experience. This 98-room majestic landmark features unique architecture and beautifully decorated suites complete with soaring ceilings, rich woodcarving and sumptuous marble. The castle is complete with secret passages, climb twisting towers, and an 800-foot tunnel leading to luxurious stables.

Visitors interested in obtaining a CityPass should call 1-888-330-5008 or visit www.citypass.com/toronto For more information about what Toronto has to offer, please visit http://www.seetorontonow.com/

Weather

In Toronto, June tends to generally warm. From June 26-29, temperatures will range from highs around 28°C to lows around 16°C with a change of thunderstorms each day.

Conference coffee/tea breaks

During the conference, coffee and tea along with baked goods will be included in the delegate fees and will be served during the morning and afternoon breaks. ***Lunch is not provided but there are a great number of eateries within the vicinity of the conference location. The closest is Ned's/Wymilwood Café located in the lower level of the Goldring Student Centre (150 Charles Street West) across the street from the College and open until 3 p.m., Mondays to Fridays.***.

Food around the University of Toronto
Please note that there are hundreds of restaurants in all price ranges serving every cuisine imaginable within walking distance, on Bloor Street west of St. George Street, Yonge Street and in Yorkville.

LUNCH & DINNER (Casual)

Spring Rolls
Style: Pan Asian
693 Yonge Street
416-972-6623

Ginger
Style: Vietnamese
695 Yonge Street
416-966-2424

Mothers Dumplings
Style: Chinese Comfort Food
421 Spadina Avenue
416-217-2008

The Friendly Thai
Style: Casual Thai
678 Yonge Street
416-924-8424

Serra Ristorante
Style: Wood Oven Pizza/Pasta
378 Bloor St. West
416-922-6999

Ethiopian House
Style: Casual Ethiopian
4 Irwin Avenue
416-923-5438

The New Yorker Deli
Style: Breakfast/Deli Sandwiches
1140 Bay Street
416-923-3354

PUBS

Duke of York
39 Prince Arthur Avenue
416-964-2441

Bedford Academy
36 Prince Arthur Avenue
416-921-4600

The Madison Avenue Pub
14 Madison Avenue
416-927-1722

Duke of Gloucester
649 Yonge Street
416-961-9704

Victory Café
Style: Craft Beers
581 Markham Street
416-516-5787

Queen and Beaver (Lunch and Dinner)
Style: British Pub / Restaurant
35 Elm Street
647-347-2712

DESSERT

Summer's Sweet Ice Cream
Ice Cream Parlour
101 Yorkville Avenue
416-944-2637

Greg's Ice Cream
Ice Cream Parlour
750 Spadina Avenue
416- 962-4734

Future Bakery and Café
483 Bloor Street West
European-style pastries
416-922-5875

Dessert Trends
154 Harbord Street
416-916-8155

COFFEE

L'espresso bar Mercurio *(& lunch)*
321 Bloor Street West
416-585-7958

Lettieri
94 Cumberland Street
416-515-8764

Second Cup
170 Bloor Street West
416-975-1723

Starbucks
110 Bloor Street West
416-963-8754

Starbucks
139 Yorkville Avenue
416- 922-8922

RESTAURANTS

Bar Mercurio
Style: Italian/Elegant Dining
270 Bloor Street West
416-960-3877

The Museum Tavern
Style: Classic American Tavern and Brasserie
208 Bloor Street West
2nd floor
416-920-0110

Pomegranate (Dinner Only)
Style: Persian
420 College Street
416-921-7557

Ciao Wine Bar
Style: Italian Wine Bar/Elegant Dining
133 Yorkville Avenue
416-925-2143

93 Harbord St.
Style: Elegant Middle Eastern Dining
93 Harbord Street
416-922-5914

Harbord House
Style: Gastropub
150 Harbord Street
647-430-7365

Messis
Style: Upscale European
97 Harbord Street
416-920-2186

DT Bistro
Style: Fusion (also serves brunch)
154 Harbord Street
416-916-8155

The Boulevard Café
Style: Peruvian and Latin American
161 Harbord Street
416-961-7676

Hemmingway's Restaurant
Style: New Zealand Inspired Grill
142 Cumberland Street
416-968-2828

The Host Fine Indian Cuisine
Style: Indian Cuisine
14 Prince Arthur Avenue
647-955-0876

Brownstone Bistro
Style: Casual
603 Yonge Street
416-920-6288

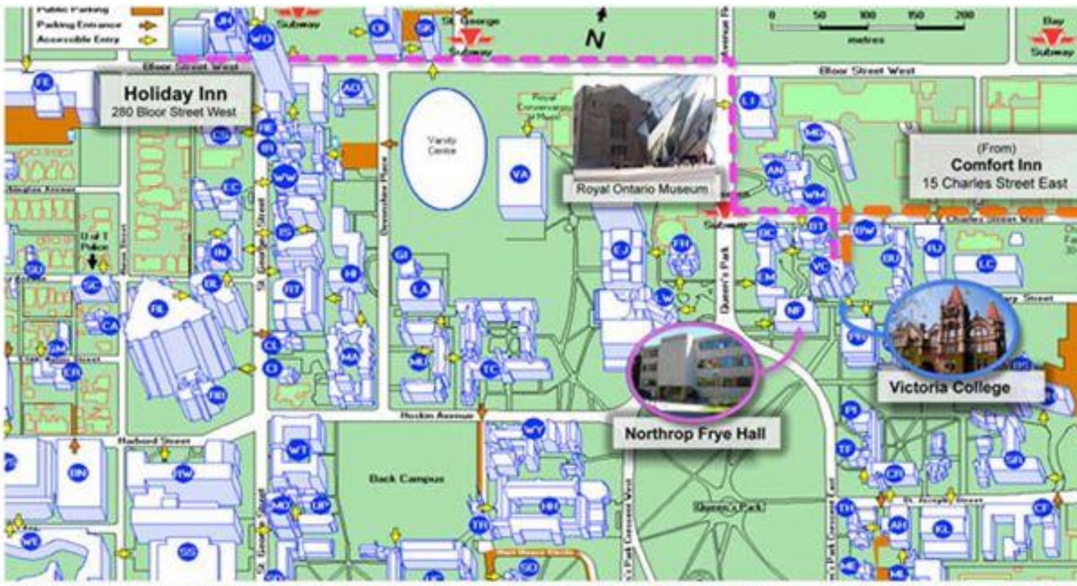
Sushi Inn
Style: Sushi
120 Cumberland Street
416-923-9992

Aji Sai Japanese
Style: All-you-can-eat Sushi
467 Queen St. West
416-603-3366

Sushi On Bloor
Style: Good Value/Casual
515 Bloor Street West
416-516-3454

Conference location and programme

The conference will take place in Victoria College (91 Charles Street West), located at Victoria University at the University of Toronto. The Registration and Information Desks will be located on the 1st floor of the College. Alumni Hall (room 112), also on the 1st floor, will be the location of the Library of Social Science Book Exhibit. The Plenary Sessions will be held in the College Chapel (room 213) and in Northrop Frye Hall (room 003), while all other sessions will be held on the 1st, 2nd and 3rd floors in Victoria College.



Directions from Holiday Inn: Go east along Bloor Street to the Royal Ontario Museum (intersection Bloor and Queen's Park/ Avenue Road). Turn Right onto Queen's Park. South one short block. Left onto Charles Street West. Go East roughly 100m past the Bader Theatre. Turn Right into the alley between Bader Theatre and Burwash Hall. The entrance to Victoria College is directly in front of you. (~15 minutes on foot.)

Directions from Comfort Inn: Walk West along Charles Street (East). Cross Yonge Street, then Bay. Continue along Charles Street. Turn left into alley between Bader Theatre and Burwash Hall. The entrance to Victoria College is directly in front of you. (~10 minutes on foot)

Location of sessions:
All sessions of the conference will be held in Victoria College with one Plenary being held in Northrop Frye Hall, located on the campus of Victoria University at the University of Toronto.

- **Victoria College** (91 Charles Street West) will be referred to in the Program and in the Book of Abstracts as **VC**.
- **Northrop Frye Hall** (73 Queen's Park Crescent East) will be referred to in the Program and in the Book of Abstracts as **NFH**.

SPSP 2013 General Schedule

WEDNESDAY, June 26

7:00-9:00 p.m.	Informal pre-conference social gathering Bedford Academy, 36 Prince Arthur Avenue (416-921-4600)

THURSDAY, June 27

9:00-10:10	Opening Remarks + Plenary Session 1
10:10-10:30	Morning Tea
10:30-12:30	Concurrent Sessions I
12:30-2:00	Lunch
1:00-2:00	Lunch meeting: SPSP newsletter interest group
2:00-3:30	Concurrent Sessions II
3:30-4:00	Afternoon Tea
4:00-5:30	Concurrent Sessions III
5:40-6:50	Plenary Session 2

FRIDAY, June 28

9:00-10:10	Plenary Session 3
10:10-10:30	Morning Tea
10:30-12:30	Concurrent Sessions IV
12:30-2:00	Lunch
2:00-3:30	Concurrent Sessions V
3:30-4:00	Afternoon Tea
4:00-5:30	Concurrent Sessions VI
5:45-7:00	Reception

SATURDAY, June 29

9:00-10:10	Plenary Session 4
10:10-10:30	Morning Tea
10:30-12:30	Concurrent Sessions VII
12:30-2:00	Lunch
2:00-3:30	Concurrent Sessions VIII
3:30-4:15	Closing Remarks, SPSP Business & Planning
4:15-5:00	Afternoon Tea

Plenary Session Abstracts

Plenary 1: ***The Roles of Mathematics in Scientific Practices***

Ian Hacking

University of Toronto and College de France

June 27. 9:10-10:10 VC 213

Mathematics is everywhere in scientific practice. But look at the papers for the previous (2011) conference: Mathematics seems seldom discussed by this Society. This paper will describe some of the ways in which we do science by doing mathematics. These range from simulation, now an integral part of every science, to pythagorean a priori picturing of what the world 'must' be like. Rather than presenting a general analysis, the paper will focus on a handful of examples to illustrate the very different ways in which mathematics is incorporated into scientific practices.

Plenary 2: ***Establishing Causes in Medical Practice: The Role of Cases***

Rachel Ankeny

University of Adelaide

June 27. 5:40-6:50 VC 213

Establishing causes in medical practice: The role of cases Although case studies and reports are central to the epistemic practices utilized within clinical medicine, they appear to be limited in their abilities to provide evidence about causal relations in part because of their reliance on individual instances or very limited sets of patients. As case studies and reports often are used as early communication devices, they tend to provide extremely detailed accounts of particular patients but are limited in terms of filtering of those attributes most likely to be relevant for explaining the phenomena observed. This paper uses a series of examples drawn from recent medical literature in order to explore how case studies and reports are brought together by practitioners in order to make testable causal predictions.

Plenary 3: ***Model Taxa as Platforms for Biological Research***

James Griesemer

UC Davis

June 28. 9:00-10:10 VC 213

The word 'model' is multivalent and has been accumulating meanings since the 18th century. In this talk, I consider organisms, species and larger groupings of organisms as models. A model organism fits something like Goodman's sense in Languages of Art of exemplary instance: as a model citizen is a fine example of citizenship, a model organism is a fine example as well. But example of what? Ankeny and Leonelli's target/scope distinction and Fox Keller's model of/for distinction register a duality of meanings: a model organism presents a fine example of a phenomenon, as in *Drosophila melanogaste*r presents a fine example of Mendelian factor transmission. *D. melanogaster* also serves a basis or platform for the promotion and construction of new work according to a system of research. Specific constructions or preparations in particular laboratories implementing a research system using *D. melanogaster* as a platform constitute models of phenomena. The species, per se, is also an exemplary citizen in a society of scientists: a model organism for the conduct and promotion of scientific research activities. In this talk, I discuss a related kind of platform for research: a model taxon. A model taxon is a (monophyletic) group, typically of biological species, used in modeling practices in which the whole clade constitutes the material platform for a model-based research system. Variation in packages of phenomena that are integrated in characteristic, yet variable ways by the organisms of each species of a model taxon constitutes variety within the model, whereas model organism research must deal with variation among species by comparisons beyond the model system. Model taxa enable and facilitate different research practices than do model organisms, especially those concerning historical, comparative, and evolutionary problems. In this talk, I illustrate modes of comparative analysis, modes of generalization, and extrapolation of methodological and inference lessons across biological specialties that intersect in research projects and programs that integrate evolutionary with mechanistic inquiry.

Plenary 4: *Toward a Political Economy of Epistemic Things*

Sergio Sismondo
Queen's University

June 29. 9:00-10:10 NFH 003

When it comes to issues in the public sphere, philosophy of science often seems to contribute something like second-hand science, buttressing specific claims with familiar justifications. How can philosophers do something significantly different from what involved scientists do? I argue that one approach, useful in some contexts, involves setting aside concerns about the truth or justification of individual or small clusters of claims. Instead, philosophy of science might attend to larger scale issues of political economies of knowledge - we can see affinities with some approaches in science and technology studies in this move. To illustrate, I provide an overview of some key points of control or influence over pharmaceutical knowledge, showing how the drug industry can affect the production, distribution, and consumption of knowledge. We see in the current knowledge regime substantial concentrations of power in few hands and strong incentives to flood the market with knowledge that serves narrow interests.

Symposium Session Abstracts

S1: De-idealization in the Sciences

Organizers: Julie Jebeile & Ashley Graham Kennedy

June 27. 10:30-12:30 VC 323

Recently there has been a lot of discussion in the literature on the role of idealization in the practice of scientific modeling. However, the practice of de-idealization, or removing the false assumptions initially included in a model, has not been discussed. While it has been more or less taken for granted, both by philosophers and scientists alike, that the eventual goal of science is to arrive at more realistic models, presumably via processes of de-idealization, the literature is surprisingly scant on the strategies involved in such processes. We therefore propose that a detailed study of the practice (and the effects) of de-idealization is needed. To that end, we will present four papers in this symposium that examine the practices of idealization and de-idealization in four branches of the sciences: astrophysics, systems biology, economics and engineering sciences.

Boon's paper will argue, by an analysis of examples from chemical engineering, biotechnology and the material sciences, that scientists use both idealization and de-idealization as an epistemic strategy for producing scientific knowledge that is manageable and adequate for the scientific modeling of concrete target systems.

Green's paper will raise the question of whether or not de-idealization is even possible (let alone preferable) in some contexts. Using as examples models in evolutionary systems biology where the search for 'evolutionary design principles' is an important aim, she will reflect on the inherent tension between the heuristic value of generalized principles and the complexity of living systems.

Jebeile and Kennedy's paper will argue, via an examination of a model in astrophysics, that, in some cases, de-idealizing a model has explanatory benefit, while in other cases it does not. Their view is that the entire modeling process - from idealization, to de-idealization, to the reflective work on the process – is necessary in order to genuinely explain certain phenomena.

Knuuttila and Morgan's paper will examine six problems that arise with de-idealization strategies in economics. They will argue that an analysis of these six problems reveals several important points not only about the strategies of de-idealization, but also about those of idealization.

Our aim with this symposium is to generate fruitful discussion of an important, yet neglected, aspect of the practice of scientific modeling.

Idealization and de-idealization as an epistemic strategy in experimental practices

Mieke Boon
University of Twente

June 27. 10:30-12:30 VC 323

In this paper, I aim to explain and evaluate idealization and de-idealization as an epistemic strategy in the context of a more general issue, namely, how scientists produce knowledge that is manageable and adequate for scientific modeling of concrete target systems such as the properties or the dynamical behavior of technological devices. The epistemic purpose of these scientific models is to enable relevant and reliable reasoning about them (e.g., towards creating a desired property, or designing, improving, optimizing or controlling a process).

Nancy Cartwright has been enormously influential in making philosophers aware of the limitations of scientific knowledge, especially when it comes to applying it to real systems. In laboratories, we develop reproducibly functioning experimental set-ups in such a way that stable, repeatable patterns of data are produced, from which we infer to laws of nature. Cartwright (1983, 1999) calls these law-producing experimental set-ups nomological machines. She rightly argues that laws of nature, and scientific models derived from them, are only true at those idealized conditions and usually do not present us with true descriptions of real systems. Nevertheless, (de-)idealization is an important epistemic strategy in the production of scientific knowledge about concrete target systems.

Idealization is closely related to some other epistemic strategies such as: conceptualization (which is the strategy to introduce conceptions of phenomena, for instance, specific physical properties, by means of operational definitions in terms of paradigmatic experimental set-ups; see Feest 2010, Boon 2012); abstraction (which is the strategy to produce representations that abstract from some of the concrete content); mathematization (which is the strategy to subsume measured data-sets under mathematical formula); and simplification (which is the strategy of neglecting in our description aspects that supposedly have a negligible contribution).

In order to explain and evaluate (de-)idealization, I will address the following questions: (I) What is idealization and how does it work in the production of scientific knowledge? (II) How does application of 'idealized knowledge' in the modeling of concrete target systems go about? (III) Why is idealization productive as an epistemic strategy?

(ad. I) I will propose that (de-)idealization concerns the way in which scientific practices develop experimental set-ups for 'discovering' natural regularities. This strategy involves technologically isolating a part of the world such that it exhibits reproducible behavior (i.e., phenomena) that can be studied at varying but controlled and measurable conditions.

(ad. II) Subsequently, conceptual and mathematical descriptions of 'isolated' phenomena, in concord with knowledge of the paradigmatic experimental set-ups at which they have been produced, enable us to identify the occurrence of such phenomena in a concrete target systems under study, and build our knowledge of this phenomenon in the model of the target system. Furthermore, the paradigmatic experimental set-up plays a key-role in investigating the phenomenon at 'non-ideal', 'non-isolated' conditions of the target system.

(ad. III) Examples from chemical engineering, biotechnology and material sciences will be presented such to illustrate this view.

References:
Boon, M. (2012). "Scientific concepts in the engineering sciences: Epistemic Tools for Creating and Intervening with Phenomena." In: Scientific Concepts and Investigative Practice U. Feest and F. Steinle (eds.). Berlin, New York: Walter De Gruyter GMBH & CO. KG, Series: Berlin Studies in Knowledge Research. 219-243.
Cartwright, N. (1983). How the Laws of Physics Lie. Oxford, Clarendon Press, Oxford University Press.
Cartwright N. (1999). The dappled world. A study of the boundaries of science, Cambridge University Press.
Feest U. (2010). "Concepts as Tools in the Experimental Generation of Knowledge in Cognitive Neuropsychology." Spontaneous Generations: A Journal for the History and Philosophy of Science, Vol. 4 (1): 173-190.

De-idealizing general principles in systems biology

Sara Green
Aarhus University

June 27. 10:30-12:30 VC 323

In recent years the aim of finding generalized formal principles of biological organization has been (re)introduced in biology, in particular with the research aim in systems biology of finding so-called organizing or design principles. These are principles whose formal relations apply to a range of systems despite differences in the systems such as fine-grained causal relations, specificities of variables and evolutionary contingencies of different organisms. Attempts have been made to start from abstract idealizations rather than to infer these from detailed empirical studies. This is often done by exploring the ability to apply models, tools and principles from other disciplines such as engineering to the study of living systems. Successful attempts of finding such principles using the tools of network modeling has provided optimism that biological systems can be understood without knowledge on many of the details of the system. However, the empirical application of many of these attempts is still to be determined. It is therefore of great importance to reflect on how the idealizations and de-idealizations are carried out in practice.

The problem of de-idealization has to do with the inherent tension between the comprehensibility and tractability of models and the complexity of biological systems that provide material resistance to any straightforward de-idealization. I shall argue that the generality of organizing principles is at the same time their strength and weakness. On one hand the simple models and generalized principles carry a great heuristic value. They provide an epistemic framework for conceptualizing and addressing a complex problem and a great potential for cross-system analyses. On the other hand, it has been argued that general relations are either trivial or draw on superficial and misleading analogies between systems, providing a simplified view on biological systems that are diverse and contingent. I argue that these considerations must be viewed in relation to different epistemic aims in the scientific practice. In the case of organizing principles, the aim of de-idealizing the models is secondary to the aim of providing “explanations in principle” or exemplars that indicate how the overall behavior of the system(s) is constrained.

This point will be illustrated using case-examples from evolutionary systems biology where the search for ‘evolutionary design principles’ is an important aim. In this field, the search for generalized principles often draws on analogies from engineering. While some see this as a progress towards a more quantitative and predictive biology, others doubt that the abstract formulations can be de-idealized in any concrete way and that the strategy to formulate such idealizations lacks important evolutionary perspectives. This approach is therefore interesting for exploring the tension between the generality of these principles and the complexity and contingency of biological systems. The paper reflects on the prospects of de-idealizations in biology in a double sense - from models to a more complex biological context and from general principles that apply to a class of systems to what counts for species or perhaps even individuals.

Explanatory Models and De-idealization

Julie Jebeile & Ashley Graham Kennedy
University of Paris; University of South Carolina

June 27. 10:30-12:30 VC 323

In addition to theoretical assumptions, all scientific models contain idealizations, i.e. deliberate deformations of properties of their target systems. Because of this they are commonly deemed misrepresentations of their targets. Nevertheless, scientific models are often used to explain the systems that they represent. How does one reconcile what seems to be a contradiction here? According to a commonly-held view of idealization (hereafter the “received view”) scientific models, albeit false, can be said to be explanatory as long as future corrections of their idealizations are possible (McMullin, 1985; Laymon, 1995; Jones, 2005; Nowak, 1992).

In this article, we will argue that the received view faces two major problems. First, it assumes that throughout the process of de-idealizing a model there remains a constant core of assumptions within that model. However, as Morrison (2005) has shown, this is not always the case. Further, this assumption of a constant core is in conflict with what scientists do in actual practice. Scientists often change the laws within a model when aiming to improve its accuracy, thereby changing the core assumptions of that model. Second, the de-idealization thesis claims that only the de-idealized version of a model can be explanatory. However, we will show that in some cases the idealized version of a model is required for a full explanation of the target system in question.

Our arguments will be supported via a study of the example of accretion disk simulations in astrophysics. (The study of accretion disks in energetic objects, from systems ranging in size from low-mass binary star systems to super massive black holes in active galaxies and quasars, requires the use of models because the theory that describes them is, out of necessity, highly simplified and does not allow for the solution of the time-dependent equations describing the disks, which requires numerical techniques (Hawley, 2000).) This is an example in which the original model was subjected to two types of improvement, one of which - the transition from a 2-dimensional to a 3-dimensional simulation - can be understood as de-idealization of the sort described on the received view and one of which - the transition from Keplerian dynamics to General Relativistic theory within the model - cannot.

While both our view and the received view propose that de-idealization has a place in modeling and explanation, our view differs from that thesis in two important ways. First, we argue that model improvement does not require a constant theoretic core or stable structure that survives the improvement process. And second, we argue that idealization and comparison, in addition to a de-idealized model, are often needed for successful explanation. Our view is that the entire modeling process - from idealization, to de-idealization, to the reflective work on the process – is necessary in order to genuinely explain certain phenomena.

References:
Jones, M. R. (2005) "Idealization and Abstraction: A Framework," in Jones, M.R. and Cartwright, N. (eds.), Idealization XII: Correcting The Model. Idealization and Abstraction in the Sciences (Amsterdam: Rodopi, 2005), pp.173-217
McMullin, E. (1985) "Galilean idealization." Studies In History And Philosophy of Science, 16 (3):247–273.
Morrison, M. (2005) Approximating the Real: The Role of Idealizations in Physical Theory in Jones M. R. and Cartwright N. (eds.).
Nowak, L. (1992). The idealizational approach to science: A survey. In J. Brzezinski and L. Nowak (eds.), Idealization III: Approximation and Truth, volume 25 of Poznán Studies in the Philosophy of the Sciences and the Humanities, pp. 9–63. Rodopi, Amsterdam and Atlanta, GA.

Six problems of de-idealization in economics: What does de-idealization tell us about idealization?

Tarja Knuutila & Mary S. Morgan
University of Helsinki; London School of Economics

June 27. 10:30-12:30 VC 323

The literature on 'idealization' covers widely different notions of idealization with different goals and justifications. Typically the theme of idealization is discussed in the context of modelling. One standard example is the so-called Galilean experiment, a procedure which is assumed to be transferable to model-building (e.g. McMullin 1985; Cartwright 1989; Boumans 2003, Weisberg 2007; Mäki 2009). Interestingly, different philosophers have underlined different aims of Galilean idealization: While for McMullin and Weisberg the primary goal of Galilean idealization is simplifying theories in order to make them computationally tractable, Cartwright and Mäki discuss Galilean experiment from the causal perspective as an attempt to study how a cause operates on its own unimpeded by other causes. Another recurrent theme in the discussion on idealization has been the attempt to distinguish between abstractions, idealizations, simplifications and other operations that are used in the process of model construction typically rendering the model unrealistic in view of real-world systems.

In contrast to the discussion on idealization, little attention has been given to the topic of de-idealization. It has been more or less taken for granted, both by philosophers and scientists alike, that the eventual goal of science is to arrive at more realistic and full-blown, or usefully concrete or particular models (e.g. Nowak 1992). Yet the literature is surprisingly scant on the strategies involved in such de-idealization processes. It therefore seems instructive to consider what kinds of problems arise in the processes of de-idealizing some idealizing assumptions.
Our examples come from economics where the unrealistic assumptions underlying economic theories/ models have been a subject to lively discussion since the nineteenth century. While the discussion on idealization in economics has mainly concentrated on theoretical models (see however Cartwright 1989 and Boumans 2005), de-idealization often (but not always) involves a move towards econometric or policy models (different kinds of applications of models). These examples enrich our understanding of de-idealization under six headings: (i) de-idealizing distorting simplifications (Hausman 1992), (ii) de-idealizing mathematical abstractions (Boumans 2005) (iii) de-idealizing contradictory assumptions at different levels of idealization (Hoover 2008; Kirman 1992), (iv) de-idealizing the causal structure of a model in econometric work (Heckman 2000; Hoover, 2001, 2008; Cartwright 2006), (v) de-idealizing various ceteris paribus assumptions (Boumans 2005), and (vi) de-idealizing the conceptual content of models.

These six problems, we suggest, reveal a number of important points not only about the strategies of de-idealization, but also about those of idealization. Firstly, they direct our attention on how idealization and de-idealization work in actual modelling practices, and whether, or when, de-idealization can succeed. Secondly, such investigation also sheds more light on the various suggested distinctions between idealization, abstraction and simplification. Thirdly, and related to the second point, we suggest that some more fine-grained distinctions might also be needed with respect to de-idealization.

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S2: Scientific Research Funding: Practices, Problems, Philosophies
Organizers: Maureen A. O'Malley & Chris Haufe

June 27. 10:30-12:30 VC 215

Scientific research funding affects what is researched and what is not. The funding process is governed by evaluations that appeal to conceptions of what sorts of research projects ought to be funded. We suggest that these conceptions of funding-worthy science are philosophical in nature. Many of the themes that have preoccupied philosophers of science over the last several years (e.g., the nature of science; normative claims about best practice; how values influence scientific activity) are very usefully contextualized in an examination of scientific funding. So far, however, very little philosophical attention has been devoted to understanding philosophical issues in scientific research funding.

This session will examine generally from a range of perspectives how the actual funding process works, and why it works the way it does. We will show that an examination of research funding gives insight into not only the criteria by which specific projects are funded, but also the general goals to which funding strategies are directed. More specifically, we will draw out how philosophical commitments deployed in the funding process influence a diverse aspects of scientific practice, including the pursuit of specific research problems, the styles of inquiry employed, the manner in which research results are communicated, and even the strategies used to allocate funding. Presenters will draw on a range of data about the scientific funding process, from funding policy statements and application guidelines to funding statistics, in order to elucidate the varieties of philosophical issues inherent in this crucial area of scientific practice.

Our first speaker, Gregory Petsko, will discuss the notion of translational research, a theme currently much favoured by funding agencies. He will outline the many problems inherent in conceiving research practice as divided into applied and basic science, and delve into the hidden philosophies that have led to the malaise of today's model of scientific funding. Shahar Avin will focus on the uncertainty of scientific funding by showing that it is never clear what should be funded, and what the social impact of any funded research will be. It is thus unlikely that peer review of grants could outperform funding by 'lottery'. Randomized scientific funding could, in fact, be a healthy alternative to peer review of grants, with merits beyond cost saving. Maureen O'Malley will discuss the funding of particular research programmes and how broader approaches and even fields are brought into existence by funding agencies. In trying to understand how that happens, a philosophy of 'scientific promise' seems to legitimize scientific funding decisions and the ways in which scientists conceive their research for grant applications. Chris Haufe will take this theme further in his account of 'fruitfulness' and how it guides funding strategies, grant review criteria and the success of particular applications. He suggests that fruitfulness, understood as an epistemology, is what guides theory choice rather than some of the other virtues on which philosophers have focused. By connecting these different themes, the session will offer an exploration of how science works practically and philosophically.

Lost In Translation: How Michael Jordan, the Atomic Bomb, and Zombies Are Undermining the Effectiveness of Scientific Research

Gregory Petsko
Weill Cornell Medical College

June 27. 10:30-12:30 VC 215

I hate the term 'translational research'. To be fair, I am an equal-opportunity hater: I also hate the term 'basic research'. By categorizing research in this way, we create the artificial, and I think fundamentally incorrect, impression that there are two distinct types of research, that one is better than the other, and that they must compete for research funding. We should excise these terms from our vocabulary. There is only 'research', and it comprises a seamless path from discovery to application. Without discoveries ('basic research'), there would be nothing to translate; all advances in application would be incremental. And without practical applications ('translational research'), it would be difficult, if not impossible, to persuade the public to support the research enterprise at all.

But there is more than an issue of diction involved when we ask what the philosophical underpinnings are behind our current model for research support. Accepting, for the moment, that the scientific enterprise has outgrown the support structure that has evolved for it, that funding decisions are too often excessively conservative and risk-averse, and that translational and basic research are locked in an internecine struggle for the souls and dollars of those who make funding decisions, I want to explore the hidden, unspoken philosophies that are responsible for this situation.

The first is what I call the Michael Jordan Effect. When an individual receives an enormous amount of fame and money because of a particular way of doing things, there is a natural tendency for others to imitate that style, rather than developing their own. In basketball, this can lead to a decade of stagnation in the development of the game. In scientific research, it can lead to a lengthy period in which projects that mimic the original fare disproportionately well in attracting funding, regardless of their intrinsic worth. I argue that this has happened as a result of the success of the Human Genome sequencing program.

The second is the Manhattan Project Metaphor, in which it is assumed that 'top-down', targeted efforts are the best way to achieve big results in science, because that is how the atomic bomb was developed. I will show that this assumption is based on a misunderstanding of what the Manhattan Project was, and offer some comments about what kind of problems are, and are not, most effectively tackled in this way.

The third are the Zombie Ideas and Zombie Programs: these should have all died a long time ago, but they continue to shamble on, threatening to eat our brains. Among them are the notion that all data are equally valuable; the Protein Structure Initiative; and Genome Wide Association Studies. My discussion of these will lead to my final argument, which is that we need to have an in-depth discussion, at both the philosophical and practical levels, about how we assign priorities in scientific funding.

How I Learned to Stop Worrying and Love the Scientific Lottery

Shahar Avin
University of Cambridge

June 27. 10:30-12:30 VC 215

I argue that the most important lesson anyone interested in scientific funding can learn from philosophy of science is that, in the majority of real cases, no one has the necessary information needed to decide effectively what should be funded. I will propose a philosophy of science funding that leads naturally to randomized allocation of resources for research: a science lottery.

A call for a science lottery, as a possible alternative to grant peer review, was made by Greenberg (1998, Lancet, 425). The claims he makes about the high cost and low effectiveness of peer review have been supported by Graves et. al. (2011, BMJ, d4797). They estimated the costs and analyses, using statistical methods, the reliability of grant review panels of the National Health and Medical Research Council of Australia. The worry about high costs and difficulty in choosing between equally good proposals have led the Foundational Questions Institute (FQXi), an international non-profit organization, to use a lottery for its mini-grant funding scheme (Kanipe, 2007, http://fqxi.org/data/articles/MiniGrants_Major_Benefits.pdf).

My main argument is that peer review of grants could not outperform a lottery, at least given realistic time and resource constraints, because the amount of information required to make good judgments about funding is rarely present. The argument requires a short detour to establish the characteristics of grant peer review, and to suggest for what I think should count as the necessary information. Briefly, I argue that in order to rank proposals, national funding bodies need to predict the eventual social good that will arise from the social uptake of the actual products of the proposed research. This means the prediction needs to cross two deep chasms of uncertainty: the highly non-linear process of scientific investigation, and the highly non-linear process of social uptake of scientific results. If grant peer review cannot outperform a lottery, the costs of peer review make the lottery the better alternative.

Some worries need to be addressed before we can adopt a wide-scale lottery. We can avoid unreasonably low success rates by limiting access to the lottery to holders of relevant credentials. Abuse of the system can be prevented by light-handed gatekeeping. We can avoid neglecting research in significant areas by creating multiple lotteries, some dedicated to more specific interests and others accepting a broad range of applications.

If implemented wisely, a science funding lottery can have benefits that go beyond cost saving. If the lack-of-knowledge argument is right, then the image of "rational" or "impact-led" science funding is misleading. A science lottery would communicate the healthy message that science funding is fallible. It would also ease the psychological burden of having research proposals rejected. I hope that by the end of the talk you too will learn how to stop worrying and love randomized science funding.

Philosophies for the Funding of Scientific Research Programmes, Or, Why Some Research Areas Attract So Much Investment

Maureen A. O'Malley
University of Sydney

June 27. 10:30-12:30 VC 215

The main theme of this session is that particular philosophies of science guide research funding strategies. These philosophies can be as broad as, for example, the normative position that the best science is hypothesis driven and not 'merely' exploratory (O'Malley et al. 2010, Cell, 611). Other philosophies of funding are more targeted, favouring specific research approaches over others, both in regard to methodologies and institutional arrangements. The fairly recent emergence of systems biology is a good example of this kind of targeted funding, which has ensured the rapid rise of systems biology research teams, departments, journals, conferences and teaching programmes across the world (Powell et al. 2007, Hist Phil Life Sci, 5). While for many people, systems biology is understood as just another variant in the turn towards 'big science' rather than individual laboratory-based science, this diagnosis does not explain why systems biology has proved so attractive to funding agencies, and nor does it take into account funding agency reasons behind funding decisions.

In order to provide some of this explanation, I will focus not on the broad and diverse field of systems biology, but on a cognate area of research called metagenomics. This set of practices has many parallels to early systems biology, in that its main focus is currently cataloguing parts (the DNA sequences of microbial communities), but its aim and ongoing challenge is to provide quantitative mechanistic accounts of dynamic interactions in complex molecular, organismal and ecological systems. Despite the field's current lack of explanatory models, and regardless of the fact its practitioners are 'drowning in the flood of data' (Fierer and Ladau 2012, Nat Meth, 549), metagenomic projects are funded to an extraordinarily high level and have a very visible public profile (especially when the microbial communities being scrutinized happen to occupy human bodies).

My presentation will examine the ways in which funding agencies have justified the financing of large metagenomic data collection exercises, and how metagenomicists pitch their projects in order to be successful grant applicants. My aim is to draw out the philosophies of science underpinning what at first glance appears to be highly exploratory science. I will suggest that indications of novel causal forces, combined with basic commitments to understanding multilevel biological systems, have managed to overwhelm the usual suspicions of exploratory science as not 'real' or merely preliminary discovery science. What has happened in the case of metagenomics, and also more broadly in regard to systems biology, is that a 'philosophy of scientific promise' has been crucial to funding decisions. I will elaborate on what this means for a philosophical understanding of contemporary scientific practice, as well as for how scientists write their grant proposals.

The Epistemology of Fruitfulness

Chris Haufe
Case Western Reserve

June 27. 10:30-12:30 VC 215

Examining the process by which research programs are judged to be good candidates for funding adds a new dimension to our understanding of how scientific research is evaluated more generally. For grant proposal reviewers, choices between different research programs are not — at least, not directly — choices about which program has the best success record (by any measure). They are choices about which program is most likely to exert, in the words of the National Institutes of Health, "a sustained, powerful influence." In this case, traditional epistemic desiderata like truth, accuracy, and problem-solving capacity are shunted to the background while proposal referees — of whom the overwhelming majority are scientists — attempt to assess how fruitful a proposed research project is likely to be.

This paper has three aims: (1) to articulate at a basic level the processes by which judgments of fruitfulness are made in the funding process; (2) to connect the factors involved in those processes with general features of the concept of fruitfulness; and (3) to bring the points made in (1) and (2) to bear on more general considerations about theory choice in science.

To achieve (1), I attempt to explain the results of a recent internal study conducted by the NIH that analyzes the statistical relationships between the "Overall Impact" score received by a grant application and the various "Review Criteria" ("Approach," "Significance," "Innovation," "Investigator," and "Environment") scores it receives. If overall impact scores are reflections of grant proposal reviewers' assessments of potential fruitfulness (a point for which I argue in the paper), then the relationship that a particular review criterion bears to overall impact can be understood as that criterion's contribution to the probability that a given project will prove fruitful. For each criterion, I explain at a conceptual level why it bears the particular relation to fruitfulness that it apparently does.

In developing aim (2), I show how the considerations reflected in review criteria judgments are explicable as particular instantiations of deeper, more general properties of the concept of fruitfulness. Drawing on patterns of inference in science and mathematics, I argue that the principles employed in the evaluation of proposed research projects overlap in substantive and surprising ways with the principles employed in the evaluation of existing research projects.

The final section of the paper argues that this overlap in principles suggests a revised understanding of the dominant epistemic goals governing theory choice in science. Viewing the epistemology of science as the epistemology of fruitfulness (a) unifies patterns of theory choice in the sciences with patterns of proof style choice in mathematics, (b) unifies patterns of theory choice in scientific investigation with patterns of research program choice by grant proposal referees, and (c) provides a common epistemic explanation for the tendency for working scientists to adopt a theory or theoretical framework in advance of much of the sort of evidence that would attest to the theory's truth, accuracy, or problem-solving capacity.

S3: Taxonomic Practices in the Scientific Study of Cognition: Do Valid Constructs Matter?

Organizers: Muhammad Ali Khalidi

June 27. 10:30-12:30 VC 212

Psychology and neuroscience share the goal of illuminating the processes by which humans acquire knowledge about themselves and the world. In psychology, investigators place a high value on "construct validity". In other words, they aim to develop tasks that measure or individuate those aspects of cognition (e.g. intelligence) or those cognitive processes (e.g. working memory) that they intend those tasks to measure. Additionally, in the ideal case, that feature of cognition or cognitive process that a task measures actually corresponds to a real feature or process in the natural world. For example, intelligence tests are supposed to measure intelligence, which is taken to be a real attribute that a person can have to varying degrees.

Given the importance of construct validity in the cognitive sciences, this panel aims to address several questions by considering case studies from both psychology and neuroscience. First, what are the distinguishing features of a valid construct and how are valid constructs differentiated from invalid ones? Second, what role do constructs play in psychological explanations, and how important is construct validity for the success of such explanations? Third, given that psychology and neuroscience both aim to explain cognition, how important are valid constructs in psychology for integrating psychological and neuroscientific explanations of cognitive phenomena? Fourth, what advances have there been over the past few decades in articulating a notion of construct validity in the cognitive sciences, and is there room for convergence with recent philosophical accounts of natural kinds in the special sciences? Finally, how does the demand for valid constructs relate to the distinction between folk and expert concepts, and to the need to disseminate scientific knowledge to the general public?

The papers in this symposium consider different positions on the importance of construct validity for the successful practice of cognitive science. There are some indications that failure to articulate consistent standards for construct validity can impede scientific progress, but there is also evidence that scientific research can be successful in the absence of such standards. When do concerns about construct validity, whether voiced by philosophers or scientists, simply impede scientific research? And at what point does the failure to employ valid constructs serve to hide conceptual confusion and theoretical incoherence?

Natural Cognitive Kinds: Innateness as a Case Study

Muhammad Ali Khalidi
York University

June 27. 10:30-12:30 VC 212

Though it originated as a folk category, innateness has featured prominently in contemporary controversies in cognitive science. There are debates concerning whether the linguistic faculty is innate to the human species, the extent to which numerical, spatial, and causal cognition are innate, and the relative innateness of moral and religious concepts, among others. Yet, some cognitive scientists and philosophers have doubted the very concept of innateness, questioning its suitability for rigorous scientific theorizing.

Various attempts have been made to provide an analysis of innateness that accords with contemporary cognitive science, including analyses based on canalization (Ariew 1999), entrenchment (Wimsatt 1999), psychological primitiveness (Cowie 1999; Samuels 2002), triggering (Khalidi 2002; 2007), and process invariance (Weinberg & Mallon 2006), among others. However, all these attempts have been deemed beside the point by other theorists, since innateness has been criticized as an obsolete folk concept associated with a kind of biological essentialism (Griffiths, Machery & Linquist 2009). Moreover, rather than being a unified concept, it is held to be a multivalent category, combining a number of disparate criteria (Griffiths 2002). However, the first critique is not decisive since many scientific concepts originate as folk concepts before being refined and revised in order to make them suitable for scientific theorizing. The second critique is also not fatal, since innateness may be a polythetic category or a cluster concept, which combines several of the features posited by the analyses mentioned. Though this possibility has been objected to on the grounds that the innateness concept consists of a “clutter” rather than a cluster of criteria (Bateson & Mameli 2007; Mameli 2008), I will argue that the objection does not succeed. What vindicates the category of innateness and enables it to play a role as a valid construct in contemporary cognitive science is its appearance in robust causal processes or “nomological networks” (Cronbach & Meehl 1955).

The claim that innateness is a cluster concept has also come under attack recently, for several reasons. First, it is said to lead to faulty inferences from one of the properties in the cluster to another (Shea 2012). But this is a risk associated with many other scientific concepts that pertain to exception-prone empirical generalizations or causal processes that are not strictly deterministic. Second, it is claimed that even though there is a general clustering of properties with respect to innateness, the clustering is especially unreliable when it comes to human beings, given the flexibility and plasticity of human cognitive and behavioral traits. But this observation does not undermine the theoretical utility of the concept, at least in cognitive science. Third, it is sometimes said that the innateness concept ought to be eliminated because it has had a pernicious effect on popular discourse concerning intelligence and other cognitive capacities, as witnessed by the widespread use of such pseudo-scientific expressions as “hard-wired”. However, even if the concept has had a negative impact on lay discussions, I will argue that scientists ought to be guided by epistemic purposes in their taxonomic practices, not by moral or political considerations.

This paper will conclude with a general proposal for identifying cognitive kinds that is based on taxonomic practices in cognitive science. Like kinds in other sciences, both basic and special, kinds in the cognitive sciences are validated by the role that they play in causal networks. When the instantiation of a property or, more commonly, the co-instantiation of a cluster of properties leads causally to the instantiation of a multitude of other properties in recurring causal processes, we identify such a property or set of properties with a natural kind. These natural kinds then enable us to explain the occurrence of the properties that they cause and to predict the occurrence of those properties, which is what makes natural kinds so central to the scientific enterprise.

Is construct validity necessary for mechanistic explanations of cognitive functions?

Jacqueline A. Sullivan
Western University

June 27. 10:30-12:30 VC 212

Mechanistic explanations of complex phenomena are taken to require the integration of results across areas of science purportedly situated at different levels of analysis. This is thought to be particularly true with respect to mechanistic explanations of cognitive functions (e.g, Craver 2007; Picinnini and Craver 2011). To date, philosophical discussions of how such integration culminates in mechanistic explanations have been silent with respect to whether methodological differences across levels of analysis may be regarded as obstacles to providing complete mechanistic explanations. This paper is concerned with one such methodological difference, namely, variations in the emphasis placed on construct validity across those areas of neuroscience directed at the study of learning and memory. An investigator may be said to value “construct validity” (e.g., Cronbach & Meehl,) just so long as she aims to develop tasks rigorous enough to individuate discrete cognitive functions and to delineate actual processes that occur in the natural world (so-called “natural kinds”). However, construct validity may operate as one constraint on experimental/task design and it may not always be regarded as the most important or fundamental constraint. The question I aim to address in this paper is whether or not construct validity is necessary for providing integrative mechanistic explanations of cognitive functions.

In order to address this question, I focus on two areas of contemporary neuroscience directed at the study of learning and memory: cognitive neuroscience and cognitive neurobiology. I appeal to the case study of recognition memory in order to demonstrate that cognitive neuroscientists prioritize the validity of their constructs. Specifically, they aim to design cognitive tasks that may be used effectively in conjunction with electrophysiological recording and imaging techniques to localize cognitive functions to specific areas of the brain. Importantly, progress in the field is taken to involve the refinement of cognitive tasks in an effort to ensure that the function individuated by a given task and the brain area(s) involved in the task are sufficiently discrete. Achieving these goals is clearly important for identifying a phenomenon to be explained and circumscribing the region of interest (ROI) in the brain in which the mechanism that realizes that phenomenon is housed. Cognitive neurobiologists, in contrast, are far less concerned about the validity of their constructs and far more concerned with the cellular and molecular mechanisms that bring the functions about. I support this claim by revisiting the case of spatial memory (Sullivan 2010).

I first address the question of whether this difference in emphasis on construct validity means that cognitive neuroscience is on firmer epistemological footing than cognitive neurobiology. I establish by appeal to several case studies that the answer to this question is “no”. Then I take up the question of whether the absence of construct validity in cognitive neurobiology is an impediment to integrating results across levels of analysis in neuroscience–i.e., whether this difference is an obstacle to providing mechanistic explanations of cognitive functions.

Taxonomic practices in the scientific study of cognition: A view from the trenches

Kristina Visscher
University of Alabama at Birmingham

June 27. 10:30-12:30 VC 212

No abstract available

S4: Research systems and the organization of practices
Organizer: Elihu M. Gerson

June 27. 2:00-3:30 VC 323
Research practices are not isolated from one another; rather, they are organized and contingent upon one another and their institutional contexts. Moreover, these organizations and contexts are persistent. In recent years, many scholars have described these organized research efforts under names such as laboratory system, experimental system, and so forth. We use the more inclusive term research system to indicate the class of related organized persistent inquiries even when they don't involve experiments or laboratories. A research system is an organized group of efforts devoted to a particular problem or family of closely related problems, and embodied in or realized by one or more concrete research organizations such as laboratories, museums, observatories, centers, or field sites. As part of their work, research systems develop, revise, and deploy both new and established practices. These new practices in turn function as new or increased capacities in the same or different settings. Research systems also include people and the resources needed to carry out studies and interpret their results. Research systems are thus the means by which scientists juxtapose and articulate multiple actors, materials, and ideas so as to reliably produce and control phenomena of interest.

The session will begin with a very brief discussion of the concept of research system in order to provide necessary groundwork. The session then continues with three papers that provide perspectives for analyzing research systems. Sterner examines the concept of research problems, a crucial individuating factor for research systems. He argues that articulating the conceptual structure of a problem allows scientists to institute a rationalized, collective schema for coordinating effort. DiTeresi reconsiders the notion of "types" or "schemas", which he conceptualizes as reference standards that are richer than numeric measures, but less rich than concrete exemplars. Schemas allow for variation and plasticity in the conduct of research, while ensuring commonality of conception. Schemas are one crucial way that local practical achievements can at the same time be contributions to the growth of a collective capacity. In this way, they constitute key components of institutional mechanisms for the processes of integration and innovation. Gerson's paper makes use of these two ideas (research systems as problem-solving institutions and as inquiry schemas) to address the problem of conceptualizing the relationship between the "epistemic" and "organizational" aspects of research. This process is one of understanding how the variable practices of concrete organizations can generate a series of conventional repertoires that collectively instantiate a robust institutionalized pattern of conduct (the schema).

Structuring Problems, Coordinating Research

Beckett Sterner
Field Museum of Natural History, Chicago

June 27. 2:00-3:30 VC 323
I argue that we can analyze research systems by looking at the institutional implications of how scientists articulate shared research problems. Research problems set the motivations, presumptions, and aims of communities of scientists. However, problems are also always subject to re-articulation as scientists revise their beliefs and commitments. As a result, these changes pose a constant challenge for the coordination of research work: re-stating a problem in a new way can affect the significance of existing research projects, threatening their viability as lines of work. One solution is for scientists to organize problems into parts that can be pursued quasi-independently, rationalizing their research to make progress possible on multiple fronts and scales. In this way, problems become a place for scientists to coordinate the epistemic and organizational aspects of their work.

The conceptual articulation of research problems both causes and expresses the institutional structure by which scientists make progress as a group. When scientists endorse a problem articulation, they entrench it as a shared tool for guiding and evaluating research across the community. They also put into effect (i.e. institute) local norms for what counts as a good answer. The conceptual structure of the articulation therefore organizes the thinking and actions of scientists, and in the converse direction, scientists use the results of their actions to re-articulate the problem structure.

I argue that a general comparative approach to research problems in their institutional context should distinguish at least two attitudes a community may have as a whole toward a problem articulation. In the first case, the community recognizes a jointly held problem, but the scientists have no consensus on an adequate articulation of that problem. Alan Love's recent work on problem agendas has focused on evolutionary development as a particular kind of institutional interaction -- what Elihu Gerson has called a "juncture" between specialties -- and I suggest evo-devo's question, "What is evolutionary novelty," falls under this first case of community attitude toward a problem.

In the second case, there is a community consensus on a single articulation of the problem that is taken as a complete representation of the problem's meaning. I claim this is the appropriate way to understand Philip Kitcher and Sylvia Culp's work on "normal forms of problems" and "explanatory schemas" in an institutional context. The schemas they describe connect empirical concepts from the science with abstract variables that can be filled in under certain rules to produce an explanation. Problem schemas rationalize research in a community by standardizing its questions and segregating the parts of problems into individual, independent variables. As a result, problem schemas offer a common framework for a community to become its own, semi-autonomous specialization.

Coordinating Collective Research in Developmental Biology: Types as Reference Standards

Christopher A. DiTeresi
George Mason University

June 27. 2:00-3:30 VC 323

Describing and comparing as epistemic activities have been relatively neglected by philosophers of science. In this paper, I propose a practical account of ‘types’ as tools for coordinating collective scientific practices of comparison and description. Rachel Ankeny has recently shown how what she terms ‘key cases’ (e.g., the chick, the frog life cycle, diapause in insects) are used in developmental biology to describe typical patterns of phenomena that serve as reference points for the field. In order to refine and to supplement her discussion, I attempt to answer two questions regarding these ‘key cases.’ First, what exactly are the cases? And second, what is it to be a key case or to serve as a reference point? To the first question I consider a number of possible responses, including whether the case is a kind of organism (the chick), or an instance of a general phenomenon in one kind of organism (the frog life cycle), or a scientific achievement (the work that constituted the case), or an idealized model of typical development abstracted from the study of a variety of individual organisms (a series of developmental stages). After rejecting each response, I suggest that the challenge to understanding cases such as these is that they are ongoing foci of investigation, and as such the conceptualization of the typical pattern is itself in flux. I then turn to the second question as a way of addressing this challenge. Ankeny has persuasively argued that key cases serve as baseline typical patterns that are used to detect and describe themes and variations in other organisms. Building on her discussion, I argue that given the work they do in scientific practice as reference points, ‘the chick’ and its ilk are better understood as types rather than as cases. I propose to define types functionally by the roles they play in coordinating collective practices of description and comparison. Types work via reference standards that allow the local articulation of versions of the type to situations, while at the same time enabling the versions to be recorded or collected, and then accessed by others with whom one may not share much. Types are thereby one crucial way that local practical achievements can at the same time be contributions to the growth of a collective capacity. Types are held in common but are not fixed; they are maintained and updated. This feature of types suggests analyzing them in terms of Hasok Chang’s notions of epistemic iteration and semantic extension. I argue that types require modifications to both notions, and accordingly I articulate two new notions - semantic plasticity and versioning – that correspond to semantic extension and iteration.

By way of a conclusion, I point to two implications of my view of types that I take to recommend the institutional perspective of research systems. First, both types and reference standards are institutions, and not primarily either idealized representations or concrete objects. And second, since types are components of research systems that can be shared across different lines of research, they can serve to institutionalize explanatory relevance between specialties and thereby foster scientific integration.

Research systems as institutions and organizations

Elihu M. Gerson
Tremont Research Institute

June 27. 2:00-3:30 VC 323

Research systems do a variety of things in order to address a problem: they develop models, characterize phenomena, and collect and analyze data. The problem that individuates a research system, and the conventions that shape the way it is addressed, constitute a reference standard or inquiry schema in the sense of DiTeresi; that is, a way of framing a set of concerns and issues, and formulating a set of concepts and procedures for addressing them. A research system is thus an institution in process of development; when the development stops, it's not research any more. Research organizations such as laboratories and museums enact the schema in the same way that orchestras perform the music in their repertoires. Like jazz, each performance in a research system is also a composition, not merely a rendering. A research system then, is a developing institution instantiated by one or more organizations. There are thus two kinds of practice associated with a research system: those associated with the schema, and those associated with the constituent organizations. The organizations and the schema establish different practical constraints on the research. Organizations impose contingencies such as budget and local feasibility; schemas impose epistemic contingencies such as relevance and accuracy. Some kinds of contingency, such as testability, are jointly imposed by both organizations and the schema.

Schemas and their constituent organizations can vary independently to a considerable degree. Changes in the schema (e.g., elaboration of a new concept, refinements of a model) aren't necessarily reflected in the structure of a constituent organization. The structure of research organizations is often flexible enough to accommodate many different kinds of research activity. Similarly, changes in an organization (e.g., a new hire or new administrative policies) aren't necessarily reflected in the structure of the schema. And of course, organizations vary in their local arrangements independently of one another as well. As a result, schemas and their organizations respond in different ways to different intellectual and administrative events. For example, losing grant funds might be very important to an particular laboratory, but only mildly consequential to the development of the schema.

These considerations are illustrated with examples from a continuing study of the Museum of Vertebrate Zoology at Berkeley. The Museum's history offers many rich examples of the ways that schemas and local organizational arrangements both support and limit one another. More generally, the notion of research system is an aid to developing a rigorous comparative natural history of the research process. Such a natural history will enable us to take account of how new or changed practices influence local organizations, broader institutional patterns of research organization and the development of inquiry types. It will also aid analysis of more specific problems such as the integration of practice.

S5: Meeting the brain on its own terms?
Organizer: Philipp Haueis

June 27. 2:00-3:30 VC 215
In this session, we aim for a philosophical, historical and scientific assessment of the thesis that the human brain can be studied without involving the vocabulary of cognition – that it can be “met on its own terms”, as it were. Our project is part of the interdisciplinary research initiative “Critical Neuroscience”, which studies neuroscientific practice from the interlocking perspectives of methodological, historical, philosophical, as well as political and sociocultural approaches – bringing in view those multiple contexts that have made the modern social, cognitive and affective neurosciences (hereafter: ‘SCAN’ disciplines) such an influential discursive player concerning questions about bio-technology, human nature and the transformation of society (Choudhury and Slaby 2012). As it is a crucial part of this initiative to practice critique as an activity that is not externally imposed upon, but itself a part of neuroscientific research, our theoretical discussion will be guided by the question of how the phrase “meeting the brain on its own terms” can be made fruitful for actual practice, while at the same time being philosophically justified and historically grounded.

One of the working hypotheses in the contemporary SCAN disciplines is that the “mind is what the brain does”. Therefore, most researchers readily assume that investigating cerebral functions or organizational principles of the human brain is equivalent to studying the mind. But it is a priori unclear whether the question of how the brain works is in research practice substitutable with the question of how the mind works. Our aim in this panel is to show that, partly because of the missing disambiguation between questions regarding the mind and questions regarding the brain, contemporary research in human neuroscience is so far lacking a discussion about which concepts are adequate to describe the complex system of the brain. Our constructive point is that there are good methodological reasons, supported by literature from history and philosophy of science, to meet the brain on its own terms, i.e. to do exploratory research which investigates cerebral structure and function without using cognitive or psychological vocabulary. The structure of the panel session is to move from a general framework to increased concretion with each talk. First, we assess that the ontological and practical validity of psychological concepts used to describe the “mind” in current neuroscience is at least questionable, despite attempts to formally classify cognitive vocabulary. Second, we argue that even if a better method to operationalize the concepts with which humans describe their mental life were available, it is still unclear how to map such constructs upon measured differences in brain activity. This is chiefly because the physiological principles governing the mesoscopic scale of brain functioning are largely unknown, but they are urgently required to connect neuroscientific knowledge about micro- and macroscopic dynamics of cerebral function. In the absence of better psychological constructs and physiological knowledge, we suggest that the neuroscientific community is currently in a stage where exploratory experiments – i.e., experiments that do not test theoretical hypotheses – are advisable to better understand how the brain works. Finally, we will explicate this in a case study about resting-state functional connectivity research, showing that such experiments can articulate new concepts that describe previously inaccessible aspects of reality.

Letting Be – Constitution in Scientific Practice

Jan Slaby
Free University Berlin

June 27. 2:00-3:30 VC 215
In the first talk, Jan Slaby will provide a conceptual clarification of how something can be met “on its own terms”, by introducing some key notions from phenomenological and existentialist perspectives on scientific practice. At the core of his talk stands the idea that every object of inquiry demands a vocabulary which is adequate to describe its structure, and that it is a priori unclear whether cognitive or more broadly psychological concepts adequately characterize how the human brain works. Slaby thus contends that the common assumption of many neuroscientific practitioners that “the mind is what the brain does” is problematic, especially because the ontological and practical validity of psychological concepts used to describe the “mind” is questionable (Turner 2012). The opening talk concludes by arguing that finding new and more adequate concepts to describe the brain has to coincide with a change in what it means to be a neuroscientist. If the human brain can be investigated

meaningfully without using psychological concepts, then what researchers are committed to in practice are assumptions about the biological principles governing cerebral function, rather than cognitive models of the human mind. Finding a vocabulary for these principles in empirical science is especially difficult, since researchers here deal with aspects of reality for which there are no concepts in everyday language (Rouse 2011), such as for the anatomical structure and organizational principles of the brain. The way in which scientific objects such as the brain are conceptualized is thus inextricably linked to the methods, rules and institutionalized traditions that govern research practice. This linkage can be explicated by a non-constructivist notion of constitution, where meeting something on its own terms means "letting" an object "be what it is" (Haugeland 1998; Rouse 2002).

Exploratory Experiments in Neuroscience? – Lessons from History

Philipp Haueis
Wesleyan University

June 27. 2:00-3:30 VC 215

In the second talk, Philipp Haueis will directly follow the trail opened by Slaby's analysis and complement it with the discussion about exploratory experiments in history and philosophy of science (Hacking 1983). While there is an extensive literature discussing examples from biology where experimenters proceeded without testing well-developed theoretical alternatives (Rheinberger 1997; Burian 1997), a systematic discussion of exploratory experimentation in neuroscience is so far missing (as noted by, e.g., Franklin 2005). Haueis will use the existing historical studies to argue that the neuroscientific community is in a stage where exploratory analysis is advisable to better understand how the brain works. The reason is that the physiological principles governing the mesoscopic scale of neuronal functioning are largely unknown; however, exactly these are needed in order to connect neuroscientific knowledge about micro- and macroscopic dynamics of cerebral function. For instance, if an area shows net increase in the ratio between oxygenated and de-oxygenated blood, it is still unclear whether the neurons in that area largely inhibit or exhibit received signals, or do both (Logothetis 2008). Given the lack of this knowledge or of any general theory of how the brain may work, it is most fruitful to conduct experimental series which vary a large set of different parameters in order to map out the characteristic features of neuronal phenomena. Such a systematic but largely 'theory-free' research could help find a novel conceptual framework which might structure later inquiry (Steinle 1997).

Exploratory Brain Research: The Case of Resting State Functional Connectivity Studies

Daniel S. Margulies
Max-Planck-Institute for Human Cognitive and Brain Sciences

June 27. 2:00-3:30 VC 215

In the third talk, Daniel Margulies will give a concrete example of how exploratory neuroscientific experimentation in practice might look like. Margulies will report about his recent neuroimaging research in resting state functional connectivity studies, where large-scale brain dynamics are measured without testing a cognitive task. While this neuroscientific subfield is gaining increasing significance for a variety of issues such as inter-individual variability or age differences in functional cerebral architecture (Biswal et al. 2010), using resting state functional connectivity for discovery-based research also faces institutional challenges, for example related to the acquisition of funding. The work of Margulies and others provides an example of successful exploratory experimentation, as they found new functional subdivisions within the cortex not present in previous anatomical parcellation schemes (Margulies et al. 2009, Nelson et al. 2010). Moreover, the 'functional connectivity' approach may be a candidate for a new conceptual framework which meets the brain "on its own terms". The term 'functional connectivity' had been used already in earlier electrophysiological studies, before it was adopted by the neuroimaging community to investigate macroscopic aspects of neural activity (Friston 1994; Haueis 2012). Thus, the application domain of 'functional connectivity' ranges over different levels of neural activity (e.g. single firing neurons, neuronal assemblies, cortical networks). Together with other techniques (e.g., EEG, MEG), resting state fMRI studies can help to find general empirical rules that govern the cortical organization at these different levels, without the need to settle for a particular cognitive or psychological interpretation of human brain function.

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S6: Mathematical (and applied mathematical) Conceptual Practice

Organizer: Kenneth Manders

June 27. 4:00-5:30 VC 323

Mathematics provides conceptual support for many scientific disciplines. Mathematics is also internally conceptually innovative. Because foundational analyses re-cast in a way that is not necessarily sensitive to this phenomenon, we must directly study it in mathematical practice.

The case studies in this session explore what goes into furnishing improved mathematical conceptual structures. Morris and Avigad follow the 19th-century development of the notion of a group character in the proofs of Dirichlet's Theorem on primes in arithmetic progressions, as an instance of the emerging abstract function concept. Hunt explores the differences between group-theoretic and group-avoiding theory of atomic Spectra. Manders synthesizes several case studies into an account of how features of mathematical language usages enhance intelligibility of specific problem areas.

Expressive means and Mathematical Conceptualization

Kenneth Manders
University of Pittsburgh

June 27. 4:00-5:30 VC 323

Philosophy of mathematics has often treated representation on the paradigm of notational variants: for philosophical purposes one recasts, subject only to ``can I get this in my system?''. Differences among such alternatives are ``inessential''.

Case studies, including the Cartesian transformation of Geometrical reasoning, Knot theory and Algebraic Number theory, bring out effects of representational differences, and indicate that these differences can matter philosophically:

Mathematics shapes special-purpose contents, by the expressive means it deploys (and avoids) in special contexts, that we call MODES, especially suited to certain aspects of problems. Such expressive means are paradigmatically deployed by combining linguistic restriction with instituting Attentional Foci (eg. equational degree in Descartes) that make appropriate contents available.

Mathematical power resides, not only in proof, but in coordinating suitable modes to overall purposes.

Group Theory or No Group Theory: Understanding Atomic Spectra

Josh Hunt
University of Pittsburgh

June 27. 4:00-5:30 VC 323

Philosophers have started constructing accounts of how mathematics plays a role in scientific explanation. Beneath this problem lurks the largely neglected matter of how mathematics contributes to scientific understanding and intelligibility. In many of the cases under discussion, it is difficult to discern exactly what mathematics distinctively contributes compared to the other components of the explanation. A promising way to gain traction into this problem is to examine cases where additional mathematics is added to an already mathematized theory. Group theoretic and non-group theoretic approaches to atomic spectra provide just such a case study.

In the early 1900s, a key motivation for developing quantum mechanics was to explain the atomic spectra of elements more complicated than those covered by the simple Bohr model. This work culminated in Condon and Shortley's classic work, Principles of Atomic Spectra (1935). Yet even before 1935, Eugene Wigner, Hermann Weyl, and others had realized that group theory—a branch of abstract algebra—could be fruitfully applied to quantum mechanics. While Condon and Shortley acknowledged this group theoretic approach, they, like most other physicists, chose to ignore it for as long as possible. Nevertheless, the existence of these two approaches—group theoretic and non-group theoretic—serves as a useful case study for determining what mathematics contributes to scientific practice and how this contribution is made.

With the non-group theoretic approach serving as a benchmark, the contribution made by group theory can be isolated. Based on this analysis, I will argue that although the two approaches have markedly different virtues, they are both viewed as being explanatory by the physics community. This is because they enable physicists to answer different kinds of why questions. As recently argued by Molinini (2011), such diversity of explanation motivates the adoption of a pluralistic model of explanation. I will argue that this case study lends further support to this model.

In juxtaposing the group theoretic and non-group theoretic approaches, I will draw on both historical and more recent treatments of atomic spectra. Employing a pluralistic model of scientific explanation, I will examine the epistemic and pragmatic benefits of the non-group theoretic and group theoretic approaches. In particular, the non-group theoretic approach often provides a more mechanistic picture of the underlying physics, and pragmatically it requires knowledge of less mathematics. Yet by applying group theory to quantum mechanics, physicists were able to achieve a more unified treatment of atomic spectra, although at the cost of heightened abstraction and a steeper learning curve. To go beyond the debate in philosophy of science concerning causal-mechanical and unificationist models of explanation, I will argue that both accounts provide acceptable explanations in their own ways. This can be phrased more generally in terms of why questions and patterns of reasoning. Taking this analysis one step further, I will examine how the conceptual resources enabled by group theory enhance scientific understanding. Such research has implications for science pedagogy, particularly with regards to how quantum mechanics and physical chemistry are taught to undergraduates.

Character and Object

Jeremy Avigad & Becky Morris
Carnegie Mellon University; Carnegie Mellon University

June 27. 4:00-5:30 VC 323

In 1837 Dirichlet proved that there are infinitely many prime numbers in an arithmetic progression whose first term and common difference are coprime, a result known as "Dirichlet's theorem". However, although there were no questions over the legitimacy of Dirichlet's proof, various mathematicians published their own presentations, including Dedekind in 1863, de la Vallee Poussin in 1895/6 and 1897, Kronecker (whose work was edited and published by Hensel) in 1901, and Landau in 1909 and 1927. The central ideas invoked in, and the general line of argument of, these various presentations are essentially the same as Dirichlet's original.

What we today recognize as particular functions called characters play a central role in each of the proofs. These are, in modern terms, homomorphisms from a finite abelian group to the multiplicative group of non-zero complex numbers. However, the treatment of the characters in the various presentations are strikingly different both to fully modern presentations and to each other.

In our paper (Avigad and Morris 2012) we examine the presentations of Dirichlet's theorem by Dirichlet, Dedekind, de la Vallee Poussin, Kronecker, and Landau, as well as fully modern presentations. In particular, we attempt to identify, in a precise way, a number of axes along which the treatment of characters vary in the different presentations. We then explore the advantages and disadvantages of treating the characters in these various ways and bring these considerations to bear on questions as to why mathematicians gradually moved away from the original treatment to the modern one we use today. More specifically, we argue that the differences in the way the characters are treated influence how successful the presentation of Dirichlet's theorem is at satisfying the following goals:

To ensure that only clear, well-defined rules and norms are used and that these are able to cope with complex and subtle reasoning.
To promote an "efficiency of thought"; for example, to ensure that the cognitive burden on the reader is reduced by suppressing irrelevant information in the presentation, drawing attention to important features, and exploiting similarities to other, more familiar domains.

Moreover, we suggest that an attempt to satisfy both of these goals is what drove mathematicians to gradually alter their approach.

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Jeremy Avigad and Rebecca Morris. Character and object. Available on ArXiv arXiv:1209.3657 [math.HO], 2012.

S7: Interdisciplinary Integration: The Real Grand Challenge?
Organizers: Sophia Efstathiou & Annamaria Carusi

June 28. 10:30-12:30 VC 215

There are several "grand" challenges for academic and industry research: hunger, poverty, climate change, alternative energy, infectious diseases, personalized medicine or ageing. For example, the UK Research Councils have specified the following six priority areas as in need of extra, co-ordinated research across various disciplines: food security; digital economy; energy; safety uncertainties and security for all in a changing world; environmental change; lifelong health and wellbeing[1].

Calls like these from the Research Councils of the UK or elsewhere express an interest in humanity and the environment, in helping establish what constitutes the physical and socioeconomic well-being of people, nations and individuals. Evident in these and similar calls is a growing awareness that none of these grand challenges can be broached without special organisational and institutional infrastructures, to think about the best ways to join academics and partners in industry or government, and to ensure that proposed solutions are ethical and sustainable locally. For problems to be tackled at this large scale, inter-disciplinary, inter-institutional and inter-sector cooperation and collaboration are required, and this crucially includes interdisciplinarity across natural, social and human sciences. Yet Nancy Cartwright is as correct today as she was in 1999 when she wrote:

> But we have no articulated methodologies for interdisciplinary work, not even anything so vague and general as the filtered-down versions of good scientific method that we are taught in school. To me this is the great challenge that now faces philosophy of science: to develop methodologies, not for life in the laboratory where conditions can be set as one likes, but methodologies for life in the messy world that we inevitably inhabit. (1999:18)

Interdisciplinarity then, understood as integrative, collaborative work across university disciplines, industry and policy[2], is critical for addressing the grand challenges we face as societies and people. Yet interdisciplinarity itself raises many questions:

- When is integration successful?
- Can successful integration be replicated?
- What are the potential benefits and particular challenges of including philosophy and science studies in the integrative mix?

This session will explore different aspects of integrative, interdisciplinary research, focusing especially on research that integrates scientific and sociohumanist perspectives. Our aim is to explore integration as it can occur across different levels of research, for different purposes and within different forums. Through examples of research that aims to integrate sociohumanist perspectives in the practice of scientific research we aim to raise questions about the character and forms of integration in place across the sciences and humanities, its benefits and difficulties, and the actual and envisioned aims of such integrative, interdisciplinary research.

Values, data and institutional practices: The evolution of Public Health and City Planning

Giovanni De Grandis
University College London / University of Copenhagen

June 28. 10:30-12:30 VC 215

Public Health (PH) and City Planning (CP) are academic disciplines with a rather unique and distinctive history and character. Both disciplines are very practical in their interests and aspirations: they aim to better human life through improving population health (PH) and through improving the urban built environment (CP). The furtherance of their goals requires the use of scientific data, theories and sophisticated techniques. They are thus applied disciplines that make use of the resources provided by a variety of academic subjects and attempt to influence public policy. They can therefore be seen as early examples of interdisciplinary enterprises that aim at bringing scientific

[1] RCUK 2012, http://www.rcuk.ac.uk/research/xrcprogrammes/Pages/home.aspx

[2] We are here being rather permissive in our use of the term interdisciplinarity to describe what is often called "transdiciplinary" work. See Klein 2010 for taxonomies of interdisciplinarity: "A taxonomy of interdisciplinarity", in Frodeman et al (eds) The Oxford Handbook of Interdisciplinarity, pp 15-30.

knowledge to bear to social affairs.

Looking at the history of these disciplines in the last two centuries provides a very interesting case study to explore how scientific knowledge gets translated and incorporated into social policy and into the practices of governmental bodies.

Although both disciplines have a very long history that can be traced back to antiquity, they have both achieved their modern shape between the second half of the 19th century and the beginning of the 20th, largely as responses to the challenges of mass urbanization. The modern consolidation of both disciplines is the result of the interplay between growing scientific data and understanding on the one hand, and of ethical concerns and socio-political ideals on the other hand. Ethico-political values have prompted scientific inquiry and scientific advances have changed both the perception of reality and the practical options available to governments and social reformers. In short the two processes have fed into each other producing an interesting blending of science and values.

The peculiarities of these origins have profoundly affected the mission, the ethos and the values of these two disciplines and provided interesting challenges to the traditional view of the separation between facts and values, and between politics and scientific inquiry.
Several factors have contributed to the different balancing between a scientific ethos (aspiring at objectivity and detachment) and commitment to ethico-political values and aspirations. Among them socio-economic circumstances and individual personalities have played important parts, but how both disciplines have been institutionalized is also very important and has contributed to the transformation of the professional ethos and practices. The interactions and the dialectic between social circumstances, dominant ideas, individual personalities and institutional organization is illustrated in some detail and through relevant examples.

While no normative conclusion is drawn from this historical reconstruction, it is argued that the historical examples provided by PH and CP show that the interaction between sciences and social policy does not follow any simple linear pattern. A greater awareness of the ways in which these processes have occurred in the past can help contemporary scientists engaging in applied science and multidisciplinary research to understand how they can play an active role in shaping their context of research. Of particular importance is paying attention to the institutional organization of their areas of research and to how the power attached to their knowledge is used by them as well as by others.

Mapping interdisciplinarity in neuroeconomics

David Budtz Pedersen
Aarhus University

June 28. 10:30-12:30 VC 215

This paper investigates the concept of interdisciplinarity from the perspective of philosophy of science. Interdisciplinarity and inter-field dynamics play a pivotal role in shaping the future of the scientific system. Governments and policymakers around the world increasingly call upon the scientific community to deal with the Grand Challenges of contemporary society, such as climate change, the ageing population, international governance, resource management etc. Many of the most exciting and influential academic ventures today are seen as interdisciplinary, for example neuroscience, behavioral economics, bioengineering, etc. However, despite the growing practical interest, there currently exists no generally agreed-upon definition of interdisciplinary research or how interdisciplinary science should be conceptualized in terms of its epistemological foundations. The leading candidate for characterizing interdisciplinarity is the notion of integration. Importantly, what counts as a ‘good’ or ‘successful’ interdisciplinary collaboration involves some type of integration – whether it is the attempt to integrate multiple disciplinary approaches to a common problem; the attempt to integrate academic and non-academic stakeholders; or the attempt to develop genuine cross-disciplinary models and multi-level explanations (Holbrook 2012). Interdisciplinarity inevitably encompasses a set of ontological, epistemological, and methodological claims about the integration among different scientific domains (Budtz Pedersen 2012).

In this paper, I consider the concept of interdisciplinary integration from the perspective of a recent case study of neuroeconomics. Drawing on cross-citation analysis, I show how neuroeconomics as a field has evolved combining economic models with brain imaging techniques. From the beginning, the ambition was «to create a unified ('consilient') science encompassing neuroscience, psychology, and economics», moving decision-theory away from standard notions of 'ordinal utility' and 'revealed preference' into neuroeconomics (Glimcher 2004). Nevertheless, 10 years after the installation of the field, economists and neuroscientists have not integrated. The paper demonstrates how the intellectual traffic in neuroeconomics has gone in one direction only – from economic modeling to neuropsychological processes. Economists, on the other hand, have not picked up to use these results to refine their explanatory models (cf. Aydinonat 2010).

From this study, I draw three conclusions that can be used as pointers for an epistemology of interdisciplinarity. In short, I conclude (i) that not all interdisciplinary research is integrative in nature; (ii) that a theory of interdisciplinarity needs to account for differences, rather than only similarities, and (iii) that integrative interdisciplinarity, although it is not the sole criterion of interdisciplinarity, is an important feature of interdisciplinary collaboration. In order to sustain successful interdisciplinary research, I conclude that integration and coordination among explanatory schemes need to take place. Interdisciplinarity will flourish only when researchers from all participating disciplines take an equal role in the formulation of explanation-seeking questions. Departing from the case of neuroeconomics, I thus suggest that some degree of epistemic mutuality is relevant when designing interdisciplinary research programs.

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If 'Integration' is at the heart of Integrative Interdisciplinary Research; what is it? Some Philosophical Reflections

Michael O'Rourke & Stephen Crowley
Michigan State University; Boise State University

June 28. 10:30-12:30 VC 215

Collaborative cross-disciplinary research (CCDR) is increasingly seen as crucial to solving the "grand questions" facing humanity at the beginning of the 21st century. Doing CCDR well then is a good idea, which in turn implies the importance of thinking about what is involved in doing CCDR well. Such thinking is significantly more valuable if it gives rise to insights that can improve the practice of CCDR. In this paper we argue that philosophy is ideally placed to contribute to just this sort of practically valuable thinking about CCDR. We base our argument in part on our experience using philosophically-framed dialogue to facilitate CCDR and in part on our developing theoretical understanding of key CCDR concepts (e.g., common ground, integration). Our focus in this paper is theoretical; since this focus draws on our empirical work we will offer a quick sketch of that material.

Our empirical work is part of the Toolbox Project, which originated in a NSF-sponsored Integrative Graduate Education and Research Traineeship (IGERT) project at the University of Idaho. This project involved students and faculty committed to CCDR and well-supported institutionally, but it encountered a critical but under-appreciated barrier to CCDR, viz., that students from different disciplines had difficulty working together in teams due to their distinct, tacit styles of doing science. The treatment, developed by the IGERT community in 2005, is the Toolbox workshop, a structured dialogue among team members about their styles of doing science. Philosophy of science provides the structure for these dialogues, illuminating the core commitments that frame different scientific styles. The response to participation in the more than 90 Toolbox Workshops has been overwhelmingly positive. This suggests, tentatively, that the Toolbox Project is on to something, and we explore that in our theoretical work.

Two key notions in the theory of CCDR are common ground and integration. Both are philosophically under-theorized. This is in part due an emerging consensus among CCDR theorists that these concepts are so plastic and contextual that there is little to be said about them at a theoretical level. We think that this is a mistake; both common ground and integration have core meanings that are worth theorizing. Such core meanings are largely procedural in nature. In this paper, we focus on integration. Integration is a notion that lacks any sort of theoretical account, to our knowledge; what there is a kind of “shell game” in which integration is cashed out in terms of synthesis which is cashed out in terms of unification and so on…. We argue that the way to avoid such a “shell game” is to begin as simply as possible with a model of integration as involving no more than relation. The next step in the process of theorizing involves enriching the basic notion sufficiently to make it do real work without focusing it so tightly that it fails to capture critical features of the phenomena in question. Our presentation offers both an initial definition and a sketch of the enrichment process.

Integration, Method and Applied Ethics

Rune Nydal
Norwegian University of Science and Technology

June 28. 10:30-12:30 VC 215

This paper discusses the call for integration with reference to three idioms for thinking about science, technology and society. Drawing on Andrew Pickering’s The Mangle of Practice, we may refer to the first two idioms as the "representational idiom" and the "performative idiom". The representational idiom in Pickering's words "casts science as, above all, an activity that seeks to represent nature, to produce knowledge that maps, mirrors, or corresponds to how the world really is" (1995:5). The performative idiom, in return, emphasises the material and technological mediators of scientific performance. In this idiom science is “regarded as a field of powers, capacities, and performances, situated in machinic captures of material agency" (1995:7). The performative idiom replaced the representational idiom but should in turn now be replaced by what Sheila Jasanoff in an article title called "The Idiom of Co-Production”. Co-production in Jasanoff’s words “is shorthand for the proposition that the ways in which we know and represent the world (both nature and society) are inseparable from the ways in which we choose to live in it” (2004:2).

The performative idiom casts science in terms that came to undermine the crucial distinction between the technical and the social. The blurring of this distinction implies a critique of the way both epistemic and ethical-political normative issues had been identified, separated and scrutinized within the representational idiom. The performative idiom has been criticized by Jasanoff and others for having left us without much of an alternative. This talk further rests on Charles Taylor’s normative diagnosis of the need for such an alternative: the representational account of knowledge has come to be an unfortunate barrier for normative approaches.

Taylor suggests that the epistemological tradition needs to be seen as part of the background that has staged the discussions of the point or worth of various practices we live by; they are epistemologically modelled (including the sciences and relations/divisions of labour between them). The exclusivity of the epistemological model, asTaylor discuss in his paper "Philosophy and Its History", exposes a “forgetting” of why the model once arose in the Renaissance as a liberating response to rise of modern science (1984:30) The forgetting implies that the epistemological model does not appear as a normative model whose organizational principle may be questioned, but as the only conceivable option constituting a necessary foundation for a range of different practices.

Difficulties of integrative research are explained against this background. Given this analysis the worth of the scientist’s practices, along with the identities of the practitioners, would be called into question in integrative research. Integrative research with a possessed normative aim, like found in applied ethics/philosophy circles, has a joint theoretical and methodological potential. It may provide a platform for working out a viable alternative to the representational idiom that cast science as a pure epistemic activity. It may do so through integrative research efforts that provide viable answers to current normative challenges of our time.

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Interdisciplinary Groups and Intercultural Meaning-Making

Zara Mirmalek
Boston, MA

June 28. 10:30-12:30 VC 215

Interdisciplinary collaboration is a discussion among people who approach problems from different angles, and use different tools to solve them; respect alone is not enough for such collaborations to flourish (Efstathiou and Mirmalek, forthcoming). Agreeing to disagree or waiting to see how things work out on their own is also neither an instructive strategy nor a productive one. On the other hand, continuing to contest one another's ways of making sense, of gathering and interpreting data can itself be the demise of a collaborative project and thusly any potential benefit to the public. Who among us has participated in an interdisciplinary project and not been involved in a difficult discussion over what or who matters, naming schemas, or metrics? These however are the types of contestations that can turn short meetings long, extend research timelines, and even render a project benign. Indeed some classification conflicts are as corporally consequential as artillery wars when we take into account the wielding of categories for marking populations for exclusion, eradication, or extinction (Bowker and Star 1999; Thomas 2000).

Understanding disciplinary communities through the lens of cultural anthropology provides a way to look at disciplinary differences and commonalities; and it lays the groundwork for framing interdisciplinary collaboration as a process of intercultural meaning-making. Anthropologist C. Geertz defines human culture as a web of significance, self-produced and shaped by one's community (Geertz 1973). A.P. Cohen extends this to defining a community as many individuals work together to spin mutual webs of significance (Cohen 1985). An interdisciplinary group of people is necessarily comprised of people who represent distinct disciplinary cultures (at the moment setting aside personal background culture, but keeping an eye on it), who arrive with varying webs of significance that are long-standing, supportive, authoritative, and primary sources of meaning-making.

An interdisciplinary group is not necessarily attuned to how their cultural distinctions matter; indeed one commonality may be that no one participant's disciplinary culture prepared them to navigate another culture from a shared level of power. In an interdisciplinary group if no one disciplinary culture is intended to prevail then by what process does one disciplinary framework prevail, given the absence of an established, recognized intercultural one? Is it determined by those with the most funding? Or by those who are in the best position to disseminate information? In this paper I offer, through the lens of cultural anthropology, what it means to understand an interdisciplinary group as an intercultural group, and the inherent power struggles over values, practices, and instruments that must be explicitly addressed. I will use examples from my own interdisciplinary research experiences at NASA and MIT as well as publicly available accounts of interdisciplinary research successes and conflicts (Efstathiou and Mirmalek forthcoming; Squires 2006, Quenqua 2012).

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Is It Possible to Give Scientific Solutions to Grand Challenges?

Sophia Efstathiou
Norwegian University of Science and Technology

June 28. 10:30-12:30 VC 215

Poverty, climate change, ageing: When called to tackle a grand challenge, most of us seem to understand what the challenge is about. Further most would expect that science, especially interdisciplinary science, can help solve these challenges.

In this paper I argue that grand, social challenges are not possible to solve through scientific research.

The problem is not only one of application: it is not only that we need science research and science results to be applied or taken up properly in target contexts. The problem is that science must already distort commonly identifiable, source problems, into scientifically solveable ones if it is to even hope to solve them scientifically. A key challenge then for tackling grand challenges scientifically is already the very conception of what the challenge is or can be.

This account relies in part on Heidegger's discussion of science as operating in a "Grundriss" or a groundplan (2002 [1938]): Heidegger argues that science operates within already defined categories and metaphysics, and he pessimistically says that it cannot ever get out of its own mazes (of amazing creations). I am not as pessimistic as Heidegger: I argue that there are definite processes of transfiguration, through which seemingly everyday or non-scientific objects and ideas get embedded or "founded" within scientific contexts; these founding processes are historical and social and possible to trace and study, scientifically (Efstathiou 2012).

Tracing such processes enables us to link the realms of the scientific and the ordinary, in historical and cultural narratives and frames. At the same time, expecting that scientific research on "grand" or otherwise "shared" challenges can grasp that common problem and offer answers to it is indeed logically flawed. Perhaps some supplementary historical, sociological or other science studies work can help design better solutions to shared problems, given it works to retain links to problems perceived as common. But even so, we might just be inherently limited as the sharpness and rigour of most scientific research approaches rely on developing specialized rearticulations of everyday phenomena.

So what is to be done, to solve grand challenges? If we were to re-articulate these challenges in scientific vocabularies it might be harder to see (or be moved) by their common relevance, but it may be more reasonable to expect issues to be solved through scientific research. At the same time, it is important to enable the motivations and drives of science to contribute to common problems. Perhaps supporting work that can help bridge scientific research with its context of social, political, organizational and other epistemic-cultural applications from the very start of the research can help better meet challenges that we perceive as shared, through particularized scientific handlings. Such interdisciplinary or integrative work should strive to connect with the actual practical contexts where solutions are to be sought and grander social milieux. Though this work may enrich and inform science, the work itself will only make a difference if it is not, or not only, scientific work.

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S8: Methods for Making: Synthesis and Theoretical Structure in Chemistry
Organizer: Julia Bursten

June 28. 10:30-12:30 VC 323

A central activity of chemical practice is synthesis, the creation of new substances and materials. The centrality of synthesis in chemistry means that chemistry differs from many scientific activities, because most systems or objects of chemical study must first be made before they can be measured, described, or fitted within a system of causes or natural laws. There is no a priori reason to think that the theoretical structures — theories, models, explanations, laws, etc. — that guide synthetic practice should be the same as the theoretical structures that guide the more descriptive or classificatory aspects of scientific practice.

This symposium rests on the assumption that philosophers can most fruitfully study the theoretical structures that guide synthesis by looking first to examples of chemical theory and synthesis practice, rather than setting out to fit the theoretical structures of chemistry within extant philosophical accounts of theory structure. It aims to shed some light on the theoretical structure of chemistry and synthesis. Bursten highlights the case of nanosynthesis, which aims to make materials with nanoscale features. A wide variety of theoretical structures are used to guide the various stages of successful nanosynthesis, and obtaining a clearer picture of the relationships between these theoretical structures, and between the structures and the systems they describe, can be seen as a first step toward characterizing theoretical structure in synthesis more generally.

Woody addresses synthesis and theoretical structure in organic chemistry. She uses the case of “bump/hole” modeling of organic systems to address the problem of how scientists assess the relevance of a piece of information in synthetic contexts.

Chang investigates the concept of synthesis in chemistry through a historical examination of the synthesis of water from hydrogen and oxygen — and electricity. During the Chemical Revolution and up to the years around 1800, it was unclear whether this procedure counted as a true synthesis because the role of electricity was poorly understood. Similar uncertainty continued well into the 19th century. These episodes illustrate that what counts as a synthesis at all is in need of philosophical consideration as part of the project of understanding the theoretical structures that guide the practice of synthesis.

Hendry and Goodwin shift the conversation from the concept of synthesis itself to 1
the relationship between synthesis and structure, which is another crucial component of chemical theories, models, and explanations. Ever since the Chemical Revolution, and especially throughout the 20th century, chemists have come to appreciate the central role of chemical structure in shedding light on the characteristic behaviors of synthesized substances and materials.

Hendry provides an overview of the concept of chemical structure and discusses the relationship between understanding structure and understanding synthesis. Hendry argues that, from the point of view of modern chemistry, there is no synthesis with- out structural understanding. Structure, mechanism and kinetic understanding are inseparable from the activity of synthesis itself.

Goodwin considers conformational analysis in organic chemistry. He uses a discussion of conformational analysis, a type of analysis of chemical structure, to illustrate how synthesis experiments, rather than law-like generalizations, often serve as the cognitive basis for development of new concepts and theoretical structures in chemistry.

Together, these papers provide the beginnings of a new philosophical approach to characterizing synthesis and understanding its role in chemical practice.

Epistemology and the Synthetic: Lessons from Nanosynthesis

Julia Bursten
University of Pittsburgh

June 28. 10:30-12:30 VC 323

Nanosynthesis, as an example of a synthetic science, forces a reevaluation of received philosophical views on theory structure, explanation, and models in science. I demonstrate the need for such reevaluation by considering the problem of what counts as understanding for nanoscientists. For chemists active in the field, achieving new understanding of a synthetic system is primarily characterized by the ability to do something new with the system at hand or with a similar system. Beginning from this pragmatic view of understanding, I create a detailed descriptive account of synthetic practices in nanosynthesis, in the hopes that such an account will serve as a basis for a more general account of the structures of models, explanation and theories — in other words, the epistemic structures — that operate in synthesis practices.

In this talk I argue there is no a priori reason to expect traditional approaches to the epistemology of science to adequately capture the epistemic structures that support the practice of nanosynthesis. So, instead of beginning from these traditional philosophical approaches, I take a bottom-up approach to obtaining a descriptive theory of the epistemology of nanosynthesis. I motivate this bottom-up approach with a case study of anisotropic metal nanoparticle synthesis, a prototypical nanosynthesis research program.

This case study suggests a view of epistemic structures in synthetic sciences as comprised of procedural guidelines that inform researchers how to produce a material instead of why that material is produced. This indicates a new role for scientific explanation and a need for new philosophical infrastructure to describe the mechanics of theoretical activity around synthesis.

One way of addressing this need is to look to the evolution of particular scientific concepts as they are refined for use in novel synthetic settings. With new developments in the control of nanoscale material features comes a new role for theoretical concepts such as surface and dielectric. For instance, one consequence of the smaller length scales of nanoscale materials is that synthetic scientists can no longer ignore the behavior of surfaces by writing them off as a boundary condition. Rather, modeling techniques developed to address longer (macroscopic) and shorter (molecular) length scales must be adapted to predict and describe the behavior of nanoscale surfaces. This adaptation is a form of a novel epistemic structure whose mechanics are not necessarily well-described by current philosophical accounts of theory structure, models or explanations. However, recent work by Robert Batterman (2001, 2012) and Mark Wilson (2006, 2012) suggests a promising philosophical setting for describing this type of adaptation.

Making Water — With Electricity

Hasok Chang
University of Cambridge

June 28. 10:30-12:30 VC 323

Synthesis is a crucial part of chemical practice. The domain of chemistry has grown vastly through the creation of new substances; synthesis has also served as an important method of analysis, confirming the composition of familiar substances that we can make. But understanding a chemical reaction as a synthesis is not a straightforward process, as I will illustrate through the early history of the synthesis of one of the simplest substances, namely water.

The composition of water was a significant point of contention in the Chemical Revolution. Lavoisier never managed a simple decomposition of it; Cavendish, who made water from hydrogen and oxygen (as did Priestley and Watt), interpreted the reaction as a restoration of phlogiston-balance, not a true synthesis. And electricity often entered as a crucial factor (even as a substance), creating an interpretive difficulty for those who wanted to view the synthesis of water as a straightforward bonding of hydrogen and oxygen. This complication is usually not recognized in standard histories.

Electricity was involved in the synthesis and analysis of water right from the start: Cavendish reacted hydrogen and oxygen gases with an electric spark (though one can also do it by combustion). Most scientists at the time considered electricity a substance, and there was no sure account of what happened to the electricity that went into the making of water. Further confusion came when Deiman and van Troostwijk in 1789 decomposed and then recomposed water, using electric sparks for both processes. In 1800 Nicholson and Carlisle performed the first modern-style electrolysis of water using Volta's new invention, the battery. Ritter proposed an anti-Lavoisierian interpretation, arguing that electrolysis was actually a pair of syntheses: negative electricity combining with water made hydrogen, positive electricity combining with water made oxygen, and water remained for him an element. Like

Cavendish, Ritter denied that making water from hydrogen and oxygen was a synthesis.

The place of electricity in the synthesis of water was uncertain for much of the 19th century, as the general theories of electrochemistry remained uncertain. An emblematic instance (incidentally with much practical significance at present) is Grove's work starting in 1839 on what we now call hydrogen fuel cells. Grove discovered quite accidentally that the electrolysis of water would run in reverse spontaneously (with platinum as a catalyst) when the external battery was removed from the circuit: the hydrogen and oxygen gases re-constituted water, producing an electric current in the process. This makes fairly straightforward sense in modern terms, as a conversion of chemical potential energy to electrical energy, mechanically mediated by the ions present in acidulated water. But how did Grove and his contemporaries (including Faraday) tried to make sense of the experiment, with the concepts of free ionic dissociation and energy not available to them?

These episodes illustrate that "synthesis" is a highly interpretive and contentious category. Agreeing on an account of a synthesis-reaction requires pre-existing agreement on the list and basic nature of the starting materials, and an agreed-upon theory that can specify sufficiently well what happens to those materials in the reaction.

Gaining a Foothold: Integrating Novel Concepts into the Experimental Life of Organic Chemistry

William Goodwin
University of South Florida

June 28. 10:30-12:30 VC 323

One of the most dramatic changes in chemists' conception of structure occurred during the middle third of the 20th century with the gradual realization that the conformations of molecules (not just their configuration and connectivity) had a crucial role to play in understanding their physical and chemical behavior. A molecule's conformation is, roughly, any of the three-dimensional arrangements of its constituent atoms in space resulting from rotations around single bonds. The development of conformational analysis shows how new aspects of chemical structure were integrated into the experimental life of organic chemists originally by crafting 'foothold' concepts demonstrating the importance of conformations in clear cases. Those foothold concepts were then extended and articulated throughout the domain. Derek Barton and Odd Hassel shared the Nobel Prize in Chemistry in 1969 for their contributions establishing the nature and importance of conformations. Hassel employed physical techniques (electron diffraction experiments) to characterize the structure and the nature of the intra-molecular non-bonding interactions in cyclohexane. These experiments supplied the foothold concepts that were subsequently adapted to explain and predict the chem- ical behavior of synthetically important organic molecules by Barton and others (see Barton, 1950, 1969).

After conformations were recognized to be important early in the 20th century, chemists still faced the daunting task of organizing and sorting these infinite structural variations into categories that could be inferentially connected with experimental results, and eventually lead to new experimental designs. This was not done in a top down way, by somehow deducing the implications of non-bonded interactions for chemical reactions. Instead, successfully doing this depended on finding a particular case where the conformational implications were clear and then generalizing and articulating from there.

The development of conformational analysis is philosophically interesting, then, because it provides a clear example of how important conceptual distinctions got integrated into organic chemistry by way of careful experiments on particular clear cases. These supplied the foothold concepts originally connecting conformations with experiment. Because particular molecules and their models are cognitively richer than general types of models or abstract theories, they have many specific features that can be explored for their potential inferential significance. This extra cognitive content seems to have been crucial to developing conformational analysis. The experimental significance of conformations was not deduced from some general theory of non-bonded interactions. Instead chemists isolated particular cases where this significance was clear, used very local concepts to explain and predict in those cases, and then generalized from there.

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Structure and Synthesis in Chemistry

Robin Findlay Hendry
Durham University

June 28. 10:30-12:30 VC 323

Structure is central to the design of chemical syntheses, which depends on the structures of the reactants and products, the mechanisms by which one turns into the other, and the kinetics of the relevant processes. What is a structure? At its most abstract, the structure of a complex object is just the way its parts fit together to make up the whole. To be less abstract, and correspondingly more informative, one must specify (i) what the relevant parts are, and (ii) what kinds of relationship between the parts constitute the relevant kinds of ‘fit.’ In this paper I will specify (i) and (ii) for chemical structure by investigating structural explanations in various parts of chemistry.
Although the parts of a chemical structure are pretty uniform (atoms, ions and electrons), I argue that different parts of chemistry appeal to different kinds of structural relationship, and to different kinds of physical interaction. Moreover, the same substance displays different structures at different length and time scales. To that extent, the specification of a structure is interest-relative. On the other hand, none of this undermines a robustly realist conception of the role of structure in chemical classification and explanation. Chemists focus on particular substances, and different substances are stable over different ranges of physical conditions. It should be no surprise if structural explanations concerning substance X focus on structural relationships that survive across the conditions under which X exists, and structural explanations concerning substance Y focus on different structural relationships that survive across the different conditions under which Y exists. And explanatory interests in the various processes that go on within substances determine the right scale to focus on. In short, one should be both a pluralist and a realist about structure in chemistry.
How is structure related to synthesis? I argue that structure plays such an important unifying role in chemistry that it hardly makes sense to speak of synthesis independently of structure. One reason for that is quite general: structure is what individuates chemical substances. Hence to make something just is (from the chemical point of view) to make a structure. The second reason is quite specific: the role of structure and mechanism in chemical kinetics. Kinetic data (i.e. rate laws) makes sense only in the context of possible mechanistic pathways, and mechanism itself occurs within the space of structural possibility.

The Diverse Landscape of Relevance Judgments: The ‘Bump/Hole’ Strategy in Organic Synthesis

Andrea Woody
University of Washington, Seattle

June 28. 10:30-12:30 VC 323

Issues of relevance have dogged traditional analytic philosophy of science. Hempel’s account of explanation, for example, cannot properly defend against irrelevant information and ‘old evidence’ has caused problems for multiple accounts of evidential reasoning, most notably those grounded in Bayes Theorem. This is hardly surprising, given the empiricist orientation of the tradition, because relevance is not solely, or even primarily, an empirical matter determined by the nature of phenomena alone. Rather than identify relevant factors, scientists make judgments regarding their relevance. Relevance is furthermore a relational concept; we make relevance judgments with respect to certain conditions and some set of assumptions and claims that we accept, either implicitly or explicitly. Consequently, there exists a cluster of related but distinct notions of relevance; a factor may be relevant with respect to logic, a given empirical theory, a particular set of goals, aims, or interests, a given degree of accuracy, available cognitive or material resources, etc. In most situations, moreover, our judgments treat relevance as a matter of degree. While recognizing many factors as relevant in principle, we focus on the most relevant among them in a given context.

To explore this complex landscape and get a grip on how relevance judgments are rendered in practice, this essay provides a case study of the “bump/hole” strategy for small-molecule synthesis in organic chemistry. (For a brief overview, see Stuart Schreiber (2011). “Organic synthesis toward small-molecule probes and drugs”, Proceedings of the National Academy of Sciences 108(17): 6699-6702.) Developed primarily in relation to drug design and already generating promising new drugs for breast cancer and malaria, this strategy involves selective alteration of complex molecules and relies upon genetic engineering of model systems for testing potential designs. My discussion focuses on (i) how in this research relevance judgments made in the contexts of explanatory, evidential, and theoretical vs. medical reasoning can pull apart in striking ways and (ii) how practitioners decide which factors are relevant enough to merit attention at all. While the overarching goal of this project is to display the variability and context- sensitivity of relevance judgments in practice, it also aims to contribute to a better understanding of the nature of synthetic sciences and the forms of reasoning that guide these enterprises.

S9: Scientific Representations Across History and Practice
Organizer: Chiara Ambrosio

June 28. 2:00-3:30 VC 323

This symposium presents some recent developments in the debate on the nature and role of scientific representations, and offers a range of new perspectives on the epistemic activities involved in the practice of representing. The immediate common connection between our four contributions is the assumption that representations should be investigated first and foremost as practices, and we address a variety of ways in which this simple assumption can be articulated philosophically. These include foundational questions regarding the very notion of representation, what it is for and what it amounts to, what are the challenges to current philosophical accounts of representation and model-based science, and whether the notion of representation may be best understood in a deflationary spirit. Contrary to traditional analytical approaches to representation, however, we investigate how such foundational questions play out in concrete experimental contexts. For this purpose, our contributions draw on a variety of representational practices, ranging from model-building to diagrammatic and schematic representations, and include historical examples such as scientists' private drawings and doodles. The range of epistemological and historiographical questions arising from our accounts of representation reflects the diversity of practices informing the construction and use of models and representations, and shows that there is still much to say about representing as an epistemic activity at the centre of scientific practice.

Deflationary Representation and Practice

Mauricio Suárez
Complutense University of Madrid

June 28. 2:00-3:30 VC 323

It has become commonplace that representation in science may not be analysed, but is a primitive notion (Giere, 2004; Suárez, 2004; Van Fraaassen, 2008). This is a sort of deflationism akin to the homonymous view in discussions regarding the nature of truth. There, we are invited to consider the platitudes that the predicate "true" obeys at the level of practice, disregarding any deeper, or more substantial, account of its nature. Yet, the motivation for deflationism in these two different areas is arguably also distinct. The motivation behind the move towards "primitivism" or, more generally deflationism, regarding scientific representation is the recognition that representation is first and foremost an element of a practice – the practice of model building in science. This recognition explains why the emphasis has moved in recent discussions away from considerations regarding the nature of the representational relation between the objects that play the role of sources and targets, and their shared properties. Instead, the focus nowadays is chiefly on considering the activities involved in the diverse representational practices across the sciences. This is at the heart of what I have elsewhere called the turn from the analytical towards the practical inquiry (some outstanding instances of which are Knuttila (2009) and the collection of essays in Gelfert (ed.), 2011).

But what exactly is it to hold a deflationary view of some concept X? I first define the contrary view, a substantive one, as any analysis of X, in terms of some property P or relation R, that accounts for and explains the standard use of X. I then go on to characterise a deflationary view of X, in opposition, in three distinct senses, namely: a "no-theory" view, a "minimalist" view, and a "use-based" view. I attend to how these three views have played out specifically in the philosophical literature on truth. Finally, I argue that the key to deflationary accounts of scientific representation, under any sense of "deflationary", is that representation is not a property of sources, or targets, or their relations, but is instead best understood as a set of necessary features of the practice of representing.

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Extrapolating with models: Against similarity and inferential approaches

Christopher Pincock
The Ohio State University

June 28. 2:00-3:30 VC 323

Recent work in the philosophy of scientific practice has sought to isolate a distinctive strain of model-based science (Godfrey-Smith 2006, Weisberg 2007). In model-based science scientists construct and evaluate models as a means of learning about target systems. The models range from concrete objects, as with scale models of ships, to abstract mathematical models picked out by systems of equations. A central problem for understanding this practice is extrapolation: in some cases, scientists find that a model is accurate in one respect and use this fact as evidence that the model is accurate in other respects. For example, a small concrete model of a ship may be built and investigated. The features of the model are then used to predict how a much larger ship would behave if it were built.

In this paper I argue that two widely deployed approaches to models, and how they represent, are not able to make sense of this practice of extrapolation. One approach links a model to its target system via a network of similarity relations (Weisberg 2012). A second approach posits a series of inferential connections between models and their targets (Suárez 2004, 2010). In both cases the tie between a model and its target is said to be quite open-ended and subject to a variety of contextual considerations. This has the virtue of allowing models to represent their targets in different ways in different scientific contexts.

I argue that this benefit comes at a very high cost. These accounts of model-based representation have great difficulty in accounting for the rationality of extrapolating with models. Consider the investigation of a small concrete ship that reveals the drag for a range of velocities. The larger proposed ship is indeed geometrically similar to the small concrete ship. But it is not clear how being similar in one respect is evidence of similarity in other respects. A similarity approach that aims to aggregate similarities of different sorts is not able to validate this sort of extrapolation. Analogous problems arise for inferential approaches. A model is said to license a variety of inferences from features of the model to corresponding features of target systems. But this array of inferences makes the success of one inference too disconnected from the success of other inferences in the array. Again, extrapolating with models becomes difficult to vindicate. I conclude that some substantive alternative account of model-based representation is required. To succeed, this account must help to explain the practice of extrapolation without sacrificing the flexibility of similarity and inferential approaches.

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Iconic Representations and Representative Practices

Chiara Ambrosio
University College London

June 28. 2:00-3:30 VC 323

Charles Sanders Peirce’s philosophy, and in particular his distinctive formulation of pragmatism, are gradually beginning to gain greater visibility in philosophy of science. Yet, his rich account of representations still raises a certain degree of scepticism among philosophers. In this paper, I focus on a class of representations that Peirce grouped under the category of iconicity. Iconic representations, which Peirce clearly characterised as the dynamic constituents of scientific inquiry, occupy a central place in Peirce’s philosophy, in his innovative approach to logic, and more importantly in his practice as a scientist.

I begin my discussion with a brief overview of Peirce’s own use of a broad range of representations and representational formats, and explore their role in his practice as a scientist and as a philosopher.

Through a selection of heterogeneous historical materials, including drawings, doodles and diagrams collected from his notebooks and manuscripts, I show that Peirce approached representation first and foremost as an experimental mode of inquiry. I claim that Peirce's own representations are in line with his formulation of iconicity, and that they are more broadly connected to the pragmatist philosophy that he developed in parallel with his practice as a scientist.

In the second part of the paper, I defend the contemporary relevance of Peirce's approach to iconic representations. For one thing, Peirce offers a useful "third way" between what Suárez (2010) has usefully described as the "analytical" and "practical" inquiries into the concept of representation. While the former focuses on the question of what constitutes representation, the latter implies abandoning constitutional questions to privilege the ways in which scientists use models and draw inferences from them. I argue that Peirce's account of iconic representations reconciles these two approaches. As a philosophically-minded scientist and as an experimentally-inclined philosopher, Peirce never divorced the practice of representing from questions about what counts as a representation in the first place. I claim that his account of iconic representations shows that it is the very process of representing, construed as a practice which is coextensive with observing and experimenting, that casts light on what counts as a representative relation in the first place.

S10: Talking Junk about Transposons: Levels of selection and conceptions of functionality in genome biology

Organizer: Stefan Linquist

June 28. 2:00-3:30 VC 215

Transposable elements (TEs) are mobile pieces of DNA capable of self-replicating and reinserting throughout the genome. Since their discovery, a variety of different metaphors have been used to understand the roles of TEs. They have been compared to everything from control switches, to junk, to genomic parasites, to mini-ecosystems. How have these different ideas directed empirical and theoretical research within transposon biology? Are some metaphors more appropriate or misleading than others? This session will address these questions from both scientific and philosophical perspectives. Ryan Gregory (Integrative Biology, U of Guelph) will review the history of the concept of Junk DNA. He argues that this term is often misappropriated in ways that are not only misleading, but also strategically designed to elevate the importance of certain findings. Tyler Elliott (Integrative Biology, U of Guelph) will critique the tendency toward single-level thinking in transposon biology. He argues that much of the evidence recently cited in favour of the host-level perspective is better understood from the selfish-element perspective. Stefan Linquist (Philosophy, U of Guelph), will discuss the ways that function concepts have been employed within the field of transposon biology. He argues that different research traditions operate with distinct function concepts, and that one of these conceptions (functions as selected effects) is clearly preferable to the other (functions as causal roles).

Junk and the genome

T. Ryan Gregory
University of Guelph

June 28. 2:00-3:30 VC 215

It has been known for more than 60 years that the amount of DNA in the genome bears no relation to the complexity of the organism in which it is found or the number of protein-coding genes which it contains. Once considered paradoxical, this discrepancy between genome size and gene number is explained by the massive quantity of non-coding DNA in most animal and plant genomes. If media reports, anti-evolutionists, and the authors of many scientific papers are to be believed, this non-coding majority has long been dismissed as useless “junk”, and only now is its potential biological significance being considered. But is this characterization accurate? And what is the current state of knowledge regarding so-called “junk DNA”? In this seminar, I will present the historical and conceptual background to this topic and will address the most common misconceptions about the biology of “junk DNA”. In broader terms, this seminar will examine the importance of properly acknowledging the history of scientific research, the dangers of scientific hype, and the standards of evidence necessary for ascribing “function” to biological features.

Appeal for an element-level perspective of transposable element evolution

Tyler A. Elliott
University of Guelph

June 28. 2:00-3:30 VC 215

Transposable elements (TE) are sequences of DNA that can move from place to place within the genome and are some of the most abundant and ubiquitous components of eukaryotic genomes at large. The advent of more advanced molecular techniques has seen a 30 year period where the evolutionary histories of TEs in genomes and the workings of their behaviour have been investigated. This approach has generally been from the perspective of TEs as components of the genomes in which they reside, much as genes are usually studied. While useful if one is interested in how TEs have contributed to the evolution of their hosts it may not be the best perspective to take when considering how the elements themselves have evolved. The host-centric view of TEs was challenged early on by Doolittle and colleagues with the publication of the selfish DNA hypothesis in 1980 and expansions upon it throughout the 80s. I will argue that his element-centric view did not take hold due to a misunderstanding of the selfish DNA papers, discoveries of TE-derived sequences taking on beneficial function at the host level, the inherently complicated multi-level nature of understanding TEs and several other factors. While advances have been made in our understanding of TEs using the host-centric perspective, a shift down to the level of the elements themselves might prove fruitful in helping to answer some of the longstanding questions in TE biology. Questions such as the causation of large-scale patterns in diversity and abundance of TEs in genomes. The goal of this presentation is an appeal for this element-level perspective to be taken to complement the host-centric perspective which is the default position. This will require a thorough review of the current TE literature to synthesize a theoretical framework from which to pursue this line of inquiry and to identify key questions to be tackled. From this, more specific topics will need to be addressed in detail, which could include explorations of the concept of the individual, species and breeding system at the TE level. Methods will also need to be devised to determine the effective population size of a TE lineage to determine how factors at multiple levels affect the capacity and trajectory for evolutionary change. These methods could be used to address much broader questions relevant to the evolution of TEs, such as the relationship between element effective population size, that of its host and various traits at both the host and TE levels.

Function-talk in transposon biology

Stefan Linquist
University of Guelph

June 28. 2:00-3:30 VC 215

When transposable elements were first identified by Barbara McClintock it was assumed that they must have a function. In this context, having a function meant having a beneficial effect on the host organism. At the same time, the phrase “junk DNA” was appropriated to refer to the opposing claim that transposons are not beneficial to the host – i.e. functionless. McClintock’s organism-centric perspective was later challenged with the rise of selfish gene theory. Given that TEs replicate more rapidly than their host organisms, it was argued, selection will not favour TEs for their host-beneficial effects. Indeed, some theorists regarded TEs as paradigm examples of selfish genes. This argument was duly interpreted to imply that TEs are therefore “functionless” – i.e. junk. In recent years there has been a swing back towards the organism-centric framework. It turns out that TEs are much more prevalent in some genomes than it was initially assumed. For example, over half of the human genome is comprised of transposable elements, whereas less than 2% consists of protein coding DNA. If TEs are so abundant, it is argued, they must serve some (host level) function after all– i.e. not junk. In this paper I try to make sense of this function-talk to determine what is at issue in this debate. I argue that the organism-centric perspective and selfish-element perspective differ in their very understanding of “function.” This is one place where a philosophical framework offers clarification. The organism centric perspective, I argue, adopts a causal role (or Cummins) concept of function; whereas selfish element theory adopts a selected effects (or Wrightian) concept of function. This goes part of the way in helping to understand function-talk in transposon biology. However, it leaves open the question of whether one of these function concepts is better suited to this field. I will argue for the counter-intuitive claim that a selected effects concept of function is better suited to the investigation of transposon mechanics, even when the primary aim is to develop proximate (as opposed to ultimate) explanations.

S11: Diagrams as Vehicles of Reasoning and Explanation: Perspectives from Chronobiology
Organizer: William Bechtel

June 28. 4:00-5:30 VC 323
While most theorizing in philosophy of science relies on the vehicle of natural language, figures and diagrams are the vehicle employed in many of the sciences. This is clear in the way scientists communicate their results: slides at talks are mostly filled with diagrams whereas in philosophy talks they are filled with text. Scientists do write papers, but most attention is paid to the diagrams, as becomes clear in journal clubs where discussion begins with the diagrams and attends to the captions and text only to explicate what isn't clear from the diagrams (e.g., the methods used to procure data or evidence). Our contention is that diagrams serve not only as the vehicle of communication, but also as vehicles of reasoning and explanation. They are constructed as part of the reasoning process and inferences are made based on what is represented in them. In emphasizing their roles as vehicles, we do not downplay the importance of the cognitive activities of scientists—it is scientists who must interpret and utilize diagrams in reasoning and offering explanations. What we claim is that diagrams are vital and ineliminable representational vehicles used in these activities.

The three talks in this session examine specific aspects of the use of diagrams in a single field of biology—chronobiology. As the name suggests, chronobiology focuses on how organisms temporally organize biological processes, especially those that occur on an approximately 24-hour cycle (circadian). The responsible mechanism is generally referred to as the circadian clock and although these talks will emphasize the mammalian circadian clock, clocks have been found in all orders of life and are responsible for rhythmic expression of a very large range of physiological and behavioral processes. In this as in many fields of biology, researchers use diagrams to represent the phenomenon to be explained, the relationships between variables that the research has identified, and the mechanism that is proposed to explain the phenomenon. Some of the diagrammatic practices employed in chronobiology, such as the use of line and bar graphs, are widely shared with other scientific fields. But others are specifically developed to represent the temporal organization that is central to the field—processes that exhibit a 24-hour oscillation and mechanisms capable of generating such oscillations. The three talks in this session will focus on different aspects of the use of diagrams in the practices of chronobiologists. The first focuses on graphical practice, and argues that to understand scientific practice with diagrams we need to abandon a conception of a fixed form-content relation for diagrams and focus on the interpretative activities of scientists, addressing the fluidity of form-content relations over time. The second talk focuses on one important role of diagrams in practice—the representation of “homogeneity assumptions” that guide research into complex systems. The last talk focuses on two features of some diagrams of mechanisms that reveal their role in the reasoning and explanatory practices of scientists—their incorporation of question marks and characterization of variables and parameters needed to create computational models to simulate their operation.

Graphical Practice: Scientists as Interpreters of Diagrams
Benjamin Sheredos
University of California, San Diego

June 28. 4:00-5:30 VC 323
One component of scientific practice is graphical practice (“GP”). Scientists painstakingly construct and annotate graphical media (diagrams, micrographs, line graphs, etc.) to direct attention to explanada, explanans, research methods, and more. As a few cognitive scientists and philosophers have argued, compared with bare linguistic media or “offline” cognition, graphical media subserve distinct cognitive strategies for problem-solving. Once interpreted, graphics can constrain and afford understandings of the domain of inquiry.

Formalistic accounts of explanation emphasizing laws have been rejected as inadequate in accounting for explanatory practices in the life sciences, where, as the new mechanistic philosophers have emphasized, explanatory “laws” are rare. Here I pursue a different issue, demonstrating the general inadequacy of traditional logico-formal accounts of form-content relations as means of understanding GPs.

Recent work has begun to address GPs' departures from idealized form-content relations. In early work, Perini suggested GPs be understood in light of Nelson Goodman's account of "notational systems" – a symbol system in which traditional, fixed form-content relations are enforced such that:

(i) each formal element unambiguously represents a well-defined extension,

(ii) every object (in the domain) is unambiguously represented by some formal element(s) in the symbol system, and

(iii) simple formal elements can be combined to form complex, unambiguous representations of complex objects.

In more recent work, Perini suggests that we multiply notational systems to understand the multiplicity of GPs: each system of graphics depicts part of the domain of inquiry, but no unified system of formal elements depicts the whole domain. Likewise, Griesemer has argued that GPs can constitute theoretical models which exhibit "mismatch" to empirical content: a given system of graphics might fail to represent portions of the domain of inquiry.

I argue that a diachronic approach to GPs urges further departure from traditional conceptions of form-content relations. I seek to demonstrate four such departures via an analysis of distinct graphical depictions of the same system in chronobiology. First, by using old graphical forms in novel ways, biologists can radically reconceive their domain of inquiry – the instability in form-content relations over time can be valuable. Second, such innovations can be accomplished using a variety of ambiguous graphics whose form can be coherently mapped to the same domain of inquiry in multiple ways. Third, novel GPs can involve graphics which are built to be multiply interpretable – i.e., graphics which are meant to convey more content than can be borne by their formal elements under any single, stable interpretation. Finally, pre-existing graphics are not isolated from novel GPs: "idiosyncratic" formal elements of earlier graphics can be reinterpreted as piecemeal anticipations of a new understanding of the domain.

The analysis shows GPs to be a continuous, open-ended, and backward-reaching negotiation of the domain of inquiry. This serves to underscore the inadequacy of analyzing scientific practice in terms of fixed form-content relations, and the poverty of assuming one-to-one mappings between form and content. A plausible account must recognize researchers' active roles in interpreting graphical forms as having a content, if GP (hence, scientific practice) is to be understood.

Homogeneity Constraints and Reasoning about Complex Mechanisms

Daniel C. Burnston
University of California, San Diego

June 28. 4:00-5:30 VC 323

One vital function of scientific diagrams is to constrain reasoning about the represented domain. Constraints aid in problem solving by limiting search space, and by affording particular hypotheses about the specific system in question. Here, I discuss one particular type of constraint, which I refer to as "homogeneity assumptions," that plays a role in diagrammatic representation and problem solving in complex scientific domains. Representations of homogeneity assumptions can involve both functional and structural depictions that connote the uniformity of a part type within a mechanism—they can assume that individual components of a particular type are compositionally similar (at a relevant level of abstraction) and/or contribute in similar ways to the operation of the mechanism. Several diagrammatic techniques are used to connote these types of homogeneity, including specific spatial grouping and the repetition of icons intended to characterize the function or structure of a type of part. Homogeneity assumptions can be depicted for different levels of mechanism organization, and help to shape the reasoning of scientists, both within an individual and within a field. I assess three distinct but related roles for representations of homogeneity assumptions in the investigations into the function of the mammalian suprachiasmatic nucleus (SCN), a structure in the hypothalamus that serves as the central timekeeper for the organism, synchronizing other rhythmic biological processes to time in the external environment.

In mammalian chronobiology, homogeneity assumptions play at least three distinct roles. First, they focus search in a way that directs attention to discovering the capacities and operations of a part of a mechanism. To illustrate this, I will analyze diagrams from a particular period of research in which scientists attempted to uncover the propensities of independent but connected intracellular oscillators within the SCN. Second, homogeneity assumptions can "background" (in the sense articulated by Griesemer) details about a particular part of a complex process, in order to focus attention on the details of other aspects that are “foregrounded.” I illustrate this via diagrammatic depictions of “peripheral” processes in mammals that are regulated by the SCN. Third, homogeneity assumptions allow for questioning relationships between particular possible functional decompositions of a system. I illustrate with the visual comparison of peptide expression and Period gene expression in different parts of the SCN. Importantly, homogeneity assumptions have different lifespans. The first use can last for numerous years within a field, while the other uses may serve to focus attention through only the course of a single study or reasoning process.

Finally, I will provide some examples of contexts in which questioning standard homogeneity assumptions served to promote conceptual change. Unsurprisingly, diagrammatic practices show noticeable changes as homogeneity assumptions come to be questioned, and I will show examples of this change in diagrammatic practices for representing SCN organization. I conclude that (i) homogeneity assumptions, and their diagrammatic representations, play important roles in cognizing about biological phenomena, both in an individual and a field, and (ii) that analysis of diagrams provides perhaps the most fruitful way to understand developments in these assumptions.

Reasoning with Mechanism Diagrams

William Bechtel
University of California, San Diego

June 28. 4:00-5:30 VC 323

When scientists advance a new model of a mechanism, they often represent the proposed mechanism in a diagram. They take advantage of two dimensions of space and features such as color to represent the identified parts of a mechanism and how they are thought to relate, spatially or functionally, to each other. Often arrows are used to indicate causal interactions whereby the execution of an operation by one part affects another part, although other conversions are occasionally invoked. Diagrams of this sort sometimes appear in research articles, typically as the last figure, and more often in reviews and discussion papers and talks. They are also common in graphical abstracts, a relatively new representational format introduced by some journals. They clearly constitute a major communicative device. But, as I will argue in this paper, they are often tools for reasoning by scientists, not just readers but also the authors themselves. They are not just representations of the proffered explanation, but vehicles that serve to guide further investigation. I will focus on two such ways in which diagrams of mechanisms serve this role: by identifying features of the account not yet worked out and by serving as a foundation for computational modeling.

One clue to the role of mechanism diagrams in reasoning is that a surprising number contain questions marks. I will present examples that illustrated somewhat different uses. Sometimes they simply indicate that the evidence for a given part or operation represented in the diagram is more problematic than for other parts or operations. In other diagrams question marks signal gaps in the understanding of the operations, prompting inquiry into what are possible intermediates that could connect two known operations that are thought to connect to each other. In yet other diagrams alternative components and pathways are marked with question marks to indicate that they are regarded as the possibilities for which evidence is required in order to decide between them. A further indication that these mechanism diagrams are meant to support reasoning is that they sometimes include representations of the phenomenon to be explained.

A second way diagrams foster reasoning and thinking about mechanisms is by providing the basis for computational modeling in terms of systems of differential equations. Such modeling requires transforming a representation of the parts and operations in the mechanism into identifying variables and parameters that then are incorporated into equations. This is often worked out on diagrams of the parts and operations. Although some computational models attempt to incorporate all parts and operations, many are based on selective or partial models that are employed to try to relate different components of the mechanism to different features of the phenomenon and diagrams often serve to identify the components to be included or excluded in the computational model. Finally, I will examine diagrams develop to depict the behavior of the model, showing how it links up to the phenomenon to be explained.

S12: Coherence in science after the practice turn
Organizers: Léna Soler and the PratiScienS group

June 29. 10:30-12:30 VC 211

In this symposium, our aim is to revisit issues related to coherence in science and coherentist conceptions of science after the "practice turn" in the science studies.

The notion of coherence has often been centrally involved in conceptions of science directed against foundationalist pictures of science based on correspondence theories of truth and meaning. Before the relatively recent labeling and systematic account of "coherentism" (see for example Lehrer [1974, 1990], BonJour [1985]…), the very much related notion of "holism" has most notably figured in the works of Duhem, Neurath or Quine. 'Behind' options such as foundationalism versus coherentism, the philosophical issues traditionally at stake were: the nature and force of scientific methods of justification; their ability to impose inevitably *the* right solution between a set of scientific concurrent hypotheses or theories ("theory-choice"); and on this basis, the objectivity, if not the truth, of what we identify with sound scientific knowledge ("scientific realism"), as well as the relative, possibly plural and contingent character of scientific achievements ("relativism").

In the post-positivist era, multiple "coherence theories of justification" have been developed. A core minimal idea of coherentist theories of justification is that coherence is the main, if not the only criteria available to scientists when they have to decide between several concurrent hypotheses or sets of scientific propositions. Coherence, however, is not an unambiguous notion. As most coherentists have admitted, coherence as a criterion for scientific decisions cannot be reduced to the absence of contradiction. For instance, BonJour added to logical consistency several conditions for a system to be coherent (the existence of inferential connections between propositions, etc.). A plurality of conceptions of coherence coexist, and in addition, the adoption of a given definition of coherence does still not impose one unique way to apply the definition to various situations in which scientists have to make choices. This being said, beyond differences, traditional approaches to coherence nevertheless share one important feature. In all of them, coherence is a relation between *propositional* units (or between units akin to propositions: beliefs, hypotheses, testimonies, etc.). Let us refer to this coherentism as "propositional coherentism".

With the practice turn, the focus has been shifted away from propositions to take into account many other 'ingredients' of scientific practices, such as instrumental devices, tacit bodily skills, dominant social values or the like. In this context, it has been recognized that coherence in science had to be reconceived as a relation of "mutual support", "agreement" or "good fit", not just between *propositional* units, but between many *more diverse, possibly heterogeneous if not incommensurable kinds of units*. As a corollary, it has become clear that coherentist theories of science had to be rethought accordingly.

This recognition adds complexity in comparison to propositional coherentism. At the same time, it opens new issues. (a) How should we conceptualize the units between which the coherence is supposed to hold? What is (are) the possible / most fruitful / right framework(s)? (This is part of a more general issue, namely: which framework to analyze scientific practices?). (b) What is the nature of the "glue(s)" that is responsible of the "good hanging together" of the different kinds of units? These are, according to us, the hard problems that have not been solved by the practice turn. Few authors have faced these issues directly, and further work is needed with respect to the existing proposals. Obviously, the options favored with respect to these hard problems will strongly condition the way in which traditional questions such as scientific justification, theory-choice, objectivity, realism and relativism will be reconceived.

Some important attempts have been made in the direction of specifying coherence in science after the practice turn. To give a sample of some of those that have especially inspired our reflection: [Hacking 1992] spells out what coherence applies to, and gives a coherentist explanation of scientific 'justification', in the case of laboratory science. The author introduces a notion of "enlarged coherence between "thoughts, actions, materials and marks" and describes the resulting configuration as "self-vindicating". Pickering develops a coherentist conception of science according to which elements of heterogeneous types (material, behavioural, social, propositional…) co-mature, and in which a mutual stabilization is sometimes achieved. The elements then stand in a relation of reciprocal reinforcement, and this corresponds to a "scientific symbiosis", characterized as a "selfconsistent”, “self-contained” and “self-referential” complex package. More recently, Hasok Chang identified the units to which coherence applies as actions; it is the overall aims of a system of practice that define what it means for the system to be coherent.

The existing attempts are not homogeneous, and at any rate, they are not sufficiently developed. The symposium will try to go further in some respects. Its three constitutive talks aim to contribute to the following questions: what constitutes coherence in science, and how to conceptualize the coherence of scientific practices when science is no longer primarily equated to scientific propositions? What difference does it makes with respect to traditional issues such as justification, scientific decisions, realism and relativism? The first talk offers a coherentist account of scientific observation. Observation has been a concern for coherentism as this concept is tied to the foundationalist notion of ‘epistemic privilege’ that is not very welcome in such equalitarian frameworks as coherentism. Vincent Israel-Jost presents a way to save the epistemic authority of observational results within coherentism. In the second talk, Léna Soler starts from a definition of robustness inspired by W. Wimsatt, namely invariance of a scientific result under multiple, independent determinations, and criticizes the realist reading of this configuration. She shows that this configuration is better characterized in a coherentist framework, and sketches how a coherentist reading suggests that our science is both genuinely robust and truly contingent. In the last talk, Regis Catinaud starts with question (a) above, namely: after the practice turn, what units – or general constituents – of scientific practices are now supposed to be “hanged together”? He explores one of the possible answers, recently advocated by some practice theorists, according to which activity is considered to be a good candidate for the analysis of practices and the study of their coherence. He also analyses the cohesion mechanisms that constitute the coherence of actions.

A coherentist account of observation

Vincent Israel-Jost
LHSP – Archives H. Poincaré, University of Lorraine

June 29. 10:30-12:30 VC 211

In traditional empiricism (whether classical or logical), observation is a concept that is key to understanding how experience can provide us with the most authoritative knowledge. The traditional empiricist account of observation is foundationalist in that it presents observational knowledge as independent from already possessed knowledge; it is epistemically autonomous. This is to be contrasted with non-observational knowledge, which is derived and relies on prior knowledge. As [Fodor 1984] phrases it in his characterization of the traditional empiricist conception of observation: “observationally fixed beliefs tend, by and large, to be more reliable than inferentially fixed beliefs. […] less is likely to go wrong because there’s less that *can* go wrong.” In other words, the epistemic autonomy of observational knowledge comes in support of its epistemic authority.

Since the 1950s, this conception of observation has been harshly criticized. At the moment, philosophers widely agree on arguments of “theory-ladenness of observation” (a term coined by [Hanson 1958]) or of “the myth of the given” [Sellars 1956], which attack the notion of epistemic autonomy of (observational) knowledge. This has led to revised conceptions of observation, the most convincing of which have focused on actual scientific practices as in the works of [Shapere 1982] and [Hacking 1983]. The most prominent feature of this “new observation” is that it is liberalized in many ways. The use of instruments for instance is permitted, even encouraged, and observation is no longer tied to unaided perception. Also, prior knowledge is not excluded from the act of observing. It implies that, while observation leads to revising a subject’s system of beliefs, this system of beliefs also plays a role in the way one observes and reports on the observation. This renewed conception of observation that is aware of the actual practices thus results in an interdependence between observation and the subject’s system of beliefs. It is therefore a conception that rejects the notion of epistemic autonomy and fits a coherentist, rather than a foundationalist, epistemological picture.

In this presentation, I will focus on the question of whether renouncing foundationalism and its notion of epistemic autonomy irremediably leads also to renouncing any sort of ordering, priority or authority of different varieties of knowledge. While it has often be said that coherentism leads to the rejection of such notions, I will argue that a coherentist conception of observation can nevertheless accommodate the thesis that observation can provide us with very authoritative pieces of knowledge that can for example strongly challenge well-established theories. I will link this thesis to a condition of stability of the epistemic and material aspects of a given empirical investigation.

A coherentist, contingentist reading of the robustness of experimental results

Léna Soler
LHSP – Archives H. Poincaré, University of Lorraine

June 29. 10:30-12:30 VC 211

The talk starts from a definition of robustness inspired by Wimsatt, according to which

X is robust = X is invariant under a multiplicity of independent derivations,

and focuses on the particular case in which X is an *experimental result* and the derivations are *experimental proofs*. For the sake of pedagogical means, the discussion is applied to a simple four-elements structural configuration, called the prototypical robustness scheme, in which three experimental proofs converge on one and the same experimental result X. The intuitive, most common and quasi-irresistible reading of such kind of scheme corresponds to a foundational-like and realist reading, according to which X is independently supported (founded in that sense) by three experimental proofs, hence X is true. The paper criticizes this realist reading and discusses an alternative, coherentist and contingentist reading of the robustness scheme. In such an alternative interpretation, the robustness scheme works as a global, holistic equilibrium inside of which the robustness of each of the four elements is co-constituted. Considering the situation dynamically through time, the paper argues that the robustness of experimental results is better characterized in a coherentist perspective, that is, in terms of reciprocal stabilizations, or (in Pickering's terms) scientific "symbioses". When we opt for such a coherentist reading, the robustness of scientific achievements, although vindicated, is explained in a way that breaks up the quasi-irresistible leap from robustness to realism. Instead, we are led to consider the plausibility of the counterintuitive idea that experimental results might be both genuinely robust and nevertheless truly contingent in a non-trivial sense.

Practices, actions coherence and cohesion mechanisms

Régis Catinaud
LHSP – Archives H. Poincaré, University of Lorraine; University of Geneva

June 29. 10:30-12:30 VC 211

While many studies emphasize the importance of the notion of practice in the analysis of science, few of them have actually tried to map out the elements that compose a practice, and explain how they interact with one another.

In some attempts that have tried to address this issue head-on (like [Hacking 1992] "The Self-vindication of the Laboratory Sciences" or [Gooding 1992] "Putting Agency back into Experiment"), practice is usually conceived as a collection of general and heterogeneous characteristics (such as, for the most common: beliefs, skills, instruments, identities, shared values, methods, institutions, cultural aspects, local contexts, etc.). Other practice theorists recently approach the problem from another end. By understanding the notion of practice as an activity, these theorists were naturally led to consider that every practice could, in principle, be decomposed in a set of underlying actions, and consequently that a theory of practice requires a theory – or at least a conception – of action, which also have the great advantage to divide practices only into sets of *comparable* elements.

One could think that this strategy (often labeled as "action-" or "activity-based analysis" of practices, cf. [Chang, 2011], [Giere, 2006] *Scientific perspectivism*) only shifts a definition problem from a level – practices – to another – actions. However, the studies that give preference to this analytical option are not directly concerned with the definition of the properties of actions, or with the analysis of their causal connection or even with reduction issues from practices to actions. They are rather interested in the logic of the configuration of actions, in their arrangement, their disposition or their "grammar". From this perspective, the goal is to identify the specific factors that make a multiplicity of actions aggregate and combine in a consistent practice, that is to say to explain the *coherence of actions*. In this regard, one of the attempts of this presentation is to develop the notion of "actions coherence" by disclosing cohesion mechanisms that assemble actions together.

Building on work of practice theorists interested in this relationship between coherence and actions organization, mainly Chang's "Philosophical Grammar Of Scientific Practice" (2011) and Schatzki's *Social Practices: A Wittgensteinian Approach to Human Activity and the Social* (1996), the aim of this talk is to bring to the surface some of these *cohesion mechanisms*, usually implied in these works but not always properly featured and characterized, and to display their structuring role for actions**.**

S13: Epidemiological Evidence and Medical Practice
Organizer: Jonathan Fuller

June 29. 10:30-12:30 VC 215

Evidence, broadly construed, has always served to guide clinical judgment. Yet, evidence-based medicine (EBM) is barely past its adolescence. The dominant movement in medicine over the last twenty years, EBM was founded by clinical epidemiologists, those interested in the implications of population research for patient care. Not surprisingly, the EBM approach to finding, appraising and incorporating evidence in medical practice is concerned exclusively with the results of epidemiological studies. In fact, the first principle of EBM is that epidemiological studies, and especially randomized, controlled clinical trials, are a better guide for clinical decision-making than other kinds of evidence, such as biological mechanisms or clinical experience. Evidence-based medicine's controversial beliefs and assumptions have drawn philosophers of science into debate with physicians, with those from the clinical sciences, and with each other around the nature of medical evidence, its application in bedside reasoning, and the ethics of its production and use. The debate is enriched by active collaboration and conflict between philosophers and practitioners, to an extent rarely seen in the philosophy of science.

In this session, we present new work on the relationship between epidemiological evidence and the practice of medicine. Ross Upshur reflects on the EBM conceptualization of evidence and on how defeasible logic and the philosophy of science might enhance clinical medicine. Jonathan Fuller critiques the received logic of generalizing clinical trial evidence perpetuated by EBM and by clinical practice guidelines. Drawing on recent developments, Robyn Bluhm suggests a new role for biological mechanisms in a medicine informed by epidemiology. Finally, Kirstin Borgerson argues on ethical grounds that we should conduct fewer clinical trials, and that this may offer a solution to some the problems with evidence in modern medicine. The session will conclude with an opportunity for discussion and exploration of themes common to the four papers. Meditations on evidence, logic, mechanisms and experiments in the philosophy of medicine are both parallel to and distinct from corresponding meditations elsewhere in the philosophy of science. We hope for cross-fertilization of ideas common to both terrains as an important outcome of our session.

Does Philosophy of Science Have a Place at the Bedside?

Ross Upshur
University of Toronto

June 29. 10:30-12:30 VC 215

Clinical medicine and philosophy of science have co-evolved since antiquity with variable explicit mutual recognition. The school of Kos, most often associated with Hippocrates, focused on the whole patient and was strongly influenced by Pythagorean concepts and reasoning. The notion of balancing four humours was closely related to harmony of the celestial spheres. The rival school of Knidos focused much more specifically on particular diseases related to disturbances in discrete organs requiring specialized intervention. The approach of the Knidian school was aligned more closely with that of empiricism.

Clinical medicine is a case-based practice. All activities in clinical medicine commence with a particular patient manifesting a set of signs and symptoms and requiring explanation in terms of diagnosis, rectification or restitution of the illness in terms of therapy, and a sense of what the future holds in terms of prognosis. The philosophy of clinical medicine can be seen as a set of beliefs relating to the core tasks of diagnostics, therapeutics and prognostics. Each of these activities has been informed by emerging science throughout the millennia.

That philosophy informed early conceptions of the practice of medicine serves as a reminder of how separate the two endeavors have become over the centuries. The advent of evidence-based medicine (EBM) has stimulated considerable interest among philosophers of science. As one of the founders of EBM, Brian Haynes, stated, "… it is fair to say that not very much attention was paid by the originators of EBM to the philosophy of science… One hopes that the attention of philosophers will be drawn to these questions." Much recent philosophy of science work related to medicine has focused on issues of explanation, models and critiquing various claims about the status of randomized, clinical trials. Few accounts have been given of the relationship between philosophy of science and clinical medicine.

In this presentation I will critically examine the adequacy of evidence as conceptualized by EBM for the practice of clinical medicine. I will argue that the vision of evidence articulated by EBM is overly reliant on certain epidemiological constructs. Evidence-based medicine argues that this type of evidence is necessary for clinical reasoning, but not sufficient. I will argue that epidemiological evidence is neither necessary nor sufficient for the practice of medicine. The central argument I will advance will situate evidence within the framework of defeasible reasoning schemes. I will then draw on recent developments in argumentation theory to articulate a logic of clinical reasoning that is better calibrated to clinical practice than the proto-logic articulated by proponents of EBM. I will draw further implications of this logic for the relationship between clinical medicine and philosophy of science.

The Logic of Generalizing Clinical Trial Evidence

Jonathan Fuller
University of Toronto

June 29. 10:30-12:30 VC 215

Clinical trials are the preferred test of the clinical efficacy, or effectiveness, of modern medical treatments. Clinical trials also constitute the preferred evidence base for treatment decisions in evidence-based medicine (EBM). However, effectiveness in clinical trials does not guarantee effectiveness in clinical practice. As Nancy Cartwright has shown (2007), randomized, controlled trials (RCTs) form the basis for a valid deductive argument with the conclusion that the treatment caused the outcome in some members of the trial population. The claim that RCTs have a 'high internal validity' is justified only with respect to this internal clinical trial argument, which refers to trial patients under trial conditions. In external clinical trial arguments, effectiveness in other patients under other conditions is instead inferred from the trial results. External arguments are most relevant to physicians as it is various 'other populations under other conditions' that are actually encountered in practice. Thus, I will consider the logic, or structure, of these arguments.

Most commonly, external clinical trial arguments have the form of a generalization, especially one in which the average treatment effect, measured in the trial, is applied to a particular target population. In other words, the practitioner argues that the average effect would also be seen in this particular population context. This strategy is exemplified by clinical practice guidelines, which generalize from trial findings to wide target populations in support of their recommendations. The logic here is statistical generalization, a simple induction from the sample (trial) population to the sampled (target) population. The guideline approach seems incomplete, but guideline development panels follow evidence-based rules, so perhaps EBM can fill in the gaps. A reading of the EBM literature reveals that the normative EBM approach relies on the logic of falsification. It conjectures that the average trial result applies here and now, and then attempts to refute that generalization by searching for compelling evidence to the contrary. Proponents of EBM argue that average effects are usually stable across contexts, which suggests that all generalizations are automatically well-corroborated and that falsification is merely a last check on the process.

I will argue that neither the guideline approach nor the EBM approach provides us with good, positive external arguments. The evidence movement in contemporary medicine lacks the reasoning needed to apply clinical trial evidence judiciously in practice. Whatever the logic to this reasoning, it must get us to the ultimate conclusion about treatment benefit in a particular clinical encounter. Since clinical encounters involve individual patients rather than populations, generalization is insufficient for this purpose. To successfully apply evidence, I will suggest physicians also need particularization, an inference about an individual. An approach to reasoning that can take us from evidence all the way to the clinical encounter is elusive but essential for evidence-informed medical practice.

Mechanisms in Epidemiology and in Evidence-Based Medicine

Robyn Bluhm
Old Dominion University

June 29. 10:30-12:30 VC 215

A great deal of work in philosophy of science has focused on the role of mechanisms in the life sciences, but it is only recently that discussion of mechanisms has played an important role in the philosophy of medicine. Much of this discussion has been inspired by the claim by evidence-based medicine (EBM) that knowledge of mechanisms should play only a limited role in clinical reasoning. EBM is based on the idea that there is a "hierarchy of evidence" which says that epidemiological studies, particularly randomized controlled trials, provide much stronger evidence for clinical decision-making than knowledge of physiological mechanisms.

Jeremy Howick and Holly Andersen have both written recently about the role of mechanisms in EBM. Despite being generally supportive of EBM, Howick (2011) argues that, in some cases, knowledge of mechanisms may be sufficient to ground treatment decisions. Specifically, when knowledge about a mechanism is “not incomplete” and takes into account the complexity of physiological systems, it may provide acceptable evidence for the efficacy of a treatment. Howick acknowledges that such cases are likely to be rare, but Andersen’s arguments (2012) suggest that we may never be able to assert such knowledge confidently. She describes a study in which paracetamol was given prophylactically, to prevent fever in infants who had received an immunization. Although both the mechanism underlying fever and the mechanism of developing immunity are well-understood, it turned out that paracetamol interfered in both of these mechanisms, something that could not have been predicted based on what was known about each mechanism separately. Andersen takes this to be evidence that EBM’s approach is necessary to establish the efficacy of a treatment. She also notes, however, that it is the nature of epidemiological research that not all individuals will respond the same way to an intervention as the group does as a whole. Because of this, the results of randomized trials cannot be straightforwardly applied in clinical practice. Therefore, she suggests, physicians can, and even must, use their knowledge of mechanisms to guide the application of EBM’s results in the care of an individual patient. Yet if the gaps in our knowledge of mechanisms raise problems for making predictions at the population level, they will raise the same problems when predicting outcomes in individual patients.

I suggest that the problem with finding a place for mechanisms in clinical decision-making stems from the fact that both Howick and Andersen see mechanisms as an alternative to epidemiological research. Instead, knowledge of mechanisms should be integrated into epidemiology. In making this case, I draw on Heather Douglas’s (2009) argument that the purpose of developing explanations in science is to enable better predictions, as well as a paper by James Tabery (2009), in which he develops philosophical work on mechanisms to account for variability in outcomes. Although Tabery is concerned with genetic mechanisms, I adapt his framework for epidemiological research. I argue that my approach provides a better evidence base for clinical decision-making.

An Argument for Fewer Clinical Trials

Kirstin Borgerson
Dalhousie University

June 29. 10:30-12:30 VC 215

Twenty years after its dramatic proclamation in JAMA, evidence-based medicine (EBM) is now pervasive as a standard for clinical practice in health care settings world-wide. But the EBM of 2012 would in many ways be unrecognizable to the group of concerned clinical researchers and practitioners who set out to develop a revolutionary ‘new paradigm’ for clinical decision-making in the early nineties. The most significant shift within EBM has been one away from the individual critical appraisal of clinical research by practicing physicians to ever-more elaborate systems of knowledge synthesis and translation. The evolution of the 4S, then 5S, then 6S, approaches to evidence-based clinical decision-making illustrate this effectively. Each addition to the original hierarchy of research methods (syntheses, synopses, summaries, systems...) aims to make clinical research evidence more digestible and easier to use. Developers of the approach seem keen to get to a point where all of the information needed by clinical decision-makers would be contained in the titles of short synopses or summaries.

One driving force behind these changes has been the dramatic increase in the production of clinical research evidence. With thousands of new studies published each week in medical journals, the task of staying on top of research as an individual clinician, even in a relatively narrow sub-speciality, has become thoroughly overwhelming. But knowledge synthesis of the sort pursued by modern-day proponents of EBM has well-documented shortcomings. Given the overload of biomedical research data published every day, the decision-paralysis that often results, and the serious problems with knowledge synthesis as a solution to this predicament, how might the initial project of EBM – improving clinical practice through closer alignment with the results of clinical research – be achieved? In this paper I explore whether the following solution might be defended: conduct fewer clinical trials. Put in more positive terms, it suggests that we should conduct only clinical research of the highest quality, and prohibit all other (lower-quality) research, even when that research would seem to meet some minimal ethical standard of acceptability.

I argue that the harms associated with the over-production of low-quality research evidence are rarely included in the social value calculations conducted during the ethical review of proposed clinical trials. This happens for (at least) two reasons: first, because social value calculations – when they are conducted – focus on positive outcomes of potential trials; and second, because the requirement of social value has been generally neglected, or interpreted far too narrowly, by research ethics committees. But the overproduction of low-quality clinical research is very likely to be harmful to patients. If trials are publicly funded, we can also factor in trade-offs between the value of such trials and other social goods (environmental protection, education, social security, etc.). In these contexts, the argument against conducting poor quality research is even stronger. In sum, on ethical grounds there are persuasive reasons to endorse the position that we should conduct fewer clinical trials.

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S14: Scientific Understanding Without Truth
Organizer: Soazig Le Bihan

June 29. 10:30-12:30 VC 212

It is widely assumed that much of the epistemic value of scientific theories comes from the fact that these theories provide us with accurate explanatory knowledge of the way the world works. However, the notion of accurate explanatory knowledge is far from unambiguous, not only because many explanations contain idealizations, but also because scientific realists and anti-realists interpret it in radically different ways. It is of crucial importance to develop a satisfactory account of the epistemic value of science (beyond its predictive power) that does not depend on one's view of such a highly controversial issue as the question of scientific realism. This symposium will explore the ways in which one can defend the idea that part of the epistemic value of science is that many scientific theories afford some form of understanding that is not necessarily associated with scientific realism.

Until recently, the notion of understanding has been largely ignored by philosophers. At worst, understanding has been taken to be a subjective and often misleading feeling that plays no positive role in the scientific endeavor. At best, understanding has been considered as a by-product of accurate explanatory knowledge. This has dramatically changed in the last five to ten years, both in epistemology and in the philosophy of science. More and more often, one finds suggestions in the literature that (1) there is a notion of understanding that is distinct from accurate explanatory knowledge, (2) such a notion constitutes a distinctive epistemic goal for the scientific endeavor, and (3) such a notion accordingly deserves renewed philosophical attention. For example, it has been suggested that understanding is not a species of knowledge (Kvanvig 2003), or that to have an explanation is neither necessary nor sufficient for having understanding (De Regt 2009, Lipton 2009). The aim of the proposed symposium is to advance the philosophical analysis of understanding along these lines.

The participants explore new ways to partially decouple understanding from accurate explanatory knowledge. All share the view that trying to separate understanding either from explanation or from knowledge is not the best strategy. It is more promising, according to them, to investigate in what sense and to what extent understanding can be analyzed as related to, but also as distinct from, considerations of accuracy. Catherine Elgin maintains that the epistemic values of scientific models resides in that they are “felicitous falsehoods”, i.e. inaccurate representations that exemplify features of their target phenomena. Kareem Khalifa argues that understanding can be conceived as a species of explanatory knowledge, if cognitive utility, rather than approximate truth, is taken as the fundamental dimension of explanatory evaluation. Soazig Le Bihan defends the claim that one gains understanding of a phenomenon P via a theory if the theory provides modal knowledge of the space of possible -- not necessarily actual -- relevant dependency structures for P. Finally, Henk De Regt articulates his view of understanding as the ability to use scientific theories and models to generate predictions of target phenomena, where these theories and models are not necessarily interpreted realistically. Intelligibility of the relevant theory, not accuracy, is the crucial condition for understanding target phenomena.

The symposium's contributions thus articulate different ways in which scientific theories can provide epistemically valuable kinds of understanding, even if these theories misrepresent the world.

Exemplification in Scientific Understanding

Catherine Z. Elgin
Harvard University

June 29. 10:30-12:30 VC 212

Nature is enormously complicated. Science provides an understanding of nature by discovering orders that underlie its complexities. A common stereotype is that these orders are expressed in explanatory truths. If so, science provides true explanantia from which we can infer truths about the phenomena. This is a lovely picture, but it is false. Science develops and deploys models that are not, and are known not to be true of the phenomena they pertain to. Modeling is not treated as a temporary expedient. Although particular models may be given up with the growth of science, scientists neither expect nor desire the practice of modeling to wane. They consider models to be a good way to capture and convey scientific understanding. But if they are not true, how can they do that?

I will argue that by highlighting, streamlining, overshadowing and omitting, scientific models afford epistemic access to aspects of phenomena that are obscured by more salient, if less significant, factors in accurate representations. The value of modeling resides as much in the capacity to omit (real but) irrelevant aspects of things as in the capacity to disclose relevant ones. Models play a variety of roles. Not all are approximations. Nor is it the case that a closer approximation to the truth always affords a greater understanding. Sometimes, closer approximations introduce complexities that mask features that the simpler model reveals. For example, the Hardy Weinberg model discloses how alleles would redistribute in the absence of genetic change. To introduce evolution, genetic drift, migration, and so on would give a more accurate picture of how genetic change happens in an actual population, but it would preclude understanding how much of that alteration is due to redistribution of existing alleles and how much is due to changes in the gene pool.

If models need not be accurate representations, what ties them to the phenomena they pertain to? I will discuss three alternatives: partial truth (Yablo, Milgram), relative truth (Richard), and felicitous falsehood (Elgin). I will argue that felicitous falsehood affords the best account of how scientific models embody and advance understanding. A model is a strictly inaccurate representation that exemplifies -- that is highlights, exhibits or displays -- features it shares with the phenomena it pertains to. By so doing, it affords epistemic access to that feature and shows why it is significant. Since the feature in question may be subtle, complex, relational or dynamic, an effective model can show us something we would not otherwise see, thereby enabling us to understand the phenomena in ways we otherwise would not.

Non-Factive Understanding

Kareem Khalifa
Middlebury College

June 29. 10:30-12:30 VC 212

Ostensibly, understanding is a goal of scientific inquiry. But what exactly do scientists possess when they understand the empirical world? A common view -- shared by philosophers as diverse as Hempel, Salmon, Achinstein, Kitcher, Lipton, and Woodward -- holds that scientific understanding is a species of explanatory knowledge. Given that knowledge that p implies that p is true (i.e. knowledge is factive), this view appears to imply that understanding should trade primarily in true explanatory propositions. Some have recently challenged this view, by offering examples in which understanding is advanced without a closer approximation to the truth. From these examples, they infer that understanding is not a species of knowledge.

There are natural affinities between these debates and earlier ones concerning scientific realism. If understanding is a species of knowledge, and such knowledge requires true explanations, then theories that provide understanding by invoking unobservable entities will sit comfortably within the realist's ambit. If, on the other hand, understanding does not require knowledge of true explanations, then antirealists are vindicated.

In this paper, I will split the difference between these two positions: true explanations are not required for understanding, but understanding is nevertheless a species of knowledge. My argument proceeds in two steps. First, I argue that truth is unnecessary in accounting for advances in our understanding of empirical phenomena. Rather, these advances can be explained by increases in “cognitive utility” -- roughly, that status of an explanation that falls just short of approximate truth. Cognitive utility includes considerations of empirical support, theoretical virtues (simplicity, scope, conservatism, etc.), coherence with answers to related questions, and inferential role. However, even jointly, these considerations need not amount to approximate truth. I examine various scenarios in which the cognitive utility and the truth of our explanatory commitments change, and argue that adverting to cognitive utility alone accounts for advances in understanding at least as well as appealing to truth and cognitive utility. By contrast, advances in understanding cannot be explained nearly as well by appeal to truth without also appealing to cognitive utility. I conclude from this that truth plays a dispensable role in understanding.

Second, I argue that this conclusion is compatible with understanding being a species of explanatory knowledge. Specifically, I argue that if S understands why p, then there is some q such that S knows that q explains p is cognitively utile. Thus, understanding is not factive in the sense that an explanation's cognitive utility does not entail that it is approximately true. On the other hand, since this view holds that one must have true beliefs about an explanation's cognitive utility, understanding is factive in a weaker sense than has previously been assumed.

I conclude by briefly returning to the debates between scientific realists and their critics. On the view presented here, both parties to the debate can agree that understanding is a species of explanatory knowledge, and the primary issue is whether cognitive utility or approximate truth is the fundamental dimension of explanatory evaluation.

Scientific Understanding Beyond Truth: Modal Understanding

Soazig Le Bihan
University of Montana

June 29. 10:30-12:30 VC 212

The goal of this paper is to articulate a notion of scientific understanding such (1) that scientific theories and models can afford independently of whether or not they faithfully represent the way in which the world actually works, and (2) that has genuine epistemic value.

There have been some attempts to formulate such a notion in the recent literature, among which Catherine Elgin's work is prominent. That said, two important objections that have been leveled against these accounts need to be addressed. Doing so will allow us to articulate a clear notion of genuine understanding that theories and models that misrepresent the world can afford.

The first objection is that the only notion of understanding that one might associate to theories that provides a poor representation of the world is an understanding of how they generate the predictions that they do. Call this type of understanding understanding *within* a theory. Granted, this is an important notion of scientific understanding. That said, in addition to allowing us to understand how they entail such and such predictions, scientific theories provide us with some understanding of the phenomena they target in the world. For example, while it is true that it is epistemically valuable to understand the behavior of a massive body orbiting around another one within Newton's theory, a great deal of the epistemic value that we grant to Newton's theory relates rather to the understanding that such a theory provides of the behavior of actual mechanical bodies in the world, say Mars' orbit around the Sun. And this is true even if, as we know, Newton's theory does not provide a faithful representation of how mechanical bodies actually interact gravitationally.

In light of the above, it is proposed to distinguish between two notions of scientific understanding. The first is the one described above, understanding *within*. The second is a notion of understanding so that theories and models, even when they misrepresent the way the world works, still afford some kind of understanding of the phenomena that they represent (the phenomena that are recovered as predictions of theories and models). Refer to this notion of understanding as understanding of phenomena *via* a theory and its models. This is the notion of understanding that is the focus of this paper.

The second objection that needs addressing concerns this notion of understanding *via*. Some authors reject the suggestion above that we need to articulate a notion of understanding that theories could afford independently of whether or not these theories faithfully represent the way the world actually works. According to these authors, scientific understanding *only* arises from the fact that scientific theories and their models get something right about how the phenomena actually come about. That the representations that some theories give of the world are partially inaccurate is not what matters for how they afford understanding. These theories are also partially accurate, and that is how they afford genuine, objective understanding of the phenomena.

It will be granted that this notion of scientific understanding *via* captures an important part of what we mean when we say that a scientific theory affords us some understanding of its target phenomena, even if it provides a simplified and/or idealized representation of the way the world works. That said, it will be argued that there is a lot of epistemically valuable scientific practice that is not captured by this notion of scientific understanding *via*. The reason is that truth is not necessarily what is conducive of scientific understanding *via*. With this in hand, an expanded notion of understanding *via* will be offered and defended, i.e. the understanding that theories and models afford of *how possibly* the phenomena arose. It includes the previous notion of understanding *via* as a special case, but also captures far more than this previous notion does.

Scientific Understanding without Scientific Realism

Henk W. de Regt
VU University Amsterdam

June 29. 10:30-12:30 VC 212

Scientific realists often claim that the widely accepted view that science provides explanatory understanding commits one to a realist position. In my paper I will argue against this idea of a necessary connection between understanding and realism.

Study of scientific practice reveals that understanding is often obtained via theories and models that are unrealistic or simply false. For example, many scientific disciplines concerned with complex systems (such as economics or climate science) use highly unrealistic models to achieve understanding of phenomena. The same goes for many mechanisms that figure in biological and neuro-scientific explanations. Similarly, Feynman diagrams, which are used to understand phenomena in the domain of quantum electrodynamics, cannot be taken as correct representations of reality. Moreover, the pessimistic meta-induction from the history of science forces us to take seriously the possibility that all our current best theories are false, which would imply that we do not have explanatory understanding at all.

So we face the dilemma of either giving up the idea that understanding requires realism, or allowing for the possibility that in many if not all practical cases we do not have scientific understanding. I will argue that the first horn is preferable: the link between understanding and realism can be severed. This becomes a live option if we abandon the traditional view that scientific understanding is a special type of knowledge, namely knowledge of an explanation (S understands p iff S knows that T explains P). While this view implies that understanding must be factive because knowledge is factive, I avoid this implication by identifying understanding with an ability rather than with knowledge. I will develop the idea that understanding phenomena consists in being able to use a theory to generate predictions of the target system's behavior. The crucial condition is not truth but intelligibility of the theory, where intelligibility is defined as the positive value that scientists attribute to the theoretical virtues that facilitate the construction of models of the phenomena. Intelligibility is not an intrinsic property of theories but a context-dependent value related both to theoretical virtues and to scientists' skills.

I will show, first, that my account accords with the way practicing scientists conceive of understanding, and second, that it allows for the use of idealized or fictional models and theories in achieving understanding, as well as for wholesale anti-realist (or constructive empiricist) interpretations of scientific theories. Contra van Fraassen, however, I argue that explanatory understanding is an epistemic aim of science. I conclude that scientific understanding is an epistemic aim of science, but that understanding does not require realism. Understanding of phenomena can be obtained via theories or models independently of whether these are true representations of an underlying reality.

Contributed Abstracts

Cognitive Scientists Are Not Computational Functionalists

Mikio Akagi
University of Pittsburgh

June 29. 2:00-3:30 VC 215

It is not obvious what unifies the various disciplines and topic areas of the cognitive sciences. Plausibly, the cognitive sciences are unified by a common object of inquiry—cognition. But what is this thing we have come to call ‘cognition’? It is clearly not what is denoted by more traditional uses of the word. This is evident from the fact that many of the phenomena studied by cognitive scientists are unconscious and automatic, and that they include affective and motivational phenomena. The most prominent philosophical accounts of cognition tend to be forms of computational functionalism (e.g. those of Putnam, Fodor and Chalmers). On these views, cognition is understood to be essentially a system of algorithms. Indeed, many empirical cognitive scientists claim that cognition is just information processing, which might be understood as an endorsement of a kind of computational functionalism.

However, there are many recalcitrant objections to functionalism, including liberality objections, chauvinism objections, triviality objections, and multiple realizability objections. My worry is not that these objections undermine functionalism, but that empirical cognitive scientists do not seem to care about them. This might be because the objections ultimately fail, or because cognitive scientists are philosophically unsophisticated, or because cognitive scientists are not actually functionalists. I argue for the latter alternative. Consideration of each of the recalcitrant objections to functionalism reveals that functionalism has dialectic weaknesses that hypotheses in the cognitive sciences do not share. That is, the sort of claim that would count against a functionalist proposal is not the sort of evidence that would count against an empirical hypothesis in cognitive science. This is true even if the objections to functionalism are unsound. I do not claim that computational functionalism does not truly describe something. However, if I am right then functionalism is not a satisfying account of what cognitive scientists actually study.

Attention to the history of functionalism helps us see why it should seem natural to view functionalism as the metaphysics of cognition, but also why it should turn out to be such a poor account. Computational functionalism was most famously articulated in 1967, with an eye toward philosophical rather than empirical disputes. It was later adapted to answer questions about cognitive science, but the cognitive science of that time had many presuppositions then that have been abandoned or changed since then. In particular, the concept of cognition seems to have been reevaluated by scientists since the early 1980s.

I conclude with a plea for an account of cognition that is more adequate to the practices of contemporary cognitive science, and with a discussion of some desiderata for such an account. In particular, an account of cognition should probably abandon pretensions of extensional specificity in virtue of functional structure, and should allow for unspecified realization relations and theoretical pluralism. It should also make intelligible the recalcitrant controversies that are proper to cognitive science: about the concept of representation, the evidential significance of evolutionary considerations, and about modularity and relations between “levels” of explanation.

Social epistemology of e-prints in scientific practice

Ben Almassi
College of Lake County

June 27. 4:00-5:30 VC 206

In Peer Review: A Critical Study, David Shatz echoes Churchill's famously tepid assessment of democracy, repurposed to express the widespread notion that peer review "is the worst form of evaluation — except for all the others." The underlying judgment here is that peer review rightfully serves as a cornerstone of scientific and scholarly judgment not for its unimpeachable reliability, but because alternatives are even less so. The recent growth of arXiv and other online e-print platforms for scientific communication gives philosophers of science an opportunity to revisit the uneasy assumption of social-epistemological primacy of modern scientific peer review as actually practiced. What is lost evidentially when scientific communities move from dependence on traditional prepublication peer-reviewed journals for communication and exchange to faster, open, largely unfiltered platforms like arXiv? Absent the filter of masked review, what alternative social-epistemic mechanisms are available to scientists to gauge others' work and mitigate biases in collective scientific practice? Do the benefits of e-print publication come at the costs of institutional trustworthiness and objectivity? In considering these questions we distinguish between multiple factors, including the range of existing and emerging forms of open access, evidential and non-evidential functions of peer review traditionally conceived, and the assorted considerations cited for and against open access. We begin by identifying basic characteristics of different forms of online scholarly publication and articulating a comprehensive portrait of the functions to which peer review traditionally has been put. So situated, we then consider how these traditional functions might be met in new and alternative ways given new forms of scientific and scholarly publication. For unmet functions — particularly for concerns about gender bias and other authorial identity biases absent formal masked review mechanisms — we prioritize participants' stances, looking to criticisms and defenses made by actual arXiv users. Some unmet functions are abandoned or left to other devices; others recognized as indispensable may be incorporated in developing forms of open publication. The development of arXiv as an open, unfiltered, unmasked site of scientific communication has been responsive to issues of corroboration and gate-keeping, yet problems of gender and authorial identity biases remain comparatively ignored. We seek to unpack arXiv founder Paul Ginsparg's critique of peer reviewed publication as traditionally organized, and draw upon criticisms and defenses of Ginsparg's own model by David Shatz, Steven Harnad and Kathleen Fitzpatrick. We find that authorial identity biases constitute a persistent evidential and ethical issue for open access absent traditional peer review. We close by considering the prospects for arXival equivalents to traditional masked review, enabled by emergent informational technologies rather than contrary to them, and derived from scientific communities' specific social-epistemic commitments rather than imposed as off-the-shelf solutions.

Grouping practices from a "naturalistic" point of view: a meta-theoretical comment

Alba Amilburu
University of the Basque Country

June 27. 2:00-3:30 VC 206

The notion of 'natural kind' plays an important role in philosophy of science for understanding grouping practices on the one hand, and in scientific practice as a methodological tool on the other, because it allows and facilitates a comparison of different grouping strategies. In order to investigate the contribution of this philosophical concept, we need first to clarify what makes a kind a natural kind. In this paper I argue that the notion of "natural kind" is ambiguous: a fundamental disagreement concerns how philosophers understand "naturalness". Thus, a meta-theoretical analysis —i.e., an interpretation of the different theoretical accounts of natural kinds that conform the current debate— is a necessary step to clarify the use and meaning of the "natural kind" concept.

In a recent paper, Reydon (2010) presented an interesting meta-theoretical analysis of the current debate on natural kinds. He identifies two lines of work or traditions —called by Reydon a "metaphysical approach" and an "epistemological approach"— that interpret and address the same philosophical problem differently, as each line of work is accompanied by a different set of assumptions and ideas. In the "metaphysical approach" natural kinds are conceived as real kinds that exist in nature independently of observation and human reasoning. In this classical tradition, the theoreticians (for instance, Ellis 2001) face issues such as how we should conceive and develop the idea of natural kindhood, or which sorts of real essence correspond to the different types of natural kind that exist. In the "epistemological approach" natural kinds are considered as groups of particulars that are made by us with the purpose of being useful in a certain context. The main concern for authors close to this line of work (for instance, Boyd 1991) is to understand what it is that makes certain groupings of things suitable for featuring successfully in explanation and prediction.

In this paper, first, I examine critically Reydon's proposal pointing out certain aspects of his analysis that should be improved, and second, I present an alternative proposal, which includes and develops some aspects of Reydon's analysis but introduces new elements in order to overcome its limitations. In particular, I argue that the distinction between a metaphysical and an epistemological line of work is better understood as a distinction between an "essentialist approach" where natural kindhood is metaphysically clearly defined in terms of essentialism, and an "non-essentialist approach" in which membership condition is metaphysically undetermined because it is relative to an epistemic contribution in a certain research context. Authors close to this second line of work understand scientific grouping practices and group concepts as a decision-making calculus over where and how to draw and describe kind boundaries, as MacLeod (2011) pointed out.

I argue that this alternative proposal is best suited for a) explaining in what sense the notion of "natural kind" is ambiguous, and b) understanding the peculiarities and differences among theoretical approaches in the current philosophical discussion on groupings and grouping concepts.

Biochemical Kinds and Protein Classification

Jordan Bartol
University of Leeds

June 27. 10:30-12:30 VC 206

There is a crisis of accepted beliefs in the study of protein molecules. Investigations into proteins have long been structured by reductionist ideology, according to which microstructural descriptions of proteins should explain their chemical and biological properties. Yet a number of recent phenomena challenge this belief; it has become clear that protein function does not straightforwardly reduce to protein structure. Calls for a paradigm shift have become regularity. In this paper I discuss one of many interesting philosophical issues that arises from this crisis: that of protein kinds.

Recent discussions of proteins (Slater 2009; Tobin 2010) have highlighted how physico-chemical (microstructural) properties cannot account for biological facts about protein molecules. Biological properties appear related to but not determined by physical properties. The microstructure of a given protein often underdetermines biological function, and biological function can be realized by many distinct protein structures. I agree with these assessments, but believe that physico-chemical facts can nonetheless underwrite kind classifications in much the same fashion as with simpler chemical molecules. Given that these physico-chemical kinds do not line up with biological properties, however, we need a second theory of protein kinds; I claim that only an evolutionary theory can explain the biological properties of these molecules. Though biological properties are partially explained by physical facts, it is the evolutionary history of a protein molecule that provides the whole story. I thus offer a dual theory of protein kinds, corresponding to different properties and slightly different referents.

After introducing this theory of protein kinds I attempt to address the elephant in the room: How do these abstract-seeming issues from metaphysics bear on scientific practice? I explain why a kind classification relying on sound metaphysics is indeed solid foundation for actual investigative practice. Just as a search for a sound metaphysics of protein kinds reveals the need for two kind classifications, so too should it reveal the need for a twin-track approach to understanding protein molecules.

Socially and morally responsible cognitive neuroimaging: Mental rotation case study

Vanessa Bentley
University of Cincinnati

June 29. 2:00-3:30 VC 215

Cognitive brain imaging offers a deceptively clear and distinct window into the brain. Newspapers and popular press books sensationalize neuroscientific findings, which find their way into general society. My interest is in the neuroimaging of sex/gender differences. The concern is that giving a biological explanation for differences between men and women can be used to justify stereotypes, prescribe certain social structures, and limit resources for individuals interested in pursuing non-gender-normative pursuits. For example, showing where in the brain the difference lies between men's and women's performance on mathematics could be used to limit resources for women interested in pursuing science, technology, engineering and mathematics (STEM) fields.

I focus on the specific question of sex/gender differences in mental rotation with the hope that by paying attention to the details of the studies I can highlight problematic practices and suggest modifications to avoid harmful science.

Mental rotation is widely accepted as one of the most static and robust sex/gender differences in cognition. As such, neuroimaging researchers have searched for brain activation differences between men and women to underlie the supposed performance difference. However, most fMRI studies of sex/gender differences fail to elicit the supposed male performance advantage and there is little overlap (and certainly no consensus) on different sex/gender-linked areas or networks underlying mental rotation processing. Despite the studies' failure to demonstrate a performance advantage for men on mental rotation, the activation differences are attributed to different "cognitive strategies" used by men and women — without assessing if they are indeed engaging in different cognitive strategies.

I identify a number of problems with these studies. 1) A sexist theory regarding male performance advantage in mental rotation persists despite the studies failing to demonstrate sex/gender differences. 2) Observed activation differences are attributed to an untested, yet testable, "cognitive strategy" explanation. 3) "Natural" sex differences are supposed despite only testing individuals from industrialized, Western cultures (usually university students). 4) No consideration is given to the different gendered social environment experienced by males and females despite studies that show how activities, education and experience affect mental rotation ability. 5) Imprecise language regarding sex, gender, and biology contributes to confusions regarding causal assumptions underlying the supposed difference. 6) The data is not analyzed blind to gender.

Thus, current practice in the neuroimaging of sex differences is sexist, ignores relevant evidence from other scientific fields, and inaccurately presents its results as stemming from "natural" sex differences rather than investigating the possibility that sex differences arise from different gendered rearing environments. Using feminist standpoint theory, I suggest modifications to current practice to begin to address these problems. As a start, these modifications involve: 1) analyzing data blind to gender; 2) assessing the influence of spatial activities, science classes, and the effect of practice on activation; 3) dividing groups based on performance rather than gender; 4) broadening diversity of participants; 5) investigating the effect of strategy use on activation; 6) separating questions of proximate and ultimate causation; and 7) being reflexive in reporting results.

Why what is impossible in practice is more relevant than what is impossible in principle

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June 29. 2:00-3:30 VC 323

The debate on reductionism in biology has been traditionally framed in theoretical terms and the alternatives to theory and explanatory reductions are mainly addressing the multilevel and multifactorial features of biological processes and phenomena in terms of pluralism, integration, and interdisciplinary programs (1). My project somehow fits in between these positions and aims at clarifying what is at the core of these major transitions from an epistemological point of view. I believe, in fact, that the question about the possibility of reductionism in scientific practice has not been resolved and that the rational behind pluralism and integration in biological sciences can be further spelled out.

This paper is a partial contribution to this general project. I present an analysis of how the explanation of higher-level properties affects scientific practice both in theoretical and methodological terms. Examples are taken from cancer research and cell biology where the contrast between reductionist and antireductionist positions still seems to be far from being resolved. This contrast embodies the main issues at stake in the philosophical debate as well. The present argument is based on the analysis of the conceptual convergences among different explanatory models of cancer in order to clarify what really is at stake in the reductionist-antireductionist debate. Both reductionist and antireductionist views, in fact, have a common root in the challenge of explaining how biological processes are regulated at different levels of biological organization. From a scientific perspective the point is to provide an explanation of some robust phenomenon or higher-level properties. From a philosophical perspective, the aim is to understand the multilevel phenomenology of complex biological behaviors.

Following Schaffner's discussion about reductionism in biological sciences (2, 3), I thus clarify the requirements for partial reductions in biology. I argue that attempts to explain higher-level properties in reductionist mechanistic terms fail because they are unable to grasp the explanatory relevance of generalizations in the system-level understanding of biological phenomena. Requirements for reductions are thus analyzed on the basis of the Preferred Causal Model System element introduced by Schaffner and discussed to show how science works in practice and why this case persists.

My final thesis states that what is possible or impossible in practice is philosophically more relevant than what is possible or impossible in principle and that this general thesis has two related implications on which I focus in this paper: 1) that the explanatory process in biological sciences, through methodological reductions, is characterized by the identification of a mesoscopic level, which I will characterize in epistemological terms, and 2) that negative results in biological sciences are equally important than the positive one as they shed light on a different aspect of the biological problem.

Although the discussion and the examples I will preform necessarily focus on one aspect of the biological complexity and of the scientific enterprise, I believe that the epistemological perspective that emerges from this analysis can be exported to other areas of scientific inquiry and expand the relevance of the integrative enterprise in life sciences as well.

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Plausibility as a cognitive value

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June 27. 10:30-12:30 VC 211

Hasok Chang (2001) argues that “empiricist realism” (following e.g. Grover Maxwell), which considers the empirical success of scientific theories to be epistemically valuable and attempts to establish their (approximate) truth based on that success — is not viable by itself. Chang proposes an additional, non-empirical criterion for epistemically evaluating our systems of knowledge: “ontological plausibility”. Chang's motivation comes from our frequent inability to deny certain intuitive (non-empirical) beliefs about reality — for their denial would be implausible/ unintelligible (he doesn’t distinguish the two) to us and would make us unable to comprehend reality. These principles — these unshakable beliefs, he calls “ontological principles”: rationally compelling guiding principles for our comprehension of reality. A realism that epistemically values and pursues these principles — and takes them as a standard for evaluating validity of scientific claims — he calls plausibility realism.

Chang’s position on ontological principles in a subsequent paper (2009) is different. He discards the earlier idea of plausibility as too subjective and instead takes ontological principles to be relativized to the “epistemic activity” at hand. For instance, if we didn't assume that the objects we count are discrete (the “principle of discreteness”), the activity of counting would become unintelligible. The principle is not some unconditional truth: its roots are entirely pragmatic. We absolutely need to consider it to hold only if we choose to engage in an activity whose very intelligibility relies on it. Along with the need to (intelligibly) engage in an activity comes the pragmatic need to believe the relevant principle — hence such belief is not subjective.

Here, I reformulate Chang's plausibility arguments in terms of values: I construe plausibility as a cognitive value. This is useful since a) Chang does not invoke talk of values, so this provides a new way of understanding and critiquing his views: it sharpens Chang's arguments and provides clarifications on the differences in the roles he sees empirical evidence and ontological principles playing in science; and b) plausibility has generally not been identified as a value in the traditional values literature, so adding it to the list opens new issues there. I then criticize Chang’s original treatment of plausibility and intelligibility as interchangeable: the two need to come apart. Following distinctions from Douglas (2012), I argue that intelligibility belongs to the group of values that are “minimal criteria for adequate science”; plausibility belongs to those that when instantiated, “don't guarantee us the truth, but increase the likelihood of honing in on the truth” — which is very much in line with Chang's own views on plausibility and truth. Further, I argue that neither plausibility realism nor conditional/relativized intelligibility is a viable theory by itself. While an unconditional plausibility criterion is indeed idiosyncratic as Chang realizes in the later paper, I contend that conditional intelligibility is too minimal and inadequate a criterion. I suggest a way forward by reintroducing plausibility into his later story and give an account of what I call conditional plausibility realism, which amalgamates the two views.

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Knowledge, Ignorance, and Intellectual Property Rights: The Case of GM Seeds

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June 28. 4:00-5:30 VC 211

Intellectual property rights -- particularly patents -- are playing an increasingly important role in many areas of science, including the biological sciences and biotechnology. While there are a number of potential justifications for patenting, including Lockean labor-based justifications and Hegelian personality-based justifications, the most plausible justification is consequentialist in nature. On this account, patenting incentivizes research and thereby promotes the generation of scientific knowledge, which in turn facilitates both technological and social progress. Recently, however, a number of commentators have questioned the consequentialist justification, on the grounds that patenting is actually inhibiting research in many areas of science. In this paper, I will make a stronger argument. In some areas of biotechnology, patenting is not only inhibiting research; it is prohibiting it. In particular, I will argue that intellectual property rights are being used to prohibit research on genetically modified (GM) seeds.

While there are powerful moral reasons for ensuring that patenting does not prohibit research, the primary focus of this paper is not moral but epistemological. Recent work in the social epistemology of science has emphasized the importance of the organization of research in the production of knowledge. The case of GM seeds provides an important illustration of this. More specifically, an examination of this case furthers both the critical project and the meliorative project in social epistemology. The critical project, which has roots in the work of Karl Marx, Karl Mannheim and others, is the project of identifying features of the institutional environment of research that impede the production or dissemination of knowledge. This project, in other words, exposes how social structures give rise to ignorance. The meliorative project, based on the work of Philip Kitcher, Alvin Goldman, and others, is the examination of how these social structures can be redesigned to improve our epistemic situation. In this paper, I show how current intellectual property laws and policies allow for patent holders to prohibit others from doing research on GM seeds, and I examine ways in which these laws and policies can be changed in order to promote further research and facilitate the dissemination of knowledge.

Subjective Report and Operationalization in the Study of Consciousness

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June 29. 2:00-3:30 VC 215

There are a number of controversies surrounding the role of subject report in cognitive scientific practice. The aim of this paper is to examine, in particular, problems that have arisen as a result of reliance on subjective report in the project of unearthing neural correlates of consciousness (NCC). I argue that inflated importance of verbal reportability has hindered NCC research by limiting conceptions of conscious content to coarse-grained categories that are easily relatable through language, and further by contributing to the generation of problematic experimental designs. I begin by briefly outlining NCC research and the role that verbal report has come to play in it. I then explicate a line of reasoning that grounds the credence given verbal report in NCC research. In cashing out this line of reasoning, it becomes immediately apparent that it relies on problematic premises. After sketching some of the different lines of objection that could be pursued, I focus on two related objections in depth throughout the remainder of the paper: (1) overemphasis on verbal reportability obscures and downplays the importance of aspects of perceptual experience, which evade verbal description; (2) pretheoretical assumptions about the nature of consciousness—specifically the assumption that it is a binary property of (brain/mental) states—shape verbal report tasks in experimental designs in problematic ways. In light of these objections, I develop a positive view according to which consciousness ought to be operationalized, not strictly in terms of subjective report, but through an integrative process coordinating subjective, behavioral, and neurophysiological measures.

The Promise and Problems of Open-Source Drug Design

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June 28. 4:00-5:30 VC 211

Though all human cells possess the same genetic material, not all human cells have the same function. Epigenomic regulatory mechanisms are responsible for the differentiation of cells into skin cells (keratinocytes), liver cells (hepatocytes), and blood cells (haematocytes), among many others. Much of what we used to call “junk DNA” is, in fact, thoroughly functional—non-protein-coding, but involved in gene regulation (Nature ENCODE, September 6, 2012). The study of regulatory genomics is an exciting area of science right now, because this work promises to relieve human suffering from diseases such as cancer and HIV/AIDS, and advance the study of aging and reproduction (in particular, contraception). Turning away from “the gene’s eye view” and toward a more systematic understanding of the human genome, chemical biologists no longer aim to modify the genes themselves, but attempt, instead, to develop synthetic molecules with the aim of manipulating some part of the larger regulatory system. In short, they attempt to design molecules and, eventually, medicines that “turn genes on and off.”

Drug discovery traditionally has been done behind closed doors by for-profit corporations hoping to develop best-selling medicines that both recoup initial research investment and pass on healthy returns to company shareholders. Very recently, academic research centers have started to develop their own programs in applied genomics. Instead of keeping the structures of their molecules secret until these molecules can be developed into viable medicines, academic researchers have started to publish the structures of their molecules at a much earlier stage of development than a pharmaceutical corporation might, in an effort to encourage collaboration and speed drugs to market.

This paper examines the new practice of “open-source” drug discovery against the background of several canonical issues in the philosophy of science. First, I suggest two ways in which philosophers might help scientists think through the bumps they have encountered in open-source. Second, I suggest two ways in which the open-source movement might help philosophers think through some issues that matter to them.

A new kind of credit.
David Hull argued in "Science as Process" that science is motored, in fact, by scientists seeking credit for their achievements, often in the form of authorship credits on journal papers. Credit secures: 1) entry into prestigious intellectual communities, 2) institutional promotion, 3) responsibility/accountability, and 4) legacy. The claim is not that scientists are self-promoters, but that contemporary science simply could not operate without the assignment of credit. Because widespread collaboration is an instrumental goal of the open source movement in academic medical research, scientists working within this value system will require new forms credit and new ways of granting credit.

Fellows or free riders? Incorporating Big Pharma into the open source movement.
The distinction between Big Pharma and academic research is not as easy to maintain as one might think, given that the scientists who end up at pharmaceutical companies are trained alongside the scientists who end up in academia. Moreover, there is a fair amount of back and forth between the industries, as scientists choose different jobs for different phases of their careers. Both industries are constituted by the same population.

However, there is a difference in the institutional values and aims of each industry, as academic medical research centers aim to produce molecules that will alleviate human suffering, while corporate drug companies aim to produce drugs that will earn profits for their shareholders. This creates a dilemma for academic researchers, who must share information with free riders. Academic scientists engaged in the open-source movement are bound by their principles to share information with scientists at pharmaceutical companies, despite the fact that for-profit scientists often fail to reciprocate.

Formalizing the context of discovery.

Most philosophers of science, at least in practice, recognize a general distinction between the context of discovery, in which scientists select research topics and/or notice something new (discover a species, a star, etc.) and the context of justification, in which scientists defend their conclusions with data collected through controlled experimentation. Philosophers traditionally address the context of justification, by offering theories of evidence, confirmation, explanation, and prediction. The more contingent (psychological and historical) details of discovery are left to historians.

The open-source movement offers a more tractable approach to discovery—i.e. a directed method by which we might make discoveries happen, instead of waiting for them to happen. When many different labs study related questions at the same time, relevant data is generated and going-nowhere avenues are closed more quickly, which can mean the difference between life and death, in the medical case.

Science as an emergent system.
The collaborative example of open-source medical research can help philosophers understand and articulate the theory that science, writ large, is an emergent system, irreducible to any one, isolated element (i.e. any one lab, one discovery, one historical case, one scientist, etc.).

Systems biology and the limits of philosophical accounts of mechanistic explanation

Ingo Brigandt
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June 27. 4:00-5:30 VC 215

I discuss how systems biology is working toward complex accounts that integrate explanation in terms of mechanisms and explanation by mathematical models--which some philosophers have viewed as excluding each other. Systems biology is an integrative approach, and its mathematical models ideally combine entities on several levels of organization. Philosophical accounts of mechanisms laudably capture multilevel and multifield explanations, yet mechanistic explanation has been developed as an alternative to traditional models of explanation as derivation from laws and equations. Carl Craver has even promoted the view that mathematical models describe and predict, but do not explain, unlike mechanistic accounts, construed as the analysis of a whole in terms of its structural parts and their qualitative interactions (e.g., binding, activating). Against this, I discuss how mathematical equations can be explanatorily relevant. The paper briefly lays out three cases from systems biology, focusing on questions about qualitative phenomena (rather than the explanation of quantitative details), where equations are still indispensable ingredients of the explanation.

The worry that most mathematical models are merely phenomenological and fail to capture a mechanism's internal causal structure is unfounded in the case of systems biology, as its models are built on experimentally obtained molecular data. The model predictions (which are tested against in vivo data) include how the system behaves in case of interventions on its internal workings (e.g., mutants with modified molecular pathways), so that the model causally explains--which also stems from systems biology's aim to provide tools for therapeutic interventions. One case discussed is the development of mammalian teeth, which is modeled by nonlinear differential equations with are sensitive to quantitative parameters, so that the developmental outcome could not be predicted and explained by standard, qualitative mechanistic accounts. Another example is the modeling of apoptosis, which illustrates the general phenomenon of bistability, i.e., a system being in either of two qualitatively different states. In the case at hand, this is the apoptosis execution state (triggered by signals internal or external to the cell) and the normal, alive state of a cell (maintained despite noisy signals). The qualitative phenomenon of bistability is explained by the presence of threshold behavior, which can be foreseen only by the use of a quantitative model. The final case is the development of vertebrate segments (which form as so-called somites), which is based on the presence of regular oscillations of gene activities inside individuals cells, and its synchronization between cells. The presence of regular as well as synchronized oscillations (as a qualitative explananda) cannot be explained by the knowledge of which molecular components activate or deactivate each other, instead the quantitative interactions have to be taken into account using a mathematical model.

Apart from the fact that equations can be an essential part of mechanistic explanations, systems biology shows that a broader philosophical conception of mechanisms is needed, which takes into account quantitative changes and functional-dynamical aspects, transient entities and the generation of novel entities, complex interaction networks with feedback loops, and system-wide functional properties such as distributed functionality and robustness.

Obstacles to Interdisciplinary Scientific Research in Conservation and Natural Resources Management

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June 29. 2:00-3:30 VC 206

Scientists and philosophers of science have generally acknowledged that interdisciplinary research is a necessary precondition for successful problem-solving in applied domains like environmental science, conservation biology, and natural resources management. This recognition has emerged from the realization that no single disciplinary field alone can produce the knowledge required to craft effective environmental problem-solving strategies. Successful strategies for addressing complex environmental problems typically draw on research grounded in both the natural and social sciences. Although there are numerous instances of successful interdisciplinary collaboration, there are also many examples where interdisciplinary collaboration has been difficult or nearly impossible. In a few high-profile cases, acrimonious disputes among researchers of different disciplinary backgrounds have spilled onto the pages of peer-reviewed journals, further complicating collaborative work.

To illustrate the barriers to interdisciplinary research, I examine a case study involving efforts to preserve biological diversity in Central Africa, a biologically rich but economically poor region. These efforts have sparked vitriolic debates between conservation biologists and anthropologists. Participants and observers have cited personal differences or differences in basic ethical commitments (i.e., commitments to ecosystem conservation vs. economic development) as the reason for the controversy. Though personal and ethical factors cannot be entirely discounted, a closer examination shows that such disputes are overdetermined, since a range of disciplinary differences reinforce each other.

In contrast, I explain dysfunctional interdisciplinary collaboration by identifying three levels of dissimilarity between natural and social sciences with respect to their epistemological and normative frameworks. First, at a basic level, in some disputes disciplines formulate background theory in different ways. In this case, for instance, conservation biologists and anthropologists subscribe to different—and conflicting—theories about the effectiveness of particular conservation and development strategies. Second, their statements about the legitimacy of various research methods demonstrate that they disagree about the explicit and implicit disciplinary standards which guide scientific practice. For instance, conservation biologists and anthropologists hold different standards about the legitimate treatment of human research subjects, they endorse different epistemic values in evaluating research strategies, they have different attitudes toward inductive risk, and they use different standards for assessing evidence. Finally, they seem to prioritize different normative values which are used to direct their research goals. That is, differences in basic ethical commitments (to ecosystem conservation or to economic development) play a constitutive role in applied sciences since they are one basis for prioritizing and formulating practical strategies. I examine the second level of disagreement—disagreement over disciplinary standards—in greatest detail, since this is an area of intense inquiry among philosophers of science—though its implications for understanding obstacles to interdisciplinary collaboration have been overlooked.

I conclude that obstacles to interdisciplinary scientific collaboration are deeply rooted. Nonetheless, it is vital that researchers engaged in environmental problem-solving seek to identify the causes of—and potential solutions to—these obstacles in order to effectively address a variety of critically important environmental problems.

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Computational models and medical science: the honeymoon is over

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June 29. 10:30-12:30 VC 323

Computational modelling and simulation have for some time received a great deal of attention in medical research. For the last decade major funding resources in many countries have been channelled into developing a modelling and simulating research programme, and into bringing researchers with backgrounds in mathematics, computer science and engineering into the medical sciences. Computational modelling and simulating offered the great promise of achieving results that could not be achieved experimentally, and the medical research community has eagerly awaited these results. One of the most compelling promises made by this programme of research has been that of delivering new methods for personalised medicine (Hunter et al 2010). Yet the real challenges of achieving these results are now beginning to become apparent, and there is increasing scepticism that they can actually be met. As a senior figure in clinical research awaiting the fulfilment of the promise of computational modelling remarked, 'The honeymoon is over'. What underlies this increasing scepticism — at least in the area of physiology with its concomitant medical applications — is the scarcity of actual examples of validations of models against experimental data (Carusi, Burrage and Rodriguez 2012: H145).

This paper discusses two possible reasons for this scarcity, and their effects on the promise of personalised medicine. The first is that in physiological modelling, the effort has gone into constructing mathematical models capable of multi-scale integration (that is, integrating the different levels of a physiological process from the sub-cellular level up to the whole organ level). The role of simulations is often defined from the perspective of mathematics, as that of solving the equations of the models. That is, simulations are geared towards the models rather than towards experiments and the validation of the models is mathematically defined not experimentally defined. This is a matter of disciplinary practice, since if the construction of the models is carried out by researchers who identify themselves primarily as mathematicians (rather than engineers, for example) they will tend to be interested in the solution of the equations that make up the model, but they are not used to thinking in terms of hypothesis and test, which is what is required for entering an experimental paradigm. The shift from a mathematical to an experimental epistemic practice is the first issue that will be discussed in the presentation. A second related issue is that even when comparisons with experiments are carried out, the pervasive variability of biological processes makes it very difficult to interpret and compare experimental and computational results. Ironically, this is particularly true of the results of multi-scale integrated models, which are precisely the strength of computational modelling and simulation over experimental methods. For clinical researchers the variability of biological processes is the real issue. In their eyes, it does not help to develop a multi-scale integrated model which then becomes difficult to validate, and even more difficult to apply because of variability. The different attitudes towards variability will be the second point discussed.

A different view of the relationship between modelling and simulation is required if the promise of computational modelling for medical science is to be even part-way fulfilled. Drawing upon studies such as those of Humphreys (2004), Varenne (2007) and Winsberg (2010) , this presentation puts forward a suggestion for reformulating this relationship which has implications for the interdisciplinary epistemology of computational modelling and simulation in the medical sciences.

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The Patent System's Ontological Project

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June 28. 4:00-5:30 VC 211

The ontological status of intellectual property is a question of growing legal and social significance (Koepsell, 2003) and was a major focus of a recent issue of The Monist (Biron, 2010; Wilson, 2010; Wreen, 2010). Little attention to date, however, has been given to the relevant legal actors and institutions as builders and users of ontological systems. Each branch of intellectual property law in a given jurisdiction appears to have its own distinctive ontological project, complicating efforts to offer unified ontological theories of intellectual property.

This paper offers a detailed account of the U.S. patent system's project of constructing an ontology of inventions in which patent rights may be grounded. A distinctive characteristic of patent rights in the United States is that they flow from the timely filing of an adequate disclosure with the Patent Office, not from the creation of any physical or abstract object. I argue that the patent system uses the legal requirements for adequate disclosure to enforce its own criteria for incurring and warranting ontological commitments to claimed kinds of products and processes. A patent claim as an object of intellectual property therefore owes its existence not to a causal process of creation, but to the patent system's acceptance of an ontological commitment.

This account resolves a dilemma posed by Biron (2010) and thereby obviates a related ontological argument against intellectual property in the case of patent rights. It also addresses various concerns regarding the role of the type/token distinction in patent law and policy (Wilson, 2010; Wreen, 1998).

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What Are the Epistemic Contributions of Models? Separating the Heuristic and Evidential Roles of the DMP Model

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June 28. 2:00-3:30 VC 211

Theoretical models are praised in most sciences, including economics. A case in point is the story of Peter A. Diamond, Dale T. Mortensen and Christopher A. Pissarides, who were awarded the 2010 Nobel Prize in economics as a recognition of their work in developing a theoretical model of the labor market—i.e., the DMP model. Theoretical models are highly praised, but what do they actually contribute to the epistemic project of sciences like economics?

In this contribution, we characterize precisely four types of epistemic contributions of theoretical models. We illustrate this typology by drawing on opinions about how the DMP model contributes to our understanding of labor markets. Three of these contributions are heuristic; we call them enabling, revealing, and stimulating roles. The last one is an evidential role.

The evidential role is the one that is best known. Theoretical models are said by some—and clearly believed by practicing economists—to provide evidence for empirical claims. In labor economics for instance, the DMP model is believed to grant support to the presumed effectiveness of some policy reforms. In contrast, many philosophers of an empiricist bent doubt that theoretical models can play an evidential role. To the extent that these models have not gone through a serious attempt at empirically validating them, we should refrain from reading them as providing evidence for worldly hypotheses.

Even those who reject the evidential role of theoretical models recognize that these objects have some epistemic value. Models make heuristic contributions—i.e., they somehow help us in our goal of learning about the world. Though this proposition sounds plausible, we are in dire need of systematic attempts at characterizing and distinguishing among heuristic contributions. In our paper, we distinguish among an enabling role (the model results in conceptual innovations which enable us to think about our target systems differently), a revealing role (statements true of the model reveal to us hypotheses about the target systems that we might not have envisaged otherwise), and a stimulating role (the model points to relevant empirical data to gather and analyze, thus stimulating empirical research in novel directions). We illustrate these three roles with claims about what the DMP model has achieved in labor economics.

All in all, our project of characterizing more precisely the potential epistemic value of theoretical models leads us to conclude that these objects get their praiseworthy properties by being able to 'link up' with other tools or practices of the relevant scientists. This conclusion is obviously true with respect to heuristic contributions, which mean exactly that other research practices are furthered by the existence of the model. But the same conclusion can be extended to the evidential role: if theoretical models provide evidence for worldly claim, they only plausibly do so as weak evidential elements linked up to other such evidential elements in a diverse evidential set.

Epistemic Iteration: Enrichment and Self-Correction of Patient-Reported Outcome Measures

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June 28. 10:30-12:30 VC 212

How is it epistemologically justifiable to design a measure of a construct without some theoretical understanding of the laws governing the behavior of that construct, and how is it possible to develop such an understanding without the empirical data provided by a measure? This inherent circularity is a recurring theme in the philosophical literature on measurement. Complaints that measures lack a gold standard are common among quality of life researchers, just as a lack of foundation plagues metrological researchers in the physical sciences.

Questions of justification and scientific progress figure explicitly in Hasok Chang's book, Inventing Temperature: Measurement and Scientific Progress. Chang presents a strategy for negotiating the epistemic circularity inherent in developing temperature measures. His coherentist strategy for the scientific progress and the justification of measures appeals to a framework called epistemic iteration. Epistemic iteration, which comprises both enrichment and self-correction, involves refining measures through use in concrete situations.

Leah McClimans navigates similar territory in the medical and social sciences in her 2010 article exploring the theoretical underpinnings of patient-reported outcome measures (PROMs) — questionnaires designed to measure quality of life or subjective health status among respondents. She argues, first, that we cannot foresee the shortcomings of or defects in the questions in PROMs without putting them to work in concrete situations, and, second, that the questions in PROM questionnaires should be treated as open to interpretation — as genuine rather than merely apparent questions — and that even unexpected responses should be taken seriously.

I contend that Chang's work on progress and justification parallels McClimans' treatment of PROMs. Like temperature measures, PROMs are vulnerable to epistemic circularity. Both Chang and McClimans insist that we learn how our measures work and what their shortcomings are only through use in concrete situations. McClimans proposal that the questions in PROMs should be treated as genuine and open-ended allows for enrichment and self-correction just as Chang's strategy of epistemic iteration does in the physical sciences. However, unlike Chang's temperature measures, the constructs targeted by PROMs, quality of life and subjective health status, fail to converge on an idealized standard under McClimans' model. At some point, temperature measures are sufficiently refined for use in almost any circumstance. When we refine PROMs through epistemic iteration, however, our standard frays. We end up with as many standards as we have concrete situations for PROM deployment.

In this paper, I describe the problem of circularity inherent in measurement as well as Chang's method of epistemic iteration, which is characterized by self-correction and enrichment. I explore the way McClimans's proposal that the questions in PROMs be treated as genuine and their subject matter as imperfectly understood opens the door to something like Chang's epistemic iteration, allowing both a refinement of PROMs and a greater understanding of their target constructs. Finally, I discuss the limits McClimans's theoretical framework may place on the convergence of those measures to a unique, well-validated standard.

Clarifying Value Judgments in the Pragmatics of Explanations

Barry DeCoster
Michigan State University

June 29. 2:00-3:30 VC 211

In this paper, I begin with an overview of contemporary strategies of explanation within medicine & science. The focus on mechanism is central to many approaches to explanation of disease and scientific projects. This paper focuses on two approaches to explanation that, in addition to mechanisms, emphasize contextuality as part of explanatory structure. I begin by looking at Paul Thagard's work (How Scientists Explain Disease). I articulate how his middle-range explanatory theories require articulation of a range of value judgments, which are dependent upon the goals of the explanatory agents. Relatedly, I develop Bas van Fraassen's work (The Scientific Image) on levels of explanation within the domain of scientific explanations, more generally. Both explanatory models are deeply sensitive to contexts of explanatory agents and their goals.

While both of these theories assume explanatory success is connected to contextual requirements, neither author articulates successful accounts for how to decide between competing explanatory projects. As such, both explanatory approaches identify that contextuality is important to explanatory projects, but neither provides sufficient grounds for identifying proper contexts or the related value judgments. In order to clarify these explanatory projects, I incorporate Sandra Harding's work on "weak" and "strong" objectivity. Both explanatory strategies are presented as value-free ("weakly" objective). But to be productive, Thagard's medical explanations must be structured to take into account value judgments of both physicians and patients. Similarly, van Fraassen's scientific explanations must also take into account a range of value judgments. This move towards "strong" objectivity—the continual work to identify and re-evaluate value judgments of evidence and explanatory agents—works to show the complicated nature of medical and scientific explanations. As such, successful explanations must meet the needs of a range of interdisciplinary concerns.

Scientific Understanding in Medicine

Leen De Vreese
Ghent University

June 27. 4:00-5:30 VC 212

Recently, the topic of scientific understanding got some renewed attention in philosophy of science. However, to my knowledge, no body of literature exists on the more specific topic of scientific understanding in the medical sciences. The aim of this paper is to make a start on clarifying the necessity, the characteristics and the peculiarities of scientific understanding in the case of medicine.

The main goal of medical science can be summarized as follows: gathering knowledge that enables us to interpret, prevent and treat health problems. In other words, the goal is to scientifically understand disease states such that scientifically based interventions become possible. The medical community often equates achieving scientific understanding with gaining knowledge of reductionist causal explanations of diseases (i.e. descriptions of the pathophysiological mechanisms that lead to diseases). However, it is also clear that for a lot of "diseases", medical science does not (yet) have such comprehensive, reductionist, causal explanations. Partial understanding of a disease can nonetheless already be sufficient and practically useful in order for medicine to be able to intervene. This finding leads to questions about

(a) the necessity of scientific understanding in medicine: Is making progress in medicine a matter of aiming for "full" scientific understanding? How can "full scientific understanding" in medicine be defined? Is "full scientific understanding" really the central goal of medicine, or rather an ideal, an illusion or even a useless aim? And what is the role of "partial understanding?"

In my talk, I will further focus on questions concerning (b) the characteristics and (c) the peculiarities of scientific understanding in medicine, and try to formulate answers to the following questions:

(b) the characteristics of scientific understanding in medicine: How is scientific understanding usually achieved in medicine? Can we discern different kinds of scientific understanding in medicine, related to the use of different tools to achieve understanding (e.g. what is the role of theories, taxonomies, models, statistics, etc.)? Are different kinds of understanding dependent on different epistemic interests in different kinds of contexts?

(c) the peculiarities of scientific understanding in medicine: Does scientific understanding in medicine differ in important ways from scientific understanding in other scientific disciplines? If so, in what respects?

To answer these questions, I will start from the literature in philosophy of science on scientific understanding, as well as from theoretical writings from the medical profession, which demonstrate their own stance towards scientific understanding in medicine (e.g. articles written by medical scientists on causation and explanation, and on knowledge, uncertainty and ignorance in medicine). The insights from both kinds of sources leads to a general framework about scientific understanding in medicine, that can further be used to analyze case-studies.

When materiality does and does not matter

Jo Donaghy
Egenis University of Exeter

June 28. 10:30-12:30 VC 206

The current philosophical debate about epistemological issues raised by computer experiments versus laboratory experiments has focused on the significance of differences in materiality. Central issues have included, 1) in what sense is there a difference in materiality, and, 2) whether this relates to differences in the epistemic power of results from the two forms of experiment. The second issue has involved addressing whether the ontological similarity of the target system to the thing or process of interest affects the epistemic status of the results.

In this paper I will argue that the epistemic status of experimental results emerges from the interaction between the context of their production and the context of their use. I will do this by examining differences in the use of mathematical models of as tools for calculation, and tools for measurement. As tools for calculation, results from laboratory experiments are analysed using a mathematical model to produce a numerical interpretation. As tools for measurement, often results from laboratory experiments are used to provide some of the parameters of the mathematical model, experiments are then carried out with the model producing numerical measurements, or results. The significance, for the epistemic status of the results from the laboratory experiments, of the ontological similarity of the target system to the thing or process of interest is different in each case. In the first case, the laboratory experimental intervention is carried out when the system is in a particular state, the similarity of this state to the thing or process being researched is highly significant for the epistemic power of the results. In the second case, additional parameters and assumptions are used to construct the mathematical model in a particular state which deviates from that in which the laboratory experiments, which are supplying some of the model parameters, were carried out. Experimental interventions are then carried out when the mathematical model is in that particular state. The similarity of the laboratory experiments to the thing or process being researched is not as significant for the epistemic status of results from these hybrid computer experiments.

I will support my argument with an analysis of two mathematical models of metabolism developed in the early 1970's which involve the introduction of systems approaches. Metabolic Control Analysis (MCA) is a tool for calculating, or interpreting, the results from laboratory experiments carried out in intact systems, or components in in-vivo like conditions. Biochemical Systems Theory (BST) is a tool for simulating, and taking measurements about, the behaviour of intact systems, using results from laboratory experiments on isolated components to construct the model. In MCA the state of the system during the laboratory experiment is crucial. In BST the results from laboratory experiments are manipulated during their integration in to the model, and further assumptions and parameters are used to construct the insilico system in a particular state for experimentation. Developing this kind of perspective on the contextual basis of the epistemic status of experimental measurements is important for addressing issues arising from the highly mobile data context of current systems biology.

One Way to Use an Ecosystem Theory for Decision Making in Environmental Policy

Justin Donhauser
University at Buffalo

June 29. 2:00-3:30 VC 212

As we confront decisions about how best to cope with mounting large-scale environmental issues, it is increasingly obvious that we need to understand theory in ecosystems ecology better. We cannot ignore the need for predictively powerful theories that attend to what is going on at the spatiotemporal scales salient to making decisions about sustainable development and resource management. Yet, to date nobody has met the challenge of prominent figures in environmental policy (e.g. Mark Sagoff) and ecology (e.g. R. H. Peters and Kristin Shrader-Frechette & Earle McCoy) to show exactly how theoretical ecological models, and in particular models from general ecosystems theory, are useable for purposes of policy and management "on the ground".

The main cluster of critiques are either vague as to their intended targets, take issue with different sorts of ecological theories in different places, or focus specifically on questioning the legitimacy of particular sorts of ecological models. In consequence, though the critiques all hinge on the fact that theoretical models lack determinate referents, respondents interpret the critiques as critiques of different sorts of theories. Some respondents (e.g. Jay Odenbaugh and Stefan Linquist in some places) interpret the salient critiques as being critiques of theory in community ecology, others interpret them as being critiques of general ecosystems theory (e.g. Odenbaugh in other places), and others argue that the salient critiques apply to all theory in ecology because they are, at base, critiques of theory and theoretical modelling in general (e.g. Gregory Cooper and Sagoff).

In this talk, I suggest that the latter position appears to be correct while at the same time contending that it is most important to defend ecosystems theory in particular both for urgent practical reasons and because it is philosophically interesting with respect to what it may teach us about the epistemology of naturalistic theorizing and modelling in general. Regarding the former, the policy and resource management literature is pregnant with concepts from theoretical ecology, and making sense of ecosystems theory can only help to render the hundreds of directives that call for augmenting and sustaining properties of types of ecosystems effective. Regarding the latter, because ecosystem models are so general (i.e. about undefined thermodynamic aggregates), their potential target phenomena are by fiat on the extreme end of being unspecified. Thus, demonstrating how such models can be used to generate empirical claims provides insights into the nature and cognitive roles of theoretical models in scientific applications.

I progress by first characterizing the generative process of a novel metapopulation model and its use for guiding normative policies and land management efforts. I then make plain the core tenets of a particular general ecosystem model (the exergy theory), and explain one way that that theoretical model can be used to augment metapopulation models to generate causal explanations and predictions about species distributions. I close by offering a characterization of some epistemic functions that the models appear to play in the process of generating empirical claims in the applications considered.

Non-Epistemic Values and the Multiple Goals of Science

Kevin Elliott & Daniel McKaughan
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June 28. 2:00-3:30 VC 212

Recent efforts to argue that non-epistemic values have a legitimate role to play in scientific reasoning typically either reject the distinction between epistemic and non-epistemic values entirely, or they incorporate non-epistemic values only as a secondary “tie-breaker” for resolving epistemic uncertainty. This paper argues that even if non-epistemic values can be distinguished from epistemic values, they can sometimes play a legitimate role as factors that override epistemic considerations in scientific reasoning. Drawing upon work by Ron Giere (2004) and Bas van Fraassen (2008) on the nature of representation, we show that scientific representations can legitimately be evaluated not only based on their fit with the world but also with respect to how well they meet the needs of their users. Using examples from chemical risk assessment, wetland mitigation banking, and stream restoration, we argue that because non-epistemic values are directly relevant to assessing how well representations achieve the practical purposes for which they are deployed, non-epistemic values need not always be subordinated to epistemic values in scientific reasoning.

One might initially object to this approach by arguing that it violates the epistemic standards of science. However, we argue that it is not problematic to give priority to non-epistemic values when choosing a representation as long as one adopts an appropriate cognitive attitude toward it. For example, one could entertain a model, or one could hypothesize it, or one could accept it as worthy of further pursuit, or one could accept it as a basis for policy making. While it would be problematic to give non-epistemic values priority when believing a representation to be true, there are other cognitive attitudes such that it is not problematic to give non-epistemic values priority when adopting them.

We illustrate how non-epistemic values can legitimately override epistemic values in some cases using examples from chemical risk assessment, wetland mitigation banking, and stream restoration. For example, one of the crucial goals involved in choosing a risk assessment methodology is to minimize the social costs associated with making mistakes (i.e., either overregulating or underregulating potentially toxic chemicals). Carl Cranor (1995) has argued that the social costs of relying on current risk assessment procedures (which are fairly accurate but very slow) are generally greater than they would be if regulatory agencies relied on less accurate but much quicker methodologies for assessing risks. Thus, his analysis shows that when scientists are accepting a methodological approach for the purposes of guiding regulatory policy in ways that minimize social costs, they sometimes have to sacrifice epistemic concerns such as accuracy for the sake of non-epistemic values such as the ability to generate rapid conclusions. The wetlands banking and stream restoration cases provide similar illustrations of how non-epistemic values such as speed and ease of use can legitimately take priority over epistemic values such as accuracy when choosing models and methodologies.

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The Discovery of enzymatic RNAs: a Mechanist Approach

Jin-Yeong Ghim
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June 29. 2:00-3:30 VC 323

RNAs, including mRNA, tRNA, and rRNA, are single-stranded sequences of nucleotides involved in the transmission of genetic information from DNA to proteins. In protein synthesis, many chemical reactions cannot occur without biocatalysts called enzymes. Until the early 1980s, most biologists had believed that “all enzymes are proteins”. Against this common idea, however, Thomas Cech discovered and proved a newly enzymatic function of a segment of precusor ribosomal RNA (pre-rRNA) in Tetrahymena thermophila (Kruger et al. 1982; Zaug and Cech 1986). The discovery of RNA's function as a biocatalyst provokes additional research of other RNAs such as small RNA, short interfering RNA, and micro RNA, as regulators in gene expression. Furthermore, it provides a significant clue as to what was the original system between DNA and protein in the primitive soup. This question so called “chicken-and-egg problem” in evolutionary biology could be resolved by Cech’s discovery, answered that the original replicator was not both DNA and protein but RNA. Today, all biologists agree that RNAs can act as not only biocatalysts in chemical reactions but also transmitters of genetic information.

When Cech won the 1989 Nobel Prize in chemistry, Sylvia Culp and Philip Kitcher characterized his practice on Kuhnian stance in which scientific practices are problem-solving activities. (Culp and Kitcher 1989) According to them, Cech tried to solve a research problem that is what kinds of enzymes mediate splicing reactions in T. thermophila. Despite all his efforts to find splicing proteins, he observed splicing reactions of pre-rRNA by itself without proteins. The possibility of a catalytic property of pre-rRNA triggered to change the referent of enzyme, methodological directives, and experimental techniques from previous normal science into new one. Particularly, Cech’s observation led to abandon a widely-accepted methodological theory “all enzymes are proteins”, and to accept a new theory “many enzymes are proteins, but at least a few enzymes are RNAs”. Even if, however, Culp and Kitcher’s approach is very curious, unfortunately their approach gives rise to misunderstandings about Cech’s practice. In this paper, I point out their mistakes and discuss an alternative approach by focusing on following two issues.

The first issue is related to a historical question as to whether the initial purpose of Cech’s laboratory was really the discovery of an enzymatic function of pre-rRNA. As mentioned above, Culp and Kitcher reconstructed Cech’s practice as activities for finding enzymatic entities. However, Cech never endeavored to only acquire something that splices intervening sequences (so called intron or IVS) of rRNA. By showing historical evidence on the basis of many articles published by Cech and his colleagues from 1979 to 1989, I argue that his initial research purpose was the understanding of a splicing mechanism in nucleus, and that a catalytic property of pre-rRNA was derivatively discovered in the course of building models of the splicing mechanism.

The second issue is a philosophical question that is how a theory “RNAs can function as enzymes” could be accepted by biologists. Culp and Kitcher claim that the new theory was chosen in biochemistry because the enzymatic function of pre-rRNA in T. thermophila was disclosed by widespread techniques of recombinant DNA and because the segment of pre-rRNA satisfies three kinds of conditions of biocatalysts. However, most biologists don’t concede all results from widely-used techniques unquestioningly. It’s evident that catalytic properties of RNAs could not be accepted only on the basis of confidence in recombinant DNA technology. Besides, Cech just identified the catalytic property only in a portion of rRNA of T. thermophila, not all RNAs of all domains. If taking a mechanist approach, we sufficiently figure out the reason why the new theory could be acceptable. The enzymatic properties of RNAs could be accepted through two steps. It was the first step in identifying key structural and functional components of a mechanism in a particular entity of a specific species. Cech identified the complete 413 sequences of the IVS of pre-rRNA and the chemical formation of its phosphodiester bond in T. thermophila. And then, he showed the components of the splicing mechanism are very robust or stable. In the second step, the core structural and functional components of a mechanism should be generalized in most living organisms. Cech’s discovery of an enzymatic property of pre-rRNA could be generalized by showing that both homology of the core structures among RNAs and chemical similarity of the core reactions across domains. I argue that causal robustness and generality are essential for a new theory to be accepted in biochemistry.

Consequently, I conclude that the mechanist approach is more relevant to Cech's practice than Kuhnian approach.

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Major and Minor Groups in Evolution

Peter Gildenhuys
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June 29. 2:00-3:30 VC 323

Kerr and Godfrey-Smith (KGS) argue that two mathematically equivalent, alternative formal representations drawn from population genetics, the contextualist and collectivist formalisms, may be equally good for modeling the dynamics of some natural systems, despite important differences between the formalisms (Kerr and Godfrey-Smith 2002; 2012). I draw on constraints on causal representation from Woodward (2003) to argue for a general criterion for choosing between the contextualist and collectivist formalisms for arbitrary systems in which groups are formed. Groups whose contributions to future generations are caused by their genetic variations via the phenotypes of group members I call minor groups; groups whose contributions are caused by genes, but not via group member phenotypes, I call major groups. (The contrast is specified in a technically careful manner in presentation.) Minor groups should be modeled using the contextualist formalism while major groups should be modeled using the collectivist one. I discuss an instance of each sort of group, considering classic cases: the infamous D. dendriticum (brainworm) parasite forms minor groups while infesting ants, while organisms form major groups of sickle-cell gametes and alleles.

To argue for the connections between the contextualist/collectivist contrast and the major/minor contrast, I use a test I dub the intervention test, a test similar to, but in important ways different from, KGS's near-variant test (Godfrey-Smith and Kerr 2012). The test shows that the collectivist formalism fails to represent the causal structure of minor groups while the contextualist one fails to exhibit the causal structure of major groups, but not vice-versa.

While I am interested in cementing the connection between group types and formalisms sketched above, the major/minor group distinction has further explanatory potential for practical issues in evolutionary modeling and the study of evolutionary history. The distinction can help approach the issue of what is a lifecycle phase in classical population genetics models. The distinction might also prove useful for distinguishing groupings among non-standard systems. For instance, Dictostylium discoideum, the slime mold, forms aggregates at a critical point in its lifecycle. If we find out that these aggregates form major groups, then we know that they must be formally represented, with their own frequency terms and associated fitness variables, in mathematical models that capture causal structure. Moreover, at least some hymenoptera colonies can be treated as major groups of female/male organism pairings. Finally, the evolution of major groups may prove important to the study of evolutionary history because it represents the shedding of an important engineering limitation upon group-level adaptations. In systems in which major (but not minor) groups are formed, genes impact group development, and group adaptedness, directly and need not have positive (or at least non-disastrous) effects on the development of particle phenotypes to positively affect collective performance.

The Trustworthiness of Scientific Institutions: The Implications of Situated Knowing

Heidi Grasswick
Middlebury College

June 28. 4:00-5:30 VC 212

Though many social epistemologists have attended to the epistemological underpinnings of trust in the testimony of individuals, this paper investigates trust in the testimony of scientific institutions, considering the requirements of a 'responsible trust', where the trust in the institution matches the trustworthiness of the particular scientific institution or practice. The feminist thesis of socially-situated knowledge complicates such an analysis because it suggests that the trustworthiness of scientific institutions may not be the same from all vantage points. In particular, Naomi Scheman argues that a history of poor relations between a marginalized group and the institutions of science, including ethical research abuses and unjust practices, give such a group good reason to distrust scientific institutions epistemically (Scheman 2001), showing the failure of the institution's trustworthiness.

Adapting a situated approach to knowing, this paper investigates several conditions for the trustworthiness of scientific institutions, using as a key example the genetic research done on the Havasupai Indians by researchers at the University of Arizona in the 1990s. The Havasupai suffer high rates of diabetes, and when approached by researchers, they agreed to give blood samples and participate in genetic research investigating links to diabetes. Conflict ensued when the blood samples were later used for additional research projects, including ancestry studies and research on genetic links to mental illness, neither for which the Havasupai believed they had given consent.

The Havasupai case demonstrates the complex and fragile nature of relations between institutions of science of science and marginalized lay communities. Grasswick (2010) has argued that the trustworthiness of scientific institutions is based not just on a history of the production of reliable knowledge, but rather on the satisfaction of a broad range of expectations from lay persons, including the ability to set priorities for knowledge production and offer evidence that questions of significance for that particular group are being taken up. This case offers an excellent example of a research group who did take up a question of significance for a marginalized group and whose research was done well, but it also shows how fragile such trust can be and how it can fail when other research questions pursued are perceived as potentially harmful to the group and when principles of consent are considered to have been violated. The paper also investigates the ways in which trust and distrust can 'travel' and thus why cases such as this are so serious. That is to say, while this research might have successfully built trusting relations that would result in trust carrying over to other areas of science not directly related to issues especially significant to the group, the distrust created also travels into different research contexts, eroding the trust across a wide range of scientific projects and amongst other groups who feel an affinity with the particular community involved. For example, in the aftermath of the Havasupai case, the American Journal of Medical Genetics reported, that the "distrust of genetics research is at an all-time high among most Native American tribes" (2010). Because of such serious repercussions, it is crucial that scientific communities seek to establish their trustworthiness across a broad range of localized lay communities if they are to maintain their claims to epistemic authority. To achieve this, active engagement, cooperation, and communication are required between the expert communities of scientific institutions and variously situated lay communities.

An Argument for Local Critique in Philosophy of Economics: The Case of Rational Choice Theory

Catherine Herfeld
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June 29. 10:30-12:30 VC 206

One of the frequent and also persistent critiques of philosophy of science is that appraisal is fruitless for the improvement of science if it is used to assess a theoretical framework in isolation. Rather, theory appraisal should be conducted by taking into account the actual scientific practices and/or the context within which a specific framework becomes applied. Implementing this request, however, is not a facile undertaking. One danger is that if philosophy takes scientific practices and context too seriously, then philosophical analysis lapses into a merely descriptive enterprise with the primary focus on understanding what scientists actually do. In this case, philosophy would give up its normative function. Another difficulty is that multiple meanings have been attached to the notions of 'contextualization' and 'practice'. In order to make them useful for appraisal, they require precise specification.

The question that I address in this paper is how philosophers can fruitfully appraise theoretical frameworks while taking scientific practices seriously. Limiting the scope of my argument to philosophy of economics, I suggest that philosophers should apply what I call 'local critique' of a practice-based theoretical framework. By looking at the example of rational choice theory, I will show what local critique explicitly entails as well as which scientific practices should be taken into account. I will argue that the application of local critique allows for a better understanding of the framework under appraisal, while at the same time preserving the normative function of philosophy. This is advantageous not only for philosophers in that local critique can help to shed further light on philosophical concepts inspired by the social sciences and to inform philosophical theories. Its application can also be fruitful for economists as it enables reflection that might help them to improve their respective theorizing practices. Against this backdrop, I conclude by making a case for pursuing case study research in philosophy of economics.

Biology as technology? The ontological perplexity of synthetic biology products

Sune Holm
University of Copenhagen

June 27. 10:30-12:30 VC 206

Scientific research and rapid advances in technology is accelerating our ability to manipulate biological systems. The core aim of the emerging field of synthetic biology is to enable the design of living systems with new functions that do not exist in nature and the redesign of already existing functions. One of the goals driving much research in synthetic biologist is to turn biology into an engineering discipline by applying engineering principles to biological systems and to do biology in a standardized, systematic way guided by rational design principles. Engineering biology means introducing abstraction levels, separation of production and design, knowledge-based design of a particular function, and in this way synthetic biology represents a significant development compared to genetic engineering understood as traditional trial-and-error recombinant DNA work. Still, the fundamental insights from molecular biology and advances in a range of other disciplines such as computer science, mathematical modelling, electronic engineering, are an essential part of current developments in synthetic biology.

While there has already been a lot of attention directed at ethical and societal issues that might arise in connection with synthetic biology research, little investigation has so far been focusing on the status of the products that synthetic biologists announce that they will construct. A recent book on synthetic biology and its promises and perils proclaims that biology is technology: Organisms and their constituent parts are engineerable components of larger systems, and the possible products of synthetic biology are commonly described as living machines. While these locutions are extremely effective when it comes to proclaiming and communicating the engineering aspirations of synthetic biology, they are also philosophically perplexing. In this paper I explore the ontological nature of synthetic biology products. The question concerns how to conceive of synthetic biology products and what to make of their status as technology or machines. In particular I examine the notion of a biological artifact in relation to theories of function in biology and technology.

The paper consists of three parts. In the first part I review the central features of the epistemic and ontological dimensions of the engineering approach to biology, and I characterise synthetic biology products as Paley organisms - living systems originating in intelligent design. In the second part of the paper I discuss whether Paley organisms fit into the domain of paradigmatic technical artifacts such as watches and airplanes. In particular I investigate how to account for the biological functions of Paley organisms, and whether and, if so, how Paley organisms can be ascribed technical functions reviewing some of the main theories of biological and technical function. In particular, I examine the role of intentional design for acquisition of technical functions, and I argue that the emphasis on designers' intentions in accounts of functions in artifacts is problematic. In the final section I relate the discussion of synthetic biology products to the case of bred and cultivated animals and plants.

Methodological Preservationism

Robert Hudson
University of Saskatchewan

June 27. 10:30-12:30 VC 211

Preservationism is now the orthodox response to the sort of problem that the pessimistic induction raises for scientific realism. It is often claimed that the preserved elements in a series of empirically successful theories are the true candidates for a realist interpretation, and that the pessimistic induction has no force regarding them.

In this paper, I do three things. To begin with, I argue that the preservationist strategy has a strong affinity to a rule of reasoning often ascribed to experimental scientists, called robustness reasoning. A successful analogy along these lines, however, has the unfortunate consequence of exposing preservationism to the same set of problems that afflict robustness reasoning. Notably, one can argue that the preserved elements that carry over from one (empirically successful, but defeated) theory to a subsequent (empirically successful, but not yet defeated) theory have their source in a lack of independence between the two theories, due perhaps to their joint adoption of a unique cultural perspective, their joint source in a unique feature of human cognition, or their joint expression in a presentist historical interpretation (these sorts of criticism of preservationism have been put forward by Kyle Stanford and Hasok Chang).

My next task is to consider what promise preservationism holds for on-going scientific research. Here I recount some recent scientific, historical episodes with the goal of illustrating how little is preserved in theoretical terms when a scientific advance is made. Fundamental changes in scientific ontology, I suggest, are common and thought to be progressive, despite the fact that they flout the sort of epistemic virtue heralded by preservationists who look for ontological continuity and eschew fundamental ontological rifts. Correlatively, in their future research, it would be practically nonsensical for scientists to always cleave to the preservation of past theoretical ontologies just for the sake of interpreting these ontologies realistically. The question should always arise whether an ontology is accurate, whether or not this ontology is preserved in subsequent theorizing.

Finally, having criticized preservationism in its traditional form (call this ‘theoretical preservationism’), I formulate and defend a different form of preservationism, called ‘methodological preservationism’. Following work by Gerald Doppelt, I note that the methodologies employed by scientists have a better track record of being preserved than the ontologies of successive scientific theories. This insight suggests that whereas ontological preservation may not hold (assuming it is even desirable), methodological preservation is a genuine possibility. To this end, I set forth a general taxonomy of preserved observational methods one finds in science, starting from primordial, naked-eye observation and leading to various fairly uncontroversial, preserved extensions of naked-eye observation that are either reason-based enhancements (e.g., targeted testing and calibration) or technological modifications (e.g., telescopes, microscopes). The point of this taxonomy is to note that the objects of a realistic interpretation of science are those objects revealed in a series of preserved observational methods. My main point of setting forth this form of realism is to show how it contains the resources to effectively block the pessimistic induction.

Longino's criteria for objective communities and pharmaceutical research: The case of SSRIs

Saana Jukola
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June 28. 4:00-5:30 VC 212

In her books Science as Social Knowledge (1990) and The Fate of Knowledge (2002) Helen Longino aims at constructing a philosophical theory of science that is sensitive to knowledge gained by sociological and historical studies of science. In the paper I discuss Longino's criteria for objective communities and argue that the criteria do not succeed in offering a tool that can be utilized in the context of current science — contra what Longino states. I do this by discussing a case of biomedicine: this case highlights the epistemically alarming features of commercialized science while escaping Longino's criteria.

According to Longino, objectivity of science, which does not mean value-neutrality, is dependent on the possibility of intersubjective criticism and questioning the background assumptions steering research: it can be secured by subjecting evidence, reasoning and results to the critical scrutiny of the pluralist community. A central part of her theory is the criteria for objective communities, the purpose of which is to define which features a community should have in order to produce reliable knowledge. The criteria are:

> 1) In order to be named as objective, the community must have publicly recognized forums where evidence, methods, reasoning and assumptions can be criticized.
>
> 2) Beliefs and theories of the community must change in response to criticism.
>
> 3) Members of the community must have some shared and publicly recognized standards, by reference to which hypotheses, observational practices and theories can be evaluated.
>
> 4) Those communities should not be qualified, where irrelevant factors, such as political, social or economic power of individuals or groups, have an effect on which assumptions are accepted.

According to Longino, these criteria are "features of an idealized epistemic community" (Longino 2002, 134), and when they are followed, the influence of biasing factors on accepted views can be kept in check, even if individuals were behaving in a way that would not be qualified as objective by traditional standards.

I argue that the criteria do not guarantee adequate conditions for critical interactions. The criteria do not forbid individual conflicts of interests. I argue that this is a problem by invoking the case of research on Selective serotonin reuptake inhibitors (SSRIs). I use the case to demonstrate how the criteria are lacking in recognizing how the biases caused by individual conflicts of interests cannot be disposed by critical discussion in the context of one-sided research funding.

I suggest that Longino's criteria should be supplemented with a policy for regulating the ties individual researchers have. Also the political context of research, including the decisions affecting research funding, ought to be considered when the conditions for the objectivity of research are scrutinized. The discussed case indicates the significance that funding has for promoting the diversity of opinions in research communities. Accordingly I argue that research communities are not self-sufficient in tending the conditions for objective research.

The target of climate model assessment

Joel Katzav
Eindhoven University of Technology

June 29. 2:00-3:30 VC 212

My question here is what the target claims of useful climate model assessments might be, where climate model assessment is understood to be assessment of one or more climate model versions. A climate model version is a climate model in which equation parameter values and equation initial conditions are specified. Assessing a climate model (version) involves testing some of its simulation results against empirical data as well as assessing its assumptions with the help of background theory.

I look at three answers to the paper's question. The first answer, which I will call the standard view, tells us that useful climate model assessments are primarily of the truth or, at least, approximate truth of climate models, though it allows assessment of other more specific targets of confirmation, such as individual model predictions, to be of importance in special cases. Lloyd (2009 and 2010), I will argue, is committed to a version of the standard view. The second answer, which is based on Parker's (2009) view and which is called the adequacy-for-purpose view, is the view that useful climate model assessments are primarily of climate model adequacy for specific representational purposes. The third answer, which I will call the conservative view, tells us that useful climate model assessments primarily aim at confirming all those uncertain assumptions and results of assessed models that have not been epistemically undermined prior to assessment.

I make progress in the discussion of each of the above three views. I show that the current case against the standard view is incomplete and further develop this case. We will see that the standard view might still turn out to be viable. We will also see that it will be viable only if we can provide a notion of approximate truth that is sufficiently weak so as to allow climate models to be approximately true despite their substantial limitations and that, nevertheless, provides real guidance as to which assumptions and implications of an approximately true model we can trust by virtue of its being approximately true. With respect to adequacy-for-purpose assessments, I argue that they should be characterised by their being epistemically demanding in a certain way rather than, as proposed by Alexandrova (2010), by how specific and local the purposes involved are. I also describe a direction in which assessing the adequacy-for-purpose view should proceed. I argue that this view needs to address the worry that, in typical cases, it forces us to rely on climate models in order to assess climate model limitations and thus threatens, in such cases, to make establishing adequacy-for-purpose impossible. The outline of the adequacy-for-purpose view will help me to introduce a novel view, namely the conservative view. Here, again, I will point out the direction in which critical discussion should proceed. We will see that the conservative view must address the worry that it allows confirmation on the basis of evidence that may not be reliable. In the concluding discussion, I point out that the worries that the adequacy-for-purpose and conservative views face can be bypassed by views according to which assessment of climate models aims at confirming epistemic attitudes that are weaker than belief/acceptance.

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Historical narratives and narrativized actions. On the role of narrative in the reconstruction of scientific practices

Katherina Kinzel
University of Vienna

June 27. 2:00-3:30 VC 206

My talk addresses the question of how historical narratives represent scientific practices. I explore the conflict between constructivist accounts of historiography that take historical narratives to impose an external and often ideological meaning on the events they report, and more recent approaches that highlight continuities between the structure of scientific practices and the structure of narrative. I argue that the latter approaches, quite contrary to their own self-conception, do not contradict, but rather strengthen central constructivist points.

My talk has three parts. In the first part, I present Hayden White's conception of historiography and its implications for the historiography of science. White argues that the intelligibility of historical discourse depends on the embedding of historical events in a narrative structure. He claims that (a) the narrative structure and therefore the meaning that historical reconstructions bestow upon historical events is external to the events themselves, (b) multiple incompatible, but equally epistemically acceptable narratives of the same historical events are possible, and (c) choices between different narratives are primarily motivated by the moral and ideological conclusions these narratives allow. These insights were subsequently used for revealing the ideological and rhetorical character of reconstructions in the historiography of science (William Clark, Rivka Feldhay).

In the second part of my talk, I explore more recent accounts of narrative that oppose White's radical constructivism. These accounts highlight the continuity or structural similarity between the narrative mode of representation and the processual dynamics of scientific practices themselves. For example, Joseph Rouse argues that (historical) narrative should not be thought of as imposed on an unnarrativized series of events. On the contrary, actions already belong to a narrative field, and narrative reconstruction is an active component of dynamical scientific practice. This idea finds a stronghold in accounts that conceive of narrative as the natural expression of temporal experience (Paul Ricoeur), contingency (Marie-Laure Ryan) and the structure of intentional human actions (David Carr). Such accounts suggest that the continuity between the temporal structures of scientific practices on the one hand and the narrative structures of their representation on the other imposes certain constraints on possible narrative emplotments of scientific episodes. The denial of White's external meaning thesis (a) is thus taken to place limits on the multiplicity (b) and maybe even on the ideological dimensions of historical narrative (c).

In the third part of my talk, I argue that this line of reasoning is mistaken. Rouse himself makes clear that the narrativization of scientific practice proceeds in a field of conflicting interpretations which are related to the evolving goals and prospects of scientific research. Rather than imposing constraints on the multiplicity of narrative representations of scientific practice, such an account merely shows that the multiplicity of narrative emplotments emerges already before the historian starts to reconstruct past scientific practices. And rather than denying the ideological character of historical narratives, it helps to explain how narratives are systematically tied to the evolving goals and interests of the different actors that participate in the narrative (re)construction of scientific practice.

Models and Modelling in Learning Science: Mediating Between Distributed and Personal Cognition

Ismo T. Koponen
University of Helsinki

June 28. 4:00-5:30 VC 206

The philosophical accounts of models and modelling as they appear in scientific practices have found their place and applications in science education as a model-based view (MBV) of science education. Much of the inspiration of the model-based view derives from the notion that models are central knowledge structures in science and vehicles for developing, representing and communicating ideas. All the different views within MBV are more or less related to the Semantic View of Theories (SVT), which describe the theory as a cluster or collection of models which have close "family" resemblance to each other Giere (1988, 1999). Such model-based-views rooted in SVT have enjoyed lot of attention in contemporary science education literature.

In closer look, it is somewhat unexpected that the views within SVT have gained so much attention, because they mainly address the models from viewpoint of research, doing science and from the viewpoint of science community — from a viewpoint of distributed cognition. Learning, however, is to large degree a matter of individual or personal cognition, which affects the development and possession of learners' own concept and conceptual change. Curiously, the contemporary model-based-view for science education says little if anything how models and modelling work in that area of personal cognition, how models mediate between the learners' personal conceptual worlds and experiential world, and how distributed and personal cognition may affect each other.

Within the science education, this distinction of distributed and personal cognition, and on the other hand, when distinction is noted and acknowledged, also interplay, has not been clearly discussed or analysed since the seminal works by Nersessian (1995, 2008) and Gooding (1990, 1993), who both emphasised the cognitive aspect of using models for conceptual learning, concept formation and conceptual change.

In this presentation it is discussed how the distributed and personal cognition are related in a learning process. The viewpoint combines the model-based-views on science rooted in SVT and views of cognitive science and psychology of concepts, concept learning and conceptual change (Machery 2009). The core component of this combined viewpoint is to construct understanding how a learner uses models as vehicles of personal cognition, through which concepts are projected on real phenomena, and on the other hand, through which the conceptual development and change takes place, and how individual learner's knowledge of models as artefacts of distributed cognition affects the use and construction of personal models. It is argued that such a balanced view which mediates between distributed and personal cognition can give much deeper understanding of the processes of learning than either one of them alone.

Seeking consensus, and how to account for dissent in the meantime

Laszlo Kosolosky; Jeroen Van Bouwel
Ghent University

June 28. 2:00-3:30 VC 206

Different types of organizations, e.g. National Institute of Health (NIH), Intergovernmental Panel on Climate Change (IPCC), Canadian Association of Gastroenterology (CAG), aspire to establish scientific consensus in different ways, through different processes. By drawing on these examples from scientific practice, this paper scrutinizes the actual practice of consensus-making with two goals: (a) develop clear criteria/concepts/tools to compare different ways of consensus-making, and (b) apply this 'toolbox' to reevaluate existing claims on consensus versus dissent in science.

First, we introduce a rudimentary continuum to deal with consensus-seeking organizations, arguing that the continuum ranges from consensus conferences to systematic review. The ground for comparison are the structural characteristics of the procedures and the functions they serve. As understood today, one deciding factor for the place of consensus-seeking organizations on the continuum is the extent to which they appeal to, what we will call, deliberative interaction, consisting of inter- and intralevel deliberation among different types/layers of participants, and deliberation after direct confrontation.

Second, we use these insights to shape further philosophical discussion on the aim of aspiring consensus versus the need for uptake of dissent. On the one hand, when push comes to shove, establishing a scientific consensus is imperative to solve controversies, such as global warming. Establishing a consensus on the causes and the extent of global warming could facilitate policymaking and, moreover, send a convincing signal that doing nothing will have dire consequences. On the other hand, studies carrying attention for plurality and heterodoxy have raised questions concerning the ideal of the scientific consensus, and, connected to it, the neglect of dissent (Longino, 2002; Solomon, 2006; Van Bouwel, 2009). In solving this tension between plurality and consensus, which is not always made explicit in knowledge-based accounts of consensus (Gilbert, 1987; Miller, 2013), we point at the meta-consensus or meta-agreement in play. Thus, instead of focusing on consensus on the simple level (that is, as the result of alternative theories/models tested against one another eventually leading to some consensus outcome) we shift to analyzing the meta-consensus that stipulates the procedure to be followed in consensus-making.

A meta-consensus can guarantee, on the one hand, that divergent opinions are heard (without having to endorse a group consensus) and that consensus (in the absolute sense of the term) is no longer regarded as an end in itself. On the other hand, this approach allows us to maximize consensus (understood here in a relative sense) by going through the established procedure and afterwards portraying the present consensus through known democratic methods (such as majority rule, voting, aggregation and negotiation). The underlying account of consensus will thus be a social-procedural one (not stipulating the characteristics the outcome should have, but stipulating the social procedure that has to be followed).

The two parts taken together imply that consensus comes in degrees, depending on the extent to which a procedure has been followed, repeated, etc. Moreover, combined they enable us to reinvestigate current claims on consensus-making in consensus conferences as not bringing about rational consensus (Solomon, 2007 & 2011).

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The gap between epistemic and institutional practices in multidisciplinary research

Rebecca Kukla & Bryce Huebner
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June 29. 2:00-3:30 VC 206

When we secure, transmit, and dispute knowledge claims in a social context, we bring a roster of epistemic notions to bear. Legitimate knowledge claims must be capable of being publicly justified, those who make them must be able to respond to challenges and retract the claims when appropriate, the claims must be secured through a process that is free from distorting biases and interests, and so forth. But epistemic labor is highly distributed in much contemporary research - especially in multidisciplinary, multi-site research in biomedicine, climate science, and the like. In these cases, particular scientists don't typically have epistemic command over the entire project. They aren't in a position to offer a justification of the completed research, and they cannot know all of the interests and biases their collaborators may have had. Thus the products of such research aren't traditional epistemic contributions.

But institutional mechanisms are emerging that are supposed to secure knowledge claims in such contexts, using legal and procedural means to produce institutional correlates of familiar epistemic notions. For example:

- The epistemic notion of a distorting interest is replaced with undisclosed sources of funding.

- The epistemic notion of justification for a result is replaced with the transparency of the process leading up to the result.

- The epistemic notion of an author who produces a claim and is epistemically accountable for it is replaced with 'authors' who can document their role in the production of a publication.

None of these institutional notions are directly epistemic. Disclosing your funding sources, for example, is not the same as having no distorting interests. But the (typically implicit) assumption is that honest, epistemically skilled people who follow the proper procedures for satisfying these institutional correlates will produce secure contributions to knowledge; likewise, it is assumed that when participants are incompetent or dishonest, these procedures will reveal the illegitimacy of the result. We argue that it is a substantive assumption that these institutional correlates are reliable functional substitutes for their epistemic analogues, and that this assumption is unjustified in much contemporary research.

In practice, research is often designed, organized, and/or managed by funding institutions such as pharmaceutical companies or industrial lobbying associations. As a matter of institutional fact (no matter how well-meaning the actors involved are) such research is animated by goals other than securing knowledge, such as improving product marketability, increasing efficiency or productivity, protecting shareholder interests, protecting an industry's image, or securing a patent or FDA approval. Epistemic objectives like accuracy, replicability, reliability, and justifiability often contribute instrumentally to the fulfillment of these goals; we typically expect research designed to further these goals to produce true claims along the way. But practices governed by these non-epistemic goals are not, properly speaking, epistemic practices. When research practices are governed by non-epistemic goals, institutional correlates do not reliably operationalize epistemic activities, they simply replace them. Furthermore, the procedural checks typically proposed by those concerned with scientific accountability are merely checks on the institutional correlates and not on their original epistemic analogues.

We describe several institutional correlates in detail and explore how they function. We give concrete examples to show that when research is organized by non-epistemic goals, we cannot count upon the institutional correlates and the procedures we have for managing them to produce results that reliably model secure, justified knowledge claims.

Double-speak in science: Scientific standards versus peer review practices

Carole J. Lee
University of Washington, Seattle

June 27. 4:00-5:30 VC 206

Peer review is science's primary mechanism for self-governance: it vets scientific research for publication and allocates limited grant dollars. In an ideal world, peer review should be the flywheel that pushes scientists — who want to publish and win grants — to undertake innovative projects and publish true claims. Instead, it is peer review that stands accused of thwarting science's goal of discovering new truths. Authors, in chase of statistical significance, increasingly engage in scientifically questionable and fraudulent research practices, which give rise to false positives. Even if we were able to stop these gaming behaviors, the disproportionate publication of positive results — coupled with the disincentive to undertake replication research — has diminished the scientific community's ability to measure true effects accurately through meta-analysis. Innovation has also taken a hit: grant applicants and agency directors have voiced concerns about reviewer bias against projects with high transformative potential.

Science is now in a situation of double-speak. On the one hand, science values truth and innovation. On the one hand, peer review hampers its ability to meet those goals. In this talk, I'll review the empirical research on bias in peer review to discuss the role reviewer bias serves in hampering truth-seeking and innovation in science. And, along the way, I'll propose a new idea about why this is happening at the level of our peer review criteria.

To see the basic idea, consider how review processes work for journals. Surveys of journal editors at the top science journals show that editors ask reviewers to assess the significance, soundness, and novelty of submitted manuscripts. Each of these criteria can be thought of having its own scale of value. However, in the end, reviewers are asked to provide a recommendation about where the submission lies along a single dimension of evaluation (with ranked options such as "accept," "accept with minor revisions," "revise and resubmit," and "reject"). To make this final recommendation, reviewers must undertake an inherently interpretive process of commensuration — they must transform heterogeneous evaluative qualities into a single scale. If reviewers weigh the novelty of a manuscript much more heavily than its soundness, this would lead to the publication distortions we see now. In contrast, if reviewers weigh soundness more heavily than novelty in the evaluation of grant submissions, then we should expect to see a conservative bias in grant review. I'll propose strategies for improving peer review qua debiasing commensuration and propose an explanation for the opposite direction of bias in peer-reviewed publication versus grant review.

What Counts as Knowledge in Plant Science?

Sabina Leonelli
University of Exeter

June 27. 4:00-5:30 VC 212

This paper examines processes of knowledge production in plant science, and singles out specific cases of research on plant-pathogen interactions as challenging traditional philosophical views on what counts as scientific knowledge and who is involved in creating and assessing it. I start by distancing myself from existing understandings of 'translational research' as a linear trajectory from the 'bench' to the 'clinic' (or, in the case of plant science, the 'field'), which are often reflected in the policies of major funding bodies such as the National Institute of Health. Rather, I am interested in developing a philosophical analysis of translational research as a specific way of knowing, which is primarily focused on improving human health, and which potentially challenges, rather than reinforcing, any straightforward distinction between applied and basic research. To this aim, I track the practices and outcomes of current research in plant science, and particularly plant-pathogen interactions, which focuses on developing sustainable technologies for environmental intervention in the short term. I put particular emphasis on the variety of expertises involved in these research communities, which typically include stakeholders not usually viewed as 'scientists' within mainstream philosophy of science (such as government officials, researchers in industry and lobby groups, farmers, landowners and other members of civil society). While in other modes of scientific research these stakeholders are not directly involved in processes of inquiry, within the projects that I examine they play a key role in determining the directions, methods and resources used to research the phenomena at hand. This inclusivity has a profound effect on what comes to count as knowledge of plant biology within these projects, and thus on the new knowledge that they produce. As I intend to show, this becomes particularly evident when focusing on how these research communities collect, handle and interpret data. Different stakeholders have diverging views on what constitutes valid and reliable forms of evidence for claims emerging from research, as well as on who should get access to such evidence. Researchers thus need to debate explicitly, at each step of their collaboration, what data need to be collected, in which ways and in collaboration with whom. This process of deliberation on what counts as evidence has important consequences on how research is performed. In my conclusion, I will outline some of its implications for philosophical views on what counts as knowledge within plant science.

Boundary Conditions, Laws, and Nomological Content in Quantum Scattering Theory

Bihui Li
University of Pittsburgh

June 27. 10:30-12:30 VC 211

Under an account of interpretation that is fairly standard in philosophy of physics, the fundamental laws of a theory capture all the theory's nomological content, and particular choices of initial conditions merely delineate the possible states of systems described by the theory. These initial states can then be evolved forward in time according to the fundamental laws in order to derive all the "possible histories" of the system. Applying the standard account to quantum mechanics, one might think that as long as one has the Schrödinger equation and a set of initial conditions for a quantum system, then one can extract everything that quantum mechanics has to say about the system. In the standard account, such information can be extracted regardless of the contexts in which particular quantum systems feature—for each context, one simply has to insert the appropriate initial conditions, which in themselves contain no nomological content.

I argue that quantum scattering theory poses some problems for the standard account of interpretation described above. In quantum scattering theory, the lawlike behaviour of the system is not contained entirely in the Schrödinger equation. Boundary conditions in quantum scattering theory tell us something about the law-like behaviour of the system and about the system's relation to its environment. I describe how these boundary conditions encode information such as our macroscopic knowledge of the steady-state behaviour of certain systems, the decomposability and localizability of certain systems, and the effective effacement of scattering systems from their environments.

The role of boundary conditions in quantum scattering theory also complicates the relationship between time-independent scattering theory and time-dependent scattering theory. Naively, one might think that time-dependent scattering theory starts with a given set of wave packets and evolves them forward in time according to the time-dependent Schrödinger equation. In this view, the time-independent theory would then just be a special approximation of the time-dependent theory, since the fundamental theory must be that which includes the true time evolution of the wave packets. However, I argue that if one looks at the contextual restrictions imposed by the boundary conditions used in both time-independent and time-dependent quantum scattering theory, then one finds that each theory has a restricted context in which it is applicable, and neither theory is more general or more exact than the other.

Connecting ontology and practices: The case of nanochemistry.

Jean-Pierre Llored
Ecole Polytechnique, Free University of Brussels

June 27. 10:30-12:30 VC 206

How do chemists make chemical bodies become intelligible? The answer of this question depends, among other factors, on the practices involved at a particular time. Lavoisier both institutionalized and enhanced a kind of chemical reasoning which connects a chemical 'whole', the parts produced by a chemical analysis, the ingredients contained within the individual under study and their respective quantities. A major epistemological shift occurred during the Nineteenth Century as soon as chemists also included chemical structure in order to account for chemical transformations. The way chemical individuals were conceptualized thus changed because composition, quantities and structure became co-defined and thus co-dependent from within chemical practices.

Bearing this historical reminder in mind, my talk then examines how the notion of structure is still at stake from 'soft chemistry' to 'integrative chemistry'. In a nutshell, I shall scrutinize how the different specialties which are currently subsumed under the label 'nanochemistry' broaden and reshape the conceptual framework within which the word 'structure' is understood. To do so, I first identify the characteristics of those new forms of chemistry by studying different up-to-date practices (sol-gel synthesis, one-pot synthesis, the design of interactive materials, biomineralization, the modeling of chemical interactions). In this respect, I shall point out that chemists must think about chemical composition, structure, size, shape, function and process, at the very same time. This new kind of relatedness is a major feature of recent chemistry. If one modifies the chemical process, one thus alters the size of a chemical body. By changing the size, one turns out to transform the structure of this individual even if its composition and the quantities of its ingredients remain the same! From the light of those new 'nanochemical' practices, one can renew the question on how the ontological status of chemical individuals can be defined. Ontology and practices can thus co-operate in a novel way.

The individuality of a chemical body does not only depend on its composition and its structure but also dwells upon its size. Furthermore, it is process or context-dependent: instrumental modes of access constitutively take part in the definition of the body. Chemical individuals are afforded by experiments to refer to Harré's terminology. Affordances are certainly products of the interaction of equipment and the world, but in many cases they are not constituents of that which affords them, neither as properties such as 'colour' nor as entities such as 'parts', nor as processes such as 'walking'. Our practical enquiry allows us to develop these ontological and mereological perspectives from the standpoint of practices!

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The Invisibility of Scientific Practice in Interdisciplinary Explanations

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June 27. 4:00-5:30 VC 215

In chapter 11 of The Structure of Scientific Revolutions, Kuhn argued that revolutions become invisible when they are described in textbook or popular presentations. This revisionary description makes it seem as if the paradigm was born mature, providing a superior framework for solving the central problems at issue without any protracted historical genesis. That a revolution occurred, including debates about what the central problems were, will not be apparent to novices and the science in question will appear to exhibit cumulative progress.

Kuhn's argument for invisibility holds regardless of whether one is convinced of Kuhn's overall conception of scientific inquiry. And aspects of the process that generates invisibility are applicable to another domain: the invisibility of scientific practice in interdisciplinary explanations. Standard discussions of interdisciplinary research (e.g., Repko 2008) focus on potential conflicts between abstract assumptions, theories, and perspectives while overlooking the significance of scientific practice, even though the latter is a key source of tensions across disciplines attempting to comprehend the same phenomenon (Love 2010).

Practices are overlooked in analyses of interdisciplinary explanation for reasons analogous to what Kuhn argues for revolutions.

> (1) Unnecessary and Unhelpful Distraction: scientists do not need to describe the practices and processes "to communicate the vocabulary and syntax of a contemporary scientific language" (136) and "could only give artificial status to human idiosyncrasy, error, and confusion" (138).
>
> (2) Exposure of Divergence in Standards: "earlier generations pursued their own problems with their own instruments and their own canons of solution" (141).
>
> (3) Context Sensitivity of Concepts: the practice-based anchorage of scientific reasoning: "scientific concepts … gain full significance only when related … to other scientific concepts, to manipulative procedures, and to paradigm applications" (142).

All three of these aspects operate in interdisciplinary explanations. The diversity and heterogeneity of practices from different disciplines are usually deemed unnecessary and unhelpful distractions when offering interdisciplinary explanations. Differences in evaluative standards are routinely minimized intentionally in interdisciplinary collaborations. The context sensitivity of concepts is often ignored because the different disciplinary contributors supply it implicitly.

To explore this process concretely, I use a particular interdisciplinary explanation from paleontology. A recent fossil find was explained as the first evidence for viviparity (live-birth) in plesiosaurs (O'Keefe and Chiappe 2011). This involved a combination of disciplinary inputs ("taphonomic, taxonomic, and ontogenetic evidence establishes that the adult was a gravid female containing a fetus," 870-1), but the relevant practices are not described: (a) there is no description of the practices from developmental biology that underwrite the identification of poor ossification and other embryonic features exhibited by the fossil, (b) differing standards of evaluation between neontology and paleontology are ignored, and (c) the context sensitivity of reasoning pertaining to population biology concepts (K-selected vs. r-selected reproductive modes) is hidden.

Therefore, just as the invisibility of revolutions obscures the complicated historical process of scientific change, so also the invisibility of scientific practice in interdisciplinary explanation obscures how science actually works. I argue this result also helps to account for the persistent neglect of scientific practice by many philosophers of science.

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Integrating Simulation and Experiment: hybrid research in Systems Biology

Miles MacLeod & Nancy Nersessian
Georgia Institute of Technology

June 29. 10:30-12:30 VC 323

Simulation is attracting an increasing amount of philosophical attention and discussion. For good reason too! Much of modern scientific practice revolves around simulating systems using ever advancing computer technology, whether these systems are meteorological, cellular or ecological. Most attention however has focused on the distinction between experiment and simulation, and processes that lead to the generation of simulations from background theory. Our studies in the new field of systems biology have revealed that in certain cases simulation and experiment are actually closely integrated into novel methodological systems that apply simulative model building without a background theoretical scaffold that describes the biological systems and indeed where the structure of such systems are often only partially known and need to be uncovered. In this talk we will detail some results of an ethnographic study of a particular researcher working in a systems biology lab. What we discovered is that she operated as a bi-modal researcher, managing a process of both developing models of metabolic and signaling pathways and also of experimenting physically on those pathways. She tightly integrated these two modes into a system of generating and validating information about her biological systems we term a discovery process. This case reveals that in certain research processes simulation and experimentation are not always distinct and disjoint activities, but can be brought together to form a methodological system in which simulation and experiment have transformed and interdependent roles. Indeed unlike the usual approach of the literature to picture simulations as mediators between a theoretical framework and phenomena, the researcher did not have theory available to any significant degree, and instead had to draw upon her methods to generate understanding of her systems in the process of investigation. These facts not only highlight the methodological innovation and flexibility of systems biology, but further suggest that simulation can have diverse cognitive functions and roles when integrated with experimentation that are not apparent in cases where simulation or simulative model-building is performed in a context removed from experimentation.

Measuring Quality of Life in the Shadow of Science

Leah McClimans
University of South Carolina

June 28. 10:30-12:30 VC 212

Measuring quality of life or patient reported outcomes as they are sometimes called is a well developed and well-funded concentration in health services research. These measures are increasingly used in both the US and UK as part of the assessment of quality health care, specifically its effectiveness. Yet despite their recent prominence the researchers who design them often suffer from insecurity regarding the validity of these measures when comparing them to measures of physical (as opposed to psychological) constructs such as blood pressure. For example, at a recent scientific conference, one paper entitled, 'Busting the Top Myths About Quality of Life Assessment in Clinical Practice' suggested that quality of life is often taken to be a philosophical concept and thus not measurable.

For many the scientific hurdle to measuring quality of life is its status as a latent trait: we cannot see it. To measure it we must ask respondents questions about it, which introduces a level of measurement error and bias not present when measuring physical constructs. The assumption is that other constructs such as blood pressure, and indeed, temperature and time, are more tangible, more scientific and thus easier to measure. In this paper I suggest that this is not true. Blood pressure, temperature and time are no more tangible—and perhaps less tangible—than quality of life. Following from the work of Chang, Tal and Van Fraassen I argue that measures of quality of life face similar difficulties to measures of blood pressure, temperature and time. This recognition has at least two consequences for the current development of quality of life measures 1) it sheds new light on the relative virtues and vices of the two competing measurement methodologies: classical test theory and RASCH analysis and 2) it suggests that the difficulty measuring quality of life is the ability to establish empirical regularities across all the dimensions of quality of life that interest us.

An Account of Epistemic Equality

Boaz Miller
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June 28. 4:00-5:30 VC 212

We acquire many of our beliefs from the testimony of others, including experts, and from social institutions, such as science, that are in charge of generating knowledge. As social epistemologists argue, knowledge is social in two fundamental senses. First, the generation of knowledge depends on an apt division of cognitive labour among researchers, and on the existence of justified relations of trust among them. Second, hypotheses must undergo a social process of critical scrutiny and evaluation, as in peer review, to acquire the status of knowledge.

Unequal social power relations may obstruct the generation of knowledge. For example, the rich may skew research priorities to areas that suit their interests, e.g., divert medical research to diseases that strike mostly white males; and powerful bodies may hinder research or artificially manufacture uncertainty that prevents the closure of controversies, e.g., tobacco companies' efforts to impede the scientific acceptance of the harms of smoking. Social-epistemic equality therefore seems necessary for mitigating the negative effects of unequal power relations. It remains unclear, however, what exactly epistemic equality means, what it entails, and how it may be realized.

We may regard the problem of epistemic equality as a distributive problem, in which participation in, and influence over, the knowledge-generating discourse are a limited good that needs to be justly distributed among members of an epistemic community. Helen Longino suggests a model in which such participation and influence are distributed according to the principle of "tempered equality of intellectual authority". The idea is that the participation in the knowledge-generating discourse should be allocated according to members' relevant expertise in the subject at hand, irrespective of social power, which is influenced by properties such as gender, race, and class.

I argue that while Longino's notion of tempered equality points at the right way toward regulating the knowledge-generating discourse, it faces two major difficulties, which call for an alternative approach. First, there is an inherent tension in Longino's model. On the one hand, Longino requires that the epistemic community be socially diverse, namely, that people of different social and ethnic backgrounds actually participate in the communal discussion. Such diversity is required, in Longino's view, to enhance the critical resources of the community. On the other hand, as we have seen, tempered equality requires that factors such as social and ethnic backgrounds should not be taken into account when allocating the good of participation in the discussion. This conflict between two competing rationales raises the worry that a criterion of relevant expertise cannot be formulated independently of social factors. Second, applying the criterion of relevant expertise is a difficult and ultimately futile task. It is often difficult to identify who are really the relevant experts, especially when different putative experts contest each other's authority. Moreover, trying to identify experts by proxy characters, such as academic education and official accreditation leads to marginalizing experts whose expertise stems from relevant life experience or alternative training. Last, there is no guarantee that even if the right experts are identified, their contribution will be positive as expected, since they may also misuse the power that is given to them.

In light of these difficulties, I argue that rather than trying to formulate a substantive criterion of relevant expertise for implementing the idea of tempered equality of intellectual authority, we should simply insist on active participation and influence of members of disempowered groups, such as women and minorities. Such an approach leads both to the satisfaction of the principle of substantive equality as a political ideal, as well as the epistemic aim of producing reliable knowledge.

Successive Refinement of Earth Models

Teru Miyake
Nanyang Technological University

June 28. 4:00-5:30 VC 206

A major achievement of seismology in the last few decades has been the progressive development of earth models—models that represent properties such as density and seismic wave velocities over the entire interior of the earth. Almost all the information we have about the deep interior of the earth is in the form of seismic wave data gathered at stations all over the surface of the earth. An incredible amount of information can be extracted from this data, but theory and models must be used to turn this data into evidence.

For example, since the velocity of a seismic wave depends upon the properties of the medium through which it has passed, travel times of seismic waves can be used to construct a model of the interior of the earth. But in order to determine these travel times, the location and time at which seismic events such as earthquakes have occurred must be known. Most seismic events occur kilometers under the ground, so their exact location and time must be estimated. This estimation must be done using an earth model. Thus, a seeming circularity is involved in the determination of earth models using travel times. In order to locate seismic events accurately, you need an earth model. And in order to determine an earth model using travel times, you need to locate seismic events. This is not a tight circle, however, because other information is contained in the seismic wave data that can be used to constrain the determination of earth structure—for example, the eigenfrequencies of the normal modes of the earth, and information about the parameters that describe the seismic source event.

The development of earth models is an example of what Hasok Chang calls "epistemic iteration"—it appears that real progress has been made over the last few decades, but we might worry about the prima facie circularity. In light of this problem, the aim of this paper is to understand the epistemology of earth models—and here I understand earth models as being related to epistemology in two different ways. First, earth models can be thought of straightforwardly as containing knowledge about the interior of the earth. Second, however, earth models can themselves be used to create new evidence—determining the location of seismic events, for example. In order to fully understand the epistemology of earth models, their creation and use must be understood in historical context. The period from the late 1970's through the end of the century was an especially productive period for global seismology, due to huge increases in computational power allowing the processing of earth models on a scale not theretofore possible. I trace out the development of earth models during this period, examining specifically why each successive new model was created, and how it was used to enable further observations of the interior of the earth.

Scientific authorship, pluralism and practice

Barton Moffatt
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June 27. 4:00-5:30 VC 206

Authorship is at the very heart of the modern research enterprise. It is the key mechanism by which credit and blame are allocated. Currently, there is no single, universally accepted criterion of scientific authorship. What is it to be a scientific author? Recently, there has been increased attention to the ways in which scientific authorship can go wrong. Practices like ghostwriting and honorary authorship are clearly unethical (Moffatt and Elliott 2007, Moffatt 2011, Shamoo and Resnik 2009). But, these judgments seem to presuppose a clear, universal account of authorship. There are several sets of overlapping guidelines, but no consensus about a single criterion for scientific authorship. Can they all be correct? Can the charge of ghostwriting be effectively refuted by showing that the unlisted person does not meet the most stringent criterion of authorship, or do you need to establish that you do not qualify as an author by any of the commonly accepted criteria? In short, should we be pluralists about scientific authorship and, if so, what are the consequences for how we think about scientific practice (Kellert, Longino and Waters 2006)?

There is considerable tension placed on the role of authorship because it inhabits the intersection between science and society. Authorship is the currency of scientific achievement both within science and between science and the world. You earn credit in your field for papers you have authored, but you also earn credit by being an author at other non-scientific institutions like universities. Your reputation is built on your publication record which in turn influences the scientific community's judgment of your work. The situation is complicated by differing sets of assumptions that guide how much credit is due to a scientific author. Your peers will have a very good sense of what you contributed on a project to be listed as an author. University administrators are likely to have a somewhat less accurate view. Other consumers of scientific publications, like doctors reading up on the efficacy of new drugs in their area of practice, are likely to have incorrect views and often make faulty inferences as a result.

Clarifying some of these lurking conceptual issues about scientific authorship is important because it: 1) provides a better understanding of the nature of science and scientific communication; 2) reduces the impact that conceptual confusion plays in empirical research about authorship practices; and 3) impacts ethical norms that guide the responsible conduct of research. In this paper, I will argue that there is no single, universal account of authorship is the sciences—nor should one be imposed by small groups of journal editors. As such, we need to revisit the empirical findings about research ethics which presuppose a single view and the ethical claims based on it. Finally, I consider the advantages of eliminating the category of "author" in favor of a credit model modified from popular contributorship approaches (Resnik 1997, Rennie 1997). On this account, the creators of papers simply list who has earned intellectual, work, and funding credit.

Genomic “Soft” Realism: a framework for understanding the use of a plurality of population categories in epidemiology

Santiago Jose Molina
University of Chicago

June 28. 4:00-5:30 VC 215

In the past decade, genomic and epidemiological research has increasingly targeted individuals from culturally diverse populations around the world for their studies. However, the criterion used to delimit and select a population varies. Examining the relevant literature in human population genetics and epidemiology reveals a myriad of different types of population categories; such as, “Ashkenazi Jew”, “Mexican”, “Zapotec”, “African American”, “European”, “Southeast Brazilian”, etc. In this talk I examine why there is a pluralistic definition of ‘population’ in genomics and ask how socially determined notions of human groups and community become biologically significant categories. Moreover, I offer insight into the way that practical interests of researchers can influence the evaluation of categories as valid representations of biological differences between people.

To do this, I (1) explain the epistemological justification behind the use of samples from diverse populations, rooted in recent methodological and technological changes of the past decade; (2) reconsider the issues of the biological reality and reification of social categories in relation to the practices of epidemiologists; (3) and offer an instrumentalist framework for understanding the value of ambiguous definitions of ‘population’ in genomics. The instrumentalist framework I describe, genomic ‘soft’ realism, illustrates a dynamic nominalism at play when epidemiologists use racial and ethnic categories in their research and when recruiting participants.

What makes this stance realist is the underlying claim about the biological reality of measurable genetic differences between human breeding groups. Because these lower-level differences can bias the outcomes of studies, researchers must evaluate the correspondence of higher-level population labels and proxies to actual genetic differences. I explain the way in which this bias, due to lower-level population structure, occurs in practice using case-control association studies as an example.

What makes the framework ‘soft’ is its flexibility to include virtually any type of population, be it delimited culturally, politically or geographically, based on the instrumental needs of the study and ease with which samples can be acquire. This ‘softness’ is also meant to permit a pluralist understanding of the local contexts in which researchers conduct their work. When considered in relation to the imperative to collect large amounts of samples from diverse individuals, this flexibility is clearly useful.

What makes this stance ‘genomic’ are the tools and bioinformatic means through which populations structure is studied and through which categories are evaluated. In order to illustrate this framework at play, I describe a bioinformatic tool, STRUCTURE, and the ways it is used in practice to analyze “cryptic” population stratification. Under the framework of genomic ‘soft’ realism the meaning of socially defined ethnic labels is open and subject to reinterpretation in light of unresolved theoretical problems, unproved hypothesis, rapidly changing technology, unknown medical applications to findings, existing social connections, local institutional conditions and difficulties in recruiting participants. As I argue, these issues drive the pragmatic considerations of PIs and post-docs in cutting edge labs when they target study populations.

Experimental Philosophy of Economics: A Study of Choice

Michiru Nagatsu
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June 29. 10:30-12:30 VC 206

There is no consensus among economists and philosophers as to the exact nature of the notion of preference, despite its central role in economic theory. It is not clear, however, what this lack of agreement means to the status of economics as a science. Is economics still in a 'pre-paradigmatic' period in which participants cannot agree on the meaning of its most basic theoretical concepts? Or does the disagreement reflect healthy plurality of the practice in economics, rather than its immaturity? To properly diagnose the situation, one will need not only to engage with subtle details of economists' daily business, but to take a wider view of the practice in the profession. However, the data that could enable such a survey have been missing from the philosophical debates regarding preference concepts. This makes it difficult to make progress towards a better understanding of economics. In this paper, I argue that a new approach called experimental philosophy of science will provide such data and complement the traditional case studies commonly practiced by philosophers. First, I will briefly describe the recent 'experimental philosophy' (X-phi) movement and experimental philosophy of science. I will then discuss how X-phi of science can illuminate the debates concerning the preference concept in economics. I will take up two examples, the recent neuroeconomics controversy and the so-called 'commonsensible' realism debate in the philosophy of economics. I shall then respond to possible objections and discuss several methodological advantages of the X-phi approach, and also sketch how one could begin to study economists' preference concepts using this approach. I hope to report some results from a pilot study as well.

Representing Scientific Investigations

James A. Overton
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June 28. 10:30-12:30 VC 212

Data from scientific investigations are stored, shared, and published in electronic formats. Scientists facilitate sharing by representing their data in standardized forms. Standardization across diverse sciences raises a number of philosophical questions about the nature of scientific data and what the data are about. Second-order questions arise with databases of scientific investigations. For instance, the Immune Epitope Database (IEDB) contains records for more than more than 14,000 investigations, almost every investigation ever published in the fields of immunology, allergy, and autoimmune disease research. The utility of such databases depends upon scientific investigations themselves being represented well, so that they can be effectively searched and analyzed, and their data can be carefully evaluated and compared. In this paper I present and discuss with examples an ongoing attempt to carefully define scientific investigations in a way that is both enlightening to scientists and useful for scientific databases.

Problems of representing scientific data in standardized ways are being addressed by scientific "ontologies". This use of "ontology" can be traced through the fields of knowledge representation and artificial intelligence research back to its origins in philosophy. These ontologies are hierarchies of carefully defined terms, linked together into a network by logical axioms, and provided in both human-readable and machine-readable formats. The Ontology for Biomedical Investigations (OBI) is an ontology that aims to describe biological and clinical investigations. It is part of the Open Biological and Biomedical Ontologies (OBO) Consortium, which includes dozens of scientific ontology projects committed to shared best practises.

OBI defines the term "investigation" as "a planned process that consists of parts: planning, study design execution, documentation and which produce conclusion(s)." Study design execution usually involves four stages: collecting specimens, preparing specimens, performing assays, and processing data. In a clinical context the "specimens" may be human subjects. Assays serve as bridges between material entities and information about those entities. Each step is performed according to some protocol, with specified objectives, variously involving agents, devices, algorithms, etc. In order to carefully compare the results of scientific investigations it is important to compare details of these protocols and processes.

In defining these terms the OBI developers face a number of philosophical and practical problems. OBO ontologies all use the Basic Formal Ontology, which defines common terms such as "process", "material entity", and "quality". OBI must fit its terms into this common framework. Many OBI terms involve information, raising philosophical problems of meaning and identity when copying and processing data, documents, and computer files. OBO projects share a logical formalism, which is both a help and a hindrance. Biologists and clinicians practising in different fields and at different sites vary in their use of language. Compromises must be reached.

In this paper I provide a brief introduction to scientific ontologies, and a first-hand account of the ongoing development of the Ontology for Biomedical Investigations and its implementation in the Immune Epitope Database. I focus on the philosophical and pragmatic questions raised by these scientific practises at the growing frontier of science informatics.

The role of scientific practice in the analysis of theories

Francesca Pero
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June 28. 4:00-5:30 VC 206

In this paper I argue that scientific practice has entered the analysis of scientific theories not only as a legitimate topic of interest, but also as a necessary condition for such analysis to be complete and, hence, effective.

Carnap (1955) claims that an analysis of scientific theories should not take into account scientists' actions, rather it should focus solely on the results of such actions: the scientific statements which make science an ordered body of knowledge. The failure of the Positivistic image of theories is a proof of how limiting could be appealing to rational reconstructions of scientific theories and identifying the rationality of such reconstructions with neglecting the role of theory builders and users.

Arising as an alternative to the Positivistic approach, the Semantic View of theories is a program of analysis identified by two main tenets. An analysis of scientific theories that aims at being complete should provide: (i) a formalization of theories; (ii) an adequate account of theories as regarded in actual practice. Advocates of the Semantic View generally agree on providing (i) by resorting to the notion of models: we do not apply theories, as linguistic entities, to a target phenomenon directly, we rather construct 'simpler analogues' both of a theory and of its target: respectively, theoretical and data models.

The cruciality here ascribed to the notion of models leads consequently to (ii), i.e., to taking into account the scientific practice, intended as model building, in order to analyse how we gain knowledge by means of models. The model building activity consists mainly in the selection of those elements of both theories and phenomena which scientists take into account as relevant in order to obtain an explanation.

It is my conviction that the tenets of the Semantic View could act as criteria for evaluating the completeness also of those analyses recently formulated within the Semantic View itself. Using Chakravartty' s (2009) labels, I refer to such analyses as informational and functional. Informational and functional analyses of scientific theories differ mainly in the justification they provide of the explanatory power of models. Informational analysis conceives the explanatory power of models as an objective (mind-independent) property of models, whereas functional analysis conceives the explanatory power of models as a function that need to be first assigned and recognized by models' competent users in order to be effective.

By deploying Brading and Landry's (2006) characterization of empirical sciences, i.e., that they have not only to present (identifying kinds of objects up to morphism), but also to represent (identifying realizations of kinds of objects), I show that the justification of the explanatory power of models provided by the informational analysis accounts for their presentational capacity only, thus living unjustified how models latch onto reality. Functional analysis, on the other hand, does justify the representational level insofar as it is conceived as the final act of model use, i.e., the application of models, which is carried out by model competent users.

Philosophy of transdisciplinarity: The limited applicability of the concept of Mode 2 knowledge production

Johannes Persson, Line Breian, & Henrik Thorén
Lund University

June 29. 2:00-3:30 VC 206

A large number of contemporary researchers claim to be working inter- or transdisciplinary. New models and concepts of inter- and transdisciplinary research are developed, especially in the social sciences. These models and concepts are sometimes adopted by researchers as descriptively adequate of the kind of research they are doing. Time is ripe, we believe, for philosophy of science to engage in some of these discussions and critically examine the emerging accounts of what inter- and transdisciplinarity is.

Our article concentrates on one recent and influential conception of transdisciplinarity. Gibbons et al. (1994) and Nowotny et al. (2001) argue that a new mode — Mode 2 — of knowledge production has emerged. The Mode 2 concept is designed to suit enquiries where researchers from different disciplinary perspectives come together to work on problems "in an applied context." Allegedly, Mode 2 is "different in nearly every respect" from traditional, disciplinary science (Gibbons et al. 1994, vii; see also Roll-Hansen 2009, 16). For instance, the Mode 2 account claims that contemporary knowledge production breaks with disciplinary boundaries and the academia/society distinction upheld by traditional — Mode 1 — researchers.

Starting with an analysis of three essential components of Mode 2 accounts, we try to show that the Mode 2 account typically cannot be applied to emergent fields within environmental research.

It is important to note that we do not claim to examine the features of Mode 2 that those who deploy the concept take to be most important. However, the three features we base our argument on are all defining characteristics of Mode 2. Their implications, we submit, cannot be escaped by anyone who deploys the Mode 2 concept to characterize their research.

First: boundary crossing. Nowadays it appears to be the rule rather than the exception that scientific activity involves boundary crossing of some sort. Second: unified framework. Gibbons et al. (1994) claims that Mode 2 implies the development of a distinct but evolving framework. These Mode 2 frameworks are explicitly said to harbour the theoretical structures and research methods that are generated in the (ongoing) projects. Gibbons et al. (1994, 29) refers to this feature of Mode 2 as a requirement of a "homogenised theory or model pool". It needs to be noted that whereas traditional unity of knowledge perspectives are global Mode 2 frameworks are centred on specific problem-solving efforts. This brings us to the next feature. Third: the tie to a local context. The local and temporary feature of Mode 2 frameworks is highlighted in Gibbons et al. (1994, 29-30).

The unified framework requirement has its intellectual roots in an influential perspective on transdisciplinarity promoted by Erich Jantsch (1972) and others. While denying that their own view on the transdsiciplinary mode of knowledge production aims to establish itself as a new transdisciplinary discipline or to restore unity of knowledge, Gibbons and his colleagues nevertheless maintain that Mode 2 knowledge production generates a theoretical and methodological core.

We argue that the combination of unified framework and tie to a local context is seldom met.

Defining Reproduction in the Age of Genetic Engineering

Monika Piotrowska
Florida International University

June 27. 10:30-12:30 VC 206

In the early 1980s, the first sheep-goat chimera was born, followed by the birth of a quail-chicken chimera a few years later. Earlier this year, Tachibana and colleagues published an article in Cell announcing the birth of “the world’s first primate chimeras.” These primate chimeras were the product of combining at least three different rhesus monkey embryos, which were merged at the four-cell stage of development. If we assume that merging early embryos counts as a reproductive event, all of the above chimeras have four (and maybe even six) biological parents that aren’t necessarily of the same species. But why should we assume such a notion of ‘reproduction’? In what way do biological parts need to be merged for reproduction to occur?

To appreciate the difficulty of these questions, consider the following experiment. In 2005, Muotri and colleagues injected 105 human embryonic stem cells into the brains of 14-day-old mouse embryos. (Since mice are only pregnant for 20 days they were advanced enough to have developing brains at the time of injection). The human cells that were injected proliferated in the mice, but they did not spread beyond the brain regions. Given these facts, and how late in fetal development the cells were merged, we can ask: Should the injection (and subsequent incorporation) of human cells into the mouse count as a reproductive event? Or is this chimera nothing more than a mouse, since it was already a developing fetus when the cells were merged? The answer to these questions isn’t clear, but intuition strongly suggests that this creature bears a genealogical relation only to mice. Why? Because in this particular experiment, merging the cells through injection looks more like an organ transplant than a reproductive event. The cells were injected so late in the developmental process that a proper mouse was already there. Intuitively, then, this is a case of transplantation rather than reproduction. If transplanting a heart valve from a pig to a human does not change the latter’s genealogy, transplanting brain cells into a mouse probably doesn’t change its genealogy either.

Of course, these intuitions have implications for questions of reproduction. If the experiment by Muotri and colleagues is analogous to an organ transplant, merging biological material does not necessarily result in a reproductive event. But as we saw earlier, the merging of cells can have such an effect. When cells are merged early enough in a creature’s development, the offspring have four (or more) biological parents. If that is right, and if there are experiments that fall somewhere in between these two examples, it is not clear when merging biological material counts as reproduction.

To address these issues, I want to investigate James Griesemer’s widely cited account of reproduction. To do so, I will apply his definition of reproduction to the production and development of chimeras, cybrids, transgenics and other genetically engineered animals. Once I have shown Griesemer’s commitments, I will argue that his account cannot adequately distinguish between cases where merging biological material results in reproduction as opposed to transplantation. Finally, I will offer a way to address the shortcomings of his account. Doing so will leave us with a more suitable notion of reproduction, which will be increasingly important as we proceed through the age of genetic engineering.

Two Problems for Science Policy

Vincenzo Politi
University of Bristol

June 29. 2:00-3:30 VC 212

Philosophers of science have recently began to look at science policy, linking the considerations about the theoretical scientific knowledge to the problems of its actual use. This talk is about the philosophy of science policy and it is in two parts.

In the first part, I examine two of the most recent philosophical prescriptions for an effective science policy - namely, Mitchell (2009) and Cartwright and Hardie (2012). Mitchell applies her integrative pluralism, already defended in her (2003), to the analysis of the policy-process for the resolution of `complex problems'. She compares the traditional `cost/benefits-analysis' with the `scenario analysis' and the `robust adaptive planning', arguing in favour of the latter methods. Her idea is that effective science policy benefits from the integration of different non-reducible sciences.

Cartwright and Hardie advance a theory of evidence for use, which aims to answer questions such as `what is evidence good for? in which context is the evidence a good evidence?'. A good evidence is supposed to be the `cause' which, when implemented by the policy, will produce the desired effect. In this account, different pieces of evidence are INUS conditions - Insufficient but Necessary part of an Unnecessary but Sufficient condition to produce a certain effect.

I show how Mitchell's integrative pluralism and Cartwright's and Hardie's theory of evidence for use are compatible. Indeed, it can be shown that the `alternative scenarios' mentioned by the previous can be described in terms of `INUS-pies' referred to by the latter.

In the second part I describe two problems with these two models of science policy.

- PROBLEM I. For Mitchell, the difference between the laws of physics and the laws of sociobiology is of grade of complexity rather than of kind (Mitchell 2009a); she does not consider whether we need knowledge from social sciences which are not as `lawful' as sociobiology. Cartwright and Hardie considers the `efficient cause'; however, when implementing policies in a social context, the fact that people may be subject to `final causes' should be considered. The failure of some policies in some particular social setting illustrate this problem - as in the examples of implementation of medical treatments in non-Western societies, experiments in Economics, and so on.

- PROBLEM II. Mitchell's and Cartwright's and Hardie's models do not tell at which point the `actual effectiveness' of a policy is granted. The problem consists on answering the question: ``How long should we wait to see the effects of the implemented policy?'' This problem may lead to the interruption of an ineffective policy which could have been nevertheless effective in a slightly longer run or to the implementation of policies which will be effective, but not as soon enough.

More than challenging the two models under examination, the two problems aim to stress some general difficulties in science policy and suggest new ways of looking at science policy.

Who is a suitably prepared model user?

Isaac Record
University of Toronto

June 28. 2:00-3:30 VC 211

Scientists now use models in nearly every aspect of scientific practice. In recent decades, philosophers of science have devoted increasing attention to models, and in particular to the question of what makes a good model. Comparatively little attention has been paid to what makes a good model user. Although there remains some disagreement about what makes a good model, I take it that most accounts are compatible with at least the following claim: A good model is easier to manipulate than its target and it affords users useful inferences about its target. If it was not "easier to manipulate" than the target, at least in certain respects, we would just manipulate the target directly. And if the model did not afford useful inferences, we would not be using it, no matter how easy it was to use. But even good models afford some useless or misleading inferences. How do users know which inferences to make and which ones to ignore? The beginnings of an answer are evident in phrases like "suitably prepared user," which appear with some frequency in the models literature. The task in this paper is to say who can be a suitably prepared user, and under what conditions. My proposal is this: A suitably prepared user is one for whom the valid and relevant affordances of a model are readily perceptible, and for whom invalid or irrelevant affordances are either hidden or easily identified as improper. Affordances are the possibilities for action that a given individual is competent to act on. Models afford possibilities for users to manipulate them in order to generate inferences. A suitably prepared user is one who readily identifies valid and relevant possibilities for making inferences and rejects the irrelevant or invalid inferences. Roughly, then, a suitably prepared user makes the right inferences and avoids the wrong inferences. I will consider four factors that contribute to a user's preparation: native human capacities, socialization, experience, and formal training. Any of these four factors can work for or against correct model use. For example, some models are purported to have a "natural" interpretation making use of native human capacities and socialization. Such a model succeeds insofar as this "natural" interpretation is salient for individual users. In other cases, users undergo training intended to overcome natural interpretations in favor of one preferred by the model designer. In addition, the more general formal training scientists undergo typically includes methods to help them rationalize their experience with a model. For example, in most fields, proper model use involves characterizing models in terms of valid regimes and measuring the quality of results within those regimes. I argue that this diversity of modelling activities can be understood with reference to the model design process, and that model design produces not only the model but also a set of "practices of trust" surrounding the use of the model. Practices of trust aid users in setting the model up correctly, interpreting the results, and characterizing their quality.

Mathematical Explanation in Practice

Davide Rizza
University of East Anglia

June 29. 2:00-3:30 VC 211

In this talk I outline a general characterisation of the role played by mathematics in generating explanations of empirical phenomena. My account of mathematical explanation focuses on forms of scientific practice in which the mediation of background theory is absent: thus, I depart from models of scientific explanations such as Hempel's D-N account or Kitcher's unification approach because I do not consider the availability of laws or similar pre-existing theoretical resources in the construction of explanatory arguments. The examples I have in mind arise in fields like operations research or the social sciences, in which certain types of design or concrete setups exhibit unexpected behaviour, i.e. behaviour which cannot be anticipated and whose range of variation is beyond control. Under these conditions, the need to explain and the need to control (insofar as possible) the behaviour of an empirical setup are interlinked and they may sometimes by simultaneously satisfied through the introduction of mathematical resources. These resources generate explanations because they deploy a formal description of empirical systems as certain spaces whose structure restricts their possible behaviour. It is only when one looks at an empirical system through the lens of a certain mathematical representation that the system itself appears to be constrained to exhibit only certain configurations. Mathematical explanations consist in the framing of suitable mathematical representations from which it is possible to deduce actual or possible behaviour.

I illustrate this idea through an analysis of Donald Saari's geometrical explanation of a puzzling property of election rules, i.e. the fact that, under the same individual preferences, arbitrarily close scoring rules may produce a complete reversal of the election outcome. This example shows very clearly how the availability of a direct and complete description of a particular type of design (e.g. a voting rule) may prove entirely insufficient to grasp the salient properties of the design itself, which are necessary to control its behaviour. Mathematics plays an important role in this context by providing a representation of the design that makes it possible to see how its outcomes are restricted. In the case of voting, scoring rules and outcomes can be represented as geometrical objects (convex subsets of linear spaces) and an election as a map that sends rules into outcomes for fixed preferences. The ordered geometry of this representation suffices to constrain all possible configurations of outcomes for certain scoring rules and systematically explains their behaviour merely by constructing a geometrical representation of their features.

This shows how mathematics can autonomously (i.e. independently of a background empirical theory) generate explanations of empirical phenomena and thus give a substantial contribution to the understanding of empirical practices and design problems.

What is the source language of translational research?

Jason Scott Robert
Arizona State University

June 27. 4:00-5:30 VC 212

Translational research is all the rage in contemporary biomedicine. But as Woolf (2008) has aptly observed, while “it seems important to almost everyone,” nonetheless “translational research means different things to different people.” Accounts of translational research “from bench to bedside to barrio” (Horowitz, Robinson, and Seifer 2009) typically comprise several phases of translation: from experimental results through clinical testing (T1) and from clinical testing through application (T2) and uptake (T3). Such accounts thus conceive of a kind of source language (results at the bench) on the one end, and a variety of clinical and practice outcomes at the other end.

Within T2 and T3, we might anticipate a range of research products: new diagnostics, devices, or drugs; clinical trials; clinical practice guidelines and strategies for their adoption; and population and public health programs, inter alia. Some observers note that the metrics of successful translation are unclear at best; especially cynical observers complain that translational research in practice actually generates more publications, patents, and research grants, and not these other promised products. I will set aside such criticisms here, for my goal is to assess not the outputs but rather the inputs - the source language - of translational research.

What is it at "the bench" that constitutes the raw materials for translation? Are these materials simply the experimental findings of basic scientists or, alternatively, the results of practically oriented or use-inspired research? Or are the architects of the translational research enterprise imagining more special forms of discovery? Is the source language of translational research adequate to the translational task, or are reforms required? If the latter, of what sort, to what end, and with what sequelae?

This paper critically explores policy documents as well as editorials, commentaries, and review articles in the scientific literature, and assesses a case study of translational research in autism, to develop an epistemology of translational research both as conceived and as practiced. The case study evidences the use of both dominant grammars and local dialects, suggesting the need for a nuanced philosophical interpretation of the epistemology of translational research. The analysis provided opens new avenues for scholarly inquiry into the heterogeneous phenomena of translational research in contemporary biomedicine.

The Journey between Discovery and Scientific Change

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June 27. 2:00-3:30 VC 212

Although philosophers of science have been concerned with scientific methodology, I argue that philosophers have overlooked a particular area of interest, namely the communication of ideas within a scientific community. Although philosophers of science don't directly concern themselves with the communication of scientific ideas, they ought to. The present paper focuses on radical thinkers, their ideas, and the struggle to communicate ideas to those within a scientific community. By following the journey from discovery to communicating that discovery, it becomes apparent that communicating a radical idea to a scientific community is a struggle and greatly hinders scientific change. Simply, a more in-depth analysis of scientific communication is necessary and will be beneficial.

I begin by pointing out a few puzzles in Kuhn's notion of the scientific process. I note that although Kuhn is mainly interested in the role that the scientific community plays in scientific change, he strangely never provides his readers with an exemplar of a scientific community. Instead, Kuhn gives repeated examples of individual, radical thinkers and their contribution to scientific progress. Also, Kuhn fails to describe the journey from discovery to the community's complete acceptance of a new paradigm. It will become apparent that I will argue for a more fluid model of the scientific process as well attempt to better explain how radical thinkers and their ideas play an important part in the shifting of scientific thought.

To better understand how important thinkers are able to contribute to scientific change, I turn my attention to scientific disciplines, scientific fields and scientific communities. In this section, I review common thoughts among philosophers of science regarding scientific disciplines as well as scientific fields. I argue for the distinction between a scientific discipline, or broad area of study, and a scientific community, the unit of scientific communication. Next, I offer several components that are required by a scientific community as a unit of communication, namely shared goals, procedures, models, theories, values, educational tradition, journals and professional societies. My focus then turns to shared mechanistic models and how these aid in the communication process within a community.

In an effort to add to the current picture of scientific change, I review two individual cases of radial thinking. To begin, some time is spent better understanding Barbara McClintock, her radical idea known as "jumping genes", and the way it shaped modern genetics. Next, I hypothesize that something similar will happen regarding Stanley Prusiner's "prion" within molecular biology. As a result of following the journey of these two biologists, a common pattern will emerge. Initially, some radical thinkers are not able to communicate their ideas to the scientific community. It is only through the work of others and a precise fit between the idea and a particular community that an idea can come to be scientific knowledge.

Values in Science: A Liberal Egalitarian Approach

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June 28. 2:00-3:30 VC 212

A number of philosophers have challenged the traditional ideal of value-free science, that is, the view that social values are not allowed to play a role in the decision-making processes that scientists are engaged in when they accept something as scientific knowledge, either individually or collectively. While many philosophers seem to agree that the ideal of value-free science is not feasible, their views diverge on the question of what the successor to the traditional ideal should be. Some philosophers introduce principles designed to guide individual scientists in their decision-making (Douglas 2009); others suggest that a normative approach to social values in science should be a form of social epistemology, guiding discursive interactions among scientists (Longino 1990, 2002) or defining an epistemically ideal distribution of research efforts in scientific communities (Solomon 2001). Also, some philosophers hesitate to take a stand with respect to the question of which social values are allowed to play a role in science; others suggest that a normative approach to social values in science should take such a stand, either by defining a process whereby acceptable values are articulated and selected (Kitcher 1993), or by advocating some social values explicitly (Kourany 2010).

I argue that a normative approach to social values in science should be a form of social epistemology that has normative implications not only for scientific communities but also for individual scientists' decision-making. Also, I argue that a normative approach to social values in science should be understood as a kind of political philosophy of science, helping individual scientists navigate among the competing and sometimes conflicting demands of different social values in liberal democratic societies. What I call a liberal egalitarian approach to social values in science aims to accomplish these two tasks.

A liberal egalitarian political philosophy offers a model for understanding what roles both individual and community level principles play in social epistemology of science and how they are connected. In liberal egalitarian political philosophy, individuals alone are not held responsible for the realization of justice; the primary responsibility for justice belongs to institutions. Background justice must be secured by institutions because it cannot be secured through individual action alone. It is simply beyond individuals' capacity to do so. However, individuals are assigned a duty to support just institutions.

To defend a liberal egalitarian approach, I discuss briefly five alternative approaches to social values in science: Miriam Solomon's social empiricism, Heather Douglas's conception of scientific integrity, Helen Longino's social account of objectivity, Janet Kourany's ideal of socially responsible science, and Philip Kitcher's well-ordered science.

1. In social empiricism what matters is the distribution of epistemic and non-epistemic values in scientific communities, not their role in individual decision-making. A liberal egalitarian approach makes justice to the insight that diversity and dissent can function as epistemic resources in scientific communities, while it assigns epistemic responsibilities to individual scientists and not merely to science policy makers as social empiricism does.

2. In Douglas's view, scientific integrity consists in keeping social values to their proper roles in individual decision-making. A liberal egalitarian approach makes justice to the insight that scientists are morally responsible for the potential harm caused by their making overly strong knowledge claims and downplaying the risk of error. Yet, individual scientists alone cannot be held responsible for the realization of scientific integrity because they are not always aware of the many roles social values can play in scientific inquiry (see also Elliott 2011). The primary responsibility for scientific integrity belongs to scientific communities.

3. A liberal egalitarian approach makes justice to the insight that a social account of objectivity is needed because individual scientists are not capable of realizing objectivity on their own. Yet, a liberal egalitarian approach does not treat various social values in science as even-handedly as Longino's "social value management ideal of science" is claimed to do (Kourany 2010; Intemann 2011). Also, a liberal egalitarian approach assigns duties to individual scientists, especially the duty to support the conditions necessary for background objectivity.

4. While Kourany's ideal of socially responsible science is vague in its answer to the questions of how scientists identify "sound" social values (see also Rolin 2012), a liberal egalitarian approach offers an answer to this question.

5. While Kitcher's well-ordered science relies on the problematic assumption that scientific experts and "tutored" deliberators can be neutral with respect to various social values in liberal democratic societies (see also Brown 2004, Turner 2003), a liberal egalitarian approach does not rely on such assumptions.

Science Worlds: Toward more expansive notions of scientific practice and participation

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June 27. 2:00-3:30 VC 212

The turn to practice and science's social nature has raised two important and not unrelated questions: (1) How should scientific practice be organized to maximize its effectiveness in achieving its aims and (2) how should scientific practice be organized to best integrate it with and make it sensitive to the broader social context in which it is embedded? The answers to these questions depend in part on who qualifies as a "practicing scientist". The first two questions have been explicitly taken up by a number of philosophers (prominent examples being Philip Kitcher, Helen Longino, Miriam Solomon) and the determination of scientific expertise has recently gained attention in the "Third Wave" of Science Studies.

In this paper, I argue that this approach privileges an unduly narrow and individualistic view of scientific practice. Such a view makes sense in the context of our interest in question (1). However, when we adopt this view in answering question (2), it offers a very narrow view of who counts as engaging in scientific practice, namely, only "practicing scientists." While an interest in how "practicing scientists" ought to be integrated into the broader (non-scientific) socio-cultural context is legitimate, it ignores the relationships "practicing scientists" have with philosophers, historians, and the broader public. In particular, this narrow view tends to circumscribe scientific practice in a way that excludes individuals whose disciplinary activities tend to involve practices other than making or justifying knowledge claims in disciplinary-specific or public fora. These individuals aren't necessarily in the business of making or justifying scientific claims, but on a broad view of scientific practice count as active participants in science.

Put another way, the narrow view of scientific practice frames science as something that only a few qualified people do, rather than something a broad and diverse range of people participate in. This last point has important implications about how we think of question (2). For example, a broad view of participation that includes "non-scientists" (taxpayers, health stakeholders, technicians, etc.) as participants might not support an analysis that understands questions (1) and (2) as separately answerable, or even distinct. More generally, I suggest that expanding our view of scientific participation has important consequences for our understanding of the relationship between scientists and the broader public, especially with regard to promoting and maintaining trust between the two. I also propose that this view illuminates a substantive role for philosophers and historians within ongoing scientific activity (especially in light of procedural notions of scientific objectivity).

My paper proceeds as follows: First, I use Philip Kitcher's Advancement of Science and Science, Truth, and Democracy as illustrations of the "narrow view" at work. Secondly, I present a brief argument for why an individual's participation in scientific practice need not involve her making or justifying particular knowledge claims. Taken together, these points establish the possibility of a broader view of scientific participation — one which includes people not typically identified as scientists. Finally, I sketch out more fully the implications of this broader view hinted at above.

Scientific Significance

Joseph Rouse
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June 27. 2:00-3:30 VC 206

Most truths about the natural world have little or no scientific significance. Insignificant truths, if noticed or confirmed empirically, would typically not be publishable as results in scientific journals, and such truths also typically play little or no role in the reasoning for other claims that are significant (since significance is partially heritable via a claim's inferential role). The implicit partition of the world into those aspects whose conceptualization and empirical assessment is or would be recognized as scientifically significant, and those that are not, plays important roles in scientific practice. Scientists normally pursue research projects whose outcome, if successful, they expect to be significant, and the significance of purported results or achievements figures prominently in decisions to regard experiments or projects as completed, as publishable, as needing citation in other contexts, as worth replicating or otherwise checking, or as opening new research possibilities that would be significant in turn.

The difference between scientifically significant and insignificant topics, claims, or achievements has received rather less philosophical attention than have the more familiar topics of confirmation, explanation, causality, or realism. To be sure, these traditional topics undoubtedly bear on scientific significance. Explanation in particular has often served as a partial stand-in for a philosophical conception of scientific significance: explanatory power and scientific significance often seem to go hand in hand. Yet explanatory power does not exhaust scientific significance, and it also to some extent presupposes a determination of which facts or laws significantly call for explanation.

Scientific significance has one further complication. The very notion of scientific significance implicitly demarcates a more limited portion of the considerations governing scientific priorities, in apparent contrast to the "extra-scientific" significance of some scientific projects, whether due to practical applicability or specific cultural or social "interests" (e.g., Kitcher's 2001, ch. 6 proposal to distinguish "epistemic" from "practical" significance, or Lenoir's 1997, ch. 2, differently inflected distinction of the formation of research fields from the broader social and institutional issues that lead to discipline-formation). Yet these efforts, while calling attention to some important differences within scientific work, nevertheless also thereby block effective understanding of how scientific significance in some narrower sense is itself responsive to broader cultural, political or practical concerns.

This paper will have two primary aims. The first aim is to provide an initial brief overview of some of the most prominent issues and problems for philosophical work on scientific significance. The second aim, within that context, is to highlight two features of the determination of scientific significance that have not received sufficient consideration. The first feature is what constitutes an intelligible domain of inquiry, as distinct from the considerations governing the formation of disciplines that might address such domains. The second feature is the prospective character of scientific significance, which often runs ahead of established knowledge and its familiar boundaries: inquiries may gain significance precisely from their promise to challenge familiar concepts, domain-boundaries, or formulations of issues, even if the specific content of that challenge cannot yet be specified.

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A non-manipulationist account of invariance

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June 28. 10:30-12:30 VC 206

Causal assessment is the problem of establishing what causes what. For instance, what are the causes and the effects of solar storms; what symptoms and diseases are caused by Escherichia coli; whether changes on pension and health care systems help with the challenges posed by ageing populations, etc.

'Causal modelling' is, arguably, the most accredited methodology for causal assessment. Despite the differences that causal models may have in, say, economics, computer science, or epidemiology, these models share some common features, namely modelling the dependencies and independencies between variables of interest and performing different kinds of tests (including invariance tests) to establish causal relations. In the philosophy of causality, some scholars, captained by Jim Woodward (2003), made tests for invariance under intervention crucial for causal assessment and for causal explanation. Simply put, (variable) X causes (variable) Y if, and only if, were we to manipulate X, Y would accordingly change, and the relation between the two would remain stable, or invariant, under a sufficiently large class of interventions or manipulations of the putative cause-variable. I call this type of invariance 'manipulationist invariance' to stress that invariance properties are tested against interventions or manipulations of the cause-variable.

It has been recently argued that a methodology based on 'invariance under intervention' cannot offer an account of causal assessment in non- experimental studies (Russo 2011, 2012). More specifically, Russo argues that 'manipulationist invariance' does not provide a suitable test for contexts in which interventions are not practically or ethically feasible, and yet some kind of invariance is key to establish causal relations, even when manipulations on the putative cause-variables are not performed. I call this type of invariance 'non-manipulationist invariance' to stress that manipulations or interventions are not essential to test invariance properties. In this paper I develop an account of 'non-manipulationist invariance' in detail.

The paper is organised as follows. In §2.1, I offer a baseline understanding of causal modelling; I focus on the characteristics that are common to different scientific areas and I use an example from social science in practice to illustrate the different steps of model building and model testing. Then, in §2.2, I locate the debate around 'invariance' in the works of the forefathers of econometrics, where the notion was first developed. In §3, I develop the account of non-manipulationist invariance in detail. First, I argue that invariance is tested across changes of the environment, rather than just against manipulations of the cause-variable. Second, I explain the meaning and import of invariance for causal assessment by comparing it with two further notions: variation and regularity. In §4, having recalled the basic features of manipulationist invariance, I discuss where we will be misled if we keep adopting manipulationist invariance.

The take-home message is that because 'invariance' is such an important notion for causal assessment, it is of utmost importance to have an account that works in non-experimental and experimental contexts alike. The 'non- manipulationist' account hereby proposed aims to do that.

Mill's Bioeconomics

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June 29. 10:30-12:30 VC 206

John Stuart Mill's Principles of Political Economy (1848) was the dominant text in economic theory for about thirty years. But its subtitle: With Some of Their Applications to Social Philosophy also captures well the sense in which Mill sought to extend economics into a broader domain of problems. Mill was concerned with what he called "The Art of Living" and this was grounded to a large extent on deeper commitments to our place as one species within the organic world. This paper will attend to some of the respects in which Mill injected biological concepts into his economics. As a result, it will argue that bioeconomics as a pursuit is of much longer standing, and that Mill had thus undertaken some very important alterations to the scope of economics. His worries about environmental decay and declining biodiversity more specifically are closely linked to his economic analysis and in that respect he provided a much broader vision than the neoclassical economists of the last third of the nineteenth century. Although there is some scholarly literature on Mill's interactions with Spencer and Darwin, there is little that takes up the evolutionary thread in his economic thought. Moreover, Mill treated capital in a highly unorthodox manner, as a process of what he called "perpetual reproduction". Unpacking this concept, which I will argue Mill intended as literal, will also serve to understand the extent to which Mill positioned economics as conjoined with the natural sciences, and biology more specifically. In sum, his pursuit of a broader social philosophy with practical applications drew significantly on his understanding of economics as a part of a broader biological domain.

Mechanisms and the pragmatics of model explanation

Maria Serban
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June 29. 2:00-3:30 VC 211

Despite the growing recognition that models play a wide range of functions in scientific practice and theorising, there is still no explicit consensus on whether models also have a genuine explanatory function within scientific investigation. One recent proposal for understanding how models contribute to the explanation of particular phenomena of interest in science is the mechanistic account. On this account, a scientific model plays a genuinely explanatory role if and only if it describes or reveals the causal mechanisms maintaining, producing, or underpinning the behaviour of the target phenomena.

To a first approximation, mechanistic explanation is a form of decompositional, constitutive explanation which consists in accounting for the behaviour (or function) of a complex system in terms of the behaviour (functions) of its component parts, their properties, relations, and their modes interaction or organisation. The new mechanists (e.g., Bechtel and Abrahamsen 1993/2010; Machamer, Darden, and Craver 2000; Craver 2006, 2007) have argued that this conception of explanation is particularly adequate for scientific fields such as molecular biology, neurophysiology, etc. and that it should be extended to cover all explanatory practices within cognitive science.

In particular, Craver and Kaplan (2011) have proposed that dynamicist and other mathematical models used in cognitive and systems neuroscience fall short of being genuinely explanatory because they do not exhibit or specify the real causal mechanisms underlying the target phenomena. They cash out their mechanistic commitments in terms of a model-to-mechanism-mapping (3M) constraint. According to this requirement, a model is explanatory to the extent and only to the extent it exhibits a mapping between parts of the target system and their organisation, on the one hand, and parts, properties, and relations-variables in the model, on the other hand.

I contend that the 3M constraint imposes too stringent of a requirement on something to count as an explanatory model of a target system. Moreover, I claim that it fails to distinguish between mechanistic descriptions tout court and genuine mechanistic explanations. The structure of my argument is threefold. First, I emphasise an important tension internal to the mechanistic account between the pragmatic or epistemic criteria for individuating good mechanistic explanations and the notion of a hierarchy of not-yet explanatory mechanisms which culminates in the real, causal mechanisms that underpin the phenomena of interest. I seek to show that these two components of the mechanistic picture undermine the direct application of the 3M constraint. Second, I insist that the 3M constraint distorts our understanding of how and why mathematical models are used in the special sciences such as systems and cognitive neuroscience. And, third, I propose that the explanatory function of models can be understood in terms of a more minimal criterion. Namely, I contend that a model can be said to be explanatory when it captures relevant stable counterfactual dependences between the variables of a target system. I conclude that while the search for mechanisms is an epistemically worth pursuing endeavour, it does not licence the hegemonist yearnings of the mechanistic program with respect to the explanatory practices of cognitive science.

The Role of Values in Science and Policy

Neelam Sethi
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June 28. 2:00-3:30 VC 212

A central critique of feminist philosophy of science is that because feminist science rests on feminist values, it cannot possibly be objective and therefore cannot produce legitimate knowledge. Elizabeth Anderson has argued at length about the mistake critics make when they deny that values can help produce objective science. She argues that the real concern of these critics is not that "scientific theories have evaluative content, but that they might be held dogmatically" (Anderson, 2004). Anderson's central point is that because values have cognitive content, argument and evidence can prompt people to abandon or revise their value judgments. Just as science is not value-free, values too are not science-free; indeed, the influence of facts and values is bidirectional. It follows from this claim that if values are not "science free" and are open to revision (i.e. they need not be held dogmatically), then it is possible to legitimately use values in science provided they "do not drive research to a predetermined or favored conclusion."

In line with this argument, I make the further claim that some contextual values can in fact lead to more fruitful research. I consider the case of family planning programs in Mexico. In the 1990s, rural and marginal populations in Mexico were especially targeted by family planning programs. Traditional studies on this issue highlighted empirical evidence that showed that increased availability of contraception reduced women's fertility, concluding that lower fertility increased the women's well-being, where well-being was defined in terms of objective criteria such as the added time and resources made available to women who, because of their use of contraception, have fewer children. A more recent study drawing on data from six rural communities in Chiapas, Mexico examines the validity of the presumed positive relationship between contraception use and women's well-being, where well-being is construed on a different set of preferences. By looking at the assumptions and values of these two studies, I engage with Anderson's claims about values and science in a specific empirical context. I then go on to show how philosophy of science can help build bridges between science and policy decisions.

Citizen science, professional scientists: Epistemic and ethical issues in public participation in scientific research

Janet D. Stemwedel
San José State University

June 27. 2:00-3:30 VC 212

Recent years have seen a marked increase in "citizen science" projects, including many in which amateur volunteers collect and report data (as in the Audubon Society Christmas Bird Count) or classify large amounts of data collected by professional scientists (as in the Galaxy Zoo projects). Citizen science has been variously described as a strategy for increasing public understanding of science, a way to "crowdsource" large computational or classificatory tasks which outstrip the available professional scientific labor pool, and even as a way to democratize science.

Scientists designing projects making use of public participation have devoted a good deal of thought to identifying tasks amateurs can reliably perform and working out mechanisms for training volunteers and assessing the quality of the data they collect or the interpretations they make. However, less attention has been paid to the role of the citizen scientists in the epistemic community of the professional scientists, or to their interests as participants in scientific research.

In this paper, I consider some of the epistemic and ethical issues that arise from different types of scientific research harnessing the efforts of amateur participants. I examine what the professional scientists hope to gain from the contributions of amateurs, as well as the sorts of criteria their projects and participants must meet to produce reliable results. I also discuss what the citizen scientists might reasonably hope to gain from their participation in scientific research projects. Finally, I consider ways in which the interests of the citizen scientists and the professional scientists are in tension, as well as the impact that these competing interests could have of the character of the knowledge built in these projects.

Since they stand outside the professional community of science, citizen scientists cannot reap the same rewards for participating in research as professional scientists. As volunteers, they are not paid for their efforts, and even if they were included as authors on published results this would not confer the same benefit for citizen scientists as it does for professional scientists. Arguably, the reward the citizen scientist expects is a better understanding of scientific knowledge-building. Yet, it is unclear that such understanding is a reliable outcome of the citizen scientist's activities.

To advance their interest in reliable data or analysis, professional scientists structure projects to maximize uniformity of the activities of the citizen scientists. But does treating citizen scientists essentially as well-calibrated measuring devices show sufficient regard for the interests of the citizen scientists? Can science secure the benefits of outsourcing labor to amateurs while including these amateurs in the epistemic community of professional scientists in a meaningful way?

Models of Interventions

Susan G. Sterrett
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June 28. 10:30-12:30 VC 206

When scientific knowledge is to be put into practice in an attempt to deal with a large-scale problem such as an invasive species or global warming, there are not only scientific models of organisms, phenomena, and processes involved: there is also what might be called a *model of intervention*. Whether the model of the (planned) intervention is explicitly stated or not, it is rightly every bit as much a matter of concern and evaluation as the scientific models are. The notion of a model of intervention occurs in the behavioral sciences; I am interested in how the notion applies to attempts to deal with large-scale environmental challenges such as invasive species or global warming.

In this talk, I consider what's involved in constructing (or reconstructing) a model of intervention. Illustrating with examples from historical case studies, I conclude:

(i) Models of interventions of such large-scale applications of scientific knowledge are necessarily interdisciplinary models. They must be informed by the natural sciences related to both the means and the goal of the intervention, yet even this is not the whole picture: those planning such large-scale interventions cannot escape questions about the sort of political and/or administrative powers required to implement the plan.

(ii) Models of interventions of such large-scale scientific activities require knowledge at a variety of levels. Their success may turn on detailed knowledge about scientific minutiae: e.g., about habits of insects of a particular species, or about how aerosols travel in various atmospheric layers in particular conditions.

(iii) Knowledge from various disciplines and levels must be integrated in such a way that: the relevant facts needed to put an effective model together are identified, the modeler distinguishes what is possible from what is not, identifies the kind of cooperation that must be sought from various agents, and pays attention to the time scale on which the intervention must occur in order to be effective.

I draw on three controversial case studies of large-scale models of interventions in the course of reaching these conclusions. First, I examine two historical case studies of interventions, one of which was successful (Malaria Eradication in Indonesia and Asia), and one of which was disastrous (The U.S. Fire Ant Program), with an eye to identifying important points on which the success or failure of the program turned. In particular, I note that in the case of malaria eradication, it was important to distinguish the goal of *malaria* eradication from the goal of *mosquito* eradication. In the case of the failed fire ant eradication program, it is fire ant *balance*, rather than fire ant *eradication*, that is the proper goal of a fire ant management program. Then, I explore how the points from these two historical case studies on which some perspective has been gained might be helpful in evaluating a model of intervention for a rather extreme proposal that is currently arousing controversy (injecting sulfate aerosols into the atmosphere to block sunlight in an attempt to counteract global warming).

Computational Models as Experimental Systems

Catherine Stinson
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June 29. 10:30-12:30 VC 323

The assumption that computational models are representations runs deep. In some sense it is obviously true, but in many cases the fact that the model is a representation has very little to do with how inferences are drawn from the computational model to the target system. In this paper I investigate cases where the role computational models play in scientific investigations has little or nothing to do with their being representations.

I argue that connectionist models are used in cognitive neuroscience as an experimental system rather than as representations of brains. The pattern of inferences and their justifications are similar to those used in experiments with model organisms. Computational models are used as physical instantiations of particular kinds of networks, just as mice are used as examples of mammals, or macaques are used as examples of primates. The results of "computational experiments" are treated as observations of how particular kinds of networks behave, and the inferences drawn from them are of the same types as inferences from observational data in other experimental contexts. Observations of samples are generalized to populations, generalizations are applied to instances, similar effects are inferred from similar causes, functions are inferred from structures, and so on. What are not found are inferences that invoke representations in any essential role.

An extended debate in the philosophy of mind literature about the use of connectionist models focuses almost entirely on the issue of representation. That debate seems to have mostly fizzled out without being resolved. I show that progress on this question can finally be made by acknowledging that despite the fact that computational modeling necessarily requires the trappings of representations, these representations do not play any significant role in the practice of connectionist modeling. Focus on the issue of representation has led to a misunderstanding of the epistemological practices of computational modelers in cognitive neuroscience.

Measurement Uncertainty and Modeling Uncertainty: Towards a Unified Account

Eran Tal
Bielefeld University

June 28. 10:30-12:30 VC 206

Quantitative scientific results are often reported with uncertainty estimates or 'confidence intervals'. Whether such intervals are comparable to each other is not always clear, especially when the results are produced by different kinds of methods, e.g. when statistical predictions of ocean temperature are compared to infrared satellite measurements, or when estimates of chemical properties obtained by ab initio simulations are compared to experimental estimates obtained in the laboratory. The mutual compatibility of such results depends on their respective margins of uncertainty; however, when uncertainty originates from different sources and estimated by different methods the legitimate worry arises that reported margins lack a common measure. In the absence of a principled method of scaling uncertainties from different sources, it is difficult to tell apart genuine agreement from overestimated uncertainty and genuine disagreement from underestimated uncertainty. Moreover, it is difficult to identify which of several inconsistent results require correction and to assess the extent of the corrections required.

A precondition for solving the problem of uncertainty comparison is a unified concept of uncertainty across experimental and theoretical sources. A central challenge in developing such a concept is that 'measurement uncertainty' and 'predictive uncertainty' are traditionally conceived as exclusive categories: the former represents an objective property of measuring instruments (e.g. the variance of instrument indications), whereas the latter represents subjective confidence in the consequences of a theoretical model. This paper offers an alternative to the traditional divide by drawing lessons from recent developments in metrology, the science of measurement and standardization. In 1995 a committee of the International Bureau of Weights and Measures published the Guide to the Expression of Uncertainty in Measurement (GUM), marking a shift away from empirical conceptions and towards an inferential view of measurement uncertainty. Under this new conception, measurement uncertainty is a special case of modeling uncertainty where the model in question represents a measuring apparatus. Measurement uncertainty arises from limited knowledge of the exact values of various parameters in the model of the apparatus, including environmental factors, fundamental constants and dimensional units. The application of this new approach is exemplified here with case studies of the standardization of atomic clocks at the US National Institute of Standards and Technology (NIST) and the standardization of coordinate measuring machines at the German Physikalisch-Technische Bundesanstalt (PTB).

Building on the approach detailed in the GUM, I develop the conceptual groundwork for a unified epistemology of uncertainty that is able to deal with the problem of uncertainty comparison. I introduce the notion of second-order uncertainty, i.e. uncertainty about uncertainty estimates, and show how it can be employed to decide which of several first-order estimates is likely to require revision in case of disagreement. A central consequence of my account is that it is epistemically justified to correct a measurement outcome to make it agree with a theoretical prediction, as long as that prediction is associated with a suitably low second-order uncertainty. I conclude the paper by briefly discussing the implications of this consequence for contemporary theories of confirmation.

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DSM-5 and Psychiatry's Second Revolution: Theoretical vs. Descriptive Approaches to Psychiatric Classification

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June 28. 4:00-5:30 VC 215

This paper examines the question of how mental disorders should be classified with reference to the Diagnostic and Statistical Manual of Mental Disorders (DSM), which has been published regularly by the American Psychiatric Association since 1952 and is currently in its fourth edition. The forthcoming publication of the fifth edition of the DSM (DSM-5), which is scheduled for 2013, has generated considerable controversy among psychiatrists and theorists of psychiatric classification. A large part of this controversy stems from the stated intention of the authors of DSM-5 of moving away from the purely descriptive and 'atheoretical' approach to classification—that has been championed by the DSM since the publication of DSM-III in 1980—towards a more theoretical approach to psychiatric classification.

This paper aims to clarify and critically evaluate the relative merits of theoretical versus descriptive approaches to psychiatric classification, while considering the epistemic and pragmatic constraints facing a system of psychiatric classification. I begin by discussing the historical factors that led to psychiatry's 'first revolution in classification,' in which the authors of DSM-III (1980) rejected the theoretical and etiological approach to classification adopted in DSM-I (1952) and DSM-II (1968) in favor of a purely descriptive approach. I subsequently argue that the historical reasons motivating this shift to a descriptive approach (e.g., increasing the reliability of diagnostic categories, removing speculative psychoanalytic assumptions) are no longer relevant in the context of DSM-5. From this standpoint, I argue that a theoretical and causal approach to psychiatric classification would offer a more promising approach than a purely descriptive approach for meeting the DSM's goal of providing a diagnostic manual that can facilitate research on mental disorders. However, a major problem with the DSM is the ambitiousness of its goals, and I suggest that the DSM would benefit by deflating and narrowing its goals. In particular, I argue that the DSM's explicit aim of providing a manual that can facilitate (1) research on psychopathology, (2) the treatment of individuals with mental disorders, and (3) communication among mental health professionals will often conflict and that the DSM's assumption that a single manual can accommodate all of these goals is untenable. From this perspective, I discuss the costs and benefits of descriptive versus theoretical systems of psychiatric classification for these various goals. Although I advocate a theoretical approach to psychiatric classification for purposes of facilitating research, I argue that to make genuine progress on psychiatric classification, psychiatry requires a paradigm shift in the direction of pluralism. In particular, psychiatry must embrace the proliferation of multiple, sometimes conflicting, systems of classification that are formulated for well-defined goals (e.g., the RDoC classification system advanced by the National Institute of Mental Health that has been formulated specifically for research purposes). Hence, while DSM-5 may be prove to be beneficial for furthering research in its shift to a theoretical system of classification, I suggest that the DSM requires a significant reorientation in terms of the goals that the manual is conceived to serve.

Untangling Fitness, Well-Being and Health in a Multidisciplinary Evolutionary Biology Collaborative Project

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June 28. 10:30-12:30 VC 212

In this paper I discuss my work on clarifying the relationships between the concepts "fitness," "well-being" and "health" during my work on an ongoing collaborative project, run through the National Evolutionary Synthesis Center. The project aims to create rigorous theoretical and methodological guidelines for research on the phenomenon "evolutionary mismatch," which has been the subject of various disjointed scientific studies. "Mismatch" is the term applied to cases in which a trait evolves in one environment but then the trait becomes more harmful when its bearer moves into a new environment (where the trait is now mismatched). While the multidisciplinary biology team already had two philosophers of biology attached to the project, they chose to add a philosopher of medicine (this author) since the team had an interest in studying mismatch's effects on not just fitness, but also on well-being/welfare.

Responding to the group's interests in both fitness and well-being, I have advocated for splitting "mismatch" into two distinct types: 1) mismatch with respect to fitness, and 2) mismatch with respect to well-being. A single mechanism may cause one or both of the two types, but the two are not inherently tied together. For example, partially heritable disease states (traits) primarily affecting elderly individuals (Parkinson's, Alzheimer's, etc.) are more burdensome in the environments of wealthy contemporary societies than they were in ancestral environments, due to recently extended lifespans. These diseases have little impact on fitness, since they largely impact post-reproductive individuals, but have enormous impact on well-being.

By taking up "well-being," the team has also committed itself to an epistemic task (laying out methodological and evidentiary guidance for research programs) that has a large and inextricable normative component. What is "well-being" and how is it measured? I argue that "well-being," in this context, can be treated as effectively synonymous with a holistic definition of "health." I have particularly proposed to the team that we apply a working definition of "well-being" along the lines of the World Health Organization's famous "health" definition, "a state of complete physical, mental and social well-being and not merely the absence of disease or infirmity."

I show how interpreting "well-being" as effectively synonymous with "health" allows the team to draw upon measurement and analysis tools from medicine, addressing one the scientists' most pressing concerns: how the philosophical challenges of studying the important, but amorphous, concept "well-being" can be managed during the creation of guidelines for applied evolutionary biology research programs. I argue that linking broad "well-being" concerns to health research allows the team to gain philosophical clarity on how to approach "well-being" and also aids in the creation of workable research programs studying mismatch's effects on well-being, in part by allowing the use of existing health metrics such as "quality adjusted life years," along with related philosophical literature. These challenges illustrate a key feature of my role as a philosopher on a team of mostly scientists, the need to promote philosophical rigor while simultaneously furthering the team's ultimate practical goals.

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In Praise of Erroneous Models

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June 28. 2:00-3:30 VC 211

Most historians of science have a favorite example of a now-discarded model whose errors themselves offered a fundamental insight. These are not mere cases of imperfect models, in the way that Copernicus' model held almost as many inaccuracies as insights, which were refined by later thinkers who nonetheless gave Copernicus credit for getting the fundamentals correct. Instead these are models that have since been resoundingly overturned, but not before something really fundamentally useful had been wrung out of the mistake. Kuhn described the Leyden jar in this way — a groundbreaking instrument first developed because it was believed that electricity was a fluid — why else you build a jar to put it in? Or consider Carnot, who seems to have been somewhat agnostic about caloric, but made extensive use of that "fluid of heat" theory, vividly describing the "flow" around the engine. Caloric was already a theory on the decline when Carnot published his "Reflections on the Motive Nature of Heat" in 1824. But the metaphor of a fluid only "falling" naturally from a higher place to a lower one, creating work not by being consumed but by it's transfer of location was a profound inspiration: it was both comfortably familiar to anyone who had ever seen a water wheel, and a such a significant leap forward that Carnot is usually credited with the first formulation of the second law of thermodynamics. A more current example is that of the all-pervading emphasis on homeostasis in ecosystem ecology in the 1960s and 70s. This tendency for biological systems to resist change and to remain in a state of equilibrium was stressed in all the best-selling textbooks of the period. The presumption that a system in equilibrium will remain in equilibrium unless acted upon by an outside force led directly to the development of quantitative methods of calculating material and energy cycling through the trophic levels of a system. While faith in ecosystem stability has been steadily undermined in the last 50 years, calculating movement of energy and material through trophic levels remains fundamental to the discipline.

For the purpose of philosophy of science in practice we might to consider whether these examples are merely lucky breaks, isolated incidents that in retrospect happened to yield a useful insight or two that could be reimagined and redesigned to fit into a "new and improved" theoretical conception. It is possible, too, that they are merely a remnant of Whiggish tendencies in the history of science, of preferencing the "correct bits" of quaint old theories in light of modern answers. But I want to argue that the existence, and prevalence, of these examples suggests that erroneous models offer something more valuable — an alternative lens on which to focus on a problem, or an alternative metaphor to spark our imagination. By considering these examples in more detail I will suggest that we ought to give our outdated models rather more epistemic significance than they are usually accorded in the philosophy of science.

What comorbidity tells about diagnoses in psychiatry

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June 28. 4:00-5:30 VC 215

The frequent occurrence of comorbidity — the presence of two or more disorders in one individual — is one of the issues puzzling professionals and researchers in psychiatry. High rates of comorbidity are reported regularly. Epidemiological studies suggest that up to 45% of psychiatric patients satisfy the criteria for more than one disorder within one year (Bijl 1998, Jacobi 2004, Kessler 2005). Examples of disorders co-occurring frequently are depression and generalized anxiety disorder (Andrews 2002). Patients suffering from both of those disorders tend to have a poorer prognosis and a disproportionally higher functional disability than patients suffering from only one disorder (Schoevers 2005). Comorbidity's high prevalence and its influence on disease severity and treatment programmes make it an important subject to study.

Comorbidity is indeed hotly debated in psychiatry. One debate concerns the question whether comorbidity is problematic for the validity of the current diagnostic system, the DSM-IV (Kendell & Jablensky 2003), and whether it can be used to reclassify disorders (Andrews 2009). In a previous paper we showed that all parties in this debate share particular assumptions on disease models and causality (Van Loo 2012). A related debate concerns the reality or artificiality of comorbidity. Some authors argue that high comorbidity rates are a by-product of our current diagnostic system only, and can be traced back to conventions in the classification choices (Maj 2005, Vella 2000, Aragona 2009). For instance, if we make our classification system more fine-graned and include more diagnoses, it becomes more probable that individuals have more than one disorder (Batstra 2002, Vella 2000, Maj 2005). Against this, other researchers in psychiatry contend that comorbidity is a real phenomenon tied up with the nature of psychiatric disease itself, pointing to commonalities in the causal background of different disorders (e.g. Andrews 2009). According to these authors high comorbidity rates are "real expectable features of [the] psychiatric domain" (Zachar 2009, 13; Zachar 2010).

Our paper focuses on the question to what extent comorbidity is due to conventions in the classification system, or a real phenomenon in the psychiatric domain. We will argue that neither view can fully explain the high rates of comorbidity, and that a middling position provides more insight into the nature of psychiatric diagnosis. We contend that the status of the DSM is best compared to that of geometry for physical space: it offers a robust picture of reality, but only relative to a number of coordinative definitions (cf. Reichenbach 1958). This position is illustrated by an empirical study: using data from the Netherlands Mental Health Survey and Incidence Study (NEMESIS, Bijl 1998) we show that comorbidity cannot be the result of classification choices only, nor of causal structures underlying psychiatric disorders. Finally, we confront these insights with the opposition between realists and constructivists (cf. Hacking 1999) concerning mental health, and argue that our middling position provides a more fruitful starting point for improving treatments and furthering research than positions towards the endpoints of the realist-constructivist spectrum.

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Scientific practice as collective undertaking: reflections on the concept of 'community'

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June 28. 2:00-3:30 VC 206

A genuine Philosophy of Science in Practice needs to reflect, and develop, the conceptual grounds on which it stands. Therefore, the question I want to raise is the following: With what conceptual tools can we approach scientific practice? More precisely, what vocabulary do we need to describe and analyse the socio-epistemic dynamics of science-in-the-making? To specify my concern even further, what unit of sociality, what social structure can we chose to investigate science as a collective endeavor?

Recently, numerous epistemologists of science have criticized the individualist bias of their field and paid increasingly attention to research teams (e.g. Matthiesen 2006, Wray 2007, Rehg et al. 2008, Fagan 2011). At the same time, philosophers of science in practice have, albeit sporadically, emphasized the need to revisit and elaborate on approaches which focus on the individual scientist and her engagement in second-person relationships (Chang 2011). Mostly, however, philosophers of science and social epistemologists have had the tendency to discuss science as a collective achievement primarily on a larger scale. Predominant so far has been an approach that focuses on 'community' and 'communties' as the relevant unit of study.

Clearly, we need a range of concepts to describe scientific practice as collaborative undertaking and we need a better understanding of their respective 'optics': of the resolution with which they allow us to see, of their strengths and weaknesses, the blind spots and biases which they might impose upon our investigations. As a first step towards this end, I will critically revisit the concept of scientific communities as exemplified in Kuhn's seminal work (1970). I argue that Kuhnian community accounts of science frequently make two idealizations both of which are legitimate and have proven helpful, but whose convenience should not lead us to endorse them blindly and adopt a biased image of collective scientific practice:

First, since peer communities are usually defined through what their members share (e.g. a common paradigm) they are characterized as overly cognitively homogeneous. Such characterizations run the risk to brush over conflicting or complementing social and cognitive differences between them. In an extreme form, such a bias can mislead to resume a perspective on science in which “totalistic” communities dominate individual scientists (Whitley 1984: p. 5).

Second, specialist communities are often deployed to denote a collective which contains and circumscribes the making of science in its supra-individual dimension completely. In this vein, specialist communities are thought of as self-sufficient and autonomous. Based on this idealization of self-containment, communities are employed as containers of those factors which serve as explanans in accounts of collective science, such as e.g. values, necessary background assumptions or the efforts which it takes to defend those. However, the picture of specialist communities as closed, self-sufficient and homogeneous impairs philosophy's capability to explain the dynamics of scientific change, the impetus of interdisciplinary research and the scope of individual creativity in science-in-the-making (cf. Calvert-Minor 2011).

To conclude, I will point out that alternative conceptual foci such as research teams or individuals in interaction do not allow for the two above-mentioned idealizations to be made. This can be an advantage for fine-grained case studies of scientific practice on social micro scale. Resorting to groups and individuals instead of peer communities in order to describe the collective dimension of science can come with the opportunity to shift emphasis from 'the shared' to 'the complementing' and from 'the self-contained' to 'the porous'.

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Collaborative Research and its Impact on Theoretical Innovation

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June 28. 2:00-3:30 VC 206

In some scientific fields over 80% of the publications are co-authored. And the list of authors for some articles cannot even be contained on a single page, as research teams get increasingly larger. Much of what we know today is a consequence of collaborative research. The only way we can effectively investigate parts of nature is to tackle research projects as teams of scientists. The variety of skills and background knowledge needed force this way of research upon us. Even the collection of some data requires years of work, work that no single scientist could be expected to complete on their own.

But there is reason to think that collaboration in science may threaten to undermine the effectiveness of science. For example, elsewhere I have argued that collaborative research undermines the accountability of scientific authors. Collaborative research teams have a way of evading responsibility when problems are found with the results reported in a co-authored article. And collaborative research projects can be a source of anxiety for scientists who fear they may not be given adequate credit for the work they do as part of a collaborative research team. Again, elsewhere I have suggested that this anxiety may discourage scientists from developing certain skills, skills that relegate them to support roles in research projects that are directed by others who stand to gain proportionally more credit for the contributions they make to a research project.

In this paper I want to examine the extent to which collaborative research stifles innovation. First, group dynamics may threaten innovation. Some groups are prone to “groupthink,” where dissenting views are voluntarily suppressed in an effort to ensure harmony in the group (see Solomon 2006; also Hudson 1996). If this does occur, we are at risk of discouraging criticism which plays a crucial role in the advancement of science. Second, the financial and emotional investments scientists make into specific lines of research may prevent them from changing direction even when such a change is needed. Because collaborative research often involves the investment of enormous financial, material, and other resources, such research projects can take on a life of their own, and create a social climate that stifles innovation (see Fuller 1999). Turning back no longer seems like a live option.

My aim is to evaluate the extent to which collaboration and Big Science threaten innovation in science, in particular the sorts of large scale theoretical innovations that are in general unsettling to scientists. I want to determine the extent to which the social structure of science is fit to combat these tendencies that threaten our pursuit of knowledge.

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Conventionalism as Contingency

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June 27. 10:30-12:30 VC 211

From the 1980s onwards, structuralists, inspired by the work of Thomas Kuhn, have emphasized the importance of scientific practices for “post-positivist” philosophy of science. This interest in scientific practices, the so-called “practice turn”, has to be understood from a prospective or a forward-looking analyses of science. For instance, Pickering's (1984 p.8) “goal is to interpret the historical development of particle physics, including the pattern of scientific judgments entailed in it, in terms of the dynamics of research practice (my emphasis).”

Contrary to the more traditional retrospective or backward-looking view on science, in this prospective framework the investigator puts herself beside the working scientists and from the cutting edge of science, she looks into the future. Similarly, Pickering (1995, p.3) seeks a “real-time understanding of practice”, a historical account of science in contemporary terms. Accordingly, many constructivist studies in science take the process of science as their object of study and they emphasize, besides scientific practices, tacit knowledge, know-how, experimenting, expertise, social and historical components, and thus the contingent features of science. (cf e.g. Nickles 2008 for the difference between retro- and prospective projects in philosophy of science).

Interestingly, Hacking in his treatise on social construction (1999) considers “contingency” to be the sticking point #1 (p.31) in the discussion between someone who “argues that scientific results, even in fundamental physics, are social constructs,” and his opponent who “protests that the results are usually discoveries about our world that hold independently of society. (p.4)” Unfortunately, the restricted attention given to the contingency question in science has as yet failed to bring forward a general accepted conceptualization of the issue (cf. Soler 2008, Martin 2012). Most generally we may say that contingentists and inevitabilists discuss the question to what extent the accepted results of successful science are contingent or inevitable.

One of the well-known complaints about retrospective science analyses is that “[m]issing from the [retrospective] scientist's account, then, is any apparent reference to the judgments entailed in the production of scientific knowledge. Pickering (1984 p.7)” My aim in this paper is to bring in a subject from retrospective philosophy of science where the judgments of scientist was extensively discussed indeed. I want to focus on the differences and similarities between the modern discussion about contingency in science and traditional conventionalism (Poincaré 1905) and some of its thoroughly discussed variants (Poincaré 1905, Einstein 1951, Dingler 1933, Grünbaum 1963).

The reason to compare conventionalism and contingency is to broaden the scope of the latter and adding a well-developed debate and its results about the various ways in which scientific judgments and choices (in this case between geometries) played different roles in the production of scientific results. Doing so I will broaden and refine the taxonomy of Joseph Martin (2012). Two other appetizers relevant for the constructivist discussions are: Hugo Dingler tried to prove that physical geometry is inescapably Euclidian since the measurement apparatuses are build upon the assumption that geometry is Euclidian, and Robert Cohen (1963) discussed the tensions between conventionalism on the one hand and materialist empiricism on the other. Moreover, the contingency in science as a so-called sticking point between constructivists and scientists seems to be an excellent environment to study the intricate ways how the retrospective and prospective projects in the analysis of science can and should be interlinked. Conventionalism helps to flesh out one of the linkages. A final provisional result of my paper is that the similarity between traditional conventionalism and more modern contingency gives well-elaborated support to the pluralist approach to science. Besides incorporating Carnap's pragmatic pluralism (1934, Principle of Tolerance, 1950 the plurality of possible “linguistic frameworks”) it does help to distinguish between cases such as (1) the choice between observationally equivalent theories ((non)-Euclidian physical space, and perhaps the Bohm and Copenhagen interpretation QM) (2) establishment of theoretical facts by theory-laden interpretations of experimental outcomes (Pickering's construction of quarks, the weak neutral current), and (3) scientific results for which the experimental data are much less theory-laden such as the discovery of plate tectonics of the elliptic trajectory of Mars.

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