EXPERIENCE AND REASONING IN SCIENTIFIC METHODOLOGY
BETWEEN ANTIQUITY AND THE EARLY MODERN PERIOD

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ARISTOTLE'S PERCEPTUAL OPTIMISM

Aristotle was a direct realist about perception: he believed that perception puts us in direct contact with the world. What exercises our senses, in the most fundamental case, are actual properties of physical objects. This secures the infallibility of our senses, at least at the most fundamental level.

These properties that exercise our senses Aristotle calls 'special sensibles' and he defines each sense with reference to one type of special sensible, e.g. vision with reference to colours, hearing with reference to sounds, etc. (the only exception is the sense of touch which is defined with reference to three types of special sensibles – hot-cold, moist-dry, hard-soft – but we can disregard that). Whatever else may be perceptible in the world, is perceptible secondarily, on account of the special sensibles.

I will argue that Aristotle's division of special sensibles into types is meant to be both exclusive and exhaustive. It is exclusive because a special sensible of one type belongs only to that type, which means that it can be perceived only by the corresponding sense. And it is exhaustive, because there are no types of special sensibles other than those with reference to which the five senses are defined.

That Aristotle's division of special sensibles into types, set out in *De anima* II.7-11, is indeed exhaustive, follows from his argument at the beginning of *De anima* III.1 to the effect that there cannot possibly be any sense-organ other than the five familiar ones that house the five senses. With that argument Aristotle secures the thesis that there are no types of special sensibles in the world to which we have no access. Of course, Aristotle would readily agree that there are many imperceptible properties, but he would deny that there are any properties which are in principle perceptible, or which are perceptible to some creatures, but not perceptible by us.

More to the point, in *De sensu* 6 and 7 Aristotle argues that there are no special sensibles too small (or, by extension, too large) for us to perceive. This secures the thesis that there are no tokens of special sensibles which are inaccessible to us. In other words, we have full access to each and every special sensible in the world.

These three theses – perceptual realism, access to all types of special sensibles, and full access to every token of special sensible – constitute what I call 'Aristotle's perceptual optimism'. In effect, Aristotle thought that we can perceive all there is to perceive. In the first and largest part of my talk, I will discuss the texts and arguments that ground Aristotle's perceptual optimism.

In the second part of my talk, I will explain the stakes and motives of Aristotle's perceptual optimism. Very briefly, I will argue that Aristotle's concept of science (epistêmê) crucially depends on the three identified theses. Should any one of these three theses fail to obtain, Aristotle's views about science would collapse.

I will end my presentation with contrasting Aristotle's perceptual optimism with Plato's perceptual pessimism.

GWELTAZ GUYOMARC'H / Université de Lyon

INDUCTIVE REASONING IN ALEXANDER OF APHRODISIAS

The problem with induction is well-known among modern and contemporary philosophers. It entails in the asymmetry amidst induction, between the particular cases which serve as underlying premises, and the universality of the conclusion.

This criticism is actually already present since Antiquity: it is formulated as such by Alexander (*In Top.* 13, 11-17 and 86.24-28; confirmed by Simplicius *In Phys.* 1075.10 sq.), who limits induction to persuasion and removes every necessity. It can also be read earlier on in Cicero's De inventione. It was shown that Cicero's treatment of deduction was greatly based on Theophrastus (Fortenbaugh 1998). It would therefore be interesting to examine whether the same applies to induction and whether Alexander's theory ultimately comes from the ancient Peripatos. However, the difference between the Ciceronian treatment and Alexander's resides in the definition of induction: inference coming from similar cases in Cicero, it is redefined by Alexander as an inference ranging from individual to universal. Alexander explicitly rejects the first definition in *In Top*. 86.9-13.

One crucial question is to know how Alexander's criticism about induction can be on one hand, compatible with the development of our intellect described by Alexander in *De anima*, as 'grasping the universal through resemblance between particular perceptibles' (83.11-12). On the other hand, Alexander considers induction numerous times as a method effectively used by Aristotle (e.g. *In Met.* 160.9-11). Finally, induction appears in the famous last chapter of *Posterior Analytics* to describe the means to seize the first principles of every science. It is held as a 'true method' by Aspasius, which allows access to principles (In *EN* 3.17; 20.22-21.3). Alexander seems to fully accept it in his commentary on *Prior Analytics* I, 30, when he states that 'knowledge of the universal which is a principle, stems from the experience of individuals' (332.22-23).

One should therefore examine whether induction is the object of a unified treatment, or if the Peripatos (and especially Alexander) is already conscious of a distinction between dialectical and rhetorical induction (the one which in VIII.14 is reserved for young people) and demonstrative induction (the one that is used in the last chapter of *Posterior Analytics*). Note that such a distinction appears later in Averroes (see also the distinction between complete and incomplete induction in al-Fārābi).

R. J. HANKINSON / University of Texas, Austin

GALILEO AND THE CONCEPT OF DEMONSTRATION

> I say that if there were a true demonstration that the sun was in the centre of the universe and the earth in the third sphere, and that the sun did not go round the earth but the earth went round the sun, then it would be necessary to use careful consideration in explaining the Scriptures that seemed contrary, and we should rather have to say that we do not understand them than to say that something which had been proven is false. But I do not think there is any such demonstration…. To demonstrate that *the appearances are saved* by assuming the sun at the centre and the earth in the heavens is not the same thing as to demonstrate that *in fact* the sun is in the centre and the earth in the heavens.

I believe that the first demonstration may exist, but I have very grave doubts about the second; and in case of doubt one may not abandon the Holy Scriptures as expounded by the holy Fathers. (Cardinal Roberto Bellarmino to Paolo Antonio Foscarini, 12th April 1615: *Le Opere di Galileo Galilei*, Edizione Nazionale 12.172; my emphasis)

Galileo was steeped in the Aristotelian tradition of natural science. In his first teaching position at Pisa he regularly lectured on the Analytics. Moreover, the language of proof and demonstration appears throughout his works and throughout the whole of his long and productive life. But he was in a difficult position. He clearly believed, and from early in his career, in the physical truth of heliocentrism. But he was well aware that it was one thing to be impressed with the likelihood of some such celestial architecture, another to establish its truth, and another still to do so to the satisfaction of his Aristotelian opponents in the schools.

Indeed, he was never going to be able to achieve the last. The demands of the Aristotelian demonstrative canons could never, at least in empirical physical science, be satisfied. No empirically responsible science was ever going to be deductively derivable from self-evidently true axioms alone. In this sense, Bellarmine was clearly right: it is one thing to show how the phenomena follow (to some suitable degree of empirical accuracy) from some hypothesis, quite another to show that the hypotheses themselves are incontrovertible and rationally compelling (for this, presumably, is what a 'true demonstration' of them would amount to). Indeed, as is equally obvious, the only way to do that to the logical exclusion of any alternative would be to establish that only on the hypothesis in question could the phenomena be saved; and establishing any such thing is a very, indeed vertiginously, tall order.

In this paper, I seek to sketch how Galileo deploys the rhetoric of demonstration both positively and negatively in trying to establish his own case. To examine the light his own practices of scientific argument shed upon his developing views regarding the nature of the establishment of physical theories. To offer a few perhaps heterodox remarks in regard to Aristotle's theory, and in particular to Aristotle's own view of its role in empirical science, and of the extent to which he himself was committed to the attainability of axioms that were indubitable. And finally to suggest that here, as elsewhere, Galileo not only a had a juster appreciation of Aristotle's views than most of his contemporaries; his position was closer to Aristotle's own, properly understood, than most modern commentators would be willing to allow.

LUCAS ANGIONI / University of Campinas

EXPERIENCE AND EXPLANATORY REASONING IN ARISTOTLE'S PRIOR ANALYTICS I.30

On the traditional account of *APr.* I.30, there is a major difficulty in Aristotle's position. The difficulty is the gap between (i) collecting all the true statements in a given domain and (ii) finding the demonstrations, i.e., identifying the appropriate explanatory factors for each explanandum.

For Aristotle, demonstrating p is much more than finding a sound deduction for p. Thus, Aristotle should have elucidated how the collection of true statements will be enough for finding the demonstrations. For it is far from obvious how a mere collection of true predications might lead one to explanatory demonstrations, and it is far from clear why one would need an exhaustive inventory of all true statements in order to proceed to explanations.

Besides, a hasty reading of the chapter in its context suggests the following interpretation. There is a balanced division of work between experience (empeiria) and reasoning. First, experience is responsible for collecting the true predications in the field: it is a matter of mere empirical observation to settle that A is attributed to C. Then, reasoning is responsible for finding the demonstrations just by organizing the data according to the method outlined in the previous chapters (as is suggested at 46a3-10).

However, the difficulty remains, for the method outlined in the previous chapters has nothing to do with finding explanatory factors (and has no clue about how to find them). The method has to do with establishing that a given predicative relation is (or is not) the case, that's all.

Besides, this reading suggests a very poor account of empeiria itself: experience will be the mere empirical observation of brute facts; experience will be restricted to establishing that a given predicative relation holds; experience will aim at amassing all brute facts to the point of reaching an exhaustive inventory of all predicative truths in the domain. Of course, I do not deny that experience involves all these features, but I claim that it goes beyond them.

Thus, my intepretation goes for a different picture. I will highlight three points, but my main focus will be on the third point, which depends on careful examination of 46a10-27 and will lead us to consider some additional passages in Aristotle.

First, the method outlined in the previous chapters should have its importance downgraded (if the aim is to find demonstrative principles within a science). Of course, the method can be used as one tool among others by a scientist. But the method is restricted to settling that some predicative relation is true, without having the power to find why it is true. Besides, and more importantly, the method is useful for attaining principles only at very general level (cf. καθόλου μὲν in 46a10-11). In order to find out the principles required to explain each explanandum (cf. 46a17 ff.), something more is needed. (More on this below). If reasoning is needed (as an addition to experience) to find out the demonstrations, it is rather an explanatory sort of reasoning that goes much beyond the method outlined in the previous chapters.

This leads me to my second point. Not only experience, but also the method itself (as employed by a scientist), are problem-oriented or explanandum-oriented. What I have in mind is this. A hasty reading of I.30 might suggest that Aristotle recommends a scientist first to scrutinize the field looking for all true predications – as though her aim should be to inventory all the true predications in the field –, and only after that to look for what explains what.

However, the method itself Aristotle has recommended in the previous chapters is not inventory-oriented, but problem-oriented: it starts with the assumption that our target is to establish the predicative relation between a given attribute A and a given subject C. The sets of propositions we should consider in the method are determined by this target (cf. 46a4-5). One does not need to look for everything that is true (cf. 46a12) – not even everything that is true in a given domain – but should look for what is true and relevant to establish the predicative relation between the terms of the problem, A and C.

Now, the same holds for experience: as its role is presented here, experience should not aim at making an exhaustive inventory of all the true predications in the domain. There is no quantifier when Aristotle talks about what experience should collect (from 46a17 to 24). There is a quantifier (μηδέν) in 46a24, but I will explain below how its domain of quantification is restricted to a subset of truths within the domain of a science. (There is also the adverb "sufficiently" (ἱκανῶς) associated with phainomena in 46a20, which seems to suggest the exhaustive inventory of facts, but I will explore this difficulty later.)

Of course, experience is important in amassing facts, in establishing that predications are true in the domain of a science etc. But I stress that experience is also important in selecting what is relevant (among the true predications) in the search for explanations. (And I am assuming that Aristotle is talking about experience of the scientist in the sense of long-term and concerned acquaintance with the facts in the domain. Aristotle is not talking about the experience of someone who has only experience and no grip on the explanations (as he does in *Metaphysics* I.1): he is envisaging here the experience as a component of the abilities that make the scientist a scientist. And that makes a difference).

This leads me to my third point, which depends on a closer examination of the text – and depends especially on a different approach to how Aristotle's terminology encodes notions that we nowadays split into different concepts, namely, the truth-value of predications, and the importance (or relevance, or fundamentality) of some predications within an explanatory context.

FRANÇOIS NOLLE / University of Geneva

THE ROLE OF ANALOGY IN ARISTOTLE'S SCIENTIFIC METHODOLOGY

Analogy is originally a mathematical concept that refers to an equality of ratios between four terms (*Nicomachean Ethics* V 6-7, 1131a29-1131b1). By defining analogy in this abstract way, Aristotle can identify and use analogies in all scientific disciplines. More specifically, analogy calls on a certain type of experience, namely the perception of an identity of relations between four terms. However, it is difficult to determine what comes out of sense perception and what comes out of reason, since in analogy one is supposed to perceive relations between things that are generically distant from each other. It seems, therefore, that discerning analogies is first of all an act of intuition, which detects unity beyond the diversity presented by sense perception. And after all, that's why Aristotle states that it needs much philosophy to grasp the similarities between distant things (*Rhetoric* 1394a5, 1412a12).

Analogy seems to play at least four different roles in Aristotle's scientific methodology: First, analogy establishes the existence of something to which we do not have access through direct sense perception; when we know three terms of an analogy, it seems that we can deduce the fourth term in the same way as in Thales' geometrical theorem. This is the case, for example, of the substrate that is deduced in the *Physics* (I 7, 191a8-22) from an analogy with other natural and artificial realities, or of the active intellect in *De anima* (III 5, 430a10-25) from an analogy between material/efficient cause and passive/active intellect.

Second, analogy plays a role in formulating general notions in the following two cases. In the first case, analogy makes it possible for us to designate a fundamental reality, in the absence of being able to define it; for example, in *Metaphysics Theta* 6 (1048a25-b9), when Aristotle explains the notions of potentiality and actuality based on an enumeration of similar cases (analogy plays, here, a role comparable to induction). In the second case, analogy is a tool for determining the properties that will be the objects of science; this is what Aristotle talks about in the *Posterior Analytics* (II 14, 98a1-24), and this is what he often puts into practice in the Parts of animals, by treating together analogical functions and organs of animals.

Third, analogy also seems to be the basis for some types of demonstrations (*Posterior Analytics* II 17, 99a1-17). Once an analogy between set A and set B is recognized in the first place, the reasoning takes the following form: if such a property or causality holds for A, then that property or causality also holds for B, by virtue of the analogy that links them. This type of reasoning can only be demonstrative if the analogy between A and B is established by virtue of an essential rather than an accidental nature.

Finally, analogy is a meta-scientific tool that in a way unifies all phenomena and sciences. From an epistemological point of view, it is a principle that makes it possible for us to move from one science to another (Posterior Analytics I 10, 76a37-76b2). From a metaphysical point of view, it unites all realities because they all share the same principles, causes and elements (*Metaphysics Lambda* 4, 1070a30-1070b35).

What characterizes all these uses, and what makes the analogy so interesting, is that it enables us to know things about which we do not have a direct perceptual experience, and also plays a decisive role in certain types of reasoning. During my talk, I will focus on the second and third role of analogy. In particular, I would like to discuss how Aristotle deals, in his *Meteorology*, with certain phenomena such as earthquakes and rainbows: the question is how the analogies that he uses on this occasion are perceived, and whether they can be the foundation of a reasoning as solid as deduction, or whether it is just a dialectical reasoning by example.

ALEXANDER JONES / Institute for the Study of the Ancient World, NYU

UNOBSERVABLE PHENOMENA AND UNOBSERVED OBSERVATIONS IN PTOLEMY'S ALMAGEST

Ptolemy's *Almagest* (or formally, *Mathematical Composition*) is a highly structured work in which *hypotheseis* (»hypotheses« or »models«) for the motions of the Sun, Moon, planets, and fixed stars grounded in the fundamental principle of uniform circular motion are deduced and quantified from empirical data, and subsequently qualitative and quantitative phenomena are derived from the *hypotheseis*. The empirical data that Ptolemy adduces are of three kinds:

> Assertions of ostensibly observable general behaviour of the heavenly bodies, applied chiefly in deducing structural aspects of the hypotheseis.
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> Measured quantities ostensibly obtained through specific observations that are not themselves individually cited, applied chiefly to quantifying a narrow range of parameters of the hypotheseis such as angles of inclination of planar elements relative to each other.
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> ndividual dated observations, including those attributed to predecessors, ranging in date from the 8th century BCE to the 120s CE, as well as those that Ptolemy claims to have made, ranging in date from the 120s to the 140s, applied to quantifying most parameters of the hypotheseis.

There is wide recognition that many of the reports of dated observations in the *Almagest*, in particular those that Ptolemy says that he carried out, were modified or fabricated with a view to obtaining predetermined results. The historical observations seem to have been less subject to such tampering, but Ptolemy's interpretations of them and his calculations based on them are often skewed. A related but less appreciated aspect of the *Almagest*'s deductive practice is that Ptolemy frequently invokes as empirical givens general statements concerning the apparent movements of the heavenly bodies that could not have been verified from any observations possible in antiquity.

In this talk I will discuss examples of several varieties of Ptolemy's problematic empirical claims, with particular emphasis on his solar theory, the simplest part of the *Almagest* from a techical point of view and the part for which we have the most abundant historical context from other ancient sources. My particular interests are in trying to determine why Ptolemy chooses to base his reasoning on pseudoempirical data, and what the implications of this practice are for his project of demonstrating the detailed structure of the heavens as mathematics of the highest kind.

MATJAŽ VESEL / Institute of Philosophy, Slovenian Academy of Sciences and Arts

COPERNICUS: EXPERIENCE AND REASONING IN THE ARGUMENTATION IN FAVOUR OF THE EARTH'S REST AND IN FAVOUR OF ITS MOTION: HOW REASON CONQUERED COPERNICUS' SENSE AND BECAME "THE MISTRESS OF HIS BELIEF"

My topic is the relative role of experience and reasoning in Copernicus' fundamental thesis that the earth moves whereas the sun is at rest in the centre of the universe. Copernicus was very well aware that the "consensus of many centuries" had spoken determinately against it. This consensus was not confined to any one particular domain or authority but was upheld by the entire spectrum of the existing articulations of knowledge. The earth's motion was refuted for theological, philosophical and experiential reasons, i.e. reasons that were based on experiences. Aristotle, Ptolemy and others (especially Buridan and Oresme in Late Middle Ages), developed a host of very subtle arguments of philosophical nature including some that were based on experiences or appearances (sometimes called also observational tests) against the rotation of the earth (i.e. its first motion) and therefore for its rest. I will focus on Copernicus' presentation and engagement with these "experiences". My thesis is that his responses to the experiential objections to the rotation of the earth and his positive physical doctrine supporting the first movement of the Earth are not very thoroughly elaborated. He is extremely short, vague, and sometimes even contradictory. From this, I will argue, it follows that for Copernicus the decisive argument for the conceptual possibility of the motion of the earth is achieved only within the discussion of the second motion of the Earth, its annual revolution around the Sun, on purely mathematical reasoning (understood in Platonist terms). In both cases, however, Copernicus had – in Galileo's words – "through sheer force of intellect done such violence to [his] own senses", and "[was] able to make reason so conquer sense that, in defiance of the latter, the former became mistress of [his] belief".

JOHN A. SCHUSTER / University of Sydney and Campion College, Sydney

DESCARTES' OPTICAL WORK: THE DISCOVERY OF THE LAW OF REFRACTION; ITS ATTEMPTED EXPLANATIONS 1628-37; AND THE ILLUSORY USE OF METHOD

> "…he [the methodological optician] will investigate the way in which the ray passes through the whole transparent body. Thus he will follow up the remaining points in due order, until he arrives at the anaclastic itself. Even though the anaclastic has been the object of much fruitless research in the past, I can see nothing to prevent anyone who uses our method exactly from gaining a clear knowledge of it."—René Descartes, *Regulae ad directionem ingenii*, Rule VIII, circa 1628

In his *Dioptrique* of 1637 Descartes presented one of the greatest scientific discoveries of the age, the law of refraction of light, applied to the development of a theory of lenses. However, Descartes' publication raised numerous difficulties and puzzles concerning evidence, theory, explanation, discovery and method. For example, Descartes deduced the laws of reflection and refraction from a model: the motion of some very curious tennis balls. Descartes' contemporaries tended not to see any cogency in this model, nor did they grasp Descartes' theory of corpuscular dynamics upon which it is based. Later, questions were raised about how Descartes had obtained the law. Had Descartes plagiarised it from Willebrord Snel? If not, and given that his tennis ball deduction seemed so dubious, where had it come from? Was it perhaps by means of some unstated use of his purported method? [Descartes had discussed this possibility earlier in his *Regulae ad directionem ingenii* which remained unpublished in his lifetime]. To all this there may be added another question: 'What kinds of evidence about refraction, if any, had Descartes employed in the discovery of the law?'

My session aims to cut a path of reconstruction through these controversies. It will involve analysis of several short texts from Descartes, with the help of a 'Guide' that will be available for study by Conference participants, which will synthesize relevant arguments from three of my previous publications about these matters.

I shall attempt to show that the tennis ball model for reflection and refraction links quite coherently to Descartes' instantaneous-impulse theory of light through his dynamics of micro-corpuscles. That dynamics was mooted in his earliest natural philosophical projects, which were forays into what, at the time, he called 'physico-mathematics. The dynamics was first worked out in some detail for *Le Mon*de between 1629 and 1633.

Nevertheless, the tennis ball model of 1637 as presented to readers posed a number of problems. The theoretical and empirical strengths and weaknesses of the model provide us clues about how Descartes first discovered the law of refraction c.1626-27. Descartes did this using tools, techniques and data derived only from the traditional mixed mathematical field of geometrical optics (that is, without any input from his ideas about light as a mechanical impulse). Given that reconstruction of the discovery path, we shall explore [1] how Descartes mobilized his early ideas about 'physico-mathematics' to literally read out of his geometrical diagram embodying the discovery of the law of refraction clues concerning the underlying dynamics of corpuscles that could in turn explain the law; and, [2] the likelihood that Descartes' story of methodological discovery and explanation in *Regulae* VIII does not accurately report either how he discovered the law, or how he constructed physical explanations for it.

DANIEL ŠPELDA / Masaryk University Brno

HYPOTHESES FINGIMUS: CARTESIAN NATURAL PHILOSOPHY BETWEEN RATIONALISM AND EMPIRICISM

In my contribution, I want to deal with the two texts from the context of the Cartesian school: J. Rohault, *Traité de physique* (1671) and P.-S. Régis, *Système de philosophie* (1690). I want to focus on the way the Cartesians evaluated the methodology of ancient natural philosophy (especially Aristotle's) and the problem of using empirical and speculative procedures in Cartesian physics and astronomy. I chose the most important passages from Rohault's "Préface" to Traité de physique (section on four sources of errors) and Chapter III from the first book, which is generally methodological. From Régis' *Système*, I chose a methodological preface to the physical part of the work, which is extremely enlightening for everyone who wants to understand the methodology of Cartesian natural philosophy.

In my analysis, I want to focus on three problem areas:

1. Methodological objections of Cartesians to Aristotelian and Scholastic natural philosophy: too much metaphysics and little experience; too much respect for the authorities; belief in empirically unrecognizable entities (i. e. occult qualities).

2. A combination of empirical and speculative procedures in Cartesian mechanism: Cartesian natural philosophy cherished the idea of deduction of the whole nature (i. e. of all natural phenomena) from several principles or axioms. Descartes himself, however, was aware that it was not possible to infer all the empirical particulars from general principles. He had to admit that we need experience if we want to explain particulars. That is why the Cartesians distinguished the deductive physique spéculative (knowledge of causes) from empirical physique pratique (knowledge of effects). If we want to create a system of natural philosophy (which was a Cartesian dream), we need to combine both types of physique. Deductive speculative physics in itself is unable to grasp particulars and practical physics in itself can accumulate partial factual findings to infinity. The Cartesian system of natural philosophy thus ultimately consists of certain deductive knowledge and incertain factual knowledge.

3. Empirical underdetermination of theories and the hypothetical character of Cartesian natural philosophy: Cartesian physics assumed that all natural processes are caused by insensible particles. Therefore, we can only guess (corpuscular) causes of natural phenomena on the basis of their perceivable effects, i. e. we can see only the clock face; we can only guess whether the clock is driven by a spring or a pendulum. According to the Cartesians, this means that, in fact, we can only invent or imagine possible configurations of corpuscles that can produce an observable phenomenon. Nevertheless, we cannot know with certainty which corpuscular configuration is correct, "because there may be more causes of the same effect," as Rohault says (p. 21). This, in turn, means that Cartesian physics is strictly hypothetical, without being able to decide empirically the correctness of the hypothesis. In Cartesian natural philosophy, the preference of one hypothesis to others was caused rather by its simplicity and consistency with general philosophical and physical principles.

By reading these texts, I would like to show why Cartesian natural philosophy remained scientifically sterile and eventually had to retreat to experimental science of the British type (Hooke, Boyle, Newton, etc.) which, as it is commonly known, did not feign hypotheses.

SOPHIE ROUX / École normale supérieure

MARIOTTE'S RADICAL EXPERIMENTALISM

Mariotte's *Essai de logique*, published anonymously in 1678, is interesting in at least two respects. First, against the historiographic prejudice according to which France was blinded by a dogmatic rationalism under the influence of Descartes, while England benefited from the lights of a full-fledged experimentalism, it testifies that there was a strong tradition of experimental philosophy in seventeenth century France. Thus, d'Alembert (1986, 178) mentions Mariotte alongside Boyle to illustrate the first developments of experimental physics: Mariotte was its French founder, as Boyle was its English founder. Second, Mariotte's *Essai de logique* was written not by a professional methodologist, as philosophers are, but by a real experimenter, who got his hands dirty in experimental practices. Thus, Condorcet (1847, II, 30-32) presents the *Essai de logique* as "a true account of the method he [Mariotte] followed in his research" and therefore opposes him to the "authors of logic" who "all too often resemble mechanics who give descriptions of instruments that they would not be able to use".

To introduce the discussion, I will focus on the three following themes:

1. A criticism of Cartesian physics. Like Rohault in *Traité de physique* (1671), Mariotte wanted to identify the causes of the slow progress that has been made in physics. But, unlike Rohault, he did not seek out these causes only in the methodological and ontological convictions of the Aristotelians, but in the Cartesian practice of retroduction.

2. The role of probable propositions and the notion of principles of experience. Because of the Aristotelian definition of science as demonstrative knowledge, it was difficult to admit that physics is only probable. Mariotte argued however that, unlike mathematics, physics gives a place to probable propositions and to what he calls "principles of experience."

3. Some difficulties of experimental practice. In the *Essai de logique*, but also in his writings on physics, Mariotte highlights how difficult experimental practice is: the result of an experiment may not conform to the theoretical calculations made in advance; it is difficult to transmit experiments to others.

STEFFEN DUCHEYNE / Vrije Universiteit Brussel

ISAAC NEWTON'S METHODOLOGY IN THE PRINCIPIA

In my contribution, I seek to explicate Newton's methodology in the *Principia*, and to shed light on his expression "deductions from phenomena." Newton's expression "deductions from phenomena" has oftentimes been considered as a rhetorical tool by which he sought to distance himself from his opponents. However, close scrutiny shows, I believe, that Newton's "deductions from phenomena" have profound methodological significance. I do not, however, endorse the view that Newton's methodology in the *Principia* was therefore ultimately non-hypothetical. Rather, what makes it methodologically interesting is that it encompassed procedures to minimize speculation and inductive risk in the process of theory formation. What is distinctive of Newton's methodology in the *Principia* is that in Book I Newton established bi-conditional dependencies between causes and their effects from the laws of motion. In other words, the causes which Newton would later infer in Book III were backed-up and constrained by the laws of motion. Given these dependencies, Newton was able to present his derivations of the centripetal forces acting in our solar system as deductions and, hence, as "deductions from phenomena." I want to emphasize, however, that Newton's proceeding from phenomena to theory, i.e. his presenting of certain inferences as deductions from phenomena, taken as such is not what makes his method essentially different from the hypothetical approach. Rather, proceeding from phenomena to theory is the by-product of what genuinely makes Newton's method distinctive from hypothetical approach: the establishment of systematic dependencies backed-up by the laws of motion. These systematic dependencies, in other words, mediate between experimental or astronomical results and the very causes which account for these phenomena. Along the way, I shall show that Newton's methodology in the *Principia* was far from being static, but that instead it changed as Newton was confronted with new challenges.