Book of Abstracts

History and Philosophy of Science in the East

Funded by

Center for Philosophy of Science, HKUST





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A Coherence Theory of the Usefulness of Theoretical Content

Jonathon Hricko (National Yang Ming Chiao Tung University)

Our best scientific theories purport to tell us a lot about aspects of reality that we cannot directly observe. Given that scientific anti-realists are typically agnostic about whether this theoretical content is true, what should they want from theoretical content? I argue that they should want content that is useful. I understand usefulness in terms of Hasok Chang's notion of operational coherence, where an operationally coherent activity is an activity whose elements fit together in a way that makes sense given the aims that activity is supposed to achieve. Theoretical content is useful to the extent that it is relied upon in the performance of operationally coherent activities. I defend the idea that anti-realists should want useful theoretical content by showing how this idea can be used to supplement a particular anti-realist position, namely, Bas van Fraassen's constructive empiricism.

The Function of 'Living Fossils': Value of Ambiguity

Qiyue Zhang (HKUST)

Although the term 'living fossils' has been widely used, it has never been well defined. Definitions vary across levels of analysis, time spans, and other criteria (Sterner 2023; Lidgard and Kitchen 2023). As a result, debates persist over whether the concept should be discarded or retained. Critics who advocate discarding the concept focus mainly on two problems: its arbitrariness (Schopf 1984), and its misleading implication that some species do not evolve (Yong, 2013; Carnall 2016). Defenders of the concept adopt two main positions. Monists argue that 'living fossils' should be retained and given a fixed definition (Turner 2019). Pluralists argue that we should not seek a fixed standard. We should regard the concept not as a categorization but as a research agenda (Lidgard and Love 2018, 2021) or an investigative kind (Sterner 2023).

In this paper, I argue that the concept of "living fossils" is best understood not by fixing its nature, but by examining its function in scientific debate, where its very ambiguity can productively define problems and invite reflection on certain species' evolution. I first review these accounts and then analyze them, showing that each is problematic or unsatisfying. Second, I offer an alternative account that shifts the question from the nature of the concept (e.g. a taxon, a hypothesis, a research agenda) to its function. Instead of analyzing different cases of living fossils, as the existing literature does, I examine the dynamic process of debates over whether a certain species is a living fossil. Then I argue that the ambiguity of the term can foster problem-defining (Shan 2019) and encourage reflection on the relationship between different aspects of evolution. I use the case of the coelacanth to illustrate this point: it was initially labeled a living fossil (Smith 1939; Forey 1990; Thomson 1992), later doubted (Holder et al. 1999; Amemiya et al. 2010; Higasa et al. 2012; Naville et al. 2015; Casane and Laurenti 2013), and more recently described as "almost living fossils" (Cavin and Alvarez 2022). In debating the concept of "living fossils", researchers shifted the focus of discussion from whether stasis occurs to which traits are conserved and which are evolving, and explored the relationships among different concepts such as morphological similarity, lineage antiquity, and evolutionary stasis.

In conclusion, the ambiguity of 'living fossils' should be seen as a productive feature rather than a defect: as the coelacanth case shows, debates over whether it is a living fossil can contribute to defining new problems and invite reflection on the interrelations between various aspects of evolution.

Reconsider the Vitalistic Tradition in the Study of Animal Heat: a Revised Historico-critical Account

Bohang Chen (Zhejiang University)

The admirable historian of science Everett Mendelsohn gave a classical account for the development of the theory of animal heat in his elegant masterpiece, *Heat and Life: The Development of the Theory of Animal Heat* (1964). The classical account can be usefully summarized with three theses: C(1) Before the Scientific Revolution, the doctrine of vital heat, which treated innate heat in the animal body as the foundational source of all vital capacities, dominated the scientific or natural philosophical discourse; C(2) During the Scientific Revolution, Rene Descartes, J. B. V. Helmont and others dismantled the age-old doctrine of vital heat and treated it as no longer offering a unified principle, instead they attempted to explain various phenomena of animal heat through mechanical and chemical manners, leading to two inter-connected research traditions of iatromechanism and iatrochemistry that outcompeted the vitalistic tradition in the study of animal heat; C(3) at the end of the 19th century, the vitalistic approach to explaining animal heat essentially disappeared, driven by scientific achievements from A. Lavoisier, J. Liebig, and many others.

While agreeing with the classical account on all its essential insights, I attempt to suggest three slight revisions as a minor historico-critical contribution: R(1), historically, although the vitalistic tradition was far less popular than mechanical and chemical approaches in the study of animal heat, it never really disappeared until the early 20th century (with notable physiologist representatives such as G. Stahl at the turn of the 18th century, J. Hunter towards the end of the century, W. Carpender during the 19th century, and Hans Driesch at the start of the 20th century), a time when the prominent physiologist M. Rubner was still declaring, curiously, the death of vitalism, with his work on respiration and mechanism; R(2) From the critical point of view, physical (mechanical and chemical) explanations had often failed to achieve exact experimental results, and the gap between the heat used and the heat supplied had occasionally invited vitalistic speculations; R(3) Such vitalistic speculations were of two kinds, with the first positing vital forces as a source of animal heat (Hunter and Carpenter), leading finally to the doctrine of vital energy conforming to the first law of thermodynamics, and the second treating vital forces as capable of transferring heat in a way that moves beyond the second law of thermodynamics (Stahl, Driesch).

In this presentation, as a historical contribution I will use original sources from the physiological discourse after the Scientific Revolution to refute several false claims regarding the history of the study of animal heat from earlier historians of science. I also supply a logical and critical lesson to make sense of the persistence of vitalistic speculations regarding the production of animal heat: indeed, recent proposals from some theoretical biologists have not moved beyond Stahl and Driesch in their speculatively positing bio-specific thermodynamic principles for the realm of living things.

‘Guan’ over ‘Ji’: Yan Fu’s Reception of the Concept of Organism

Shuhan Xiao (University Paris 1 Panthéon-Sorbonne)

This paper examines Yan Fu’s (嚴復) preference for translating ‘organism’ as ‘guanpin’ (官品) rather than ‘youjiti’ (有機體), focusing on the conceptual distinction between guan (官) and ji (機). Previous scholarship has examined how his translation of ‘organism’ and his reception of Western organicism reshaped the traditional Chinese understanding of the relationship between the individual and the state in the late Qing period. By revisiting the competing Chinese translations of ‘organism’, this paper asks what conception of the physiological organ was at stake when Yan Fu chose guan over ji, and how his choice brings into view different ways of understanding what an organism is.

In modern Western physiology, the organ came increasingly to be understood not simply as an instrument of the soul, but as a differentiated part characterized by the function it performs within an organized living whole. Yan Fu’s preference for guan foregrounded this functional dimension. In classical Chinese, guan designated an office or charge and was also applied to bodily parts understood as having their respective offices within the body. Its semantic structure therefore differs from that of ji, which refers more readily to a mechanism, device, or mode of operation. By choosing guan, Yan Fu could emphasize that organs do not merely operate, but occupy differentiated roles within the living whole. This also explains his preference for guanpin over youjiti, since a body possessing ji or mechanisms could equally well be a machine.

The distinction between guan and ji thus brings into view two different questions that can be asked about the physiological organ and, by extension, the organism. Ji foregrounds the question of how a part operates—through what arrangement, mechanism, or process it produces a given effect, that is, fulfils its function. Guan, by contrast, foregrounds a functional why—what role this part plays within the organization of the living whole and in virtue of what role it is identified as this particular organ. Unlike in the case of a machine, understanding a bodily part as a guan does not require that its function be externally assigned in advance; an organ’s role may itself arise through the organization and differentiation of the living body. Yet once understood as a guan, the part is individuated through its contribution to the whole rather than merely through its mode of operation.

Yan Fu’s translation therefore reveals a tension internal to the modern concept of the organism itself. To understand an organism is both to explain how its parts operate in terms of ji and to understand how the functions realized through these mechanisms contribute, as the activities of guan, to the maintenance of the whole. The choice of guan over ji was thus not merely lexical: it privileged one dimension of physiological intelligibility over another and, in doing so, made visible the conceptual relation between mechanism, organization, function, and the individuality of the organism.

Standard Objections to the No-Miracles Argument and Replies to Them

Seungbae Park (Ulsan National Institute of Science and Technology)

The aim of this talk is to lure nonexperts into the scientific realism debate. I introduce the following standard objections to the no-miracles argument and replies to them. Successful theories will be ousted as they were in the past (Laudan 1977; Stanford, 2006; Wray, 2007, 2010, 2013, 2018). There are historical counterexamples to scientific realism (Laudan, 1981). The success of science does not cry out for explanation (Hendry, 2001). Inference to the best explanation is not a compelling rule of inference (van Fraassen, 1989; Ladyman et al. 1997; Wary, 2008, 2012; Khalifa, 2010). There are antirealist explanations of the success of science (van Fraassen, 1980, Fine, 1986; Leplin 1987; Musgrave, 1988, Kukla, 1998; Stanford, 2000; Lyons, 2003). Scientific realism is an ad hoc hypothesis (Frost-Arnold's, 2010). Scientific realism originates from the end of history illusion (Nickles, 2017). This talk is intended to be useful to those who have never contributed to the scientific realism debate.

Limits of the Inference to the Best Explanation in Earth Science: The Case of Wegener's Continental Drift Theory

Qinyi Wang (HKUST)

Kleinhans and De Regt (2005) argue that because of the limited temporal scope of human observation and the stochastic nature of Earth's development process, full causal explanation is impossible in the earth sciences. Therefore, they believe that most explanations in the earth sciences are abductive inferences and rely especially on inference to the best explanation (IBE), with Wegener's theory of continental drift serving as the most typical example. According to Lipton (2004), IBE proceeds from the available evidence to the hypothesis that, if true, would best explain that evidence. However, after re-examining the history of Wegener's theory, I shall argue that Kleinhans and De Regt's reconstruction of Wegener's case as IBE cannot explain the full process of how his theory was accepted and how theoretical changes happened in the earth sciences. And I shall argue that earth sciences cannot be simply reduced to abductive inference.

There are three reasons as follows: Firstly, the reconstruction as IBE cannot explain why Wegener's theory was rejected at that time. Geologists' attitudes toward the same evidence were significantly different, and both supporters and opponents possessed sufficient grounds for their positions. For example, Holmes (1929) proposed a mantle convection mechanism that supported the physical plausibility of Wegener's theory, while Jeffreys (1926) opposed Wegener on the basis of geophysical calculations and Schuchert (1928) opposed him on the basis of geological evidence. However, Wegener's theory was rejected at that time eventually; Secondly, Pellegrini (2019) pointed out that only about 22% of professional geologists accepted continental drift before 1961, but by 1977 that proportion had risen to approximately 87%. This shift occurred because of social factors, which IBE cannot explain; Thirdly, in the process from Wegener's original continental drift theory to the accepted theory of plate tectonics theory, there were at least three rounds of revision, including Daly's supplement of gravity-sliding mechanism (1926), and Holmes' supplement of mantle convection (1929), Hess' revision of seafloor spreading(1962), and Vine and Matthews' discovery of the symmetry of seafloor magnetic anomaly stripes (1963). Each round involved different scientists and different theoretical frameworks. What was finally accepted was not Wegener's original continental drift theory but an entirely new theory constructed by multiple generations of scientists. The IBE model assumes that scientists can infer the best

explanation from available evidence (Lipton, 2004), but this model cannot explain why continental drift theory underwent such a substantive revision.

Thus, I argue that IBE model cannot simply reach the explanation in the earth sciences, because based on Wegener's continental drift theory, it cannot explain why this theory was rejected at that time, cannot explain the social influences and cannot explain the complex process of finally accepted revised theory.

Antitheses to Eclecticism: Reconsidering Post-Kuhnian Philosophy of Science

Weichang Wang (Chinese Academy of Social Sciences)

Kuhn and many later philosophers of science appear to have overturned the dogmas of traditional philosophy of science, yet they remain entangled in strikingly similar patterns of reasoning. Kuhn questioned the logical positivist claim that empirical observation can confirm scientific propositions, but his own conclusions still rest on observations of historical facts about science. Feyerabend takes a few cases to show that Galileo did not strictly follow scientific method, while denying that scientific theories can be falsified by a finite number of counterexamples. Hacking argues that manipulability implies the reality of unobservable terms, but without theoretical interpretation we cannot determine what scientists actually manipulate in the laboratory. Rouse rejects the Quinean and Hessean view that all understanding can be represented as propositions or hypotheses, yet the conclusion that "not all understanding is propositional" is itself propositional. In defending "local knowledge", Rouse asserts that phenomena produced in the laboratory exist only there, but he never explains how, apart from the observation and induction advocated by logical positivists, one could rule out the possible natural occurrence of such special laboratory conditions in the vast universe. Latour borrows the term "black box" to refer to scientific facts or technological products that have been accepted as truth without further examination of their internal structure, but he does not explicitly clarify whether his own theory is such a black box, and therefore cannot fully undermine the authority of science and technology. These antitheses indicate that the revolutions in philosophy of science since Kuhn are not thoroughgoing: philosophers before and after Kuhn *have* revealed certain features of science, but none of them can characterize science as a whole with sufficient representativeness. Therefore, it is better to adopt an eclectic view when it comes to philosophical issues of science.

Idealisation and Exemplification in James Nasmyth's Images of the Moon

Chenxi Ma (University College London)

Scientific representations do not always reproduce the actual state of their targets at a particular moment. They often select, reorganize, or idealize features in ways that depart from direct observation. This raises an epistemological question: how can such departures still contribute to scientific understanding? This paper addresses this question through James Nasmyth and James Carpenter's *The Moon: Considered as a Planet, a World, and a Satellite* (1874). Previous studies have shown that its lunar images were highly mediated and carefully constructed. However, the fact that these images were constructed does not by itself explain their epistemic value.

The first case is Plate IV, the lunar "picture-map" made by Nasmyth. It presents different lunar formations under approximately the same angle of illumination, although this collective condition never occurs on the Moon itself. Drawing on Catherine Elgin's account of felicitous falsehoods and idealisation, together with Angela Potochnik's emphasis on epistemic purpose, I argue that this departure should be evaluated in relation to the purpose of this operation. By adjusting the shading and creating a common illumination condition, Plate IV places different formations on a more comparable visual basis. Its idealisation is therefore epistemically useful because it reorganizes appearances for a specific task of observation and comparison.

The second case is Nasmyth's wrinkle analogy between the surfaces of an apple and a hand and selected lunar formations. Elgin's account of exemplification helps explain how the photographs make a visible pattern of ridges, folds, and furrows salient. These objects function as exemplars of a particular surface pattern and guide attention to a related "wrinkle character" in selected lunar formations. But the exemplification has limits. Following Frigg and Nguyen's distinction between exemplification and imputation, exemplification alone does not establish that the exemplified feature applies to lunar formations. Attributing this feature to lunar formations requires a further representational step. Nor does it prove the contraction mechanism proposed by Nasmyth and Carpenter. The visible similarity, structural attribution, and causal interpretation must therefore be treated as distinct epistemic steps.

The final section examines how these constructed representations support further reasoning about the Moon. Van Fraassen's discussion of selective resemblance shows that representational success does not require resemblance in every respect. Suárez's inferential conception provides a further step by focusing on the specific inferences that competent and informed users can draw about a target. Nasmyth and Carpenter use Plate IV in this way when they draw conclusions about the heights of lunar formations from shadows, infer connections between craters from their linear arrangement, and recognize wrinkle-like structures in certain lunar ridges.

I argue that the epistemic value of constructed representation does not come from construction itself, nor from overall fidelity or visual resemblance. It depends on whether selective operations serve a clear epistemic purpose, make relevant features and relations salient, and support constrained inferences about the target. Therefore, the key question is not simply how far a representation departs from observation, but what epistemic work that departure makes possible.

Bamboos, Cicadas, and the Explanatory Argument for Mathematical Platonism

Alan Baker (Swarthmore College)

Arguments for platonism, the philosophical view that abstract mathematical objects exist, have centered in recent years around an explanation-based version of the Quine-Putnam indispensability argument. According to this argument, we ought to believe in mathematical objects because they play an indispensable explanatory role in science. However, if this argument is to be effective, convincing examples of mathematical explanation in science (MES) need to be found. For the past two decades, the philosophical discussion has revolved around the same handful of examples, many not particularly convincing. I hope to change this by introducing some new case studies, which I will present and then discuss the implications of.