ARISTOTLE AND THE HYPOTHETICAL-EXPERIMENTAL METHOD IN NATURAL PHILOSOPHY

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Introduction

My purpose in this article is to inquire whether Aristotle had any idea of the hypothetical-experimental method as this is practiced in the modern sciences. It is an initial exploration of the question that would have to be completed by many more examples than I have been able to give here.

It is generally thought that Aristotle uses two procedures in his natural philosophy to establish explanatory principles:[1]

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[1] « Lorsqu'on parcourt les traités dont l'objet direct est une recherche positive (*Ciel*, *Météorologiques*, ouvrages de sciences naturelles), on s'aperçoit assez vite de la place importante occupée par une forme de raisonnement dont l'examen logique n'est institué nulle part et qui consiste en ceci : déterminer la nature d'un fait insaisissable directement à partir d'un autre fait dont la cause est manifeste. » (L. Bourgey, *Observation et expérience chez Aristote*, 1955, p. 113)—"Some have wanted to distinguish those scientific works in which Aristotle's method is dialectical (including typically the scientific works we regard as most philosophical such as the *Physics*, *De Anima* and *Metaphysics*) from others in which he uses, or also uses, non-dialectical empirical methods." (R. Bolton, *The Epistemological Basis of Aristotelian Dialectic*, in Devereux and Pellegrin ed., *Biologie, logique et metaphysique chez Aristote*, 1990, p. 186)—"Reasoning is defined in the *Topics* as dialectical 'which reasons from noted opinions (*endoxa*)', where *endoxa* are understood to be 'things which are accepted by everyone or by most people; or by the wise—either by all of them, or by most, or by the most famous and distinguished'. Clearly, new empirical data uncovered by, say, the working expert biolo-

One is a dialectical procedure, starting with opinions, common beliefs or *endoxa*, as for example in the discussions of *De Anima* I or *Physics* IV, a procedure less often associated by modern commentators with the method described in the *Topics* than one might expect. For example, Terence Irwin claims that the *Topics* is an immature treatise and that the dialectic we find in those mature treatises is another dialectic, a stronger one. Others simply attribute a different purpose to the *Topics* and speak of them as principally describing rules for debating tourneys held at that time in Greece.[2] Although I am in complete disagreement with these views, I will not enter into a consideration of them here. We should note, however, that since in their view it is not a question of any use of the *Topics*, these authors will most often have recourse to those methodological exhortations found scattered through the philosophical treatises themselves in order to build their conception of this non *topical* dialectic.[3]

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gist doing dissections on the members of some heretofore unexamined or improperly examined species could easily fail to fit into any of the subclasses of *endoxa* permitted to figure in dialectical reasoning. Aristotle himself makes clear in various places that such a researcher might well arrive at, and use in his theory construction, results which contradict all standing opinions or, more often perhaps, results which have not occurred to anyone before." **(**R. Bolton, *Definition and scientific method in Aristotle's 'Posterior Analytics' and 'Generation of Animals'*, in Gotthelf and Lennox ed., *Philosophical Issues in Aristotle's Biology*, 1987, p. 122)— "Aristotle's inquiries, described in very general terms, should be ways to reach what is known by nature from what is known to us. He claims, equally generally, that an inquiry should begin from 'appearances', *phainomena*. But his different comments on appearances indicate the differences of method (…) These passages suggest some distinction between empirical inquiry and the relatively non-empirical method of dialectic, two methods of inquiry dealing with two different sorts of appearances." (T. Irwin, *Aristotle's First Principles*, 1988, p. 29)

[2] E.g., Grote, Cherniss and Brunschwig.

[3] E.g., "Let us first go over what others have thought **(***tas tôn allôn hupolèpseis***)** for the demonstrations of their opposites are difficulties for the contrary opinions. Besides, those who have first heard the pleas of our

The other procedure commonly claimed to be found in Aristotle's natural philosophy is described as a more empirical type of inquiry starting with facts, perceptual appearances, data or observations, and is supposed to be what is going on in the more detailed treatises, such as the *Meteorologica* and the biological treatises. This procedure is often associated with a text of the *Prior Analytics*.[4] It is also typically associated with a very large number of places in these treatises where Aristotle explicitly discusses methodological principles, such as that one must seek theories starting from the pertinent objects of sense perception, and take account of those facts whether pleasant or not.[5] We also

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adversaries will be more likely to credit the assertions which we are going to make. We shall be less open to the charge of procuring judgment by default. To give a satisfactory decision as to the truth it is necessary to be rather an arbitrator than a party to the dispute." (*Cael.* I 10.279b5)—"In our inquiry about the soul we shall have to raise problems for which we must find a solution, and in our progress we must take with us for comparison the theories (*doxas*) expounded by our predecessors, in order that we may adopt those which are well stated, and be on our guard against any which are unsatisfactory.' (*De an.* I 2.403b20) —See also *Eth. Eud.* I 3.1215a5; *Metaph.* III 1.995a27; *Eth. Nic.* VII 11.1145b2.

[4] "It falls to experience [*empeiria*] to provide the principles of any subject. In astronomy, for instance, it was astronomical experience that provided the principles of the science, for it was only when the phenomena [*phainomena*] were adequately grasped that the proofs in astronomy were discovered. And the same is true of any art or science whatever." (*An. pr.* I 30.46a17)

[5] "We must make up our minds about the method (…) should the student of nature [*phusikos*] follow the same sort or procedure as the mathematician follows in his astronomical expositions—that is to say, should he consider first of all the phenomena [*phainomena*] which occur in animals, and the parts of each of them, and having done that go on to state the reasons and the causes; or should he follow some other procedure? (…) I said earlier that we ought first to take the phenomena that are observed in each group, and then go on to state their causes." (*Part. an.* I 1.639b5; 640a13)—"We must now deal with haloes, rainbows, mock suns and rods, explaining what they are and what are their causes; for the same causes account for all of them. First, we must describe what the actual characteristics of each of these phenomena [*phainomena*] are." (*Mete.* III 2.371b18)—"A few

find advice to whomever would study natural things to have close and intimate contact with these things. If the scientist is unable to establish broad principles, says Aristotle, it is due to the insufficiency of his experience.[6] Particularly significant are those other texts which suggest that Aristotle is aware that not all perception of phenomena can count as worthwhile opinions or *endoxa*—even if the inverse is true (when it is a question of an opinion about something natural, obviously)—and that it is the perceptual phenomena by which we must ultimately test the merits of our principles in physical science, and not by means of accredited beliefs or opinions.[7]

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natural philosophers have dealt with the question of breathing; some of them have offered no explanation why this phenomenon occurs in living creatures; others have discussed it without much insight, and with insufficient experiment on the facts [*apeiroterôs tôn sumbainontôn*]." (*Resp.* 1.470b5)—"What has just been said has been stated thus by way of outline, so as to give a foretaste of the matters and subjects which we have to examine; detailed statements will follow later; our object being to determine first of all the differences that exist and the actual facts in the case of all of them. Having done this, we must attempt to discover the causes. And, after all, this is the natural method of procedure—to do this only after we have before us the ascertained facts about each item, for this will give us a clear indication of the subjects with which our exposition is to be concerned and the principles upon which it must be based." (*Hist. an.* I 6.491a7)

6 "The reason why we have not the power to comprehend the admitted facts is our lack of experience. Hence those who have lived in a more intimate communion with the phenomena of nature are better able to lay down such principles as can be connected together and cover a wide field." (*Gen. corr.* I 2.316a5)

7 "By positing indivisible bodies they cannot help coming into conflict with mathematics, and undermining many accepted beliefs and facts of observation (ἀνάγκη μάχεσθαι ταῖς μαθηματικαῖς ἐπιστήμαις ἄτομα σώματα λέγοντας, καὶ πολλὰ τῶν ἐνδόξων καὶ τῶν φαινομένων κατὰ τὴν αἴσθησιν ἀναιρεῖν)." (*Cael.* III 4.303a20)—"Such appears to be the truth about the generation of bees, judging from theory and from what are believed to be the facts about them; the facts, however, have not yet been sufficiently grasped; if ever they are, then credit must be given rather to observation than to theories, and to theories only if what they affirm agrees with the observed facts (οὐ μὲν εἴληπταί γε τὰ συμβαίνοντα

I was greatly surprised when I learned more about the history of what seems to be one of the favorite questions of the contemporary interpreters of Aristotle, namely that of the account of the discrepancies between Aristotle's theory of the *Analytics* and its practice in his philosophy. The biological treatises have been assigned a special role in this history. I seemed to find myself unable to really quite agree with anyone.

When discrepancies between the *Posterior Analytics* and the biological treatises are evoked by contemporary exegetes, they are not only thinking about the thoroughly empirical character of the latter, which manifestly contrasts sharply with the rigor of the *Posterior Analytics* and its numerous mathematical examples,[8] but also the admitted absence of syllogisms in these trea-

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ἱκανῶς, ἀλλ᾽ ἐάν ποτε ληφθῇ, τότε τῷ αἰσθήσει μᾶλλον τῶν λόγων πιστευτέον καὶ τοῖς λόγοις, ἐὰν ὁμολογούμενα δεικνύουσι τοῖς φαινόμενοις)." (*Gen. an.* III 10.760b27)—"It is absurd, because it is unreasonable that one element alone should have no part in the transformations, and also contrary to the observed data of sense, according to which all alike change into one another. In fact their explanation of the observations [*phainomena*] is not consistent with the observations. And the reason is that their ultimate principles are wrongly assumed: they had certain predetermined views, and were resolved to bring everything into line with them. It seems that perceptible things require perceptible principles, eternal things eternal principles, corruptible things corruptible principles; and, in general, every subject matter principles homogeneous with itself. But they, owing to their love for their principles, fall into the attitude of men who undertake the defense of a position in argument. In the confidence that the principles are true they are ready to accept any consequence of their application. As though some principles did not require to be judged from their results, and particularly from their final issue! And that issue, which in the case of productive knowledge is the product, in the knowledge of nature is the unimpeachable evidence of the senses as to each fact." (*Cael.* III 7.306a3)

[8] "The relation between Aristotle's official account in the *Posterior Analytics* of the nature of scientific knowledge and of the means by which it is reached and his actual practice in arriving at the results presented in his special scientific writings has long been a topic of considerable study. In the recent history of attempts to account for the discrepancies between Aristotle's theory and his practice or to explain away the apparent discre-

tises.[9] Now, we know that demonstration is a kind of syllogism. With this I am not in agreement, but I agreed at least with the claim that there were some differences, so I started to look at the two principal explanations of these in contemporary commentators.

Some, putting aside the question of the syllogism, do not deny that there are differences, but they have devoted their efforts to illuminating the likeness to be found between the procedures in the biological treatises and at least certain of those

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pancies, the biological works have been assigned a special role. (…) it is still widely supposed that (…) there are empirical elements in the method practiced in the biological writings to which no role is given in the *Analytics*." (R. Bolton in GL, p. 120)

9 "The method which Aristotle follows in his scientific and philosophical treatises and the method which he prescribes for scientific and philosophical activity in the *Posterior Analytics* seem not to coincide. The task of explaining this apparent inconsistency is recognized as a classical problem of Aristotelian exegesis. (…) In the *Posterior Analytics* (…) Aristotle expounds his theory of 'apodeictic' or demonstrative science. The theory concerns the logical form which the sciences do or should exhibit (…) The details of Aristotle's theory are obscure, but its outline is clear; a demonstrative science is an axiomatised deductive system comprising a finite set of connected *apodeixeis* or demonstrations. A demonstration is a sort of syllogism; that is, it has the form of one of the fourteen/syllogistic moods which Aristotle acknowledged as valid. (…) This points to the problem; for in the whole of the Aristotelian corpus there is not, as far as I am aware, a single perfect example of a demonstration. The Posterior *Analytics* quotes arguments which come close to demonstrative form (…) but there is no perfect example. In the other treatises there is scarcely a syllogism. There are arguments which might be said to show a degenerate syllogistic form; and there are arguments which can be brought into perfect syllogistic form without much violence to the text; but even these cases are rare, as will be clear to anyone who tries to formalize any of Aristotle's arguments. If the *Organon* were lost we should have no reason to suppose that Aristotle had discovered and was mightily proud of the syllogism. This, then, is the problem: on the one hand a highly formalised theory of scientific methodology; on the other, a practice quite innocent of formalisation and exhibiting rich and variegated methodological pretensions of its own: how are the two to be reconciled?" (J. Barnes, *Aristotle's Theory of Demonstration*, *Phronesis*, 1969, p. 65)

described in Book II of the *Posterior Analytics*. For instance, one part of the influential collection of essays edited by Gotthelf and Lennox in 1987, *Philosophical Issues in Aristotle's Biology*, is devoted to that. Those efforts have been followed by many others, among whom Devereux and Pellegrin in 1991. Not having studied all of the articles in detail, I reserve judgment on the totality of their arguments, but I have a great deal of spontaneous reticence in the face of attempts to tie the *Posterior Analytics* too closely to the methodological practices to be found in the biological treatises, although the search for definitions by the use of differences and division, likeness and induction to be found, for example, in the *History of Animals*, does seem to be in conformity with many of the general principles outlined in Book II of the *Posterior Analytics*. Yet some of those favorable to this reconciliatory approach go so far as to claim that the whole theory of science in the *Posterior Analytics* was specifically designed by Aristotle with a view to its application in biology.[10]

This seems to me to be clearly false. But my hopes that this might lead me to an agreement with those who held the opposite view were soon crushed as well, for some of them seemed to be

[10] See Wians in *Aristotle's Philosophical Development*, 1996, p. 132: "More recently, this explanation [Aristotle was under Plato's influence] has come under attack. Many commentators have drawn attention to the presence of examples from the natural sciences, especially in the second of the *Apo's* two books where nonmathematical examples predominate. Ferejohn, for instance, takes such examples as evidence that Aristotle intended the theory to cover his full scientific interests. (…) Other critics cite the nonmathematical examples to bolster their rejection of genetic hypotheses altogether. This is the case especially with those seeking to lessen the tension between the theory and practice of Aristotelian science, the notorious gap between what Aristotle outlines in the *Apo* and the methods he actually employs in his philosophical and scientific treatises. (…) Kullmann has perhaps gone the furthest in this direction. Based on his study of the *Apo's* examples, he concludes that the theory of science was designed with its application to the natural sciences, and particularly to biology, in mind. The mathematical illustrations, he claims, function as a disguised placeholder for the natural sciences."

going as far as to deny any likeness—something I could hardly admit—and further, I am in disagreement with the accounts they gave of the differences. One main thesis is Jaeger's developmental hypothesis.[11] According to him, the empiricism of the biological works marks the final step in Aristotle's emancipation from the Platonic mathematical and idealist view of scientific knowledge.[12] The other main thesis is that of Barnes,[13] according to which the theory of demonstration and the whole of the *Posterior Analytics* is only an account of how we might systematically present scientific knowledge, once it has been discovered. It thus contains no recommendations as to how it is to be discovered.[14] Now, I see no serious reason to adhere to developmental hypotheses, and Barnes' thesis on the goal of the *Posterior Analytics* seems extremely problematic and in need of clarification at best, given Aristotle's description of it and the testimony of all the ancient commentators, for whom demonstration does imply discovery of new knowledge, because there is a dis-

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[11] On the identification of Jaeger's thesis as a solution to the problem: see Bolton in GL, p. 120 and Wians 1996, p. 133: "Jaeger assigned Aristotle's researches in biology to the final, second Athenian phase of the philosopher's career, regarding them as the culmination of his movement away from Platonism."

[12] See Jaeger, *Aristotle: fundamentals of the History of His Development*, 1948, p. 337-341.

[13] On the identification of Barnes as the solution to the problem: see Bolton in GL, p. 121.

[14] "The problem only arises if it is assumed that the theory presented in the *Posterior Analytics* was intended by Aristotle to give an account of the sort of activities which his treatises report. Although this assumption has not often been expressed, it is clear that without it no problem arises; for if the *Posterior Analytics* was never intended to provide the theoretical substructure for Aristotle's scientific research, then there can be no question of inconsistency between the research and the theory. But the assumption is false: the theory of demonstrative science was never meant to guide or formalise scientific research: it is concerned exclusively with the teaching of facts already won; it does not describe how scientists do, or ought to acquire knowledge: it offers a formal model of how teachers should present and impart knowledge." (Barnes 1969, p. 77)

covery of properties. Which means that properties can now be affirmed of a subject in the conclusion of a demonstrative syllogism, thanks to the definition as middle term. Such conclusions were not *thus* previously known, that is with certitude, and so they constitute new knowledge. Otherwise we would have to believe that the geometer knows all the properties of the different figures before demonstrating them starting from a definition of a figure. Or that whoever affirms a property of a figure when he does not yet know the definition already knows it scientifically. If one only knows scientifically once the demonstration has been made, there is a discovery of the conclusion.

I have quite another reason to think that associating Aristotle's more concrete and detailed treatises with the demonstration described in the *Posterior Analytics* is wrong. These treatises are among the ones whose subjects imply the most matter, as Aquinas remarks each time he divides the natural treatises.[15] And since matter is the source of the corruptibility and contingency of things, these treatises are also those whose subjects involve the least necessity. They cannot, therefore, lend themselves to the kind of certain knowledge aimed at by Aristotle in the *Post-*

[15] E.g. *In Arist. Libros De Sensu et Sensato Comm.* I 1, 2: "Unde et scientiam naturalem incipit tradere ab his quae communissima omnibus naturalibus, quae sunt motus et principium motus, et demum processit per modum concretionis, sive applicationis principiorum communium, ad quaedam determinata mobilia, quorum quaedam sunt corpora viventia: circa quae etiam simili modo processit distinguens hanc considerationem in tres partes. Nam primo quidem consideravit de anima secundum se, quasi in quadam abstractione. Secundo considerationem facit de his, quae sunt animae secundum quamdam concretionem, sive applicationem ad corpus, sed in generali. Tertio considerationem facit applicando omnia haec ad singulas species animalium et plantarum, determinando quid sit proprium unicuique speciei. Prima igitur consideratio continetur in libro *de Anima*. Tertia vero consideratio continetur in libris quos scribit de animalibus et plantis. Media vero consideratio continetur in libris, quos scribit de quibusdam, quae pertinent communiter, vel ad omnia animalia, vel ad plura genera eorum, vel etiam ad omnia viventia, circa quae huius libri est praesens intentio."

erior Analytics.[16] The idea that the *Posterior Analytics* are concerned with a necessary object is not one limited to the older commentators, but is also to be found in Grote.[17] Thus, well into the nineteenth century we can find the claim that Aristotle had mainly in view in the *Posterior Analytics* the more exact sciences like arithmetic, geometry and astronomy.[18]

Yet an objection rose rapidly to mind: in *Posterior Analytics* Book I, is not Aristotle maintaining the possibility of knowledge that is certain about corruptible things? For although each of these material things taken individually is corruptible, he explains, insofar as they are considered as members of a species, as is the case when we demonstrate something about them, they have a certain permanence. But precisely, we must be able to apprehend what it is in these things that belongs to their essence and is permanent. This is what it seems to me must be said, namely, that Aristotle was aware that it is not possible for us to seize totally and perfectly the definitions of very particular things, the knowledge of which requires much detail, and that thus we cannot furnish the kind of propositions that answer to the criteria necessary for demonstration.

And thus I maintain my position that Aristotle did not think that demonstration as described in *Posterior Analytics* Book I was possible in biology, and that therefore he did not even attempt to carry them out there. At most one might see some relation with what is discussed in Book II in respect to procuring definitions. What I am going to propose, however, is to link some of these well-known empirical passages of the biological treatises to another method used by Aristotle, which is neither that of the *Analytics* nor that of the *Topics* (nor, for that matter, a dialectical method in any strict sense of the expression).

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16 Aristotle affirms that for certitude in knowledge, the object must be necessary.

17 If I am correct in thinking that Grote takes ‘exact’ more or less in the sense of ‘certain’.

18 See Grote, *Aristotle*, 1880, p. 578.

It seems to me that a lot of these empirical passages follow a method which is similar to that of the modern experimental sciences: we observe facts (*phainomena*), and try to furnish an explanation by positing a hypothesis from which we can deduce the observed facts, as well as other facts to be verified by further observations in order to confirm or infirm the hypothesis. (From now on, I will call this third method the 'hypothetical-experimental method' to avoid confusing it with the methods of *Posterior Analytics* Book I which I will call 'demonstrative method', as well as with the methods of *Posterior Analytics* Book II, which I will refer to as 'division' and 'induction'.) I think that Aristotle, even if he made no systematic study on it, did have a certain conception of this hypothetical-experimental method. There is, I will hold, a sufficiently elaborate conception of such a method, although it is neither a complete nor a clear one, and that is one reason why he did not always apply it rigorously.

Let us start by examining in a very general way the general picture of Aristotelian natural philosophy.

Certain objects of our sensible experience are more or less common ones, for example, change, nature, causality, motion, place, time, and in a more restricted area, life, the soul and its powers. These objects are fundamental and to be found everywhere in their particular domains—time, place and motion, for example, in all parts of nature; the soul and other aspects of living things amongst these kinds of natural beings. What also characterizes these things is that we all must necessarily have an experience of them; we cannot avoid it. Our first knowledge of them, which comes through our immediate sense contact with them, is a very certain knowledge, although a very vague one also. Thus, an intellectual analysis of this experience can eventually allow us to come to know the natures of these things with certitude, since it will be resolved back to this first certain knowledge of them. Then, when we have arrived at the definition of such things, we can proceed to demonstrate their properties. And this is what Aristotle does in those treatises of natural philoso-

phy where such objects of common experience are considered, such as in the *Physics* and the *De Anima*, and to a more limited extent in the ones which immediately follow these.

Now, the less the objects studied are common, and the more they are particular, the less our experience of them is certain. A sign of this is that we can be mistaken about particular objects, even when they are all around us, and commonly brought to our attention. Thus, although we cannot be mistaken about an experience of motion, we most certainly can be about the particular motion of a particular thing. Thus, although all astronomers, and all people for that matter, agree that something is moving when we note the passage of the sun in the sky each day, the problem is the more particular one of what it is that is moving. About the first fact of motion itself there is no doubt, but about the second it is easy to make a mistake. This kind of more particular sensible experience, since it itself lacks certitude, cannot lead us by an immediate process of analysis to certain knowledge about the nature of the object under consideration, nor, consequently, to a rigorous demonstration of its properties. And these are the kind of things that Aristotle takes up in the more particular treatises such as the *De Caelo*, or the *Meteorologica*, and all the more detailed biological works.

Now, how does one get to this more or less certain knowledge of the definitions of this diversity of natural objects, according to Aristotle? We always start with sense experience, that is with facts. When it is a question of very common experience, it will sometimes suffice to start with this personal experience, and using divisions and inductions of the kind discussed in *Posterior Analytics* Book II, to thus come to a knowledge of their definitions that is certain. More often, however, even with very common things, we will have to proceed to such divisions and inductions not only starting with our own personal experience of them, but also with the opinions of others, namely what

seems true to others, as Aristotle explains in the *Topics*.[19] For as soon as one starts to interpret his personal experience and to describe it in words and opinions, there is the risk of making a mistake or of neglecting some aspects of the experience. Whence the need for help. Such a dialectical procedure can also eventually lead to seizing the definition in a necessary way, but this will still be due to the initial experience as it is reflected in the opinions. It is no doubt no easy task to draw a clear line between those cases where we can readily arrive at a certain and correct definition without dialectic, and those where at least some dialectic will be necessary. (Examples of the former might be that motion is an act, or that nature is an intrinsic principle of motion, or that the soul is a substance.) In most cases, Aristotle will use dialectic to get to the definition. So much, then, for the problem of defining very common things.

Now, the more one is concerned with particular objects, the less our initial sense experience of them will be certain and sufficient. And thus the more one will be required to use a diversity of opinions to examine the question, and the more one will have to be content with only a probable knowledge of their natures. Such is the case, for example, of our knowledge of the natures of the functions of animals and plants, and of more particular natural phenomena. But even here we soon go beyond what can even be a reasonable object of opinion; nothing seems true anymore. What is one to do, then, when faced with such particular objects that our initial sense experience of them can really lead to no valid opinions, that is, can suggest nothing that seems true; things, in fact, which may not even fall within the range of the experience of most people? Well, Aristotle seems to

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[19] To mark the difference between the search for a definition by the *An. post.* only and also by the *Top.*, I always speak of the recourse to the opinions of others and to *endoxa*; but it is also true that in the *An. post.*, there is no syllogism discussed (as is the dialectical syllogism in the *Top.*), but only division, induction, likeness and differences. Further, there does not seem to be any putting into question of the elements of the definition to be established in the *An. post.*

have foreseen that the situation, although a difficult one, is not without hope. After all, one might just make a guess about the things, that is, suggest something which, although at first sight not particularly likely, does not seem to be manifestly false, and might just explain the object under consideration. And then one could confer some legitimacy on such a hypothesis by checking to see if the consequences logically derived from it are confirmed or not in experience. Given the logical form of such arguments however, one could never end up with anything but provisional and uncertain knowledge of the causes involved, and such a knowledge could in no way furnish a principle for demonstration. Such knowledge, however, is certainly preferable to complete ignorance of the thing.

This gives an overview of my general position on Aristotle's conception of natural philosophy: it is a vast discipline involving many different kinds of questions and problems which cannot be dealt with in the same way, and thus it requires diverse methods. But still all of these are parts of one discipline, having one general purpose, which is to get to the knowledge of natural things (specifically, of their first principles and causes, and then of their elements, as it is said at the beginning of the *Physics*).

It is not possible to establish all the aspects of this view here, nor that it also corresponds to that of Aquinas. Let us limit ourselves to discussing the part concerned with the occasional use by Aristotle of elements of the hypothetical-experimental method. Since there is much likeness between what happens in dialectic and in this hypothetical-experimental method,[20] and as it often seems that Aristotle is using dialectic where this other method would in fact be called for, a brief review of what dialectic is and how it proceeds will be useful. After this we can examine in more details than before the hypothetical-experi-

[20] More in fact than between the method of the *An. post.* and that of experimental science.

mental method in its general lines before proceeding to an examination of some examples of its use in Aristotle.

Development

Dialectical discussion

Dialectic, according to Aristotle, starts with a problem presented by way of opposition: 'Can virtue be taught or not?' To this question an interlocutor ventures an answer: 'Virtue cannot be taught.' This is his opinion on the question, what seems true to him. He may support it by other opinions: 'Virtue cannot be taught, because it is a habit, not a science.' His interlocutor is then going to try to test this position against other opinions that are in some way considered more established. For every opinion advanced by him, his interlocutor will be required to accept or refuse.

Digression: dialectic also goes towards conclusions

But, one could say: "This particular example of a probable argument seems to be about a conclusion, whereas until now you have been speaking of dialectic only as a way of reaching principles, definitions."

Now is time for some clarification. This way of speaking was adopted because the present predominant *Barnesian* view of demonstration is that it is only a way of presenting knowledge, as previously stated, and this goes together well with the idea that dialectic is only concerned with principles. Indeed, given the Platonic distinction between going to the principles and coming from them, it seems more natural to associate the way from the principles with the method which explains, and thus the way towards the principles with the method which discovers. Now, that dialectic is concerned with discovery is more or less how the *Barnesian* view is interpreted; it must be the method for finding those explanations which demonstration will use in its presentation of knowledge.

But, as demonstration also implies discovery, as already briefly explained, so too it would seem that the dialectician, once

he has discovered definitions in a probable way, can use these definitions to explain and present properties, even if this will not be with the certitude with which the demonstrator justifies properties. In fact, it is rather evident that the dialectician can reason from probable premises bearing or not on the definitions of a subject to establish in a probable way that some property pertains to this subject: *Topics* I 2, for example, does not indicate that Aristotle thinks dialectic is useful only for the establishment of definitions or principles. But dialectic seems to be appropriate as a way to reach conclusions either when one does not have the certain knowledge of the definition from which he would be able to demonstrate, or when he does not even have a probable knowledge of that definition.

Now the same will be true of the hypothetical-experimental method, namely that it is not only concerned with principles. That would even seem to be essential to it: once the scientist has been able to confirm a hypothesis, he is going to use it as an established theory to explain phenomena. That is why he formulates hypotheses in the first place, so that proceeding from them he might be able to explain the already-observed facts and come to discover others he was not yet aware of.

Return to the dialectical discussion

Let us examine an example of dialectical argument concerned with a definition. And let us choose it from the area of natural philosophy: 'Is the place of a body its shape and volume, or not'? A probable support for the position that place is the form might run like this: "A body is always in a place, and the place is not any smaller or larger in any direction than the body; now, a body is always in its shape and volume, and these are neither bigger than, nor smaller than the body". And a probable argument against this position could go like this: "A body can change places by moving out of its first place and entering into another; now, no body can leave its own volume and shape. Therefore, the volume and shape are not the place of the body".

Dialectic, thus, turns around opinions, that is, propositions that the intellect accepts without certitude, and this happens either when it does not seize the necessity of a necessary proposition, or when a proposition is contingent.[21] But more precisely, dialectic turns around reputable opinions as in the *Topics* where Aristotle talks about *endoxa*, i.e., what seems so to all, to most, or to all or most of the wise, or to the most famous of them. For the opinions about which it is a question there are the principles from which one is to proceed in order to argue about every problem, which is the purpose of the topical method. Now, it is not just any opinion that Aristotle will admit as a principle of such research. In fact, it will be necessary that the fact of being an opinion constitute a sign of the possible truth of what is opined. And for this to be so, it will have to be, according to Aristotle, an opinion shared by many people—and the middle term of this argument is reason's natural capacity to know the truth. It must be held by everyone, therefore, or the majority, or lacking a consensus among the many, it will have to be the opinion of those who are most competent.

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[21] This is how *doxa* is defined in the *Posterior Analytics*: "Ὥστε λείπεται δόξαν εἶναι περὶ τὸ ἀληθὲς μὲν ἢ τὸ ψεῦδος, ἐνδεχόμενον δὲ ἄλλως ἔχειν." (I 33.89a2)—"Et ad exponendum quid sit opinio, subiungit quod opinio est acceptio, idest existimatio quaedam, immediatae propositionis, et non necessariae. Quod potest duobus modis intelligi; uno modo sic quod propositio immediata in se quidem sit necessaria, sed ab opinante accipiatur ut non necessaria, alio modo, ut in se sit contingens." (Aquinas, *In Lib. Post. Anal. Expositio*, I 44, 399)—Back to the example: the form following the body is not a certitude for me if I cannot demonstrate it starting from the definition of a form or a body. It is an opinion I have drawn from experience. That a place does not follow the body is not a certitude either, since at this point I do not even have the definition of place, nor do I know that of body. It is also an interpretation of my sense experience. If these opinions are also those of everyone, or most people, or the wise, and so on, then we may speak of opinions in the sense of the *Topics.*

Scientific problems

Therefore, the kinds of things that dialectic lends itself to are rooted in a fairly widespread experience, so that ordinary people may have experience of them and form opinions about them. As Aristotle explains, the dialectical problem arises when the common people have one opinion which contradicts the opinion of the experts, or when there is such a contradiction within one or the other group. We can see thus that even if both the dialectical and the scientific methods attempt to explain things and to solve problems, a dialectical problem is still not a scientific one. For the latter bears on very particular, precise, complicated and detailed kinds of things about which most often only the experts have any experience.

Also, about those kinds of things, it is most often the case that we cannot go directly to something that seems true as an explanation of them, and are obliged to have recourse to conjectures not initially likely. For example, how do certain species of pigeon navigate in any weather? It seems impossible to formulate any probable opinion about such a matter. If we tried, they would be too general and remote to be taken as a start from which to discuss fruitfully and we would be discussing '*logikos kai kenos*', using an empty dialectic, to speak as Aristotle does (*De Anima*, 403a2; *Eudemian Ethics*, 1217b13). At most, we can formulate a guess—e.g., these pigeons have a magnet somewhere, and a capacity for detecting the influence of the earth's magnetic field on this magnet. And again, we could do this only if we had sufficiently observed the very particular species of pigeons in question.

Here is what I take the principal steps of the scientific method to be. I do not deny however that there are and have been a large number of diverse conceptions of how modern science proceeds and should proceed, but we can still speak of a method, the hypothetical-experimental one, to the degree that there exists a certain consensus about certain of its most fundamental traits, if not among philosophers—after all, philosophers never agree on anything—at least among the scientists them-

selves. This can be seen more in their practice perhaps than in their writings, but in the latter as well:

1) The scientist starts with a fact of nature that he is unable to explain in terms of what he already knows with any degree of confidence, and about which he often cannot even formulate an explanatory opinion that seems probable to him.

2) When the investigator cannot propose some explanation that seems probable to him, he must imagine an explanation whose only initial virtue is to offer what seems to him to be a possible explanation for the fact. In such cases, he can confer some likelihood on his hypothesis, for example, by bringing up what is admitted to be true about things that are like those he is trying to explain. The important point here is that in such cases experience alone about the things to be explained cannot of itself render his hypothesis likely.

Such hypotheses truly differ from the opinions the dialectician will use as premises. It is important to see that they are really excluded from the definition of the latter. Indeed, the first reaction of people is often: "Aren't such hypotheses simply opinions of the wise and of the experts, such as we have in dialectic?"

We have just looked at an essential difference: dialectic proceeds from opinions, that is, from propositions which, when compared to experience, seem to make sense, and from reputable opinions, that is opinions that seem to do so for all, or most people, or the wise, and sometimes in fact to certain of them. Now, often (we could say even most of the time), the scientific hypothesis that the scientist proposes is not an opinion of this scientist. Pierre Duhem insists much on this constraint under which the natural philosopher works, but from which the scientist is free: "The geometrical contrivances we use to save the phenomena are neither true nor likely."[22]

[22] Duhem, *To Save the Phenomena*, 1969, p. 21.

There is also the fact that hypotheses often bear upon measurable aspects of things (and thus imply calculations), and that they are often squarely at odds with common opinion. For example, that the earth is round, or that it revolves around the sun. Now, Aristotle places as a condition for the dialectical probability of the views of the experts that they not contradict common opinion.[23]

Moreover, the explanations used by the scientist are less well known than the facts they are supposed to explain. Indeed, these are things about which, at least at that point, he is certain and has no doubts whatsoever. Thus if the scientist sometimes explains facts by what seems to him likely, and even when this is not contrary to common opinion it is still the case that he will be more certain of the facts he is attempting to explain than of the explanation he is offering for them, so that such an explanation can never qualify as dialectical. Indeed, Aristotle insists that dialectical premises must always be more probable than the position they are establishing.[24]

3) Since these conjectures are less well known than the facts to be explained, the scientist must somehow put them to the test. He first tries to see what consequences follow from them. These consequences are ultimately statements about what is to be found in experience, and these empirical facts are not just the

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[23] "A dialectical premise is the asking of something acceptable to everyone, most people, or the wise (that is, either all of them, most of them, or the most famous), provided it is not contrary to opinion (for anyone would concede what the wise think, so long as it is not contrary to the opinions of the many.—Ἔστι δὲ πρότασις διαλεκτικὴ ἐρώτησις ἔνδοξον ἢ πᾶσιν ἢ τοῖς πλείστοις ἢ τοῖς σοφοῖς, καὶ τούτοις ἢ πᾶσιν ἢ τοῖς πλείστοις ἢ τοῖς μάλιστα γνωρίμοις, μὴ παράδοξος· θείη γὰρ ἄν τις το δοκοῦν τοῖς σοφοῖς, ἐὰν μὴ ἐναντίον ταῖς τῶν πολλῶν δόξαις ᾖ." (*Top.* I 10.104a8)—"Τὰς μὲν οὖν προτάσεις ἐκλεκτέον ὁσαχῶς διωρίσθη περὶ προτάσεων, ἢ τὰς πάντων δόξας προχειριζόμενον ἢ τὰς τῶν πλείστων ἢ τὰς τῶν σοφῶν, καὶ τούτων ἢ πάντων ἢ τῶν πλείστων ἢ τῶν γνωριμωτάτων, μὴ τὰς ἐναντίας ταῖς φαινομέναις, καὶ ὅσαι δόξαι κατὰ τέχνας εἰσίν." (*Top.* I 14.105a34)

[24] See *Top.* VIII 5.159b7 and 11.161b27.

initial ones. Of course, the hypothesis must explain the initial facts. It must allow the deduction of consequences which are in conformity with the already observed and questioned phenomena, but it is more importantly new facts that it should allow us to predict: e.g., if some pigeons have a magnet and are capable of detecting the earth's influence on it, then they should be able to navigate without using sight. Thus, if they happened to be blinded, they should still be able to do so. And as a counter proof, if a magnetic field was placed around them, they should no longer be able to navigate.

4) These consequences are then verified by a comparison with experience. When they are in conformity with experience, as we are able to obtain it, and to the degree of precision that we have about it, then we may say that they have passed the test, at least for the moment**.** When, however, the facts of experience do not confirm all the deductions, we can be sure that this hypothesis, at least in principle, needs to be changed, or even totally abandoned.[25] This follows from the nature of the hypothetical syllogisms used in science, which have either the form: If A, then B; not B; therefore not A. Or, if A, then B; B, therefore, perhaps A. It is also important to realize that this experimental verification of the consequences of truly scientific hypotheses requires the kind of detailed and controlled experiments (a kind of experience) that only a scientist can make. Everyone is able to see that the place of a body does not follow the body that moves, but it is only a scientist that is likely to appreciate how an ornithologist (say) verifies the consequences of a hypothesis about magnetic orientation in pigeons.

Thus, the question about Aristotle's use of the hypothetical-experimental method must involve determining not only whether he suggested hypotheses, but what role experimentation played

[25] Even given the Duhem-Quine principle that when experimental results do not agree with the predictions of a hypothesis, the fault may be with prior hypotheses, it is still some or all of the hypotheses in the total explanation that would have to be put into question.

for him in determining the value of such hypotheses. In other words, to judge of how Aristotle was proceeding, we must not only ask if he made observations and proposed scientific and non dialectical explanations for them, but whether he put these two steps in sufficiently close relationship to one another so as to use the facts as controls for his a priori ideas.

My position on this is that, even if Aristotle was too often content with dialectical explanations, he did propose hypothetical explanations for certain particular natural phenomena he observed. On the other hand, he rarely if ever made predictions or carried out experiments to test these hypotheses. That is, he took what he considered to be the initial facts, often gleaned from accounts by others,[26] and he was content to judge his hypotheses by these. In this respect, he would not have used the scientific method fully and properly.

Now, among the possible predictions, and even among the initial facts taken by Aristotle on authority from others, or not considered at all, some could have been rather easily verified by him, and others not. To illustrate Aristotle's use of the method as I described it, I will use two kinds of examples drawn from his efforts to explain rather minor scientific problems. In some cases (1 and 2), he could have run controlled experiments, whereas in others (3), he could not have done so.

Examples

Case 1-The formation of hail: *Mete.* I 12

The first case concerns his explanation of the formation of hail, itself part of his more general explanation of the formation

[26] Aristotle is often rightly criticized for having taken the observations of others on faith when he should not have had confidence in them. While it is quite in keeping with the scientific method to divide the work, and thus for the theoreticians to take the results of the technicians, this is possible because the latter have an adequate formation in experimentation and observation and are subject to a strict protocol—something that cannot be said of many of Aristotle's sources.

of bodies by refrigeration in the clouds (the others being water and snow).

This case is interesting for us in that although Aristotle would have had no way of observing what was going on in the heavens, or of carrying out experiments there, he still might have performed one crucial experiment [to get one more initial fact] that he did not. Moreover, he does not proceed by induction or division, nor does he proceed dialectically. First, his explanation is not an opinion; it cannot appear true by direct comparison with sensible experience. It's more like a guess Aristotle made and on which he conferred some verisimilitude through an analogy with what he took to be similar but better known cases.[27] One can also see that he formulated his guess using some of his previous theories as a guide and framework. His hypothesis supposes his conception of humid exhalations and of the formation of rain and clouds. It indeed assumes that the vapor caused by the heat due to the reflection of the sun's rays from the earth is cooled and condensed as it rises and gets further away from the source of heat, so that clouds result from a production of watery vapor from the air. Secondly, his hypothesis is certainly much less known than the fact that hail exists, as we will see.

Hail is ice, he first says. That is one of the unproblematic facts, in contrast with the more puzzling facts which, as we shall see, are hard to reconcile with the most evident and undeniable facts of the first kind. Hail is ice, yet, (paradox 1) he first notices, it forms especially in the spring and autumn, not in winter, and in milder regions. (paradox 2) And it falls less

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[27] Things presenting a likeness with clouds, that is, things that are also cold and immersed in hot air, undergo a compression from the surrounding heat. This is the phenomenon of antiperistasis, the meaning of which here would be 'mutual repulsion' or 'to oppose by surrounding, to compress, to be compressed'. The cold cloud, in the upper region, is surrounded by the hot air, just as cold caves are surrounded by warm air in summer. Thus the cold of the cloud would be compressed (and intensified) by the hot surrounding air, just as the cold of the cave is compressed (and intensified) by the hot air surrounding it.

frequently in very high places, where it is nonetheless colder and where there is more snow. (paradox 3) Also, it is odd, he notes, that water should have time to freeze in the upper region, since a quantity of water such as is necessary to form hailstones the size of those observed can not remain suspended and would start to fall immediately.

To explain these paradoxes, he supposes that things must go on in the upper region as they do elsewhere. Now, he says, we can note that hot and cold have a mutual action on one another. Aristotle is referring here to what he calls antiperistasis, namely, the fact for a body to oppose or compress and it seems to intensify another by surrounding it, and he gives as an example the subterranean places that are cold in hot weather and warm in frosty weather. Thus, he continues, hail must be formed when the clouds descend into a warmer region which compresses and concentrates their inner cold, which then becomes so intense that the water which it forms does not have time to fall before being frozen (paradox 3). Furthermore, this coldness will be greater as we get closer to the earth; and that explains that there is more hail formed there (paradox 2). Also, we are now supposed to understand why hail falls more in warm weather (paradox 1).

Yet we can see that even if it would have been possible for Aristotle to push his observations further, he did not do so. For example, he might have cut a hailstone in two—that would have been at least as easy as verifying when and where it falls the most. He would have then been able to observe that the hailstone is made up of successive concentric layers of ice, and that the bigger the stone, the more the layers. Now, it seems he would have then been forced to admit that his hypothesis does not explain this fact. It indeed attributes the freezing of the water to a more intense cold than that which causes the condensation of air into rain; and therefore, as a larger droplet of water is due to a more intense cold, likewise, a larger hailstone should be explained, according to him, by more intense cold:

> Now we know that hot and cold have a mutual reaction on one another, which is the reason why subterranean places are cold in

> hot weather and warm in frosty weather. This reaction we must suppose takes place in the upper region, so that in warmer seasons the cold is concentrated within by the surrounding heat. This sometimes causes a rapid formation of water from cloud. And for this reason you get larger raindrops on warm days than in winter and more violent rainfall (…) The process (…) takes place when clouds descend into the warm air and is most violent when the clouds descend farthest. Sometimes…the cold is even more concentrated (*anteperistê*) within by the heat outside it, and freezes the water which it has produced, so forming hail. This happens when the water freezes before it has time to fall. For if it takes a given time to fall, but the cold being intense freezes it in a lesser time, there is nothing to prevent it freezing in the air, if the time taken to freeze it is shorter than the time of its fall. The nearer the earth and the more intense the freezing, the more violent the rainfall and the larger the drops or the hailstones.

Now, a more intense cold would explain that the hailstones were bigger, if they were only made of more ice. But it is not evident at all what relation more intense cold could have with the fact that bigger stones have more layers of ice. Such a fact seems to suggest that the hailstone has gone through successive exposures to cold, thus accumulating frozen water, rather than that it has been exposed to a more intense cold.

Case 2-The nature of seawater, *Mete.* II 3

The second example concerns seawater: why it is salt?

We can see that Aristotle starts by rejecting the hypotheses of others by confronting their consequences with the facts. For example, if the salinity of the sea came from a mixture of earth coming into it by means of the rivers, these rivers, which contain less water, should also be salty; now, they are not salty (at least, no one at that time could detect any salt in them). And if the sea had been generated by the sweating of the earth, as another thinker proposed, then this should still be the case, says Aristotle. Now, when earth is dried, nothing bitter comes out of it. Thus it is impossible (*adunaton*) that the sea should be the sweat of

the earth, he concludes, before announcing that he is now going to explain the salinity by means of a principle he has already expounded. Let us note that what disqualifies the aforementioned explanation in Aristotle's eyes is not the fact it is improbable, that is, that it does not appear to be true when directly compared with experience, but that it is not possible. Here we have a sign that what Aristotle expected of a good explanation is that it should be possible—no more.

Language consideration

I would like to make a comment in passing here: one of the signs of the use of the hypothetical-experimental method by Aristotle is his vocabulary. Whereas the vocabulary of dialectic would talk about *endoxa* or the like, that by which he often characterizes what I claim to be hypotheses, as well as the adhering of the intellect to them, is of a different and significant kind. We saw *adunaton* said of a hypothesis to be rejected. Among other words that he denies to theories that are to be rejected and affirms of those to be adopted, we also often find *phainomenon*, which is cognate with the Greek word for light and first means what appears to the senses, and then what appears to the mind. We also find *eulogon* and *eikos*, all of those words being often and correctly I think translated by 'plausible' or 'conjecture', because those are expressions implying less certitude than 'probable'. The frequent use of these same words and the absence of *endoxon* in passages which seem to be more scientific than dialectical might well indicate that Aristotle did distinguish clearly enough the two kinds of explanation.

Return to the salinity

Aristotle starts by formulating a general hypothesis, namely, that the salinity of a liquid is due to a mixture with some other thing. Although this is really not an opinion—its apparent truth does not come directly by looking at immediate experience—he does give it some verisimilitude by recourse, among other things, to living bodies. Their urine and sweat have a salty taste because of another substance secreted at the same time, he says.

Guided by his previous theory about the two kinds of exhalations, he then applies this general hypothesis to the sea. The exhalations mix together, he recalls, such that a certain quantity of the dry and earthy exhalation is always found in the clouds and in the water that is formed by condensation and descends once more to the earth with the rain. This process comes about with regularity, and this is the cause of the salinity of seawater.

Then we have a kind of proof by convergence: we learn how these hypotheses allow us to explain all sorts of common observations. Among others, that the rains that fall first are the most acrid; that eggs that would sink in fresh water float in seawater, that boats that have a deep draught in river water float higher once in the sea, that in the Dead Sea one cannot even sink because of the salinity. All of this is due to the heaviness that the mixture of the dry exhalation gives to salty things. And even if he does not mention it, Aristotle certainly was aware of the commonplace fact that seawater, when it evaporates, leaves a salty residue behind.

But he is not content to rest his case here. In fact, he comes up with a prediction of new facts and an experiment, recounted by others and not questioned by him, that a wax bottle, stoppered and left in the sea overnight, will be found to have fresh water in it the next day. The explanation would be that pure water passed through the very small pores, and that the earthy material was left behind. What is strange about this story is that, as we well know nowadays, things will not happen this way. Either nothing will go through, or if there is a hole big enough somewhere, the seawater will go through unaffected. Now, Aristotle could very well have carried out the experiment himself, and then he would have found that the story was not true, but he did not, in spite of the fact that he uses this story in two different treatises (*Meteorologica*, II; *Historia Animalium*, VIII).

But he himself did apparently perform an experiment to further confirm his hypothesis. He speaks about vaporizing seawater and then collecting the condensate. This, he claims,

was fresh, not salt. And so it is. But then he generalizes this conclusion to include any liquid that is water with something added, such as wine. Now, this will not work with wine, since the condensate will actually be richer in alcohol. Thus we must conclude that this generalization was based on an extrapolation from the case for seawater, and that here once again Aristotle did not perform a possible experiment, one which this time would have invalidated one of his conclusions.

Case 3-The speed of light, *De an.* II 7:

Let us now go on to another typical case where he only used a common prior observation as a confirmation of a hypothesis, instead of using predictions of new facts. In this case, however, he cannot be blamed for it.

The problem is that of the speed of light. The question was debated before Aristotle's time, not so much to determine what precise speed it had, but simply to know if it was a motion with a finite speed, or whether it was instantaneous. Aristotle claimed light was not a body, nor did it participate in motion, strictly speaking, but that it was propagated instantaneously in a straight line.

Now, he gave a sort of observational proof for this. Although the texts referring to this are vague, the one from the *De Anima* (418b24) is clear enough for us to imagine what kind of observation he was talking about, and to understand why it appeared true to him that light, in contrast with sound, was instantaneous. The Greeks knew that the speed of sound was great in comparison to everyday speeds, such as that of a horse or even an arrow, but they also knew it was finite. (In fact, they could even have come to some estimation of its value.) They also knew that relative to the speed of sound, that of light (if it had one) was necessarily much faster. But how much faster would it have to be? If one looks at the tips of mountains far-away to the west at the moment the sun rises in the east (and this is the observation Aristotle is no doubt talking about), one would perceive the sunlight reflected from these distant mountains at the

same time as the rays of the just-rising sun in the east appeared. Now, for the light to go to the far horizon and return in a time so short as to be imperceptible, either its speed must be incredibly great (and we must take this word as very literal for a Greek of this time), or it must be instantaneous. The more likely answer was that it was instantaneous.

Obviously, no one should fault Aristotle for not trying to go further and attempt the kind of measure to determine the possible speed of light that could only be carried out for the first time in the 19th Century. We might thus consider that we have here an example where Aristotle was wrong, but not neglectful of experience. Every scientist, in fact, operates in the framework of certain presuppositions that render certain things possible and probable, and others impossible, or at least extremely unlikely. There is also, obviously, the limits placed on observations by the absence of appropriate instruments.

Conclusions

The problem then with Aristotle's method does not seem to be that he did not perform experiments that were impossible for his time, but that he did not even do those that would have been possible, because of negligence, or because some hypotheses seemed to be in agreement with some prior theory. In other words, he not only lacked instruments and experimental protocol, but, it seems, even the conviction that such controlled experiments might be necessary.

Now, here are the conclusions that these and other like examples seem for now to suggest:

—Aristotle made use of hypotheses similar in kind to those used in the modern experimental sciences to explain certain more particular or difficult-to-explain natural phenomena.

—He did use numerous observations, some of which he himself made, but many of which were taken from others.

—He neglected experimental controls for his hypotheses; i.e., he did not try to verify the consequences of his hypotheses

by means of controlled experiments. He seemed quite content to use the initial observations, and did not even always check out those he had not made himself.

Finally, let me suggest three reasons why he might have proceeded in this way:

—He was overly confident in the value of ordinary experience and of the dialectical method (being excusably ignorant of the great complexity of natural phenomena and of their underlying causes).

—His belief in the correctness of some of his more general theories made him confident that particular hypotheses in keeping with these had a very good chance of being correct. In fact, one has the distinct impression that what was most important for him was to explain phenomena as particular cases of his more general hypotheses. Thus, we find his theory of the four earthly elements and of the "ether", of the nature and disposition of the heavenly bodies, with their influence on earthly change, being used as a background for the more particular hypotheses he formulates.

—He lacked a serious interest in the kinds of question that his hypotheses were intended to answer. Although he hoped to complete in some reasonable way his initial project for natural philosophy, as set out at the beginning of the *Physics* (and this is attested to by his introductory remarks in the *Meteorologica*), he did not consider all parts of this project as of equal value. Thus, these last considerations on the material and efficient causes of very particular phenomena on the earth would have necessarily taken last place in comparison with the problems of the heavenly bodies, the first mover, or even the investigation of the final and formal causes to be found in living things.

I believe that a more thorough investigation of his work in the particular parts of the natural sciences will confirm my ex-

planation for the way Aristotle proceeded there. I hope to carry out this task in a future publication.©

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