ABSTRACTS

Joint Commission 1

Descartes and mathesis universalis - the rise of the modern algebra

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My paper of the history and philosophy of mathematics is aimed to create the clear picture about the radical changes that happened in 17th century mathematics: the gap between the ancient geometry and modern algebra is wide, but how exactly Descartes was led to the algebraic approach in his mathematical work Geometry? What were the philosophical thoughts behind the ancient Greek and early modern era?
This paper reveals that although Descartes did not value Euclid and his followers, Descartes believed that other ancient mathematics, like Diophantus and Pappus had the secret method, mathesis universalis they kept hidden. Descartes also claims that the rise of the early-modern algebra is the revival of this ancient method. This research presents the model about this ancient method that might have been in Descartes' mind while developing his new mathematics.
My main claim is that tor Descartes axiomatization of geometry itself is only a trickery that is meant to hide the true nature of mathesis universalis. In modern philosophy it is often suggested that Descartes was a precursor to the Newton's axiomatic method, but in my paper I prove this is a misunderstanding.

A note on the role of physical reasoning in Ptolemy's mathematical astronomy

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One of the main criticisms directed by Copernicus against Ptolemaic geocentric planetary theory addresses the latter's lack of unity, as expressed by the popular monster metaphor. However, although Copernicanism allowed a greater unification of astronomical hypotheses, most notably in the explanations of planetary retrograde motions, one might still ask if such criticism is fair within the framework of the Almagest. The present paper aims to provide some insight into how Ptolemy regarded the issue of the diversity of hypotheses in astronomy. The subject integrates a larger discussion about the epistemological status of mathematical hypotheses at Book IX of the Almagest, which is understandably rich in meta theoretical content since it introduces Ptolemy's most original work, the planetary models with an equant circle. The core of the argument consists in the assertion that circular motion is preserved for all celestial phenomena, without exception, in spite of the fact that these phenomena are not all alike. This solution provides a key for understanding the role of celestial physics in Ptolemy's mathematical astronomy, and challenges Pierre Duhem's instrumentalist interpretation.

Lyons, Kepler, and the commitments of deployment realism

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Timothy Lyons argues that Psillos' "deployment realism" should be committed to the truth of all the components actually employed in reaching successful novel predictions. He then explains that Kepler made novel predictions (e.g., that the Sun spins, that a planet's speed is highest at its perihelion and lowest at its aphelion, etc.) reasoning from the false assumptions that
(1) the planets tend to rest, but the Sun rotates, and transmits this rotation to them through rays whose force decreases with the distance.
This, concludes Lyons, refutes deployment realism (T. Lyons, "Scientific Realism and the Stratagema de Divide et Impera", BJPS 2006).
I reply that in abduction we should postulate only the weakest cause sufficient to explain the effects. Equally, in explaining a novel prediction, we should assume the truth of only the essential components, i.e. the weakest ones sufficient to reach the prediction.
Kepler abductively inferred (1) from facts he knew:
(2) the planets move around the Sun on the same plane and in the same wise, and their velocities are in the inverse order as their distances from the Sun.
This could suggest that the solar system rotates as a coherent (but viscous) disk, whose periphery is slower than the centre. Of course (1) was unnecessarily strong as an explanation of (2), but it had a weaker core, better supported by (2), and true:
(3) the solar system moves around (a point close to) the centre of the Sun, due to something which is inversely related to the distance from it.
Since Kepler's new predictions could already be derived from (3), (2) was not essential to them, while (3) was

essential but true, and this confirms deployment realism.

D’Alembert’s doubts

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Newton discussed in the ‘Optical lectures’ (1670-1672) two potential laws enabling to explain the phenomenon of dispersion of white light through a prism. The first law, quadratic, had been elaborated from considerations on speed of lighting corpuscles in the frame of gravitation theory. The other law, formulated by elaborating a quantitative colors scale, had a linear expression. On a quantitative point of view, these two laws did not differ for the results they led to, in any case insufficiently so the differences are detectable with the methods of measure that Newton had at his disposal. It is only much later, when he had elaborated his theory of light that Newton, based on the analogy with the results of musical harmony, finally adopted the linear law of dispersion. He decided on the impossibility to be able to build refractor achromatic systems.
In 1747, Euler criticized the Newton’s point of view and proposed a logarithmic-type law to explain the phenomenon of dispersion. Based on these ideas, the Englishman John Dollond managed to build achromatic systems and published his discovery in 1758. D’Alembert, in the 20 and 49 ‘Mémoires’ of ‘Opuscules mathématiques’, dedicated to achromatic lenses, discussed the matter and expressed doubts about theories respectively exposed by Newton and Euler.
He raised also the epistemological issues, notably linked to different possible choices of a law and its necessary or contingent nature that we propose to discuss.