4th Sydney-Tilburg conference on the philosophy of science

"The Authority of Science"
8-10 April 2011, University of Sydney

Abstracts

Keynote speaker

Session: Keynote **Room: 106** **Time: Sun 9.00-10.00**

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Evidence and Policy Formation – The Challenge of "Post-Normal Science"

Plenary speakers

Session: Plenary 2 **Room: 106** **Time: Sat 1.30-2.30**

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The Voice of Science in Society

Science and the technologies it spawns are integral to nearly every element of modern life. Decisions that involve scientific and technological considerations are being made daily at many levels of society. They occur in law, medicine, business, legislation, regulatory rule-making and other realms. At a personal level, science impacts on religious beliefs, decisions with respect to health matters, and how we deal with a variety of economic and ethical issues. Yet the expert or moral authority of science is frequently questioned and often rejected outright.

Science is but one voice among many. To exercise influence in society, it must often challenge the authorities of competing social sectors. The arguments advanced by science in debates about many social policies as well as legal and ethical practices often generate conflicts with other traditions. For example, conflicts between science and religion are an obstacle to full social acceptance of theories and experimental findings widely held within science. As another example, scientific findings in genetics and the behavioral sciences have challenged prior understandings of race, gender or mental illness, with all of the attendant implications for social policies and practices. In this lecture I will discuss the concept of authority: what kinds of authority there are; how science justifies its authority; how its exercise of authority is limited, and the distinction between epistemic and moral authority. Some recent examples of the exercise of both epistemic and moral authority by science lead to consideration of the idea of cultural authority and whether or how science might acquire a stronger voice in societal affairs.

Session: Plenary 1 **Room: 106** **Time: Sat 9.00-10.00**

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Session: Plenary 3 **Room: 106** **Time: Sat 4.30-5.30**

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Rescuing climate science from politics

Scientific American recently featured an online poll on global warming. The poll apparently found that 56.1 per cent of *Scientific American* readers believe that the Intergovernmental Panel on Climate Change (IPCC) is 'a corrupt organisation, prone to groupthink, with a political agenda.' Furthermore, in response to the question 'Which policy options do you support?' 42 per cent of the respondents chose the answer 'keeping science out of the political process.' In response, Andrew Leonard writing in *Salon* in an article entitled 'Barbarians at the climategate' exclaims:

> 'Say what? Keep science out of the political process? *Science?* I thought it was supposed to be the other way around; that the goal was the keep politics out of science... to flat-out reject science as a guide to policy is beyond medieval. It's a retreat to pure superstition, a surrender to barbarism. We might as well be reading omens in the entrails of sacrificial animals. Our wealth as a country, our incredible technological wonders – the Industrial Revolution! – were built upon scientific discovery.'

How did we get to this point? Following the 2009 'Climategate' debacle, the Intergovernmental Panel on Climate Change's was found in 2010 to have made an unsubstantiated finding that the Himalayan glaciers would melt by 2035. As a result, not only has the reputation of climate science been substantially diminished but climate scientists have come under increasing pressure. Witness what has happened to Michael Mann, one of the climate scientists implicated in 'Climategate'. His university, Penn State, received numerous emails, phonecalls and letters obliging it to investigate him for research misconduct, albeit that in 2006 the US National Academy of Sciences concluded his scientific methods fell well within accepted practice. The University cleared him of all wrongdoing. Yet, in May this year, the Virginia Attorney-General issued a Civil Investigative Demand for documents against the University of Virginia, Mann's former employer, alleging that Mann used five taxpayer funded research grants to undertake his 'fraudulent' climate change research. Worse still, in February, US Senator Inhofe, a well-known climate skeptic, threatened criminal prosecutions against 17 climate scientists, many of them lead authors of IPCC reports, for alleged breaches of the *FOI Act*, *President Obama's Transparency and Open Government Policy*, the *Federal False Statements Act*, the *False Claims Act (Criminal)* and *Obstruction of Justice: Interference with Congressional Proceedings*.

The pressure placed on climate scientists is not new and, moreover, it's inevitable. In 2006, lawyers, scientists and philosophers in Wendy Wagner and Rena Steinzor (eds) *Rescuing Science from Politics: Regulation and the Distortion of Scientific Research* showed how whenever science interfaces government regulation – tobacco, pharmaceutical drugs, pollution – scientists' work is deconstructed and dissected. Research time is taken up responding to requests for access to data while the essential underpinnings of methodologies and findings are queried by scientists employed by those threatened with regulation. While the regulatory battle over climate change has been underway for some time, what is new is the power of the blogosphere. Here climate scientists are abused via email and 'unmoderated' comment stands alongside peer reviewed climate science. Consequently, as James Hansen, the world's leading climate scientist at NASA, observed recently in Sydney 'in the last six months the gap between what is *known* by scientists and what is *understood* by the public has widened.'

How should the law respond to this seeming crisis in climate science? The private law tort of 'civil conspiracy' has already been attempted against ExxonMobil for misleading the public about climate science. It has been suggested that the fossil fuel industry's misinformation campaign amounts to a 'crime against humanity'. What possible remedies does public law offer? What science and public law share is a demand for openness, transparency and accountability. Is it time for scientists, universities and legislators to discuss whether FOI exemptions, which protect certain types of government information, ought to extend to protecting aspects of the scientific endeavour, such as 'raw data'? What should be done about the intimidating yet anonymous cyber-bullying which climate scientists face around the world?

Finally, beyond the reach of the law what is the media's ethical responsibility in all of this? Dr Lesley Cannold, a researcher and medical ethicist, reported recently in the *Sydney Morning Herald*, that Lord Monckton, a climate change denier, appeared in the media 455 times during his Australian visit while the visit by James Hansen, referred to earlier, warranted only 61 mentions.

This paper will attempt to offer some insights into the vexed issue of protecting climate science from politics.

Papers

Session: A2 **Room: 102** **Time: Sat 10.30-12.00**

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Science as a rhetorical device: The authority of 'science' in the evolution/creation debates

One recent debate that beautifully illustrates the authority of science is that between creationists and those in favour of teaching evolutionary theory. Despite continuous creationist claims to the contrary, there exists no controversy in the scientific community over evolutionary theory; the debate takes place entirely within the public domain. Because of the authority science is credited with within public space, each camp is careful to present its own position as genuinely scientific, and equally intends to portray its opponent's position as unscientific claptrap. On the one hand, creationists point out the alleged gaps in evolutionary theory to support the claim that evolution is 'just a theory', amounting to nothing more than naturalistic philosophy or a godless religion. Evolutionists, on the other hand, intend to demonstrate that creationists have only religion and not science on their mind, and that their scientific pretentions are absurd to the highest degree. The evidence, they explain, does not support a world-wide flood nor the intervention of an intelligent designer.

Apart from presenting real or supposed countervailing evidence, both camps often resort to yet another strategy to undermine each other's position. They put forth a definition of science and proceed to argue that the ideas of their respective opponent fail to meet the criteria set by that definition, and thus lack what it takes to be called a science. It is on this tactic we will focus our attention here, and this for two reasons. First, by these definitions of science , the authority of the term 'science' is invoked directly, without relying on those methods and properties science itself owes its authority to. Instead, in this public setting, science becomes a rhetorical device, a term you can define according to your needs.

Second, philosophers of science have been deeply involved in this debate. Some of them (e.g. Karl Popper) merely unintentionally, because at least one of the camps seized upon their definition of science. Others have been more actively engaged and proposed definitions by which creationism could be deemed unscientific. Both efforts have the unfortunate effect of giving the false impression that there exists a philosophical consensus on the definition of science or on the demarcation of science from religion or pseudoscience. Also, both bring yet another authority in play, that of philosophy (or the philosopher) of science, that, in this particular case, somehow derives its authority from science, but, again, does not result from the methods or properties the authority of science itself is based on. When philosophers act as experts in the debate, they reinforce both effects considerably.

We discern several disadvantages in the latter approach and we suggest that in this debate, by any available means (e.g. education, popular science, ...), science should speak for itself so that its authority can be understood and rightly assigned; simply invoking science as a rhetorical device tends to counteract its authority.

Session: A1 **Room: 100** **Time: Sat 10.30-12.30**

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Elements as Instruments of Analysis: Exploring the Authority of Instruments in Early Modern Chemistry

In his *Sceptical Chymist* (1661) Boyle advanced a fierce and influential attack against traditional Scholastic and Paracelsian matter theories through a pragmatic critique of chymical analytical methods and instrumental practices; in particular, distillation or 'fire analysis'. Towards the end of the seventeenth century, the German chymist Johann Kunckel criticized the legendary universal solvent of Paracelsus and Van Helmont, arguing that "if the alkahest dissolves everything, it should dissolve the vessel which contains it." Traditional distillation and solution techniques, however, persisted as the main analytical instruments in the hands of early modern chemists. Both fire analysis and alkahest, in various guises, were being defended and discussed well into the eighteenth century. The paper considers some of these tensions and their transformations: from Boyle's suspicion that fire—as an instrument of analysis—was creating new substances rather than decomposing old ones, to Herman Boerhaave's chemico-mechanical view of 'elements-as-instruments' alongside his promotion of solvents and vital 'menstruums' (1720s-30s). Set against various physical, metaphysical, technical, and medical concerns, these changes showcase aspects of the distinctive interplay between early modern chemical theory, experimentation, and expertise. The limits of chemical knowledge were being determined at once by both practical instrumental manipulations as well as by theoretical definitions of instruments.

Session: A2 **Room: 102** **Time: Sat 10.30-12.00**

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Nec plus ultra - restricting the epistemic authority of science

Philosophers of science have long been aware that the separation of science from non-science resists an easy and clear-cut solution. However, not only are the borderlands between science and pseudoscience often fuzzy, but the demarcation line between science and philosophy is also difficult to draw. As many contemporary discussions in philosophy of science bear witness, both domains have a considerable shared territory, in which philosophical and scientific strands are inextricably intertwined. Moreover, as Daniel Dennett remarked, "There is no such thing as philosophy-free science; there is only science whose philosophical baggage is taken on board without examination". Philosophical concepts and principles may derive support from scientific knowledge, and conversely, scientific theories invariably rest upon certain philosophical underpinnings.

Despite these and other complications, unfruitful attempts continue to be made to restrict the authority of science in largely artificial ways and disentangle it from philosophy. This has become particularly apparent in the ongoing debates about the conflict, if any, between science and religion. For example, a number of philosophers have argued that the epistemic reach of science is strictly limited to providing natural explanations for the natural world. From this perspective, interpretations of scientific knowledge (e.g. is evolution blind or goal-directed?) and questions relating to the implications of science for a broader picture of the world (e.g. was the world created by God?) belong squarely to the domain of philosophy and not that of science.

However, this division of labour between science and philosophy is largely artificial and disregards the many ways in which both are inextricably involved with each other. More importantly, narrowing down the authority of science so as to exclude all supernatural claims from its purview is unfeasible and historically inaccurate, as it ignores the fact that many such claims are perfectly testable and have been tested by straightforward empirical investigation (e.g. telepathy, the healing power of intercessory prayer, remote viewing). The position fails to distinguish between 'science' as a set of successful methods used to investigate the empirical world, and the corpus of established knowledge that has *resulted* from these methods.

Many critics of these efforts to reconcile science and religion have attempted to drag religious claims back into the realm of science, arguing that 'the God hypothesis' is not just a matter of philosophical speculation but can be rejected on purely scientific grounds (e.g. Richard Dawkins, Jerry Coyne, Taner Edis). Defenders of the first position have retorted that, by relying on the principles of parsimony and testability to attack religious claims, these attacks of religion have left the domain of science altogether and enter the realm of philosophy. Ironically, by struggling over the proper borderlines of science, both sides of the debate have unwittingly fuelled the common misconception that only that which carries the label 'science' can possess epistemic authority, whereas 'philosophy' is reduced to idle speculation or a matter of personal opinion.

Although we challenge the claim that religion and science cannot be in conflict, our intent is not to further *extend* the authority of science to the detriment of that of philosophy, but rather to make clear that there is simply no clear-cut way to disentangle both epistemic domains. This entails, inter alia, that safeguarding religion by artificially restricting the epistemic authority of science and philosophy, though convenient from a political and juridical perspective, does not hold up to philosophical scrutiny.

Session: A3 **Room: 105** **Time: Sat 10.30-12.30**

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Expert performance and the psychology of status

Typically, experts are defined by their qualifications, track record and experience. Society broadly expects experts to have privileged access to knowledge and to use it effectively. We evaluated how well experts perform on sets of questions and compared these results with the status of the experts. The relationship between status and actual performance is weak. The results suggest that the concept of expertise should be broadened to include people without relevant qualifications or extensive experience, if structured question protocols and feedback systems are used.

Session: A1 **Room: 100** **Time: Sat 10.30-12.30**

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Modelers and Makers, Midwives and Medical Men: Debating the Authority of Birthing Instruments in Enlightenment France

This paper sketches the material culture of 'obstetrics' in France through an examination of instruments presented in the 1760s and 1770s before two of France's most important medico-scientific edifices: the *Académie royale de chirurgie* and the *Académie royale des sciences*. The analysis will centre on the reception of four instruments whose uses ranged from the theoretical to the practical: the obstetrical 'phantom' (teaching dummy) of the itinerant midwife, Mme du Coudray; the life-size female waxworks of the modeler, Mlle Biheron; the breastfeeding pump of the Italian instrument-maker, M. Bianchi; and the 'pelvimètre' of the medical doctor, Pierre-Victor Coutouly. We will investigate these implements and their makers for the stories they tell us about the authority of both instruments and instrumental practice in the medical world of Enlightenment-era France. Valued for their pedagogical, aesthetic, and utilitarian virtues, these instruments resided at the intersection of conflicting approaches to birthing. While older traditions emphasized natural practices in the classroom and in the delivery chamber, newer ones embraced the apparent expediency and safety of artificial interventions. Through an exploration of the themes of manual dexterity, women's anatomy, and regimes of the 'natural order', it will be suggested that the stabilization of instrumental authority in obstetrical practice was ultimately frustrated by an imperfect understanding of the 'machinery' of the female body.

Session: B1 **Room: 100** **Time: Sat 2.30-4.00**

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Democracy deficits and epistemic deficits in the assessment of new technologies

Science-based regulatory frameworks used by most OECD countries preclude non-scientific considerations from the assessment of new technologies. This demarcation usually has a statutory basis. Yet as the decision stakes transcend the individual to the public level, and as deleterious consequences shift from the merely bad to the irreversibly catastrophic, the 'risk society' described by Beck is having a greater say in how technologies are assessed.

Public engagement is now recommended or required in many jurisdictions' regulatory assessments of new technologies, particularly for technologies that are known to be, or are foreseeably controversial. These include: genetically modified organisms, energy technologies like nuclear or carbon capture and sequestration, or data and communication technologies which pose threats to privacy like biobanks or electronic health records. One view of public engagement exercises locates them as belonging to the realm of politics, where the act of, or the results from, an exercise finds political outlets but otherwise lies outside the science-based assessments.

This view reinforces the autonomy of science as much as it confirms scientific authority or expertise. Another view emphasizes the role that engagement of the public should have in shaping technology assessments. From this perspective new technologies introduce new risks that are themselves associated with epistemic uncertainty. Not all effects might be known, the pathway of exposure unknown, and their magnitude or frequency uncertain. Because risk characterization, assessment, and management is done to protect people and their environment from harm, who better than the public to frame the questions about what constitutes harm? On this view, the lay-expert distinction dissolves blurs, and the autonomy and authority of science is undermined.

Nine standard models of public engagement can be judged against the democratic ideals that motivate them (Castle and Culver (2006). They can also be compared against five main policy objectives for public engagement (Peters 2009). Engagement is issue-driven and no panacea for the democratic deficit. Attributing direct or even indirect impact of engagement on decision-making in policy circles remains a challenge. What of the capacity for public engagement to address epistemic uncertainty in technology assessment? Given the uncertainties involved in assessing new technologies, regulators are constantly challenged to demonstrate that the 'best available' or 'high quality' or 'sound' science is being used by scientific experts. A further assessment of public engagement must confront whether, or to what extent, public engagement reframes scientific assessment of new technology, erodes legitimate scientific authority or obscures the presence of illegitimately claimed scientific authority. A typology of different meanings for 'sound science' can be compared against various models of democratic engagement, revealing the potential for problematic interaction in identifiable cases. Preliminary analysis suggests there is no definitive answer to whether reliance on autonomous scientific expertise is a preferred default to the aspirations to wrest authority away from scientists and have 'sound science' open to public debate.

Session: B2 **Room: 102** **Time: Sat 2.30-4.00**

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The Science of Decision

The question of the authority of science is usually posed as one concerning our confidence in scientific knowledge: how does science know that we are in the midst of a mass extinction event?; what reason is there for science to proclaim that there is global warming?; should the scientific voice have more authority than other voices in the ensuing debates? But science is not merely in the business of making categorical statements about the world. In the cases of climate change and mass extinction, in particular, the question of the authority of science is important because it is so closely related to policy decisions about what to do about the phenomena in question. But it is important to note that science is also in the business of advising on how rational decisions ought to proceed. In this paper I will outline some of the recent work on implementing formal decision methods in environmental management and public policy and defend the use of such methods.

Session: B3 **Room: 105** **Time: Sat 2.30-4.00**

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The scientific authority of public health in the philosophy-free zone.

Public health policy imposes sociological framing of public health issues through scientific discourse, public behaviour change programs disseminated via mass media and through legislation. Public health policy impacts upon a wide range of issues that affect the daily life of many citizens such as obesity, quarantine, sexual activity, disease screening, smoking, exercise and diet to name just a few. Unfortunately, public health practice is mostly conducted without consideration for an underlying philosophical basis or rationale. Practitioners often falsely assume that their actions and policy activities are "value-free" and "rational". Given there are significant ethical issues involved in imposing such policies it could be expected that the public health literature would be replete with references to the ontology and epistemology of public health practice. An online search of major public health journals for the words "ontology, epistemology, ontologic, ontological, epistemologic, or epistemological" appearing anywhere in the title, abstract or full text of the journal was conducted. The number of discreet publications referring to any of these words over 10 years were: American Journal of Public Health 34, Journal of Epidemiology and Community Health - 33, Australian New Zealand Journal of Public Health – 7, European Journal of Public Health -5, BMC Public Health – 1, Journal of Public Health – 1. This assessment may understate the actual numbers of such publications due to limitations of the journals online search facility, however, it is clear that among these journals the study of the validity of public health knowledge and or the ontology of practice is not of major interest. Many of the articles found through this search focus on this deficit in public health policy and practice.

The impact of operationalising public health policy and practice in the philosophy-free zone is that their adverse impacts upon society are either ignored, dismissed or considered as acceptable "collateral damage. In this paper I will explore the ethical issues associated with obesity control and anti-smoking policies and propose how philosophical enquiry and education supported by contemplative practice could redress the current situation.

Session: C1 **Room: 100** **Time: Sun 10.30-12.00**

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The Authority of Science and H. Weyl's Epistemology: a Kantian Legacy? Observation on the 'anthropological criterion'

This paper investigates the questions of whether and when the authority of science is deserved from the perspective of the history of the philosophy of science. H. Weyl, the father of gauge theory, claimed that mathematics and physical science gained their privileged position in virtue of the solidity of the symbolical-theoretical construction that shows the consecration and dignity of every creative act of man (the *Open World*, 1932). After the Second World War, Weyl revised his view. In the *Address at the Princeton Bicentennial Conference* (1946) he stated: "science is neutral towards good and evil […] What would really be needed to offset the menace of the progress of natural science is a development, not of social science, but of social behaviour and more responsibility, of our whole attitude towards life". I shall investigate to what extent, according to Weyl, the nature of science confers epistemic authority on scientific opinion, and what are the scope and limits of that authority. In the second part of the paper I show that the solution Kant developed in 1790s, according to which science and art realize themselves in the public and political spheres of our society, was grounded on a strong anthropological assumption. Conclusively, I try to assess a possible pathway of interaction between science, art and ethics that has been inspired by an interpretation of Kant's *Critique of Judgment*, by introducing the relevance of an 'anthropological criterion' in the current debate. I try to show that this criterion can have direct relevance for the practice of contemporary science and its application in public policy.

Session: C3 **Room: 105** **Time: Sun 10.30-12.00**

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The fundamental power and limits of science are those of its uniquely constrained SDAL cycling organisation

The orthodox position is that science is as effective and authoritative as it is, and limited where it is, because of its method. We can accept that formulation but note that it requires science having methodological instruments adequate to the task. The orthodox conception of those instruments - that method is applied logical inference - is demonstrably inadequate to the task. In this paper we first open up the space of cognitive processes by comparing and contrasting the research process to the design process, and explain a new conception of the cognitive learning bootstrap (learning how to learn, while learning) required by both processes: self-directed anticipative learning (SDAL). We then defend the view that basic research method is SDAL cycling through showing its capacity to illuminate the cognitive conduct of, and epistemic status of, scientific investigations (e.g. ape linguistic capacities). Next, third, we show how SDAL cycling in science is constrained in specific ways deriving from its cognitive objectives. These distinctive constraints contribute essentially to the greater power of the scientific process to pursue epistemic ends in comparison to that of the less constrained general design process to achieve its somewhat different ends. It is then possible to provide an alternative understanding of the power and limits of science as those of its uniquely constrained SDAL cyclic organisation. The far-reaching implications of this re-orientation are explored, both for science in itself and for science in its social context.

Session: A3 **Room: 105** **Time: Sat 10.30-12.30**

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Evaluating extreme risks: expert authority and data-free statistics

Extreme risks, such as the risks of terrorist attack, major natural disaster, massive bank fraud and pest invasion, are especially difficult to evaluate. Since they are generally risks of events that have not yet happened, there is either no relevant data, or there is little data and it is of contestable relevance. Often one must extrapolate beyond the range of existing data. Therefore it is essential to use expert opinion to supplement the lack of data. But expert opinion is subject to a number of well-known biases and needs to be calibrated in some way to ensure a degree of reliability. The most effective attack on the problem of melding expert opinion and small data sets has been in the Basel II international compliance regime for bank operational risks. To evaluate the risks of major events such as massive internal fraud, banks are required to model risks according to specified mathematical methods including Extreme Value Theory, and to modify the answers by scenario analysis conducted by committees, with the whole process then submitted to a potentially hostile national regulator such as the Bank of England. We give an overview of the process and explain why it provides a good template of an "advocacy model" of extreme risk evaluation suitable for use in other fields. The theoretical perspective of the analysis includes the objective Bayesianism defended in the author's *What Science Knows: And How It Knows It* and psychological findings on where expert opinion is and is not reliable. The work has been done in collaboration with the Australian Centre of Excellence for Risk Analysis, whose main interest is in quarantine risks.

Session: A1 **Room: 100** **Time: Sat 10.30-12.30**

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Radical Instrumentalism

Galileo's *Assayer* (*Il saggiatore*) is perhaps the most famous manifest of the instrumentally-aided observation. It is written in the context of a debate with Jesuit scholars over the observations of comets and usually understood as part of Galileo's battle for Copernicanism and against Aristotelian dogma. In fact, Galileo's adversaries are fully supportive of the telescope and hold much more 'advanced' views about comets than his curiously Aristotelian analysis of their makeup and position. A closer look at the *Assayer* and the controversy reveals that Galileo is defending a radical new form of sensory experience, in which the instrument completely replaces the eye. To justify this Galileo presents the human sense organ as a fundamentally flawed instrument, whose mediation distorts and deceives, and the instrument, in contrast, as an embodiment of a purely mathematical relation, which allows the intellect to read the "mathematical characters" of nature unmediated. The Jesuits, on the other hand, approached observation instruments as aids to or extensions of the eye, with the human organ, divinely assigned, being always the final adjudicator, and naked eye observation always the preferred choice. The controversy between the mild and radical instrumentalism continued along these lines through the 17th century, with its original themes still present in an elaborate form in the debate over telescopic sites between Hooke and Hevelius in the 1670s.

Session: C2 **Room: 102** **Time: Sun 10.30-12.00**

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Towards a universally applicable evolutionary methodology: a new look upon the units and levels of evolution debate

Evolutionary epistemology can provide a unified scientific methodology that enables scholars to study the evolution of life as well as the evolution of cognition, science, culture and any other phenomenon displayed by living organisms. Three heuristics are provided that allow for a thorough search for the units, levels and mechanisms of evolution. Contrary to previous approaches, units, levels and mechanisms are not identified by pointing out essential features, but rather ostensive definitions are preferred. That is, units are considers as such if a level of evolution and a mechanism of evolution is identifiable. Levels are levels if one can point out units that evolve at that level according to evolutionary theories, and mechanisms are considered as such if one can point out units and levels where the mechanism is active.

How to examine whether x is a unit of evolution

How do we know that x (e.g. a gene, meme, replicator, reproducer, symbiont, symbiome, chromosome, phenotype, trait, behaviour, system, pattern, organism, etc.) is a unit of evolution (table 1)?

Table 1. Is x (a feature, trait, space, event, element, … that is presumed relevant for evolution) a unit in/of evolution?
(read from left to right and top-down)

?	Try to prove that it is a unit of evolution (1 example suffices). Thus go to **yes**.		
Y	**Where?**	Not one level found? X is not a unit, go to **no**.	
E S	At which **level** is x the subject of evolution.	One/multiple level(s)? Identify them all. (Justifies that x is a	Via which **evolutionary mechanism(s)**? Identify them all.

		unit.)	Via which **evolutionary mechanism(s)**? Identify them all.
	Since **when**?	When did x first originate in time and when did it become a unit of evolution?	
	How does this unit x **interact** with other units?	Can this unit be divided into one or several **subunits**? If so, are they also units in evolution?	
		Can this unit be absorbed into one or several **superunits**? If so, are they also units in evolution?	
	Can this unit also be regarded as a **level** and/or **mechanism** of evolution?	? & yes: try and treat the unit as a level and/or a mechanism, go to **level and/or mechanism**.	
	Relevance?	Is the unit x **sufficient** and/or **necessary** for evolution?	
NO	Level and/or mechanism?	? or Yes: go to **level** and/or **mechanism**.	
		No: treat x as irrelevant for evolution until proven otherwise.	

How to examine whether x is a level of evolution

How do we know that x (e.g. the environment, the brain, culture, etc.) is or is not a level of evolution (table 2)?

Table 2. Is x (a feature, trait, space, event, element, … that is presumed relevant for evolution) a level in/of evolution? (read from left to right and top-down)		
?	Try to prove that it is a level of evolution (1 example suffices). Thus go to **yes**.	
YES	How many/which **units** evolve at this level?	Not one unit, x is not a level of evolution, go to **no**.
		One/multiple unit(s)? Identify them all. (Justifies that x is a level.)
	How many **evolutionary mechanisms** are active at (not on) this level?	Equals the question: how many evolutionary mechanisms are active upon the units that evolve at this level. (testing device)
	What is the **ontological status** of the level?	The level is an **abstract notion** that facilitates theory formation/ an **exiting entity**.
	Since **when**?	Locate the origin of x in time or when it becomes necessary to invoke x as an abstract notion in the theory of evolution
	How does this level x **interact** with other levels?	Can this level be divided into **sublevels**? If so, are they also levels in evolution?
		Can this level be absorbed into s**uperlevels**? If so, are they also levels in evolution?
	Can this level also be regarded as a **unit** and/or **mechanism** of evolution?	? & yes: try and treat the level as a unit and/or mechanism, go to **unit and/or mechanism**.
	Relevance?	Is the level x **sufficient** and/or **necessary** for evolution?
NO	**Unit** and/or **mechanism**?	? or Yes: go to **unit** and/or **mechanism**.
		No: treat x as irrelevant for evolution until proven otherwise.

How to examine whether x is an evolutionary mechanism involved in evolution

If we have an x (e.g. natural selection, symbiogenesis, drift, the ratchet effect, the Baldwin effect, niche construction, …) of which we do not know that it is or is not an evolutionary mechanism, how do we begin examining whether this x is an evolutionary mechanism?

Table 3. Is x (an evolutionary mechanism, feature, trait, space, event, element, … that is presumed relevant for evolution) an evolutionary mechanism involved in/on evolution?
(read from left to right and top-down)

?	Try to prove that x is an evolutionary mechanism involved in evolution. Thus go to **yes**.	
YES	On how many **units** is this evolutionary mechanism working?	Not one unit: x is not an evolutionary mechanism involved in evolution.
		One/miltiple unit(s). Identify them all. (Justifies that x is an evolutionary mechanism involved in evolution.)
	At (not on) how many **levels** of evolution is this evolutionary mechanism active?	Equals the question: the units that are subjected to this evolutionary mechanism, at how many levels are they subjected to it?
	How does the mechanism work? Which **conditions** need to be met in order for the evolutionary mechanism to occur? Answer requires (universal) **EE formulas** of the workings of the mechanism.	
	Since **when**?	Locate in time when these conditions are met regarding each unit and each level = when the evolutionary mechanism became a mechanism involved in evolution at that unit and/or level.
	How does this mechanism x **interact** with other mechanisms?	Can this mechanism be divided into **sub-mechanism(s)**? (Depends on the presence of subconditions.) If so, are they also mechanisms of evolution?
		Can this mechanism be absorbed into a **super-mechanism(s)**? (Depends on the existence of a mechanism that allows to combine different mechanisms into one single mechanism.) If so, are they also mechanisms of evolution?
	Can this mechanism also be regarded as a **unit** and/or **level** of evolution?	? & yes: try and treat the mechanism as a unit and/or level, go to **unit and/or level**.
	Relevance?	Is the mechanism x sufficient and/or necessary for evolution?
NO	**Unit** and/or **level**?	? or Yes: go to **unit** and/or **level**.
		No: treat x as irrelevant for evolution until proven otherwise.

Session: C2 **Room: 102** **Time: Sun 10.30-12.00**

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History and Philosophy of Biology meets Public Understanding of Genetics

Over the last 25 years public understanding of science researchers have rejected a 'deficit model' in which the aim of science communication is to bring the views of the public into line with those of scientific experts. Instead, it is widely held that the aim should be to empower a disparate range of publics to construct representations of science that effectively serve their own needs. In the history and philosophy of biology, however, the shortcomings of prevalent ideas about genetics and genomics remain the focus of considerable concern. I argue that these concerns should not be tarred with the same brush as the deficit model. They do not result from 'privileging' the representations of scientific experts, but rather from questioning the adequacy of current representations of genes, genomes and gene action by all groups, expert and non-expert alike. Pigliucci and Kaplan have recently characterized much work in the history and philosophy of biology as 'science criticism' – I explore this interesting but problematic formulation. I suggest that, ultimately, these two literatures complement one another, and that the aims of science communication can legitimately include amelioration as well as empowerment.

Session: A3 **Room: 105** **Time: Sat 10.30-12.30**

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Voting, Deliberation, and Truth

There are various ways to reach a group decision. One is to simply vote and decide what the majority votes for. This procedure receives some epistemological support from the Condorcet Jury Theorem. Alternatively, the group members deliberate and will eventually reach a decision that everybody endorses - a consensus. While the latter procedure has the advantage that it makes everybody happy (as everybody endorses the consensus), it has the disadvantage that it is difficult to implement, especially for larger groups. What is more, a deliberation is easy to bias as those group members who make others change their mind may not necessarily be the best truth-trackers. But even if no such biases are present, the consensus may be far away from the truth. And so we ask: Is deliberation a better method than simple majority voting if the group's goal is to track the truth? To address this question, we propose a Bayesian model of rational deliberation and compare it to the straight forward voting procedure. By doing so, we illustrate how work at the interface between philosophy and political science can contribute to policy-related debates. The talk is based on joint work with Soroush Rafiee-Rad (Tilburg).

Session: C3 **Room: 105** **Time: Sun 10.30-12.00**

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On the distinctive power and limits of working with complex systems models in science

Complex systems present unique features that challenge traditional scientific methodology and, with that, the authority of science. These features include multiple (phase-, state-, constraint-, perturbation-, ...) context-dependent dynamics; dynamical phase thresholds, self-organisation and phase transitions; non-standard (fat-tailed, ...) statistics; and globally constrained, functionally coherent dynamics. Such features yield inherent (but complex) predictability limits; disparity between prediction and explanation; disparity between model and machine learning; multiple condition-dependent laws; multiple level-dependent explanations; inter-twining of reduction and emergence; transforming notions of mechanism; problems of explanatory inference and data validity for entities with unique histories; and so on. On the other hand, complex systems models have provided the explanatory basis and methodological means for some of the profoundest breakthroughs in modern science, from genomic dynamics - e.g. now permitting the first accurate predictive models of genetic modification in cellular industrial processes - to the linkages between atmospheric and ocean flows, ultimately permitting the basic understanding of the dynamics of climate change.

Over the past 30 years complex systems models have expanded from isolated curiosities to be a central feature of virtually every science, and with that the issues above have become a prominent feature of their practices. They haunt climate science and molecular medicine (e.g. gene therapies), for instance, which are essentially wholly dependent on complex systems modelling, and their features are commonplace issues for practice elsewhere (traffic dynamics, ‘smart’ grids and ports, ecological and geodynamics, robotics and multi-agent social dynamics, etc.). And, as this makes clear, they lie behind some of the most difficult public policy decisions facing humans today, where the delicate authority of science plays crucial roles.

Philosophy of complex systems is still younger than the explosion of their use across science. This paper will systematically review the issues they raise, working off recently completed (and partly unpublished) pioneering multi-disciplinary research led by practicing scientists as well as involved philosophers (e.g. http://www.elsevier.com/wps/find/bookdescription.cws_home/720424/description#description).

The paper will critically assess the nature and status of the philosophical issues they present to developing an adequate philosophy, especially methodology, of science and the distinctive new mix of authority and tentativeness that marks systems-based science.

Session: C1 **Room: 100** **Time: Sun 10.30-12.00**

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The failure of objectivism in science: context and complexity

Traditionally science has concerned itself with the modelling of systems for which an objective outcome can be defined. Thus, the results obtained by performing a measurement upon a system of interest are assumed not to depend upon the choice of measurement performed. The world `out there' is assumed to be somehow ontologically independent from our observations of it, which probe that reality rather than actively participating in it. This has led to a divide between the hard and soft sciences that could be understood as founded more upon the `analyticity' of the subject of interest than upon any true distinction between the two classes of science. This paper will explore this naive objectivism through reference to the epistemology of Complex Systems Science (CSS).

There are two avenues of research suggesting that science must move beyond its naive objectivism. Firstly, despite its original emphasis upon concepts such as wholism and non-reducibility, modern CSS has become a science more concerned with the modelling of behaviours and the application of certain well defined techniques. However, a well respected undercurrent of researchers have been known to make statements amounting to the declaration that the complexity of a system can only be defined with reference to an observer. The failure of CSS to rigorously define notions such as complexity and emergence suggests that these features are somehow dependent upon the process by which they are observed. The way we look at complex systems apparently matters, and science has not found a way around this problem.

Secondly, it is increasingly the case that many fields of science find themselves concerned with systems for which objective reality is very difficult to define. These systems come from cognitive, social and even physical science, hence lie within the gamut of CSS. They are characterised by violations of the law of total probability, states which are impossible to define as pre-existing "elements of reality" and combined systems that exhibit apparently non-separable and non-decomposable behaviour which is sometimes termed wholistic.

Sceptical and relativistic attacks upon the authority of science are often generated from the above two problems, but this article will argue that a middle way is possible. Science must move beyond its naive dependence on objectivity, but this need not require a lapse into subjectivism.

To motivate this approach, a class of complex system will be discussed which exhibits strong contextual dependencies (a) between components, (b) between system and observation, and (c) between system and environment. A method for describing this contextual behaviour already exists, in physics; Quantum Theory. This paper will discuss a number of approaches in the scientific literature which extend the quantum formalism beyond its current domain of application, into the realm of CSS. Examples from psychology, linguistics and cognitive science will be discussed, and a possible theory of emergence proposed.

Session: B1 **Room: 100** **Time: Sat 2.30-4.00**

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The Problem of Meta-Criterion of Demarcation Between Science and Pseudo-Science

In its heyday the problem of demarcation between science and metaphysics was considered a crucial question in philosophy of science. It was Popper's opinion published in the mid-thirties that started long lasting and vivid argument on conditions of scientific status—later the place of metaphysics was taken by pseudoscience and its use for economic and political purposes.

For the supporters of the criterion of demarcation it has manifold and undeniable virtues. The older demarcationists thought it would allow to set the limits of scientific cognition and remove metaphysics both from science and from so-called 'scientific philosophy.' That should automatically catalyze the development of science. The modern ones are in general reconciled with the presence of metaphysics in science and see the merits of demarcation in a possibility of comparative appraisal of scientific theories. Such criterion of demarcation, they say, still allows grasping the 'essence' of science and it also has obvious practical values for state and scientific institutions. It has *if* it satisfies a commonly accepted meta-criterion. Is there, however, any?

While there is a vast number of works where demarcation is at issue, only a few of them ask for a meta-criterion that would constitute a stable platform for a discussion of particular proposals of demarcation.

In 1983 Laudan publishes his famous paper on 'Demise of Demarcation Problem.' To compare different approaches offered since antiquity he sets a metacriterion that appeals to very basic intuitions we have about science or scientific knowledge. In every considered case Laudan reaches negative results about the possibility of demarcation that satisfies all the conditions framed by his metacriterion. This meta-criterion, being based on the fundamental insights into science, its aims and methodology, seems to have an ultimate character which renders Laudan's conclusion as highly skeptical—this corresponds well with the actual history of the problem that seems to be losing more and more the interest of philosophers of science.

Perhaps, however, the meta-criterion proposed by Laudan is simply too strong.It consists of two kinds of conditions—two formal and a pragmatic one. If one those groups is dropped out we will not only make the meta-criterion homogeneous but we will also reject Laudan's skeptical conclusion.

The first aim of this paper is to show that when the pragmatic condition is rejected we are able to find at least one advanced theory that gives acceptable definition of science. Secondly, it will be investigated if such rejection is acceptable in a democratic society.

Session: C3 **Room: 105** **Time: Sun 10.30-12.00**

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When Randomized Trials

Randomized trials are seen to carry the authority of science. This is because suitably conducted randomized trials provide a method for rigorously testing the effects of an intervention. In medicine, randomized trials are required for establishing the efficacy of medicines for regulatory purposes, and for much of the past two decades, have been seen to provide the best evidence for clinicians making decisions about patient care. Moreover, it is the authority given to randomized trials that is seen to justify the random allocation of patients to treatment with an experimental intervention or control.

But adopting randomized trials as the standard for assessing claims about the effects of interventions is both too restrictive and too permissive. Many older medical interventions have not been tested in randomized trials, and new interventions will only have been tested in select patients under strict (and often unrealistic) conditions. Clinicians need to rely on a much boarder range of evidence to treat their patients. Conversely, the success of an intervention in a randomized trial need not boost the likelihood that an implausible theory is true (the success of retroactive prayer is a well-known example, Leibovici 2001).

The limits of randomized studies are increasingly well recognised, and a range of specific problems have been discussed extensively. What is missing is a more general account of when randomized trials provide an appropriate standard for testing the effects of interventions. This paper clarifies when, and for which questions, randomized trials are best-suited. Better demarcating the situations in which randomized trials are superior, better identifies when the authority given to randomized trials is appropriate.

Session: B3 **Room: 105** **Time: Sat 2.30-4.00**

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Authority and Popular Science

Anecdotes abound about the relationship between a researcher's scientific status and their popular status. Claims post that the more 'popular' a researcher is, the less authority that researcher has in scientific circles. Conversely, some claim that popular success can increase authority in political circles of science and encourage benefits, like funding, that have real impact on the generation of scientific knowledge. While there are empirical data (or at least 'cases') supporting both claims (there also seem to be data against), the larger questions of the legitimacy of the relationship between scientific authority and popularization have only been considered piecemeal. Further, critics of popular science have largely decided in the negative; popular science should have no impact on scientific authority within science. Put slightly more formally, there is and should be no necessary relationship between popularization and the epistemic status of a researcher's work.

This paper will argue against the normative part of this claim. Using a variety of examples, I will support the normative claim that there should be a necessary relationship between popularization and the epistemic status of scientific research. I will also suggest why it is that this is currently not the case (largely agreeing with the empirical part of the above claim), leading to many of the current criticisms of scientific popularization. Finally, I will outline the character of an epistemically rewarding relationship between popular science and scientific authority and point to where such a relationship is currently found.

Session: A2 **Room: 102** **Time: Sat 10.30-12.00**

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Ground on which to stand: biology and the authority of science

The process by which science comes to have the authority it has in the domain of human action—in public support for policies based on science, for example—does not appear to be relevantly guided, much less determined, by specialized epistemic considerations. This appears to be the case even among highly educated, philosophically knowledgeable individuals, which is why 'science says' arguments can be made to support radically different policy positions. I will argue that the authority of science (such as it is)—like the authority of the church before it— relies in large part on the psychological need for certainty, or, conversely, the need to reduce uncertainty in a complex world. Higher degrees of certainty induce greater feelings of personal control (Wurzbach, 1992), and the ability to exert control over one's environment and/or circumstances appears to be "essential for an individual's wellbeing" (Leotti, Iyengar, & Ochsner, 2010). Appeals to reliable authority thus may meet a biological need for eliminating uncertainty that is common to most (possibly all) organisms, but especially in mammals, whose physiology and life-ways are extremely energetically costly. While science is arguably the most rational and reliable knowledge generator humankind has yet produced, its authority may well rest on highly labile, emotional foundations involved with feelings of security. The very nature of science as an evolving, open-ended knowledge enterprise that relies as much on human probity as it does on human inventiveness arguably makes it a less-stable refuge for emotional confidence than, say, the church was for many centuries. A recent study found that threats to personal control, which undermine perceptions of the world as orderly and structured, increased belief in an external agent (i.e. God) and reduced belief in evolutionary theory (Rutjens, van der Pligt, & van Harreveld, 2010). However, confidence in evolutionary theory increased when it was presented "in terms of predictable and orderly processes" (p. 1078). This largely hidden, bio-psychological dimension arguably has helped to prevent a public (and public policy) consensus forming behind the overwhelming scientific consensus concerning the anthropogenic nature of climate change. The East Anglia email scandal, which alienated a substantial proportion of climate change supporters, and the results of the multinational review of the Intergovernmental Panel on Climate Change that followed in its wake will be used as a case study.

Session: B2 **Room: 102** **Time: Sat 2.30-4.00**

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Interest and opportunity: Disagreeing authorities in climate change economics

How much should the current generation spend on climate change mitigation policies? Economists disagree about the answer to this question. Of the two leading authorities on this topic, Stern argues we should spend substantial sums of money on mitigation, while Nordhaus disagrees and labels Stern's policy view as "extreme". At its core, the disagreement is over discounting. Discounting is an economic tool that decreases future costs and benefits by a yearly rate, the discount rate. While most economists agree that there should be some discounting in decisions over climate change policies, what they disagree about is how much we should discount by, and why we should discount. One of the most commonly found reasons for discounting is opportunity costs which is defined as "the value of the best alternative use of a resource". In this paper I will consider the extent to which opportunity cost can be used to justify discounting in social environmental decisions with long time horizons such as climate change mitigation policies. I will argue that the standard use of interest rates as a measure of opportunity costs is unhelpful on three counts: 1) an appeal to interest rates does not single out a rate as there are numerous rates available, 2) an appeal to interest rates attempts to derive a normative claim about political behaviour from a descriptive claim about market behaviour, and 3) an appeal to interest rates is limited by the financial instruments we currently have available, and those do not have sufficiently long the time horizon to inform climate change policy decisions. I then consider two possible replies to my objections. One based on appealing to the theory of interest and one based on using a historical analysis of interest rates. I reject both suggestions and conclude that in decisions with long time horizons like climate change policies, interest rates do not offer a successful measure of opportunity costs or of the discount rate.

Session: B3 **Room: 105** **Time: Sat 2.30-4.00**

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Fluoridation in Victoria: the authority of science, public health policy, and culture

The recent fluoridation of Geelong's water supplies in Victoria's Southeast has provoked much criticism from interest groups, along with death threats to public servants. This reflects a long history of sensationalism in the fluoridation debate in Victoria. The standard of the dialogue within the fluoridation debate has been contemptible on both sides. Petulant, unmeasured scare-mongering tactics have been employed by anti-fluoridation lobbies. Rhetorical language is utilised to entice anti-state sympathisers, certain that the government wants to poison them. On the other side, the evidence-base which supports fluoridation is not infallible. Indeed, there have been vast improvements in dental health in the last few decades. Nevertheless, the evidence that this comes from fluoridation is not conclusive.

Proponents claim that fluoridation is a major public health achievement — an all-encompassing measure which fights tooth decay and improves oral health across all echelons of society. Opponents claim that the efficacy of fluoridation is grossly overstated, dangerous, and scientifically unsound. Brian Martin explains that after the 1950s, when some key respected public health bodies worldwide had endorsed fluoridation, "the scientific issues were treated as closed … Fluoridation was considered scientifically proved and, furthermore, criticisms of fluoridation were treated as political rather than scientific. Opponents were classified as cranks rather than as rational critics." The authority of science reigned supreme. This is further reflected in the fact that pro-fluoridationists continue to be better represented by a long list of authoritative institutions, while anti-fluoridation groups tend to be less authoritative.

The evidence on both sides of the debate seems far more equivocal and anecdotal than concrete. Interest groups from differing cultural horizons band together and become more of a mass body fighting for a similar cause. The anti-fluoridationists are a coalition of bizarrely connected groups all linked by the one agenda — "The state doesn't have the right to medicate us!". This is what tends to happen in these polarized debates. It is often perceived that the lower echelons of society protest the impositions implemented by the higher echelons of society. Proponents call opponents "quacks" and decree that "Science says". Opponents claim insidious government conspiracy and throw political charges at the authorities.

Karen Block explains that "While *opponents* also attack the scientific evidence for fluoridation, they often define their battle as explicitly *political*. *Proponents*, on the other hand, are far more likely to frame their arguments in *scientific* terms". The discursive elements of these differing characterisations emerge as essential to understanding the sensationalism of the debate. By reducing the question of fluoridation to a scientific issue, proponents implicitly transform what is an inherent ethical consideration to a different realm. This naturalising tendency is nothing new in the history of science — by depoliticising an ethical issue through authoritarian scientific methodology, the power of scientific ideology is underscored.

This paper intends to elucidate the complex cultural convictions that are inextricably tied up in the fluoridation debate and to connect these with the question of the authority of science. Such authority is a powerful weapon in the implementation of public health policy, but when its evidence base is inconclusive, it engenders highly charged debate with sensationalised claims, only hindering the possibility of constructive, informed dialogue on the issue. Public health measures are never implemented in a social vacuum, and the fluoridation debate highlights this fact.

Session: B2 **Room: 102** **Time: Sat 2.30-4.00**

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Conduct Unbecoming: Eddington on Intuition versus Physics

Session: C1 **Room: 100** **Time: Sun 10.30-12.00**

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The Authority of Science vs. The Demarcation of Inquiry

The call for papers for this conference claims that 'the founders of modern philosophy of science, including Sir Karl Popper… saw it as part of their role to explain the authority of science'. It continues by declaring that 'A key motive for Popper's "demarcation criterion" distinguishing science from "pseudo-science" was to restrict the authority of science to disciplines which used the scientific method.'

However, a closer look at Popper's writing shows that this widespread view is incorrect. In fact, Popper declares in the postscript to the *Logic of Scientific Discovery*:

> [I]t is all guesswork, *doxa* rather than *epistēmē*… Science has no authority… It represents… our hope of emancipating ourselves from ignorance and narrow-mindedness, from fear and superstition. And this includes… the superstitious belief in the authority of science itself. (Popper 1983, 259–260)

After briefly defending my contention, with reference to the work of Bartley (who edited the postscript) and some of Popper's statements elsewhere, I will argue that Popper is better understood as attempting to demarcate inquiry from non-inquiry. I also hope to show that this humbler goal is worthwhile, especially when it comes to resisting calls to teach so-called 'creation science' in schools.

Unfortunately, Popper has become associated with the relatively simplistic view that (empirical) falsifiability is a demarcation criterion. But Popper recognized, even in the first edition of *Logic of Scientific Discovery*, that falsifiability alone does not suffice. He was cognisant of the problem posed by Duhem's thesis, and therefore stated:

> A system such as classical mechanics may be 'scientific' to any degree you like; but those who uphold it dogmatically—believing, perhaps, that it is their business to defend such a successful system against criticism as long as it is not conclusively disproved—are adopting the very reverse of that critical attitude which in my view is the proper one for the scientist. (Popper 1959, 50)

This would appear to be why Popper (1983, 5) wrote of 'the non-existence of scientific method' above and beyond 'the one method of all rational discussion, and therefore of the natural sciences as well as of philosophy… stating one's problem clearly and examining its various proposed solutions critically' (Popper 1959, 16). In short, the critical approach is a crucial part of the demarcation puzzle.

In the remainder of the talk, I will develop this solution to the demarcation problem in response to several possible criticisms. Chief among these is that 'creation scientists' (and similar individuals) do inquire but only with narrow scope (like, perhaps, Kuhnian 'normal scientists'). To address this criticism, I will draw on some of my recent work on the roles of criticism and dogmatism in science (Rowbottom 2011a, 2011b); and in particular, I will consider if looking at matters from the group level can help.

Session: C2 **Room: 102** **Time: Sun 10.30-12.00**

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Pitfalls and strengths of adaptation in biology education: how can philosophy of science help

The concept of adaptation in evolutionary biology is a clear example of how philosophy of biology and general philosophy of science can contribute to science education. Philosophy of biology may provide, e.g., critical conceptual taxonomies of complex and polysemic notions, like adaptation, which are peculiar to the discipline. But philosophy of science can do more for education than guard from conceptual pitfalls: it can help transforming terminological clarifications into access points to how science works. Philosophy of science encompasses the reasons of the semantic diversification of adaptation, revealing the role concepts play in scientific research. This, in turn, can contribute in structuring didactic laboratories that make students experience the nature of science and the foundations of its authority.

From the standpoint of teaching evolutionary biology, adaptation is a “false friend”: the familiarity of the term may inspire confidence as a common ground on which to build students’ understanding of evolution, but the everyday use of adaptation - as several analyses show - is manifestly non-technical. Moreover, even within evolutionary biology the word adaptation is used with several different technical meanings. In a case like this, a kind of philosophical analysis, namely conceptual taxonomy, can give important insights and guidelines for science education.

A conceptual taxonomy by our group recognized and defined seven different meanings of adaptation in biology. We also pointed out some common cross-meanings issues, e.g. (a) the long-standing state/process dichotomy: adaptation can mean both a process and its product, and such ambiguity can lead to some theoretical problems; (b) the relationship between structure and function: the relative importance of the two terms can be conceptually balanced in different ways; (c) the necessity - and non-triviality – of boundaries established between adapting entities and an environment. We also noticed that the polysemy of adaptation is related to a series of levels of description, e.g. trait, individual, population, macro.

The overall conceptual map of adaptation resulting from our philosophical analysis is, we think, important for teaching effectively. But philosophy have far greater potentialities. A deep philosophical analysis goes into the reasons why adaptation underwent semantic diversification, thus giving access to the dynamics of biology as a scientific enterprise, where new approaches and fields of research re-decline adaptation without ruling out its previous meanings. Philosophy of science may inspire didactic laboratories for students (see Pievani & Serrelli 2008) that simulate scientific work: in our case, adaptation - with its various meanings - has to be taken not as a matter of fact, but rather as a to-be-built concept capable to guide the students’ construction of real problems, puzzles and questions, and the search of the best arguments and answers. Laboratories of this sort put teachers and students into the interdependence between theoretical context, scientific practice, and discoveries, and seem the best way to help them not only navigate through concepts, but also understand and discuss the “nature of science” (McComas 1998), as well as the foundations of its authority.

Session: A3 **Room: 105** **Time: Sat 10.30-12.30**

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Applying the Precautionary Principle to various forms of uncertainty

In environmental management, the Precautionary Principle is frequently cited, but its concrete implications for analysis and decision-making are rarely discussed. The principle states, in a nutshell, that uncertainty should not be used as a reason for delaying or dismissing protective measures. This uncertainty can manifest itself in three major forms: semantic uncertainty (e.g., what does it mean that a species is "endangered" and not only "threatened"?), epistemic uncertainty (how reliable are our analyses?) and judgmental uncertainty (what is the appropriate decision model?). This prompts the question of whether the Precautionary Principle should be applied to all forms of uncertainty, or just to some of them. I argue that the Precautionary Principle should primarily be understood as a decision principle, not as a modeling principle, shedding a critical light on attempts in conservation biology that implement a precautionary approach by propagating various kinds of uncertainty throughout their model. But either way, taking the principle seriously makes a case of stronger involvement of scientists in policy decisions.

Session: B1 **Room: 100** **Time: Sat 2.30-4.00**

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The authority of scientific evidence in Health Technology Assessment models

Health technologies have come to constitute an indispensable component of health systems, functioning in areas ranging from prevention, diagnosis, and treatment, to disability alleviation (WHO 2010). At the same time, however, health technologies drastically increase health system expenditures: Australia's Productivity Commission, for example, estimates that health technologies accounted for approximately one-third of the total health expenditure increases in Australia from 1992 to 2002 (Productivity Commission, 2005). Concerns about the fiscal impact of health technologies on health systems are credited with giving rise to the field of Health Technology Assessment (Office of Technology Assessment, 1980; Banta, 2003, Walley, 2007). Health Technology Assessment (HTA) is widely regarded as an *evidence-based* input into health policy, both by scholars and by policy-makers (e.g. Velasco Garrido et al, 2010; EUR-ASSESS, 1997; Government of Australia 2009, CADTH 2010, Gallego et al, 2009). The evidence that is usually assessed by the HTA programmes can be broadly categorised into two types: scientific evidence (e.g. randomised control trials), and nonscientific evidence (including: economic, safety and quality evidence, socio-ethical considerations, shareholder values) (Government of Australia 2009, ten Have 1995, Walley 2007, CADTH 2010, etc).

It is generally recognised in the literature, however, that this evidence is not assessed in a vacuum. This recognition usually takes on the form of arguments that *contextual factors* (such as the types of constituencies participating in the assessment, political climate, health system funding model, etc) affect which evidence categories are accepted or rejected, how the evidence categories are prioritised, etc (e.g. Dobrow et al 2004; Banta 2003, Head 2010). On the other hand, thus far, very little attention has been paid to how the specifics of the *function* of HTA impact the treatment of evidence in the HTA process. The aim of this paper is to fill in some of that gap.

Although HTA's function is generally (and generically) put in terms of input into health policy, a scan of HTA models adopted by various jurisdictions reveals that a distinction needs to be drawn between those HTA models whose input into health policy is descriptive, and those HTA models whose input is prescriptive. The HTA models whose input is descriptive usually focus solely on establishing the strength of the evidence for or against adopting a particular technology, while those whose input is prescriptive additionally produce policy recommendations (e.g. vis-à-vis funding of a particular technology). In this paper, the descriptive and prescriptive approaches will be fleshed out using, respectively, England's National Institute for Clinical Excellence (NICE), and Australia's Medical Services Advisory Committee (MSAC). We will argue that although a closer analysis of the two approaches reveals that the categories of evidence considered by each type of model largely overlap, scientific evidence is more authoritative in those HTA models whose input is descriptive, than in the HTA models whose input is prescriptive.

Session: A1 **Room: 100** **Time: Sat 10.30-12.30**

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Thomas Edison and the authority of inventions

During his lifetime Thomas Edison acquired iconic status as an inventor and innovator, his authority in these persisting after his creative powers had waned and even after his death. This paper examines the source of his authority by contrasting two incidents from Edison,s early career, his invention of the Phonograph in 1877 and his claim, two years earlier, to have discovered a new force of nature that he named Etheric force. The initial evidence for each was meagre, but it persuasive enough for Edison to devote considerable research effort to exploring its potential. Despite the similarities in their origins and in Edison,s approach to each, the outcomes were radically different. In the case of the Phonograph, Edison became a national celebrity but his Etheric force claim, though receiving some popular support, led to ridicule, and threatened to alienate his most important sponsor. This opposition led Edison to abandon Etheric force and in so doing, missed the possibility of adding inventor of wireless telegraphy to his list of achievements. In each case, the outcome was a consequence of the way in which Edison developed the initial, meagre evidence and the way in which he built, or failed to build, the resulting knowledge into successful artefacts.