

Thursday, June 24 PM 1:30-4:30, Room 141

**Session 1 : Early Modern Scientific Method**

Daniele Cozzoli, “Analysis And Synthesis In Descartes’ Method”

Eric Schliesser, “Berkeley's response to Newton”

Saul Fisher, “Gassendi’s regressus Method and the Barometry and Vacuum Experiments”

William L. Vanderburgh, “Non-Pascalian Probability before Bacon—and after”

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**“Analysis And Synthesis In Descartes’ Method”**

**Daniele Cozzoli**

The standard view on Descartes’ method looks at it as a two-steps method combining analysis and synthesis. For the standard view, Descartes’ second rule of the Discours would be nothing but the analysis of ancient geometers, and the the third rule would be the axiomatic euclidean method. Descartes’ idea would be: 1. to reduce complex notions to more elementary ones and 2. to repete the steps of the reasoning in the opposite way in order to put all the notions we have found in a more natural order. The analytical step would be the reductive part of the method, where the synthetic step would be the ordering part.

Though this interpretation has a long history, since we can find it in Nicolas Poisson’s Remarques sur la méthode de M. Descartes of 1670, and has been held in a somehow fascinating philosophical framework by Imre Lakatos, I am arguing that it is a rational reconstruction that does not reproduce Descartes’ thought.

The aim of this paper is to reconstruct Descartes’ original ideas on analysis and synthesis. I will be providing both a philological and a theoretical evidence in order to show that Descartes looked at his method in the Regulae as well as at his four rules of the Discours as a kind of analysis. Descartes aimed at giving a universal method apt to discover and justify everything could fell under the realm of knowledge. In order to accomplish this task he tried to renew the analytical method, used by ancient geometers to solve problems in constructing curves, and he developed a form of this method where reducing and ordering where two simultaneous and not consecutive actions.

I am drawing this conclusion both on a philological and theoretical basis: 1. Descates has never called his method a method of analysis-synthesis, but he has always referred to it as a method of analysis; 2 there is no proof in his Géométrie following this alleged method in two steps. On the contrary, Descartes’ way of proving in the Géométrie can easily been explained in terms of a notion of method where reducing and ordering are two simultaneous activities.

Descartes has always given a negative evaluation of the synthetic method, since he has always considered it a method useful only to explain what has already been discovered in some different way. He explicitly claimed he used the synthetic method in the Principia, in order to show his phisical theories, nevertheless he didn’t use it in a strict mathematical sense but only in a broad and informal sense, since Descartes didn’t believe in the usefulness of the axiomatic method.

This paper is divided into four sections: in section 1 I will recall the main features Pappus attributed to the two different method of analysis and synthesis; in section 2 I will report Descartes' evaluation of them; in section 3 I will outline an alternative interpretation of the role of analysis and synthesis in Descartes' *Discours de la méthode* and in his *Regulae*; and finally in section 4 I will have a look to the way of proving in Descartes' *Géométrie*.

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## **“Berkeley’s Response to Newton”**

**Eric Schliesser**

In the third of Berkeley’s *Three Dialogues between Hylas and Philonous*, Hylas offers an indispensability argument (hereafter IA) for why the existence of “matter” should be accepted; he deems it essential for the practice of Newtonian science. This IA is not an isolated occurrence in Berkeley’s philosophy: he considers very similar objections (that is, the sixth and tenth) in *Of the Principles of Human Knowledge* (hereafter *Principles*): the “notions” that Berkeley advances must be false because they are “inconsistent with several sounds truths” of natural philosophy and mathematics. In response to this objection, Berkeley offers an ‘Instrumentalist’ reinterpretation of the achievements of natural philosophy and mathematics. That is, he denies the separate claim to authority of these sciences over philosophy or at least independent from it. But he can only do so by limiting the aim of these sciences to predictions alone.

Berkeley is not the first to consider an IA. My concern in this paper is thus not to establish Berkeley’s originality, but rather to call attention to the fact that he is among the first (if not the first) to recognize that Newton’s achievements could serve as a separate and authoritative source of justification within philosophic debates. More intriguingly, Berkeley also seems to realize that these achievements threaten the conceptual unity between first philosophy and natural philosophy presupposed by his Early Modern predecessors (recall Descartes’ tree of knowledge).

Moreover, Berkeley is acutely aware that, in his time, a further gulf has opened up between modern philosophy and what he calls “common life.” This gulf leads, he believes, to skepticism not only about the existence of God but also about the use and relevance of philosophy. This explains the ultimate significance of Berkeley’s treatment of the IA.

First, this paper analyzes Berkeley’s formulation of the IA and draws out implications for Berkeley’s larger project. Second, it offers some context for claiming that Berkeley is distinguishing between philosophy and science against those that would claim this distinction is anachronistic. It then explains how Berkeley’s Instrumentalism is a natural response to the indispensability argument. Finally, I suggest that Berkeley’s “philosophical therapy,” which is supposed to reconcile philosophers with common life, ends up alienating them from Newtonian science.

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## **“Gassendi’s regressus method and the barometry and vacuum experiments”**

### **Saul Fisher**

Pierre Gassendi proposes a variant on the regressus method in his *Institutio Logica* but to what extent does he employ that method in his own scientific practice? I examine Gassendi’s most detailed reasoning about experimental episodes—his accounts of the barometric and vacuum-related experiments of 1648. His scientific practice in these accounts would conform to the method of the *Institutio* just in case they met two conditions. First, if the schema of their core reasoning could be construed as plausible instances of Gassendi’s ‘probabilist’ deductive brand of argument; and second, if such accounts could be construed at least loosely as comprising resolution or composition steps performed to realize discovery tasks. This much would be a banal achievement, however, if the fit were so loose that these schemas also conformed to methods competing with Gassendi’s model. More significantly, it turns out, these experimental accounts relate reasoning that runs against the prescribed method of the *Institutio*. In Gassendi’s reconstruction of the Pascalian barometry experiment, the search for the middle term is rife with mystery—there is no univocal way to reconstruct the reasoning that underlies each account in syllogism form. In addition, it is a daunting if not unfeasible task to find a judgment-realizing step that corresponds to the discovery-realizing step which is the primary feature of these experimental accounts. Even more importantly, these experimental analyses reveal a central methodological issue not as much as glanced upon in the *Institutio* perspective: what are the guidelines for hypothetical reasoning? Though Gassendi maintains a vision of science based on information from the senses, his own reasoning in these and other writings relies on hypothetical assumptions for which we have no direct sensory evidence. Indeed, he follows a familiar method of hypothesis, embracing substantive assumptions about the kinds of causes there may be, deducing their effects, and then explaining the data as effects of such causes. (Notably, he assumes that only a corpuscularian-mechanical picture and an interparticulate void allow for the motion of bodies.) I close by sketching Gassendi’s views—quite apart from his regressus method of the *Institutio*—as to what proper role such assumptions may have in advancing empirical inquiry, and how we are to judge their merits and admissibility.

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## **“Non-Pascalian Probability before Bacon—and after”**

### **William L. Vanderburgh**

Probable judgments in light of partial evidence are as ubiquitous in science as they are in ordinary life, and have long been a concern of philosophers of science. Since Pascal, probability has usually been treated in mathematical terms, as formalized in the Kolmogorov axioms. It is easy to show that Bayes’ Theorem (for updating one’s degree of belief in some hypothesis in light of new evidence) follows as a simple consequence from the Kolmogorov axioms. It is thus not at all uncommon to find philosophers of science who think that the degree of probability (degree of belief) one ought to attribute to (have in) a given scientific hypothesis in light of a given body of evidence is to be determined through Bayes’ Theorem. L. Jonathon Cohen argues in *The Probable and the*

Provable (Oxford, 1977) that there is a non-Pascalian conception of probability that is not only perfectly coherent and respectable, but which is moreover presumed by any set of assumptions strong enough to generate Pascalian probability. This non-Pascalian probability has a sphere of application quite different from that of mathematical or Pascalian probability—Cohen 1977 argues that the law is one area which is and ought to be treated in terms of non-Pascalian probability. In “Some Historical Remarks on the Baconian Conception of Probability” (Journal for the History of Ideas, 1980), Cohen argues that Ian Hacking was wrong to accuse Francis Bacon of having no concern with probability or inference under uncertainty, and that Bacon and later writers influenced by him, notably David Hume, “were very much concerned with probabilities, though not with probabilities structured in accordance with the mathematical calculus of chance.” (Dorothy Coleman used this interpretation of Hume as a non-Pascalian in her defense of Hume’s argument against miracles at HOPOS 2002, and in her “Baconian Probability and Hume’s Theory of Testimony,” Hume Studies 27 (2001).) I am persuaded that there is a respectable non-Pascalian conception of evidential probability, and that Bacon and Hume were advocates of its application in the epistemic evaluation of scientific hypotheses; I will cite arguments and textual evidence in support of both assertions. However, in light of James Franklin’s *The Science of Conjecture: Evidence and Probability Before Pascal* (Johns Hopkins, 2001), I will correct some of Cohen’s historical claims: the tradition of non-Pascalian evidential probability is in reality much older than Bacon, and Hume is not the first of Bacon’s followers to use it in the evaluation of empirical hypotheses. I will also offer some further arguments to the effect that Hume, et al., are essentially correct (and therefore many philosophers of science today are wrong): the non-Pascalian approach to probability is the proper one to apply to the epistemic evaluation of scientific hypotheses.

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