Workshop

Scientific Reasoning In Action – From The Early Modern Period To 1900

Book of abstracts

Date: 10-12 Oktober 2018
Venue: Panamarenko room Zebrastraat events centre

Organization:
Centre for Logic and Philosophy of Science
(Ghent University)
the Centre for Logic and Philosophy of Science (Brussels)

Chairs

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About the workshop

This workshop is organized within the series "Logic, Reasoning, and Rationality":
`http://www.lrr.ugent.be/`

The series is supported by the Research Foundation Flanders (`http://www.fwo.be/`) through the scientific research network on Logical and Methodological Analysis of Scientific Reasoning Processes (LMASRP):
`http://www.lmasrp.ugent.be/`

Abstracts

Invited speakers

Maarten Van Dyck

Physics as problem solving activity

The scientific revolution is often linked with the idea of "mathematization". To properly analyze the contribution of mathematics to the new sciences, it is important to start from the sixteenth century understanding of this discipline. I will argue that it was most importantly mathematics as a problem-solving practice that offered new ideals of knowledge, which would profoundly change the nature of physics.

Marij van Strien

Reasoning from Metaphysical Principles in 19$^{\text{th}}$ Century Physics.

In this talk, I examine what an ideal theory looks like according to physicists, and how this shifted in the late nineteenth century. Physics being the most fundamental of the natural sciences, it is often expected to give an ultimate description of nature; the ontology put forward in physics is then the basic ontology of the natural world. But in the late nineteenth century, many physicists, including Mach and Poincaré, explicitly restricted themselves to giving mere descriptions of phenomena, while being agnostic about ontology. I examine the background of this shift in the aims of physics, and how it mattered for scientific practice.

Charles Wolfe

The Idea of "Philosophy of Biology before Biology".

Contributed Talks

Iulia Mihai

Brook Taylor, Analogy and Newtonian Isochrony.

Brook Taylor (1685–1731) gives a mathematical proof by which he claims that the vibrating string moves 'like the pendulum in a cycloid'. This is also regarded as the first successful mathematization of the string, a celebrated contribution to eighteenth-century vibration theory. However, Taylor's claim does not merely compare the motion of the string and the motion of the cycloidal pendulum, nor does it just place the two objects in the same class of isochronous bodies. Instead, I argue that (1) a crucial part of Taylor's argument for the isochrony of the string is based on an analogy between the pendulum and free fall and that (2) this is a Newtonian analogy which (3) Taylor uses in a creative way as he constructs the ontology of the ultimate parts of the string. Here, the analogy between the motion of the pendulum and free fall is constructed, and analogical reasoning establishes synchronicity under specific Newtonian conditions, which constitute a theoretical import into Taylor's mechanical treatment of the string.

The argument I put forth here aims at illuminating the relationship between the accounts of Taylor and Newton. Each of them has been largely neglected: Taylor's concise style has had an undeserved reputation as 'obscure', whereas Newton's treatment of the cycloidal pendulum hardly got any attention, having been considered derivative of Huygens'. As I show, Taylor's treatment of the taut string can rightfully be taken as an extension to Newton's Section 10 of Book 1 of the *Principia*. While Newton did not take up the problem of the vibrating string, mathematicians such as Taylor use theoretical tools from the Principia to solve problems which go beyond Newton's discourse of point-masses. Taylor had long been known to be a Newtonian, but his treatment of the isochrony of the vibrating string has so far been read programmatically in an anti-Newtonian way by historians of vibration theory.

Andrea Bréard

Analogy or Induction? Translating and interpreting Li Shanlan's Argumentative Mode

Attitudes towards reasoning in mathematics were seldom spelled out explicitly in texts from early modern China. A historical shift nevertheless can be observed after the second wave of influx of Western modes of argumentation and proof during the second half of the 19th century. Taking Li Shanlan's 李善蘭 (1811-1882) Comparable Categories of Discrete Accumulations (Duoji bilei 垛積比類, 1867) as a case study, this paper investigates the epistemology and history of a specific mode of reasoning underlying the combinatorial procedures listed rhetorically and illustrated visually in this work, arguing that they reflect a specific mode of nineteenth-century mathematical argumentative practice, merging philosophical concepts of "analogical" reasoning from the Neo-Confucian philosophers with practices of (incomplete) induction found in foreign mathematical texts, which Li Shanlan himself had translated into Chinese. Li is a particularly interesting and original figure since he taught and worked in mathematics in a compartmentalized fashion: either in purely traditional Chinese style or by adopting a syncretistic mathematical language with strong cultural components, mixing, for example, more specifically in his combinatorial work, practices of diagrammatic representations of series with rhetorical inductive arguments. This kind of mathematical discourse places Li Shanlan historically within the broader issue of the construction of modern global mathematics and reveals him as an individual mediator who established connections that crossed seemingly insurmountable epistemological borders between European and Chinese scientific practices and linguistic cultures that were claimed to be incommensurable.

Henk de Regt

Intelligibility and metaphysics: understanding gravitation

In my presentation I will analyze the famous debate between Isaac Newton and his contemporaries (notably Christiaan Huygens) about the alleged unintelligibility of Newton's theory of gravitation, and the subsequent development of physicists' views on contact action versus action-at-a-distance in the eighteenth and nineteenth century. Metaphysics played an important part in these debates, and physicists' metaphysical commitments and their views on the intelligibility of theories were intricately related. I will interpret this historical episode in terms of my philosophical theory of scientific understanding.

Ivan Malara

Writing Philosophy in the Early Modern Period. Galileo's Juvenilia as a Study Case

What did it mean to write a scientific text in the early modern period? How was natural philosophy taught and learnt in academies before the advent of the New Science? In this presentation I will try to approach these questions by analysing a peculiar text ascribed to the hand of Galileo: the *Juvenilia*. This work still remains an unclear chapter within the entire Galilean work, but I argue that, if well scrutinised, it could become an important source to study the practices through which academic texts on natural philosophy were often produced.

Jonathan Regier

Johannes Kepler and Method as Justification

Kepler's work has long been a resource for historians and philosophers interested in scientific method. One reason why is that we can find exactly what we want in it, if we look hard enough: abduction, regressus, reasoning by analogy, gritty inductions and inspired deductions. Such a methodological plenitude exists because Kepler's activities spanned many disciplines, from optics and astronomy to physics, music theory, astrology, and a kind of Platonic cosmology that was all his own. But when considering Kepler and method, there is a point deserving of emphasis: his entire career was oriented toward justifying the Copernican world order and grounding it in what he hoped would be an unshakable causal framework. His methods were always directed toward those ends. In other words, he was not an impartial seeker of truth; he was an intellectual partisan of passionate and sustained convictions. In my talk, I will consider Kepler's first treatise, the youthful *Mysterium cosmographicum* (1596), particularly the narrative of discovery therein, where Kepler gives us a carefully edited account of his own practice as a mathematician and astronomer. I will discuss how his portrayal of method here, as elsewhere, is a construction meant to address a number of epistemological and disciplinary questions. Method will be considered as a practice of intellectual self-justification and promotion, as much a product to be exhibited as the results that it had supposedly generated.

Chiara Ambrosio

In Pursuit of History: Lessons from C.S. Pierce's History of Science.

In the late 1890s, the American philosopher and scientist Charles S. Peirce embarked on a monumental task: to write a history of science in one volume for the publishers Putnam's Sons. Peirce had been working on historical themes for his entire life, but the 1890s had been particularly prolific on that front. In 1892, he had delivered a series of lectures on the history of science at the Lowell Institute in Boston. Following the success of the lectures,

Peirce's projected volume promised to deliver –- in the space of a hundred thousand words! –- a comprehensive narrative of key scientific milestones and achievements from high antiquity to 1898, the very time in which he was writing. Alas, Peirce's volume was never published; but his historical manuscripts offer a glimpse of what his history of science might have looked like.

If there is one aspect of Peirce's history of science that is best known to historians and philosophers of science, it is possibly his discussion of Kepler's discovery of the elliptical orbit of Mars. This is partly because Peirce's discussion has featured prominently as an example of abductive reasoning in Russell Norwood Hanson's (1958) *Patterns of Discovery*. Peirce scholars are well aware that Peirce had consulted a broad range of historical astronomical data, including six different editions of Ptolemy's star catalogue, the works of the Persian astronomer al-Sufi, Tycho Brahe's star catalogues and Kepler's own Rudolphine Tables, along with his earlier Astronomia Nova (Eisele 1979, 106; HP1, 3-4; Dauben 1996, 55-56). However, it has been rarely pointed out in the literature that one of Peirce's key points of reference in this discussion is first and foremost a Victorian debate which would shape the course of the relationship between history and philosophy of science: the controversy that, four decades earlier, saw William Whewell and John Stuart Mill argue about the nature, methods and logic of science, in which Kepler featured as a central example (Snyder 2006).

In this talk, I will present the working hypothesis that Peirce's discussion of the case of Kepler is an attempt at adjudicating between Whewell and Mill. Specifically, I will argue that Peirce's introduction, via the case of Kepler, of his distinctive formulation of abductive reasoning exemplifies his commitment to the legacy of the (Kantian) view of ideas that Whewell placed at the core of his antithetical epistemology, and particularly his account of induction (Snyder 2006). I will advance two main claims: 1. That Peirce's notion of abduction builds on, and stretches forward, the role of "ideas" that Whewell placed at the centre of his account of inductive reasoning; 2. That Peirce's "friendly" criticism of Whewell led him to conceptualise abduction as form of inference that accounts for the growth of ideas: how habits are disrupted by experience in a way that clashes with our preconceived notions. I will conclude with a note on how this historicised conception of abduction offered for Peirce a conception of reasoning that from science could stretch to the pursuit of history.

Bohang Chen

Kant's Reasoning on Evolution

This talk reconstructs Kant's two pieces of reasoning on evolution which have long been forgotten. First, contra the traditional interpretation, I show that Kant argued for species evolution in the historical sense through abductive reason. According to Kant, to account for "the origin of the species", (1) the reference to God is forbidden, because God is not a legitimate concept in science. Moreover, (2) the appeal to *generatio equivoca* (life out of matter) is less promising than that to *generatio univoca* (life out of life). Finally, given (2), the diversity of living forms and the fact that experience only shows *generatio homonyma* (organisms out of the organisms from the same species) rather than *generatio heteronyma* (organisms out of organisms from a different species), to explain the origin of the species (i.e., the first organism of the species), one has no choice but appeals to *generatio heteronyma*, namely, species evolution. Kant's conclusion re-appears in *Opus Postumum*, "[can be thought] a priori, as sprung from a single seed...preserving its species and the species that are born from it" (1993, 22: 241). For Kant, it is reason (abductive reasoning) rather than experience that forces us to accept species evolution.

Second, I show that Kant also remained suspicious of the explanatory force of a genealogical story of evolution, which, according to Kant, "has merely put off the explanation" to the common ancestor. Kant made his case by discussing hereditary alteration (inheritable variations for Darwinism). For Kant, in empirical observation it is well confirmed that individuals of certain genera in nature might undergo some incidental changes and the altered

characters become hereditary afterwards. But Kant's reasoning goes much deeper than this observational result. According to Kant, even though these alterations must be judged as accidental, the phenomena that some finally become hereditary cannot be accidental at all and must be grounded in "the undeveloped original predispositions" (2000, 5: 420) of the original genera. Then a bold conclusion follows from Kant's reasoning. Even assume that the Darwinian genealogical story of evolution is empirically sound, that is, an organism of one species did historically evolve to an organism of a different new species, it still fails to touch the essentials of a genuine theory of evolution. Kant's point is rather that the possibility of the new species is grounded in the previous species. Then, if we push Kant's point to its logical conclusion, it follows that the possibility of all species must have already been grounded in the common ancestor, more exactly, the power attributed to the common ancestor to generate all species ("an organization purposively aimed at all these creatures", 5: 419-420). So, there is no doubt that Kant would decline to accept the Darwinian theory of evolution as a genuine theory. To illustrate this I compare the system of species with the periodic table of chemical elements.

Steffen Ducheyne

Van Musschenbroek on Newton's regulae philosophandi, Aquatic Polyps, and the Dangers of Analogy

Richard P. Feynman once described Newton's theory of universal gravitation as "probably one of the most far-reaching generalizations of the human mind." Newton's *regulae philosophandi*, which got their name in the second edition of the *Principia*, were vital in the establishment of Newton's bold generalization. The function of the *regulae philosophandi*, as Newton was well aware, is to provide justification for the ampliative conclusions that occur in Book III of the *Principia*. Correspondingly, if one seeks to understand the way in which ampliative conclusions are established in Newton's natural-philosophical methodology, one should have an adequate understanding of the *regulae*. In this talk, I will consider Pieter van Musschenbroek's reading of the *regulae*. It will be shown that van Musschenbroek was a very astute commentator who very well understood the *regulae*, but that at the same time he called attention to their potential limitations in view of the research he was pursuing at the time.

Erik Weber

The Causes and Cures of Scurvy. On James Lind's Methodology.

Based on joint work with Leen De Vreese

The Scottish physician James Lind is the most celebrated name in the history of research into the causes and cures of scurvy. This is due to the famous experiment he conducted in 1747 on H.M.S. Salisbury in order to compare the efficiency of six popular treatments for scurvy. This experiment is generally regarded as the first controlled trial in the biomedical sciences.

The experiment relates to the possible cures of scurvy, not to its causes. The traditional view is that Lind was much less innovative in his research on the causes of scurvy. In 1867 the French naval surgeon H. Rey, in his analysis and critique of Lind's Treatise of the Scurvy, complained that, while Lind relied only on experiments in matters of therapeutics, his theory on the causes of scurvy was based on "ideas that are far removed from any kind of scientific precision". According to Rey, Lind accepted some ideas on the causes on scurvy too readily, because in that domain he did not rely on experimental results.

The aim of this paper is to qualify both the idea of Lind as a modern therapist and of Lind as old fashioned in his investigation of the causes of diseases. In the first part we explain how contemporary biomedical scientists investigate causes and cures of diseases. In the second part we argue that there are important differences between

Lind's experiment and contemporary experimental practice. In the last part we argue that Lind's methodology for investigating the causes of scurvy was more modern than Rey suggests.

Geoffrey Blumenthal

Reasoning in Practice in Late Eighteenth-Century Chemistry.

At the middle of the eighteenth century, the phlogistic theories were underdetermined by evidence. In the absence of understanding that air was involved in chemical reactions and of experimental practices that would investigate this, the problem of unconceived alternatives (Stanford 2006) was very real, and the vast majority of inferences at this time in chemistry were abductive but were no better than the best of bad lots (c.f. Van Fraassen 1989, 143; pace Schupbach 2014, c.f. Dellsén 2017). The later phlogistic theories were no better and added the further problem of multiple inconsistent theories of which none could be thoroughly experimentally supported or disproved.

Lavoisier's experiments were standardly performed in closed conditions and with weighing of substances. His reasoning involved taking into account wide knowledge of preceding work as well as increasingly wide experimental testing, leading to inferences that formed a consistent set with each other. In practice, abduction was far more reliable when used in this way than when used for single experiments. But also, abductions of this sort received not only further abductive support from later experiments, but in some cases also a great deal of later inductive confirmation during the routine performance of later experiments. These points help to explain why so many of his abductions were the best of the available potential explanations, were not merely good enough (Lipton 1993) but approximately true, and have remained so at the level of the chemical elements and their compounds.

In his reasoning, Lavoisier utilised or achieved a number of types of partial unification. His methodology overtly excluded any unification involving the ontology of experimentally-inaccessible substances, and in this way was non-fundamentalist and deliberately vertically incomplete. His methodology involved compositional redescription and causal mechanisms. The partial ontological unification he did achieve concerning chemical substances was on two horizontal levels: those of chemically simple substances and of compound substances. For the first time, accurate inferences were achieved about the simplicity of most of the chemically simple substances he listed. Compounds were redescribed in terms of their component simple substances, and the two levels were partially unified in that compositional and mereological way. The only elimination involved concerning compound substances was of other types of names for those substances. The only other eliminations were of hypothetical entities such as phlogiston whose hypothesised places in a theory of chemical composition were incompatible with the full range of experimental evidence concerning experimentally-accessible substances, and which had previously been part of a spurious partial unity (Schurz 1999). In this case, the partial unification was not 'simply' a systematisation of old beliefs (contra Halonen and Hintikka 1999). The increased simplicity, coherence, systematicity and accuracy of the new compositional chemistry was related to, and in turn largely justified, the empirical taxological monism of the new compositional chemistry. The antiphlogistians rightly judged that their set of abductions was largely approximately true, but was not fully established and was subject to later correction (e.g. Guyton 1796).