THE ASTRONOMY OF HIPPARCHUS AND HIS TIME: A STUDY BASED ON PRE-PTOLEMAIC SOURCES.

Lucio Russo

Dipartimento di matematica, Università di Roma *Tor Vergata*, Italy

ABSTRACT

A study based on pre-Ptolemaic sources leads to a very unusual view about hellenistic astronomy. In particular the thesis that some dynamical ideas (including the inertia law and some form of gravitation theory) were present in Hipparchus' astronomy is proposed.

1. INTRODUCTION.

This paper attempts to reconstruct some of the astronomical ideas of Hipparchus and his contemporaries. In view of the loss of this age's scientific works, the previous attempts have been based almost exclusively on the information given by Ptolemy in the *Almagest*. Instead, in sections 2 and 3, we illustrate why it might be essential to perform a careful examination of the extant pre-Ptolemaic testimonies; such testimonies, due to the loss of the scientific originals, can be drawn almost exclusively from non-scientific sources. After some methodological considerations, contained in section 4, on the feasibility and usefulness of using sources of this type, the paper continues with the analysis of some such documents.

In sections 5 and 6 we analyse the scientific content of an excerpt from Plutarch and, relating it to a passage in Simplicius, we propose the thesis that Plutarch refers to a dynamical theory contained in a lost work of Hipparchus.

In sections 7 and 8 we examine two excerpts from Seneca and Lucretius, which are shown to be parallel to each other. Section 9 contains a comparative examination of some passages of the astronomical expositions of Vitruvius and Pliny.

As a consequence of the previous analysis we formulate, in section 10, the conjecture that the hellenistic astronomy of the time of Hipparchus contained the idea of the use of a dynamics based both on the law of inertia and on some form of gravitation theory. There would have been, in particular, a substitution of a “dynamic heliocentrism” to the “kinematic heliocentrism” of Aristarchus.

In section 11 we show how, on the basis of some testimonies, limited in number but coherent with each other (mainly extracted from Plutarch and Strabo), it is possible to obtain a partial reconstruction of the evolution of the ideas on gravity from Aristotle to Hipparchus, a reconstruction leading again to the aforesaid conjecture.

In section 12 we observe that our thesis is coherent with the information available on three apparently independent subjects: celestial globes, fixed stars and comets; in section 13 we make some observations on the compatibility of our thesis with what is referred to by Ptolemy and by some writers of late antiquity. Concluding remarks are presented in section 14.

2. THE ASTRONOMY OF THE 3RD AND 2ND CENTURIES B.C.

During the third and second centuries B.C. the hellenistic astronomy developed within a continuous scientific tradition, to which belong, among others, Aristarchus of Samos, Conon of Samos [1], Archimedes [2], Apollonius of Perga and Hipparchus.

The relation between this astronomical tradition and Ptolemy (in particular between Hipparchus and Ptolemy) has been the object of different opinions and of several controversies. While the 19th century scholars maintained, in general, that the age of Hipparchus astronomy had been methodologically superior, now it seems that the opposite opinion is taking over [3]; the more recent view can be summarized with Toomer who, talking about the *Almagest*, writes that "its success contributed to the loss of most of the work of Ptolemy's scientific predecessors, notably Hipparchus, by the end of antiquity, because, being obsolete, they ceased to be copied" [4].

Let us quickly review now the sources on which we can base our attempt to reconstruct the scientific ideas which developed in the hellenistic astronomy, from Aristarchus to Hipparchus. They can be classified in four categories:

A. Extant works of the scientists of the time.

B. Sources dated from the period between Hipparchus and Ptolemy.

C. Sources later than the *Almagest*.

D. The *Almagest*.

A. The first category contains only the work of Aristarchus *On the sizes and distances of the sun and moon* and the Hipparchian commentary to the poem of Aratus of Soli, to which we can add a passage of astronomical content from Archimedes' *Arenarius*. These works have been intensively studied and probably all the relevant information has been extracted from them [5]. The information contained in them is, however, very scanty. Aristarchus' treatise, even if very important in telling us about the scientific method of his author, can be considered a geometrical work, since it has no relation with the main problem of mathematical astronomy, i.e. the description of the motion of celestial bodies. Similar considerations apply to the Hipparchian commentary to the poem of Aratus, which can give us only angular coordinates of fixed stars. The only extant hellenistic writing which contains some information about the models used to describe the motion of planets is the famous passage in Archimedes' *Arenarius*, where we have a brief

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[1] Conon is best known to us for the reference to him made by Callimachus who, in the *Berenice's Lock*,

[2] The astronomical activity of Archimedes, besides in references contained in his extant works, is testified in a fragment of Hipparchus preserved by Ptolemy (*Almagest*, III, i, p. 195, ed. Heiberg).

[3] Among the few works exposing the opposite view we quote [R. Newton]. Newton does not attempt, though, a reconstruction of the history of the scientific method at the times of Hipparchus and Ptolemy, but makes a personal accusation to Ptolemy, charging him with ineptitude and dishonesty. In any case, Newton's arguments do not seem to have received much credit.

[4] [Toomer], p.1. A description of the different opinions on this question is in that part of [Grasshoff] where a history is presented of the issue of the relation between the Star Catalogues of Hipparchus and Ptolemy.

[5] As far as Hipparchus' commentary is concerned thanks in particular to [Grasshoff].

description of the heliocentric theory of Aristarchus of Samos [6]. It is, however, just a brief excerpt, which, in a different context, happens to refer to an astronomical question.

B. No astronomical writing dates from the period between Hipparchus and Ptolemy, apart from incidental references to astronomical subjects contained in some scientific works of the early imperial period (such as in Heron’s *Dioptra*) and from elementary works, with no reference to planetary theories, such as Geminus’ compilation. The testimonies in the present category are essentially due to non-scientists, who include references to scientific arguments (generally misunderstood) in literary works, encyclopaedias or other types of writings. Since these are sparse quotations in a vast non-scientific literature, they have been mostly ignored both by classicists (generally scarcely interested in scientific digressions) and by historians of science, giving no credit to quotations from authors with little scientific credibility and who avoid the technical aspects of the theories to which they refer [7].

C. This category is almost completely useless because of the overwhelming influence of the *Almagest* on the entire astronomy of the following fifteen centuries. The information on hellenistic astronomy given in the astronomical works later than the *Almagest* is in fact almost completely obtained from the *Almagest* itself, a circumstance which seems to indicate that the ancient astronomical writings (in particular those of Hipparchus) have been lost very soon.

D. We are left with the *Almagest*, on which are almost exclusively based the present-day reconstructions of hellenistic astronomy.

In this situation, the widespread opinion that the *Almagest* had included all (or almost all) the previous astronomical knowledge could have a tautological base; it could be, in other words, that it is a consequence of the previous astronomy being reconstructed from the information given in the *Almagest* itself.

The criteria with which works were selected and preserved in the imperial period and during the Middle Ages can be easily illustrated with a few examples: just note that while all of Chrysippus’ works are lost (and among these the works where the propositional logic has been given foundation), Epictetus’ *Manual* has been preserved; we still have some of Archimedes’ works only for a fortuitous case, as they seem to have survived for centuries in just one copy, while the complete version of Pliny's *Naturalis Historia* has never been lost and has been preserved by a rich manuscript tradition; in medicine the entire work of Herophilus of Chalcedon has been lost, certainly not because of the little interest of its content, as it is now clear after the work of von Staden[8].

The selection criteria have thus favoured not the best works, but (as, on the other hand, it is easily understandable) those which could still be read without too many difficulties in times of great cultural decline. The fact that the astronomical treatises preceding Ptolemy have no longer been copied should not to be considered, per se, a reliable indication of their scientific inferiority and might even be considered as a possible clue of the greater effort required to read them.

The view that the *Almagest* included all the astronomical knowledge contained in the previous treatises is based, of course, on the assumption that Ptolemy had read them.

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6 Archimedes, *Arenarius* (ed. Mugler), pp. 135-136.

7 Neugebauer, for instance, writes: “obviously only by a careful analysis of the technical details may one hope to obtain a valid picture of the astronomy of Hipparchus and his time” ([Neugebauer], p. 277).

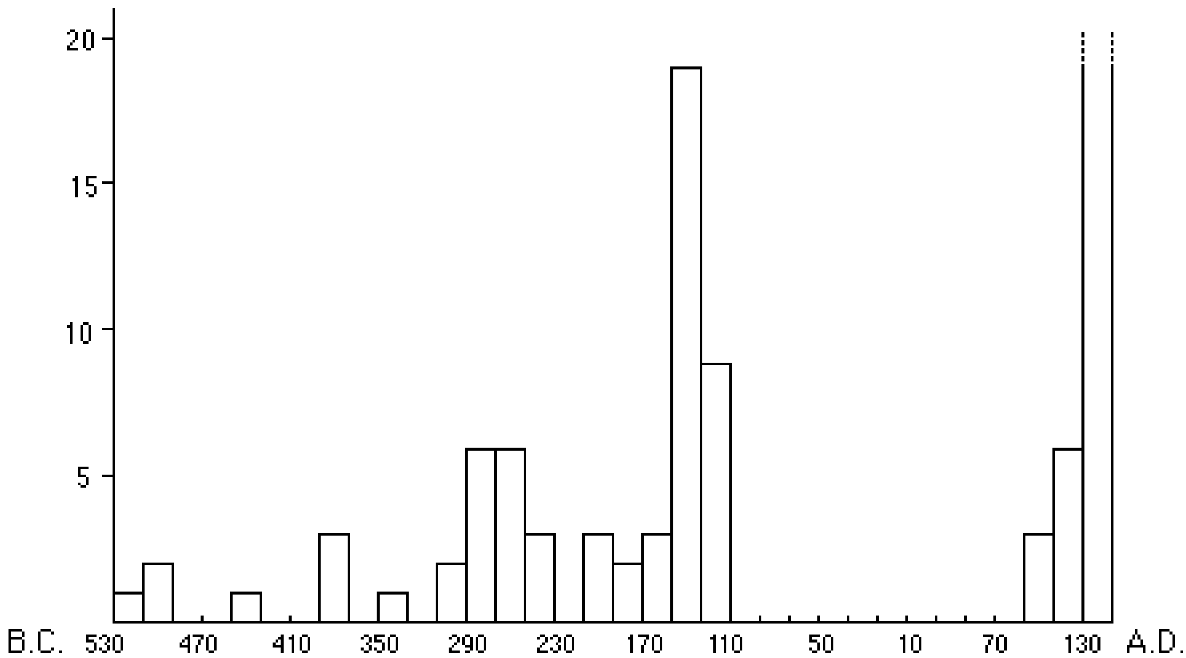
8 [von Staden].

If Ptolemy only had a partial knowledge of the preceding astronomy, the study of more ancient sources would of course become essential and could reserve many a surprise. This is the reason why in the next section we shall consider the problem of what knowledge Ptolemy had of the preceding astronomy and in particular of Hipparchus' work.

3. DID PTOLEMY KNOW ALL OF HIPPARCHUS' TREATISES?

All sources agree that Hipparchus was the greatest and the last of the hellenistic astronomers. Asking what knowledge Ptolemy had of the preceding astronomy essentially amounts to asking what knowledge he had of Hipparchian writings. It is usually tacitly assumed that such knowledge was complete.

We present in a histogram the astronomical observations recorded in the *Almagest*, classified according to their dates [9].



The histogram shows very clearly a well known, but often overlooked, fact: whereas there had been a continuous scientific, and in particular astronomical, tradition from Eudoxos down to Hipparchus, Ptolemy is centuries away from it; centuries during which the scientific activity had come to a halt and almost all the libraries, now war trophies of the Roman generals, had dissolved. The continuity of the teaching tradition

[9] The histogram is based on the index in [Toomer], where for each kind of astronomical observation all the observations recorded in the Almagest are listed. For graphical reasons, four observations of lunar eclipses between 720 B.C. and 620 B.C. have been omitted so that the horizontal scale is not too small. The first observations posterior to those of Hipparchus are about the moon: namely they are one in 92 A.D. by Agrippa and two in 98 A.D. by Menelaus. The observations from the period 130-150 A.D. are 39.

in Alexandria, in particular, had been tragically interrupted by the persecutions of Euergetes II in 145-144 B.C. [10].

The main element of continuity between the golden period and the new impulse to the studies which occurred in the imperial period was the Library of Alexandria, which survived both the events in 145-144 B.C. and the Roman conquest. After the persecution of Euergetes II, though, all the Alexandrian intelligentsia emigrated [11], and we know that there was appointed to the direction of the Library a certain Cydas ἐκ τῶν λογχοφόρων (that is, an army officer) [12]. It is then not too strange a hypothesis that the Library of Alexandria might not have had, in the following centuries, all of Hipparchus' treatises, Hipparchus himself being still working after 144 B.C. (in Rhodes) [13]. It is then legitimate to suspect that some of the hellenistic astronomical treatises, in particular some of Hipparchus' ones, might have ceased to be copied not after the *Almagest* had made them obsolete, but before the *Almagest* was even written.

It not being thus obvious that Ptolemy had full knowledge of the work of Hipparchus, let's try to find an answer to our question in the *Almagest*. Ptolemy states:

> Hipparchus did not even begin to establish theories for the five planets, not at least in his writings which have come down to us . [14]

Ptolemy's remark might seem dictated by generic cautiousness [15]. However, it seems to me that this sentence can be fully exploited by relating it to the previous passage in the *Almagest* where Ptolemy had given a word by word quotation of an excerpt from Hipparchus' *List of his own writings*[16], work that Hipparchus himself had written exactly to prevent doubts such as these and that Ptolemy clearly possessed.

Let us suppose that "the writings which have come down to us", as Ptolemy says, were all those listed in the *List of his own writings*. We would then deduce that Ptolemy was protecting himself against the unlikely case that Hipparchus' own list (presumably one towards the end of his scientific career) did not contain his main works (as one certainly should consider his possible writings on a planetary theory). It is then hardly believable, though, that Ptolemy, while discussing the issue as carefully as we are supposing he did, would not have quoted, in support of his own thesis, the testimony given by Hipparchus himself in the *List of his own writings*, a testimony which, even if not completely conclusive, should be considered at least a relevant one.

We are then led to conclude that "the writings which have come down to us", in Ptolemy's words, are not all those appearing in Hipparchus' *List*, and that Ptolemy himself, then, knew of other treatises not available in his time (at least not available in Alexandria).

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[10] The few sources referring to the persecution of Euergetes II are collected in [Fraser], vol. II, p. 166 and pp. 216 and foll.

[11] See Athenaeus, *Deipnosophistae*, IV, 184 b-c.

[12] As we know from an Oxyrhynchus papyrus (P. Oxy. 1241, II, 16).

[13] Among those quoted in the *Almagest*, the last of Hipparchus' observations has been made (in Rhodes) on July 7, 126 B.C. A list of all observations related to Hipparchus in the *Almagest* is in [Neugebauer], p. 276.

[14] Ptolemy, *Almagest*, IX, ii, p. 210.

[15] Such is indeed Toomer's impression ([Toomer], p. 421).

[16] Ptolemy, *Almagest*, III, i, p. 207: the passage has been correctly interpreted for the first time by Rehm, and Toomer is certain of this interpretation (cf. [Toomer], p. 139). Neugebauer was not certain of the existence of this work of Hipparchus ([Neugebauer], p. 338); in any case, our conclusions in the present section depend only very little on this question (and the rest of the paper is completely independent of it).

Another clue of the incomplete knowledge that Ptolemy had of Hipparchus' works is given by the measurement instruments: the dioptra described by Heron, presumably known to Hipparchus [17], was not known to Ptolemy [18]. This suggests the possibility that Heron had availability of some of Hipparchus’ writings which Ptolemy never had (as we shall suspect later on about different subjects).

Quite apart from the previous considerations, the context of the above mentioned Ptolemy’s sentence is rather important. Our quotation is taken from the second chapter of Book IX of the *Almagest*; it is in this chapter (which is an introduction to the subsequently exposed planetary theory) that Ptolemy claims of being the first to have elaborated a theory of planetary motion. Hipparchus is the only scientist mentioned in this connection, and the aforesaid sentence about him is an essential part of this priority claim [19]. Ptolemy does not restrict himself to the above mentioned sentence, but goes on to discuss the non-existence of Hipparchus' planetary theory, explaining why, in his opinion, Hipparchus had not worked on it and detailing what else he had done on the subject [20]. It seems a little odd that a scientist begins the exposition of a scientific theory by discussing at length one of his predecessors, active three centuries before, who had not worked on it at all. If, on the other hand, we assume that part of Hipparchus' treatises had then already been lost, Ptolemy’s discussion becomes easily understandable. Such a loss could indeed be the reason for disputes on what results had been actually obtained by the most famous astronomer of the past; in this case Ptolemy might in fact have been arguing against the view that Hipparchus had at least begun the formulation of a planetary theory. The existence of this second opinion could indeed easily explain the strict connection, otherwise hardly understandable, that we find in the *Almagest* between Ptolemy’s priority claim and his historical assertions about Hipparchus.

In the *List of his own writings*, as we can see from the excerpt reported by Ptolemy, Hipparchus had included, besides the titles of his works, also a brief description of their content. If none of these descriptions had hinted to a planetary theory it would be hard to believe that the mere question of the existence of some planetary theory by Hipparchus would have received the attention that Ptolemy gives to it. We can then suspect that the opinion contested by Ptolemy might have been in fact supported by some passages in the *List of his own writings* which could at least be interpreted as referring to a planetary theory.

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[17] We know that Hipparchus had used, improved and described dioptras and perhaps also other optical instruments for astronomical observations; cf. Ptolemy, *Almagest*, V, v, 369; V, xiv, 417; Pliny, *Naturalis Historia*, II, 95. Furthermore the technology described by Heron is (at least in the most cases) derived from ancient sources (cf., e.g., [Marsden$_1$], p. 3) and we do not know of developments in this sector in the period between Hipparchus and Heron (period when, as mentioned above, astronomical observations seem to lack). Hence it seems very likely that Hipparchus knew of the dioptra described by Heron. This is also Toomer’s opinion (cf. [Toomer], p. 227, n. 20).

[18] Ptolemy describes more primitive instruments used for the same purpose. This circumstance, together with the faith in the continuous technological progress, had been used as an argument to support the view that Heron was later than Ptolemy (cf. [Heath], vol. II, p. 305).

[19] Toomer remarks that Ptolemys’s statement that before him no planetary theory had been developed is certainly without basis (at least if literally interpreted), as can be shown by considering the history of Indian astronomy, which certainly was based on pre-Ptolemaic planetary theories ([Toomer], p. 420, note 6). Toomer interprets Ptolemy as meaning that the previous planetary theories were, by his criteria, unsatisfactory.

[20] Ptolemy claims that Hipparchus had simply listed planetary observations, with some useful reorganisation of the data (*Almagest*, IX, ii, p. 210).

In this situation, a plausible reconstruction, even if restricted to the main ideas of the hellenistic astronomical thought, hardly can avoid a careful examination of the extant literature dated from the period between Hipparchus and Ptolemy.

4. SOME METHODOLOGICAL CONSIDERATIONS.

As remarked above, the absence of true astronomical treatises dated from the hellenistic period forces us into trying to restore scientific ideas by using non-scientific sources. Hence we have to face the problem of what reliability could be accorded to such kind of sources and which method should be used in exploiting them.

The ideas we are interested in (the ones of hellenistic astronomy later than Aristarchus and in particular Hipparchus' ideas) belong to a well known scientific tradition. The extant works of Archimedes and Apollonius of Perga and Aristarchus' treatise *On the sizes and distances of the sun and moon* show indeed a great methodological homogeneity, independently from the particular subjects exposed. Deductions are in all cases obtained by mathematical method within a theory grounded on quantitative definitions and initial assumptions ("hypotheses" or "postulates") explicitly stated. We can be certain that the lost astronomical works of Aristarchus, Conon, Archimedes and Apollonius had used the same method and we know that Hipparchus belongs to the same scientific tradition.

The authors of the writings that we shall use, on the other hand, are non-scientists and they cannot understand (at least not completely) what they are referring to.

We may remark, though, that the incompetence of the authors, far from rendering of no value their testimonies, could be, to some extent, even of help in the reconstruction. For authors exposing subjects they ignore cannot contaminate the theory of their source with different scientific theories [21]. They can modify their source only in three ways: by inserting arbitrarily their own concepts, by altering the theoretical ideas they were not able to understand and, above all, by skipping most of the arguments.

Arbitrary insertions of the incompetent author can be, however, easily identified, since they are extraneous to the theory. If, for instance, Pliny is exposing an astronomical argument about the sun, his arbitrary additions may only contain concepts obviously associated to the sun, like heat or light, but certainly not a mathematical quantity defined within the astronomical theory. If Pliny introduces a geometrical argument we can be sure that he is using a scientific source and we can infer, at least, that the source contained a geometrical argument.

As for alterations of the source, since they do not depend on contamination with other theories, but simply by misunderstanding, they can hardly be repeated exactly in the same way in different authors. Hence if we get more than one testimony reporting the same theoretical concept we can reasonably infer that the concept was present in a scientific source.

The main troubles, of course, come from omissions: incompetent authors report indeed only scant fragments of scientific theories. A scientific theory, nevertheless, might be restored by a few fragments, at least in some essential features, much more likely than, for example, a novel; because we have "a priori" a piece of information of great value: we know the logical structure of the theory. Therefore if a passage seems to report

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[21] A different theory (scientific or not) may of course be referred in a close passage of the same text, just because the author is changing his source, but this kind of contamination hardly may happen within a single sentence.

a scientific statement, we can check it not only by looking for other authors reporting the same statement, but also by finding in other writings a logically connected statement.

After all, testimonies about scientific theories given by incompetent authors may be sifted much better than, to give an example, testimonies about a philosophical system given by another philosopher (even though in the second case for sure much more work has been done).

The aforesaid observations apply to the case of essentially literary authors, like Pliny or Seneca. Things are harder in the case of doxographical or philosophical works, which insert a reference to a scientific theory in a philosophical context (for instance, as we shall see below, in the case of Simplicius). In such cases scientific concepts are indeed translated in the philosophical language of the context and we have to look for the key for the translation. One must guard, however, in my opinion, against the common practice of attributing directly to the scientific source the aristotelian or neoplatonic statement of the philosophical author.

The aforesaid criteria need, of course, to be improved and precised in relation to each particular author, but, in our situation of almost total destruction of the ancient scientific literature, I believe that we should not neglect the bulk of information about ancient science contained in the extant literary writings.

5. SOME PASSAGES IN A DIALOGUE OF PLUTARCH. [22]

In Plutarch's dialogue *De facie quae in orbe lunae apparet* the following passage occurs:

> Yet the moon is saved from falling by its very motion and the rapidity of its revolution, just as missiles placed in slings are kept from falling by being whirled around in a circle. For the motion according to nature governs each thing unless it is diverted by something else. That is why the moon is not governed by its weight, [which is] balanced by the rotatory motion. However, there would be more reason to wonder if she were absolutely unmoved and stationary like the earth. [23]

After a while, another speaker of the dialogue says:

> To philosophers one should not listen if they want to repulse paradoxes with paradoxes and in struggling against opinions that are amazing fabricate others that are more strange and amazing, as these people do in introducing their "drift towards the centre". What paradox is not involved in it? ... Not that incandescent masses of one thousand talents drifted through the depth of the earth, stop if they should reach the centre, though nothing encounter or support them; and if they, drifted downwards with impetus, should go beyond the centre, they turn back and swing [...]? Not that pieces of matter cut off from either side of the earth should not be drifted downwards forever but falling upon the earth force their way into it from the outside and conceal themselves about the centre? Not that a turbulent stream of water drifted downwards, if it should reach the centre, a point which they themselves call incorporeal, stops suspended, moves in a circle around it, oscillating in an incessant and perpetual see-saw? [24]

I think that people who have studied some mechanics hardly could read the above passages without getting the impression that Plutarch was talking about scientific matter. Plutarch himself, however, presents his examples as different "paradoxes" of a

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[22] In the present and in the following sections most of the material is drawn from [Russo1], to which we refer for some remarks on Plutarch's text. Likewise in sections 8 and 9 material drawn respectively from [Russo3] and [Russo2] is included.

[23] Plutarch, *De facie quae in orbe lunae apparet*, 6 (= Moralia, 923 C-D).

[24] Plutarch, *De facie quae in orbe Lunae apparet*, 7 (= Moralia, 923 F - 924A).

unique theory. The remark that it is absurd to suppose that an incorporeal "point" could exercise any influence on bodies is typical of skeptical criticism of scientific theories [25]. In the same dialogue several scientific subjects, concerning in particular astronomy and catoptrics, are touched and some scientists are mentioned: for instance some passages of the extant work of Aristarchus of Samos are quoted word by word.

We have therefore several clues leading to the hypothesis that Plutarch, in the above excerpt, is referring to a scientific theory. We can check this hypothesis in a simple way: we can draw consequences (using the scientific method) from some statements of Plutarch and then examine whether the new statements so obtained are also documented in Plutarch's text. If we find a unique coherent scientific structure, we could not attribute it to Plutarch's imagination and we should conclude that Plutarch had used a scientific source.

Plutarch states: "the motion according to nature governs each thing unless it is diverted by something else."

If we want to understand the above sentence, first we have to make clear which motions are here intended as motions "according to nature" (κατὰ φύσιν).

According to Aristotle (who discusses at length the issue, in particular in the *De Caelo*) the answer depends on the nature of the moving body: heavy bodies move "according to nature" downwards (i. e. toward the centre of the earth), light bodies move "according to nature" upwards, whereas the motion "according to nature" of the celestial bodies is the circular one.

In the case of missiles placed in slings Plutarch's passage may suggest that, just like in Aristotle's writings, the motion "according to nature" should be the one downwards, due to heaviness, which should occur in absence of the rotatory motion. The distance between Aristotle and Plutarch's source is however made clear by the fact that the same consideration is extended to the moon: the source of Plutarch, evidently, maintained that the moon and the stones had the same motions "according to nature" and among them uniform circular motions were not included. The last point is a very relevant one, since the idea that uniform circular motions were the natural ones of celestial bodies (and therefore in particular of the moon) not only is usually attributed to all "Antiquity", but it was still shared by Galileo.

According to the source of Plutarch, is the rectilinear motion of bodies subjected to gravity a motion "according to nature"? I think that the answer should be negative. First let us observe that Plutarch's words I have translated as "motion according to nature" are κατὰ φύσιν κίνησις, whereas the effect of the gravity is not described as a "motion" (κίνησις) toward the centre of the earth, but as a "drift" (φορά) toward the centre. Translating φορά with "motion" (as for instance H. Cherniss does in his translation of the *De facie...* [26]) is not compatible with the circumstance that the motions described by Plutarch as subjected to a φορά towards the centre of the earth are not directed, in general, toward the centre: the only physical quantity which has in all cases such direction is the acceleration. The verb φέρω and the noun φορά seem used in this passage in a precise technical meaning. Plutarch himself, moreover, seems to indicate the scientific nature of this terminology, since he first introduces the term φορά by the words "as these people do in introducing their φορά towards the centre" and afterwards he uses the noun φορά and the corresponding verb φέρω exclusively for motions of bodies which, being subjected to gravity, vary, for this reason, their velocity; he uses, in

[25] Cf. Sextus Empiricus, *Adversus Mathematicos* (e.g. I, iii).

[26] [Plutarch], vol. XII, pp. 1-223.

our short excerpt, these two terms five times, never replacing them, in the same meaning, with any other of the many words concerning motion at his disposal.

In the case of the mass of one thousand talents which, arriving with some velocity to the centre of the earth, goes beyond it and starts to oscillate, it is clear that an effect of the gravity is the lessening of speed, since after the mass has gone beyond the centre its velocity decreases until zero; another possible effect of gravity is the increasing of the speed: otherwise the same mass could not turn back after his speed has become zero. The same considerations can be applied to the case of the stream of water oscillating forever. Let us observe that the motion of the same stream of water, subjected to the same gravity, could also be circular (just as in the case of the moon): a motion which the source of Plutarch certainly does not consider "according to nature"; on the other hand the motion of the boulder oscillating through the centre of the earth appears as far as the circular motion from aristotelian "natural" motion of heavy bodies. We are then led to the conjecture that the scientific source of Plutarch by motion "according to nature" could intend only the motion (κίνησις) of a body not subjected to any φορά, i.e. a rectilinear uniform motion. In this case the source of Plutarch should have stated something like: the rectilinear uniform motion governs each thing unless it is diverted by gravity or something else. It should have been an enunciation of the law of inertia, not to be found in any extant hellenistic treatise.

If Plutarch was actually referring to a scientific theory based on the law of inertia, his source should have maintained not only that the effect of gravity is the change of velocity, in general both in magnitude and in direction (as is illustrated by all the examples reported in the *De facie*...), but also that the same gravity is compatible with different motions (depending on the different initial velocities). Plutarch, in the case of the river near the centre of the earth, actually considers three different possible motions for the same body subjected to the same φορά (towards the centre): the rest, the uniform circular motion round the centre and the perpetual oscillation through the centre. As today we can easily check, they actually are three possible motions for a body in the conditions considered by Plutarch. Also in the case of the boulder Plutarch had described not one but two possible motions: the rest and the oscillation.

The whole excerpt of Plutarch is therefore consistent, both for the qualitative features of the described motions and for the terminology used, with the possibility that his source might have exposed a dynamics based on the law of inertia and on the idea that what is today called a "force" (in particular gravity) could not uniquely determine the motion, but only the variations of the velocity.

6. THE SOURCE OF PLUTARCH.

The passages of Plutarch that we have read suggest the possible existence (very surprising) of a hellenistic dynamics based on the law of inertia. Henceforth it seems worthwhile to search for the source from which Plutarch might have drawn his exposition. Nevertheless this problem had not been so far sufficiently explored neither by science historians nor by classicists.

Science historians have apparently undervalued the *De facie*... [27], perhaps because of the literary nature of the work. Koyré, for instance, underlines as an essential new idea of modern science the one of considering as "natural" only rectilinear motions, whereas all "ancient" physics should have opposed to the "natural" circular motion the

[27] Cf. below, sect. 14.

"violent" rectilinear motion [28]. The clear statements of Plutarch are not taken into any account. Even science historians concerned with the above excerpt have not faced the problem of the search of its source. S. Sambursky, e.g., (who even says that the motions described by Plutarch remind him of exercises in Newtonian gravitation theory) seems to consider the statements we have reported as a product of Plutarch's imagination [29].

Much more work, of course, has been devoted to the search of Plutarch's sources by classicists, who, being mainly interested in other aspects of the dialogue, have considered almost exclusively literary and philosophical writings, which do not seem sufficient to explain the origin of the scientific statements we are here considering [30]. Reinhardt argued that the source of some scientific statements of Plutarch was Posidonius [31], but his view was later contested (with apparent success) by several scholars. Concerning Plutarch's examples of motion inside the earth, it was observed by H. Cherniss [32] that they seem reminiscent of some passages of Plato's *Phaedo* [33], where also something is written about a river oscillating through the centre of the earth. Plato (who writes similar considerations also in *Timaeus*, xxvi, 62c-63a) observes that a river going down until the centre of the earth, flowing straight further, should go upwards. Plato intends to illustrate the relativity of the concepts of "up" and "down", whereas in the *Phaedo* and in the *Timaeus* both the idea of the law of inertia and the idea that different motions could be compatible with the same force are completely absent. The idea that a force could change the velocity of a body only in its direction is lacking, too. In other words all new ideas which make interesting, for their scientific content, the reported passages of Plutarch are absent in Plato's writings. Although it is possible that Plutarch's source was aware of the passages of Plato in choosing his examples, we can exclude that such a source was just Plato.

Despite the fact that the ideas reported by Plutarch about dynamics seem very far from what is usually considered the "ancient" physics, we can observe that the testimony of Plutarch is not completely isolated. Similar ideas appear indeed (as well as in some passages about hellenistic astronomy we shall consider in the following sections) both in Heron's *Mechanics* and in the pseudo-Aristotelian *Mechanical Problems*. Both these works contain an exposition of the rule of vector addition of displacements (and velocities)[34] and the two expositions are so similar to each other as to suggest strongly the use of a single source. We may observe that the above simple rule is a mathematical tool which has necessarily to be used for the description of the motion of a body subjected to a given φορά, in the meaning in which Plutarch seems to use this term. In the *Mechanical Problems* the rule is apparently applied just to the first example of Plutarch: it is there explained, indeed, how a uniform circular motion might be obtained as a continuous composition of two displacements: one "according to nature" (κατὰ φύσιν) along the tangent to the circle and another "contrary to nature" (παρὰ φύσιν) toward the centre [35]; this terminology corresponds to our interpretation of the passage of Plutarch and seems to give some further evidence in support to the conjecture of the existence of an ancient dynamics grounded on what was called later "the law of inertia".

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[28] [Koyré], III, Introd.

[29] [Sambursky].

[30] Cf., e.g., H. Görgemanns, *Untersuchungen zu Plutarchs Dialog De facie in orbe lunae*, Heidelberg, 1970.

[31] [Reinhardt].

[32] [Plutarch], vol. XII, p. 65.

[33] Plato, *Phaedo*, lx, 111, d-e.

[34] ps.-Aristotle, *Mechanical Problems*, 848b; Heron, *Mechanics*, I, viii.

[35] ps.-Aristotle, *Mechanical Problems*, 849a.

Everyday experience shows that if we want to move a body on a horizontal plane we have to apply a force. The law of inertia is compatible with the above remark if and only if frictional forces are taken into account. In other words, the introduction of the concept of friction is necessarily connected with the enunciation of the inertia law.

In his *Mechanics* Heron clearly uses the concept of friction [36]; furthermore he states:

> We shall prove that bodies in such a position [i.e. on a horizontal frictionless plane] can be moved by a force less than any given force. [37]

Since Heron in his treatise only deals with engines devised for the displacement of heavy bodies, one could hardly have expected in his work a nearer statement to the law of inertia than the above sentence.

Since the considered passages of Heron and of the pseudo-Aristotle, besides to be linked together by the parallelism of the exposition of the vector addition of displacements, both expose ideas which are necessarily connected with the inertia law, it seems possible that the two authors had used a source which was exposing the same theory alluded also by Plutarch.

The theory on falling bodies referred to by Plutarch having evidently relevance to ballistics, we may try to get some information about it by examining the technical works on artillery.

Philo's *Belopoeica* actually gives us some indirect evidence. Philo (of Byzantium) in his treatise tells us that research on ballistics was active mainly in Alexandria and Rhodes [38] and discusses in particular the problem of the dependence of the falling time upon the weight of the body. Philo mentions the possibility that the differences between falling times could depend on the different capacities of bodies in cleaving the air; this possibility, though, is referred just as one of the possible opinions and is refused [39]. In Philo's *Belopoeica* the terms φορά and φέρω are not used in the same way as in Plutarch's passage and no allusion to the law of inertia can be found. Taking the subject of the treatise into account, we may reasonably infer that the law of inertia was unknown to Philo. Since Philo's treatise may be dated about the end of the third century B.C. [40], we can infer that the theory alluded by Plutarch is probably later than this last date.

For the interruption of the hellenistic scientific tradition at the end of the second century B.C. we have mentioned above, it seems likely that the theory referred by Plutarch date back to the second century B.C. Such a theory seems to have unified the study of the motion of falling bodies with the one of celestial bodies like the moon, studying both as particular cases of motions of bodies subjected to a "drift towards the centre" (ἐπὶ τὸ μέσον φορά). The fact, told us by Philo, that research on ballistics was particularly active in Alexandria and in Rhodes suggests a possible connection with these two cities. On the other hand the circumstance that the theory seems unknown to most of the scholars active in Alexandria during the imperial period suggests that it could have been developed too late in order to be assimilated in the Alexandrian scientific tradition before the interruption of scientific activity occurred in Alexandria in 145-144 B.C. Therefore we may suspect that we are dealing with ideas developed in the second half of the century. Since the greatest scientist of that time is Hipparchus, working in Rhodes, his interest in the moon is well documented and the suspicion that

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[36] Heron, *Mechanics*, I, iv, 20-21.

[37] Heron, *Mechanics*, I, iv, 20.

[38] Philo of Byzantium, *Belopoeica* (ed. Marsden), p.108.

[39] Philo of Byzantium, *Belopoeica* (ed. Marsden), p.139.

[40] Cf., e.g., [Marsden2], p. 8.

he could have been among Heron's sources is suggested, as we have seen above, also by the *Dioptra*, [41] we are led to consider the possibility that Plutarch's source was actually Hipparchus.

Plutarch in his dialogue gives some clues supporting the above possibility.

First of all Plutarch mentions Hipparchus in the *De facie*..., explicitly attributing to him an optical theory. It is even more significant the circumstance that Plutarch, in the same dialogue, reports also some scientific results certainly due to Hipparchus without mentioning him: in particular Plutarch alludes to the possibility of measuring the lunar parallax [42] and uses numerical data coming from Hipparchus' lunar tables [43].

The speaker of the *De facie...* who ridicules the theory for its "paradoxes" is Lamprias, who in the dialogue speaks in the first person. The same Lamprias, addressing Apollonides (who in Plutarch's dialogue represents the "mathematicians"), says that the theory of vision is beyond his province and that of Hipparchus too [44]. Hipparchus is therefore treated as the main adversary in the same polemics against science to which belong the remarks about the "paradoxes" of the theory we are interested in.

Decisive evidence is given, in my opinion, by Simplicius, who tells us that Hipparchus had written a treatise about gravity with the title *Περὶ τῶν διὰ βαρύτητα κάτω φερομένων* (*On [bodies] 'drifted' downwards by heaviness*)[45]. Plutarch repeatedly uses the same terms: the boulder, the pieces of matter which force their way into the earth and the stream of water are referred to just as examples of bodies κάτω φερομένων ("drifted downwards") by gravity. Even though Lamprias, introducing the theory, does not talk about a "drift downwards", but about a "drift towards the centre" (ἐπὶ τὸ μέσον φορά), the same Lamprias, a little later, challenges at length the identification of a single incorporeal point (i.e. the centre) with the "down" (κάτω) [46].

Simplicius mentions Hipparchus' theory applying it to the case of the motion of a body thrown upwards vertically. Even though the language of Simplicius is unfortunately merely qualitative, it is nevertheless clear that Hipparchus had explained in some way why the motion was first upward with decreasing velocity and then downwards with increasing velocity. At this point Simplicius states:

> [Hipparchus] ascribes the same cause also in the case of bodies let fall from on high [47].

This sentence seems to indicate that Hipparchus' theory explained in the same way the motions of bodies thrown both upwards and downwards. This unification becomes easily understandable if we suppose that Hipparchus not only had used the same terminology reported by Plutarch, but also had given it the same meaning: it is clear indeed that in the two cases Plutarch's φορά (just as, in modern terms, the acceleration) is not different, being in particular directed downwards in both cases. It seems that only a theory which does not consider as main quantities the velocities but their variations

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[41] Cf. above, notes 17 and 19.

[42] Plutarch, just before mentioning Hipparchus, states that the lunar parallax is not negligible, a statement which goes back to Hipparchus (cf. Ptolemy, *Almagest*, V, v, p. 369). The passage about lunar parallax had been misinterpreted by Cherniss in [Plutarch] and the meaning of the sentence was first made clear by Neugebauer ([Neugebauer], p. 661).

[43] Cf. [Flacelière], p. 217; [Cherniss], p. 145; [Torraca], p. 244.

[44] Plutarch, *De facie quae in orbe Lunae apparet*, 4 (= Moralia, 921 D).

[45] Simplicius, *Comment. in Aristot. de caelo*; [CAG], vol. VII, p. 264, 25-26.

[46] Plutarch, *De facie quae in orbe Lunae apparet*, 10-11 (=Moralia, 925 E and 926 A-B).

[47] τὴν αὐτὴν δὲ αἰτίαν ἀποδίδωσι καὶ τῶν ἄνωθεν ἀφιεμένων· (Simplicius, op. cit., p. 265, 3-4).

could unify the treatment of the two motions. On the other hand the same conclusion is suggested also by the preceding passage, where Simplicius, reporting Hipparchus' description of the motion of a body thrown upward, had insisted just on the variations of its velocity.

Another passage of Simplicius is particularly useful:

> On the subject of heaviness, also, Hipparchus contradicts Aristotle; for he states that bodies are heavier the further removed they are [from the centre of the earth]. [48]

One might hardly consider Hipparchus' statement as referring to little displacements of bodies on the surface of the earth: we certainly do not have the impression of making bodies heavier by lifting them, whereas the acceleration of falling bodies might rather suggest the opposite view (which had been actually maintained by Aristotle). We must think that the weight differences Hipparchus was talking about were differences perceptible only for sensible variations of the distance from the centre of the earth. If we suppose, on the other hand, that Hipparchus was referring to bodies very far from the earth, his statement appears even stranger [49]. We can make sense of it only in one case: if we suppose that Hipparchus was concerned with bodies inside the earth, bodies which actually are lighter the nearer they are to the centre (until the weight vanishes when the centre is reached) [50].

Henceforth we have to conclude that Hipparchus in his treatise had also considered the motion of bodies " 'drifted' downwards by heaviness" inside the earth and in particular the case in which the distances covered were not negligible with respect to the distance from the centre. This is just the case of three of the examples considered by Plutarch: the incandescent masses, the pieces of matter which force their way into the earth and the stream of water.

Since the other example of Plutarch concerns the motion of the moon, a subject on which Hipparchus is certainly the main source of the dialogue[51], and, on the other hand, Hipparchus is also explicitly mentioned, the conclusion that Plutarch had drawn (not necessarily directly) also the "paradoxes" of our passage from Hipparchus' treatise may hardly be avoided.

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[48] περὶ δὲ τοῦ βάρους τὰ ἐναντία τῷ Ἀριστοτέλει φησὶν ὁ Ἵππαρχος· βαρύτερα γάρ φησι καὶ τὰ πλέον ἀφεστῶτα. (Simplicius, op. cit., p. 265, 9-11). This passage is usually quoted in the translation given by M.R. Cohen and I.E. Drabkin (*A source book in Greek science*, New York, 1948). In this translation the words "from their natural places", without any correspondence in the original, are added (without brackets) at the end of the sentence. It is true that the specification (lacking in the Greek text) of where the bodies are removed from makes the sentence more readable. It is also true that Simplicius is certainly talking about the centre of the earth, which is, for him, the "natural place" of heavy bodies. On the other hand I don't see any possible reason for attributing to Hipparchus the use of the aristotelian concept of "natural place".

[49] We might speculate about a possible theory asserting the existence of a kind of elastic force, increasing with the distance, attracting all bodies towards the earth. It seems very unlikely, however, that Hipparchus could have maintained such an opinion; it should be very far not only from traditional thinking but also from common sense, without giving any understandable help in explaining the phenomena.

[50] This statement is of course clear within any theory reducing heaviness to a reciprocal attraction between bodies. We shall exploit below, in sect. 11, some testimonia which seem to indicate the existence of such a theory (very far from Aristotle's views on gravity) in hellenistic science. Here we only observe that an independent way leading to the same conclusion could start with the remark that a body in the centre of the earth, by symmetry, has no weight at all and the plausible consideration that a little displacement of the body should not increase its weight too much.

[51] As we have remarked above, in both cases of lunar parallax and of numerical values drawn from lunar tables, it can be proven that Plutarch's source is actually Hipparchus.

7. A PASSAGE IN SENECA'S *NATURALES QUAESTIONES.*

In Seneca's *Naturales Quaestiones* the following passage (which we report in the translation by T.H. Corcoran) occurs:

> The five planets force themselves upon our attention. Occurring in one place or another they compel us to be curious. Recently we have begun to understand what their morning and evening risings mean, their positions, the time of their movement straight forwards, why they move backward. Whether Jupiter was rising or whether it was setting or retrograde (for that is the term they have given to it when it recedes) - we learned only a few years ago. [52]

Seneca is talking here about a new theory which since "a few years" had allowed to explain planetary retrogradations and stations. What is here intended by "a few years"? In this section of his work Seneca appears very interested in the progress of science during the entire human history; just a few lines before the above statements he had written, talking about fixed stars, that Greeks had begun to give them a name "since less than one thousand five hundred years". It is therefore clear that the "few years" have not to be intended too literally, but must be related to a huge time scale.

It is generally believed that Seneca had used, for his *Naturales Quaestiones*, essentially only one source, dated back to the first century B.C. We may therefore infer from the above passage that in the first century B.C. people were still aware of a new theory which had allowed to "begin understanding" (Seneca says "modo coepimus scire") planetary motions. The admiration transmitted to us by Seneca, together with the popular fame of Hipparchus and the absence of astronomers in the period between Hipparchus and Seneca, make it very unlikely that Seneca might have here referred to a theory unknown to Hipparchus. Seneca's passage seems therefore to give some indirect evidence supporting the conjecture that Hipparchus, notwithstanding Ptolemy's statement reported in sect. 3, actually did begin to establish a planetary theory.

Seneca gives us some information about the new theory; just after the above passage, he indeed goes on as follows:

> People have been found who would say to us: "You are wrong if you judge that any star either stops or alters its orbit. It is not possible for celestial bodies to stand still or turn away. They all move forward. Once they are set in motion they advance. The end of their orbital motion will be the same as their own end. This eternal creation has irrevocable movements. If they stop at any time it means that the bodies which are now maintained by a constancy and equilibrium will fall on each other [53]. What is the reason, then, that some celestial bodies appear to move backward? The encounter with the sun imposes upon them the appearance of slowness, as well as the nature of their paths and their orbits which are so placed that at a fixed period they deceive observers. In the same way ships seem to be standing still even though they are moving under full sail". [54]

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[52] Harum quinque stellarum, quae se ingerunt nobis, quae alio atque alio occurrentes loco curiosos nos esse cogunt, qui matutini vespertinique ortus sint, quae stationes, quando in rectum ferantur, quare agantur retro, modo coepimus scire; utrum mergeretur Iupiter an occideret an retrogradus esset (nam hoc illi nomen imposuere cedenti), ante paucos annos didicimus. (Seneca, *Naturales quaestiones*, VII, xxv, 5).

[53] Corcoran translates "will collide with one another", but the verb "to fall" corresponds more literally to the latin "incidere".

[54] Inventi sunt qui nobis dicerent: "Erratis, quod ullam stellam aut supprimere cursum iudicatis aut vertere. Non licet stare caelestibus nec averti; prodeunt omnia: ut semel missa sunt, vadunt; idem erit illis cursus qui sui finis. Opus hoc aeternum irrevocabile habet motus: qui si quando constiterint, alia aliis incident, quae nunc tenor et aequalitas servat". Quid est ergo cur aliqua redire videantur? Solis occursus speciem illis tarditatis imponit et natura viarum circolorumque sic positorum ut certo tempore intuentes fallant; sic naves, quamvis plenis velis eant, videntur tamen stare. (Seneca, *Naturales quaestiones*, VII, xxv, 6-7).

Since the direct source of Seneca was certainly not an astronomical treatise, we are using, at best, third hand information; on the other hand in the above passage the source seems to be reported verbatim and this circumstance strengthens our hope to find at least a trace of the information contained in the original.

The astronomical context is explicit: Seneca has made clear which celestial bodies he is talking about (his "quinque stellae" are obviously the planets) and retrogradations and stations are clearly mentioned.

Seneca states that planets can't reverse: celestial bodies are preserved in their orbits by their regular motions ("tenor et aequalitas"); they cannot stop, because in that case "they should fall on each other" ("alia aliis incident"). It just looks the same idea as the one more clearly explained, in the case of the moon, by Plutarch, but with a significant addition: gravity appears here to be conceived as a reciprocal interaction between bodies.

The "sling argument" exposed by Plutarch (i.e. what, in our language, might be said to be the argument of the balance between gravity and centrifugal force) may explain easily enough the motion of the moon around the earth, at least if, as suggested by Plutarch's passage, one is content with an approximate description, based on the use of circular orbits. The extension of the same argument to planets has however to face a very serious difficulty: why, indeed, at the time of planetary stations (when the centrifugal force seems to vanish) are the planets kept from falling? Here is the reason of the interest for planetary stations, reported by Seneca, and the meaning of his question: how is it possible (if celestial bodies can neither stop nor reverse without starting to fall on each other) that at times some planets appear to retrograde?

While the dynamical problem concerning planetary retrogradations and stations is clear, the solution referred by Seneca is not as easy to understand. I believe, nevertheless, that the argument exposed in Seneca's source can be restored.

The explanation of retrogradations to which Seneca alludes is clearly grounded on the combination of different circular orbits ("natura viarum circolorumque sic positorum ... "). Retrograde motions of planets, occasionally resulting from such a combination, are only an "illusion" ("... ut certo tempore intuentes fallant"), whereas planets never reverse in their actual motion.

Seneca's source might have explained that the apparent motion of a planet (i.e. its motion with respect to earth) can be obtained as a combination of two circular orbits, both with their centre in the sun, followed respectively by the earth and by the planet, whereas the "true" motion of the planet goes on simply on the second of the two orbits. As a matter of fact, the above description can well explain retrogradations and, having been actually proposed by Aristarchus of Samos, at the time of Seneca's source it had been well known to the hellenistic astronomers for about two centuries. Seneca's statement about a combination of circular orbits admits, of course, other possible interpretations: in particular it might suggest an epicyclic geocentric theory. Nevertheless, I believe that the heliocentrism of Seneca's source results clearly enough from the following considerations.

First, heliocentrism can actually solve the dynamical problem referred to by Seneca. The "sling argument" referred to by Plutarch can indeed be applied to the motion of planets exactly in the same way as to the motion of the moon, provided that the motion is not referred to the earth but to the sun, whereas the problem raised by Seneca seems hardly soluble within a geocentric theory.

Second, a "Ptolemaic" epicyclic theory should describe the motion of planets with respect to the earth without any reference to the sun, whereas in Seneca's passage there is an explicit reference to some participation of the sun in the explanation of planetary phenomena (Seneca mentions a "solis occursus").

Third, Seneca's statement that planetary stations are only an illusion seems to imply that the "true" motion (in which the planets never stop or revert) is not the one seen by terrestrial observers.

Fourth, the preceding point is confirmed by the example of the sailing ship which, nevertheless, seems at rest. Even though this example, too, is not completely clear in Seneca's words, we may be certain that it is an illustration of the relativity of the motion, because the same example is reported by several authors and, in particular, it had been worked out more clearly, as we shall see in the next section, by Lucretius.

Finally, we know that the possibility of a moving earth was actually taken into account by Seneca's source, since in a previous passage Seneca had reported the possibility of explaining the diurnal motion of the sky with the hypothesis of the rotation of the earth [55].

8. A PASSAGE IN LUCRETIUS.

Let us consider the following passage in Lucretius' *De Rerum Natura* (which we report in the translation by W.H.D. Rouse):

> A ship in which we sail moves on while it seems to stand still, one which remains in its place is thought to pass by; and the hills and plains, which we row by or sail by, seem to be flying astern. [56]

It appears that Lucretius is developing here, more clearly, the same example reported by Seneca in the excerpt that we examined in the previous section. This correspondence, which has been often remarked, is not the only analogy between the two passages. Lucretius goes on with the following verses:

> Sidera cessare aetheris adfixa cavernis
> cuncta videntur, et adsiduo sunt omnia motu,
> quandoquidem longos obitus exorta revisunt,
> cum permensa suo sunt caelum corpore claro. [57]

We may observe that Lucretius, just like Seneca, is talking about celestial bodies apparently at rest but actually in motion. The expression "adsiduo sunt omnia motu" is in strict correspondence to Seneca's "prodeunt omnia". By reading carefully Lucretius' verses one realizes that they cannot allude to the slowness of the apparent motion of the fixed stars, as it has been generally maintained. First of all Lucretius, again like Seneca, is not talking here about 'stars' which seem at rest, but about 'stars' which seem "to stop" ("cessare"); furthermore he introduces a link between this stopping and the return of the 'stars', link which should be obscure if he was not referring to stations and retrogradations. Planets seem to stop, though, not because they come back, but because they are on the point of doing so. If, instead of the present tense "revisunt", we had the future "revisent", the idea of the vanishing of velocity at the time of reversal should have been much clearer. Hence the circumstance that the generally accepted reading

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[55] Seneca, *Naturales quaestiones*, VII, ii, 3.

[56] Qua vehimur navi, fertur, cum stare videtur;/ quae manet in statione, ea praeter creditur ire./ Et fugere ad puppim colles campique videntur,/ quos agimus praeter navem velisque volamus. (*De Rerum Natura*, IV, 387-390) .

[57] *De Rerum Natura*, IV, 391-394.

"revisunt" comes from a correction in the codex Leidensis 30, where the original word was just "revisent" seems an important support to our suspicion: the "sidera" Lucretius is talking about, just like Seneca's "stellae", are the planets which, at the time of planetary stations, are on the point of starting to retrograde. We have to infer that Lucretius too, like Seneca, used sources explaining that planetary retrogradations are illusory phenomena, whereas planets actually continue in their regular motions.

Lucretius goes on with the two following verses, which are even more interesting:

> Solque pari ratione manere et luna videntur
> in statione, ea quae ferri res indicat ipsa.

The phrase "res ... ipsa" is evidently referring to the phenomenon described with the words "videntur in statione". Lucretius is therefore stating that the sun and the moon seem at rest, whereas just their apparent immobility (or, to say better, their appearing suspended in the sky, without falling) demonstrates their motion. As a matter of fact, it is just because of the relative motion that the moon and the sun do not come to collision with the earth, since gravity is balanced (in our language) by the centrifugal force. It seems, once again, the same idea reported both by Plutarch and Seneca.

Let us observe that, while the repetition of the same example of the ship might be considered (as usually it has been done) as an (implicit) reference by Seneca to Lucretius, a different kind of explanation is required for the much more extended parallelism between the two texts we have found.

Lucretius, in fact, cannot be the source of Seneca's statements about planetary motions, as it is made clear by the larger astronomical content of the passage of the *Naturales Quaestiones* and by its terminology. Seneca, who in particular introduces the word "retrogradus" explicitly as a technical term, is clearly quoting an illustration of an astronomical theory; it is therefore evident the use of a scientific or doxographical source. The connection between the two texts suggests, therefore, the use (maybe indirect) of a common source.

We can draw from the above analysis of Lucretius' passage the following two consequences:

1. An indirect evidence supporting our interpretation of Seneca. The two verses of Lucretius concerning the sun and the moon, indeed, if on the one hand express the same dynamical idea we had found in Plutarch, on the other hand they appear clearly parallel to Seneca's exposition, because of their proximity to the statement about the "illusory" planetary retrogradations and to the illustration of the relativity of motion by means of the example of the sailing ship.

2. The possibility of dating back at least to the first half of the first century B.C. the connection between the explanation of retrogradations as "illusory phenomena" and what we have called the "sling argument ".

9. SOME PARALLEL PASSAGES IN THE ASTRONOMICAL EXPOSITIONS OF VITRUVIUS AND PLINY.

Even though there is no true astronomical treatise dated from the period between Hipparchus and Ptolemy, latin literature of the first century B.C. and of the first century A.D. contains at least two expositions of astronomical arguments with some pretensions to be systematical: in the Book II of Pliny's *Naturalis Historia* and in the Book IX of Vitruvius' *De Architectura*.

Pliny's exposition, in spite of the author's patent incompetence, can give us some valuable information, in particular if we compare it with other sources. To give an example, Pliny underlines that no star is less "wandering" than the "wandering stars" (i.e. planets), despite their name [58]. The same opinion is explained more diffusely by Cicero, who emphasizes the regularity of the motion of the stars wrongly called "wandering", a regularity which deserves particular admiration just because it is not easy at all to be recognized [59]. We have to infer that Pliny is not exposing an idea of his own, but he is reporting a remark expressed already in sources available in the first century B.C. One gets the impression that such sources were aware of a planetary theory. This impression is confirmed by Pliny, who explicitly talks about a particularly ingenious theory, which could explain the motion of planets, in particular of the outer ones. The most interesting passage on the subject is the following:

> [The planets], struck in the aforesaid place, are prevented by a triangular ray of the sun from moving straight forward and they are drawn upwards by [its] burning force. [60]

A parallel passage occurs in Vitruvius, who (in the translation of F. Granger) writes:

> [...] the mighty force of the sun extending its rays in the form of a triangle draws to itself the planets as they follow, and, as it were curbing and restraining those which precede, prevents their onward movement and compels them to return to it ... [61]

The evident correspondence between the two passages is particularly significant in the light of the identity of the context, since Vitruvius, too, is here trying to explain the motions of the three outer planets. In both passages the idea of the sun's attraction of the planets is clearly expressed; furthermore it is particularly interesting the idea, reported by Pliny, that the result of the sun's attraction is the deviation of planets from a rectilinear motion.

That the source of Vitruvius and Pliny was heliocentric may be suggested, on the other hand, not only by the circumstance that Pliny, too, like Seneca and Lucretius, reports the idea that planetary stations are illusory phenomena [62], but also by Vitruvius' exposition of the motions of Mercury and Venus, which is explicitly heliocentric [63]. Concerning the motions of outer planets, Vitruvius, more doubtful, reports a bizarre "explanation" (which he claims to refuse) of their apparent stations and retrogradations; this explanation should be grounded on a strange "obscurity" ("obscuritas") which,

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[58] ... sidera, quae ab incessu vocamus errantia, cum errent nulla minus illis (Pliny, *Naturalis Historia*, II, 12).

[59] Cicero, *De natura deorum*, II, xx-xxi.

[60] Percussae in qua diximus parte et triangulo solis radio inhibentur rectum agere cursum et ignea vi levantur in sublime. (Pliny, *Naturalis Historia*, II, 69).

[61] ... solis impetus vehemens radiis trigoni forma porrectis insequentes stellas ad se perducit et ante currentes veluti refrenando retinendoque non patitur progredi sed ad se regredi... (Vitruvius, *De architectura*, IX, i, 12).

[62] Hoc non protinus intellegi potest visu nostro, ideoque existimantur stare, unde et nomen accepit statio (Pliny, *Naturalis Historia*, II, 70).

[63] "Mercuri autem et Veneris stellae circa solis radios uti per centrum eum itineribus coronantes regressus retrorsus et retardatione faciunt, etiam stationibus propter eam circinatione morantur in spatiis signorum." (Vitruvius, *De architectura*, IX, i, 6). Neugebauer states that "the passage in question is not only obscure but also apparently corrupt" ([Neugebauer], p. 694). The fact that Vitruvius is not able to report clearly an astronomical matter does not make plausible, in my opinion, that heliocentrism could have been introduced by him by chance. On the other hand it seems very unlikely that the "corruption" of the text, through centuries in which any memory of the heliocentrism was completely lost, could be the cause of the introduction of the heliocentric idea, so clearly expressed, in Vitruvius' work.

according to some authors, should be produced just by the sun! [64]. Doubtless Vitruvius had misunderstood his source. Probably Vitruvius (or, maybe, his intermediate source) had read the consideration that the actual motion of planets cannot be clearly identified by terrestrial observers (it is "obscured", in Vitruvius' interpretation) for some reason connected with the sun. We may conjecture that Vitruvius' source was exposing the same argument referred by Seneca too [65].

Vitruvius goes on as follows:

> [...], and to be in a "signum" of the other [out of two] triangle. Perhaps it will be asked why does the sun draw, by these heats, [the planets] in the fifth "signum" away from itself rather than in the second or third, which are nearer. I will therefore explain how this seems to happen. Its rays are spread out in the universe on the lines of a triangle with equal sides. Now [each side] extends neither more nor less than to the fifth "signum" away from its one [...] [66]

If we want to understand the above passage it is essential to understand what Vitruvius (and, more important, his source) means by the word "signum" that we have deliberately left in latin. This term has been usually intended as "sign of the zodiac" and in support of such an interpretation there is the argument, which might appear conclusive, that Vitruvius certainly uses the term "signum" in this meaning both in the same chapter, in some passages before the one we have reported, and in other chapters of the same book.

Observe, however, that the words "[...] are spread out [...] on the lines of a triangle with equal sides" ("[...] trigoni paribus lateribus formae liniationibus extenduntur [...]") clearly allude to a geometrical construction. Furthermore, the fact that Vitruvius, just after this passage, settles the issue by quoting the authority of Euripides seems a clear indication of his difficulty in following the reasoning he is reporting. We may therefore suppose that Vitruvius is trying to follow a Greek source exposing a geometrical construction. In the remainder of this section we shall try to restore the original mathematical argument. For this we have to introduce some considerations about geometrical terminology.

Hellenistic mathematicians denoted the concept of point with the term σημεῖον, whose original meaning was "sign" [67]. In latin, since no geometrical work had ever been written, there was no word corresponding to the concept of "geometrical point", whereas the term "signum" corresponded well enough to the original meaning of σημεῖον: it denoted indeed any kind of sign or mark.

When Vitruvius describes geometrical drawings, he generally uses phrases like "where the letter A shall be" [68] or "from the letter S let there be drawn a line [...]" [69]; that is to say Vitruvius uses the letters not as symbols of points, but directly for locating the places of the drawing where they are. In a passage where the word "signum" is used in a

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64 "Id autem nonnullis sic fieri placet, quod aiunt solem, cum longius absit abstantia quadam, non lucidis itineribus errantia per ea sidera obscuritatis morationibus impedire" (Vitruvius, *De architectura*, IX, i, 11).

65 Cf. Seneca' s passage quoted above, p. 16.

66 "..., in alterius trigoni signum esse. Fortasse desiderabitur, quid ita sol quinto a se signo potius quam secundo aut tertio, quae sunt propiora, facit in his fervoribus retentiones. Ergo, quemadmodum id fieri videatur, exponam. Eius radii in mundo uti trigoni paribus lateribus formae liniationibus extenduntur. Id autem nec plus nec minus est ad quintum ab eo signo." (Vitruvius, *De architectura*, IX, i, 13).

67 It was Euclid who introduced this term, replacing the previous term στιγμή (used in particular by Aristotle). The new word was regularly used by later mathematicians throughout the hellenistic age; only in the imperial period even the older term came again into use.

68 "ubi erit littera A" (*De architectura*, IX, vii, 2).

69 "ab littera S ducatur linea" (*De architectura*, IX, vii, 6).

meaning which seems to correspond roughly to the concept of point (evidently because it is used as a translation of the Greek term σημεῖον) Vitruvius writes "from that 'signum' and letter C let be drawn a line to the centre, where is the letter A" [70]. It is clear that even in this case the "signum" is not for Vitruvius a point, denoted by a letter, but simply a mark, which is near the letter.

The difference between the abstract concept expressed by Greek mathematicians with the term σημεῖον and the concrete one of "signum" (in the sense of a mark on the paper), with which a latin writer like Vitruvius replaces it, may be important for the interpretation of passages about astronomical subjects. The term "signum" could indeed be used in latin even for denoting a "sign of the zodiac" (a concept expressed in Greek with the term ζῴδιον). The latin word had in any case a concrete meaning; in most of the cases the context allowed us, of course, to understand which kind of sign was being talked about: if it was, for instance, a "signum" on the paper or, maybe, a "signum" in the sky. One can easily understand which kind of problems could arise in the case of a Greek scientific writing containing statements concerning a σημεῖον of the astronomical space. A latin-speaking reader might have perhaps thought that in this case the term σημεῖον was used as a synonym of ζῴδιον; in any case he should have been lacking in linguistic tools for getting a correct latin translation, since in latin a "signum" could be identified as a sign of the zodiac because of its location in the sky.

Of course the above difficulties could easily be overcome by means of a clear reference to a drawing, but it cannot be excluded that in some cases such a reference could be either lacking or misunderstood.

Concerning the association between a letter and a "signum", since for Vitruvius it is at most a relation of spatial proximity, it could hardly be extended to the case of a "signum" in the sky, where certainly there is no letter. Since Greeks used their letters also as ordinal numbers, in this case it could be natural to interpretate the letters in this second meaning.

We may therefore conjecture that Vitruvius' phrases "second 'signum'", "third 'signum' " and "fifth 'signum' " should correspond, in his source, to the Greek phrases used for denoting "the point Β", "the point Γ", "the point Ε"; the conjecture is also suggested by the considerations that such kind of expressions cannot be missing in a text explaining a geometrical construction, Vitruvius does not report other indications of points and it seems very hard, otherwise, to make sense of what Vitruvius says.

We shall try now, on the basis of our hypothesis, to restore the geometrical construction described by Vitruvius' source.

Remark, first of all, that Vitruvius is talking about triangles "with equal sides" ("paribus lateribus"). This phrase may seem, at first sight, to denote equilateral triangles, but since a literal translation from the Greek term ἰσοσκελές should give the same latin words, the triangles might also be only isosceles. The words "eius radii in mundo uti trigoni paribus lateribus liniationibus extenduntur" show that the second possibility has probably to be preferred, since they suggest that in each triangle the equal sides are formed by rays going out of the sun; if this is actually the case, they, of course, cannot be more than two. Furthermore, since the phrase "paribus lateribus" seems to refer to all the triangles formed by the considered "rays", such "rays" should be all equal to each other.

Our geometrical construction seems therefore to contain some isosceles triangles, whose equal sides are all radii of a circle with the centre in the sun. We note, in passing, that Vitruvius' word that we have translated as "ray" is "radius", which also had the meaning still maintained in English.

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[70] "ab eo signo et littera C per centrum, ubi est littera A, linea perducatur" (*De architectura*, IX, vii, 3).

The phrase "alterius trigoni" indicates that the considered (isosceles) triangles are exactly two (formed, evidently, by three "rays", or "radii", outgoing from the sun). Hence we are led to a geometrical construction containing at least the elements drawn in fig. 1, where we have indicated by the letter H the position of the sun.

Let us remark now that the last position of the planet is, according to Vitruvius, in the fifth "signum", which is a "signum" of the second of the two triangles. Following our interpretation, this statement should mean that the planet, at the end of the described motions, is in the point E, vertex of the second of the two isosceles triangles (the "trigoni signum" Vitruvius is talking about being nothing but a vertex of a triangle). In this way we get fig. 2.

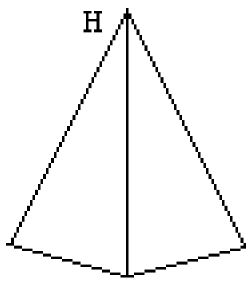

fig. 1

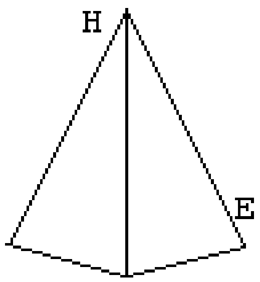

fig. 2

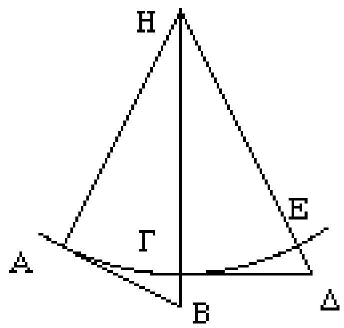

fig.3

The use of the letter E (and Vitruvius' interpretation of it) suggests that five positions of the planet were taken into account. It seems easy to identify three of them just as the vertices different from H of the triangles in the drawing, because such points certainly belong to the construction. And since they are on a circumference with its centre in the sun, their interpretation as a sequence of positions of the planet is consistent with the heliocentrism adopted by Vitruvius in his exposition of the motions of Mercury and Venus. We have now to add to our drawing two more points, corresponding to other two considered positions of the planet. Vitruvius' statement that the sun, displacing it in the fifth "signum", causes the planet to come nearer to itself ("ad se regredi") suggests that the last position considered before E (position which we may suppose had been denoted as the point Δ) was on the straight line HE and beyond the point E. Since Pliny, who seems to use the same source, states that the sun restrains planets from going straight on, we may suppose that the point Δ could represent the virtual position in which the planet should be found in the absence of sun's action; the point Δ results so determined as the intersection of the straight line HE with the straight continuation of the preceding fraction of the orbit. Since the presence of the two isosceles triangles suggests that all the construction might be obtained by explicitly repeating twice the same procedure (according to the habit of Greek mathematicians in their expositions of iterative methods) we lastly get fig. 3.

The meaning of the drawing is clear. It shows as the orbit (supposed circular) of a planet can be seen as a sequence of small fractions, each of them obtained as the composition of two simultaneous displacements: one on the tangent to the orbit (displacement which should be the actual one of the planet if, in absence of the interaction with the sun, it could proceed straight on, as Pliny says) and another one directed toward the sun.

The drawing can be used as an illustration of the idea referred, in a qualitative way, by Plutarch: the motion of the planet is indeed represented in the drawing as a result of a sequence of "drifts towards the centre". As far as the technical tool of the vector addition of displacements is concerned, we have remarked in sect. 5 that it is not only referred to by Heron and by the author of the pseudo-Aristotelian *Mechanical Problems*, but it is also used by the pseudo-Aristotle just for explaining how a circular motion can be considered as a continuous combination of a displacement "according to nature" (κατὰ φύσιν) along the tangent with a displacement "contrary to nature" (παρὰ φύσιν) toward the centre [71].

Our drawing can explain the origin of the sentences of Pliny and Vitruvius (otherwise quite hard to understand) according to which the sun restrains planets from going straight on by means of rays in the form of a triangle. We may also observe that the conjectured construction well corresponds to the words with which Vitruvius tries to explain how the "fifth 'signum' " (i. e., according to our interpretation, the point E) has been determined. Vitruvius indeed first talks about rays forming an isosceles triangle and then says that one of the sides extends "neither more nor less" (nec plus nec minus) than as far as the fifth "signum". Such words seem to correspond to a possible procedure for determining the actual positions of the planet, Γ, E, ... : they may indeed be obtained by considering first the virtual positions in which the planet should be if in the preceding section of the orbit it had not been deviated by the sun (i. e. the points B, Δ,...), joining such points with the centre H of the orbit (where the sun is) and then cutting off from these straight lines the lines of equal length HE=HΓ=HA.

Vitruvius' statement that the second "signum" and the third one are nearer to the sun than the fifth "signum" is a false statement if referred to the points B, Γ and E of our drawing. According to the reconstruction so far proposed, it has to be considered a natural consequence of Vitruvius' misunderstanding. If he had interpreted as signs of the zodiac the points of the geometrical construction and as ordinal numbers the letters used for denoting them, he might have thought that the signs of the zodiac had been ordered starting from the one in which the sun was.

10. HIPPARCHUS' ASTRONOMY.

In this section we shall try to draw from the results of the previous sections a plausible reconstruction of some ideas of Hipparchus' astronomy. Let us recall the main hypotheses so far formulated:

1. The ideas exposed in the passages of the *De facie ...* we have reported in sect. 5 go back to Hipparchus.

2. Seneca applies to the motion of the planets the same idea exposed by Plutarch in the case of the moon (and to which Lucretius alludes too): the idea that gravity is balanced by what today we call "centrifugal force".

3. Both Pliny and Vitruvius report the idea that planets are deviated from rectilinear motion by the sun.

Concerning the sources of the latin authors we have considered, let us observe that Pliny, in his *Naturalis Historia,* mentions Hipparchus as first foreign source of the astronomical exposition contained in the second book of his work and praises him repeatedly and enthusiastically; Pliny complains, in particular, about the fact that

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[71] Cf. ps.-Aristotle, *Mechanical Problems*, 849a.

nobody was able to continue Hipparchus' astronomical work [72]. Since Pliny considers the theory of the motion of the outer planets extraordinarily ingenious and he claims to be the first to divulge it [73], we may infer that Pliny was talking about Hipparchus' theory. Hipparchus is mentioned by Vitruvius too [74].

As far as Seneca is concerned, we may be certain that the astronomical ideas reported by him were known in Rome not later than the first century B.C., as it results both from the date (generally accepted) of his source and from the parallelism we have found with Lucretius' passage.

It is also worth remarking that Pliny's exposition, shortly before the description of the motion of the outer planets, contains a remark on the continuous progress of science[75] which corresponds to an analogous remark made by Seneca just before the passages we have quoted in sect. 7 [76]. We may suspect that on this subject Seneca and Pliny were using (maybe indirectly) the same source.

Let us observe now that points 1, 2 and 3, grounded essentially on analyses of different sources, strongly support each other.

If, indeed, we admit that Hipparchus is the scientific source of Plutarch's passages, we have to think that he used in astronomy the ideas about dynamics reported in the *De facie...* . Since Seneca's source could hardly expose ideas unknown to hellenistic astronomers of the end of the second century B.C. (i.e. unknown to Hipparchus) and, on the other hand, Pliny explicitly mentions Hipparchus, the proposed interpretations of the passages of Seneca and Pliny become then almost obvious.

Conversely, if our interpretation of the passages of Seneca, Lucretius, Pliny and Vitruvius (or at least of some of them) is accepted, then one can hardly avoid attributing the ideas referred in the *De facie ...* to Hipparchus, since a new important element can be added to those we had already found: the presence of the same ideas in authors who either (like Pliny) explicitly refer to Hipparchus or (like Seneca) who expose a "new" planetary theory, certainly already known in the first century B.C.

We may infer that hypotheses 1, 2 and 3 can hardly be false unless they fail simultaneously. In order to refute them, one has to refute both the identification of Hipparchus as source of Plutarch and our interpretation of all the considered passages of Seneca, Pliny, Vitruvius and Lucretius. Otherwise, one has to suppose that Hipparchus' writing on gravity was strictly related to his astronomical interests and that Hipparchus had used his dynamics in order to explain the moon's motion in the way referred to by Plutarch. On the other hand we have seen that the astronomical system used by Seneca's source appears as based, besides the ideas referred by Plutarch, also on heliocentrism. We are therefore led to the conjecture that Hipparchus might have explained planetary motions around the sun by adopting the same dynamical argument referred by Plutarch in the case of the moon. In other words Hipparchus might have replaced the "kinematic heliocentrism" of Aristarchus with a kind of "dynamical heliocentrism". The above conjecture, besides the evidence given by Pliny and Vitruvius, seems to be indirectly supported by a further consideration. In order to take into account the interaction with the sun a conceptual revolution is needed: gravity has to be conceived no longer as a tendency of heavy bodies to reach their "natural places" but as a reciprocal interaction

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[72] Pliny, *Naturalis Historia*, II, 95.

[73] Pliny's exposition of the motion of the outer planets starts with the sentence "Qui... aperienda est subtilitas immensa et omnes eas complexa causas" (II, 67) and ends with the words: "Haec est superiorum stellarum ratio; difficilior reliquarum et a nullo ante nos reddita" (II, 71).

[74] Vitruvius, *De Architectura*, IX, vi, 3.

[75] Pliny, *Naturalis Historia*, II, 62.

[76] Seneca, *Naturales quaestiones*, VII, xxv, 4-5.

between all bodies; once one has accepted this second idea, gravitational interaction with the sun appears not too remote a possibility. It seems therefore relevant to our issue the fact that the second view on gravity not only is clearly alluded to by Seneca, but it is also consistent with the statement, attributed by Simplicius to Hipparchus, that bodies are heavier the further removed they are from the centre of the earth. People maintaining that heaviness is a tendency of bodies to move towards their proper places hardly could reach Hipparchus' conclusion (which, just for this reason, seems absurd both to Alexander of Aphrodisias and Simplicius [77]). If, instead, heaviness has been reduced to a general property of attraction between bodies, it becomes then clear that a body inside the earth, being attracted only by a part of the earth (the "lower" part) is the lighter the nearer it is to the centre [78].

Note that relations between Rome and Rhodes had been not only peaceful but also very important from the cultural point of view during about one century after the interruption of the scientific activity in Alexandria (i.e. until the middle of the first century B.C.). It should be therefore not too surprising if traces of some of Hipparchus' ideas might be found more easily in the Rome of the first century B.C. (time not only of Lucretius and Vitruvius, but also of the source of Seneca and probably of the one of Pliny) than in the Alexandria of the second century A.D.

The view that in hellenistic astronomy of the second century B.C. were present "dynamical" ideas and in particular the idea of gravitational interaction is so far from the usual reconstructions that perhaps it is hard to take it seriously. In the following sections we shall try to check its plausibility by examining some further sources.

11. EVOLUTION OF THE CONCEPT OF GRAVITY FROM ARISTOTLE TO HIPPARCHUS.

In Aristotle the geocentrism is strictly linked to his concept of gravity. The centre of the earth, being also the immobile centre of the universe, is the "natural place" toward which all "heavy" bodies tend; "light" bodies tend instead upwards (i.e. they tend to go away from the centre), because of their different nature [79]. The above view (as well as many other Aristotelian views) prevailed in late antiquity and in the Middle Ages.

According to the reconstruction proposed in the previous sections, ideas on gravity should have been completely different at the end of the second century B.C.

In this section we shall try to follow a trail of the evolution of the ideas on this subject in the third and second centuries B.C.; we shall interpolate by means of logic the few extant testimonies.

Aristarchus' heliocentrism was obviously explicitly opposing the aforesaid Aristotle' s view. Had the opposition to geocentrism led also to a revision of Aristotle's ideas on gravity?

Note that, even independently from Aristarchus' ideas, Aristotle's concept of gravity had become unsustainable after the development of Archimedes' hydrostatics, at least for two reasons. First, Archimedes had shown the inexistence of lightness as a

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[77] Simplicius approvingly reports arguments of Alexander of Aphrodisias against Hipparchus' statement (Simplicius, op. cit, p. 265, 11 - 29).

[78] In this case, one can also easily understand, of course, that the attraction of the lower part of the earth for the body is compensated in part by the attraction of its upper part.

[79] Cf. Aristotle, *De caelo*, I, 3; IV; *Physica*, IV, 1.

positive property opposite to heaviness [80]: one of the main reasons for considering celestial bodies as having a different nature from the terrestrial ones had been thus eliminated. The second point is more important: Archimedes, in his treatise *On Floating Bodies*, had proved that some simple hypotheses on gravity (essentially the one that gravity was, as Aristotle had also thought, a spherically symmetric force directed towards the centre of the earth), together with some simple hypotheses about fluids, necessarily imply the spherical shape of the oceans at rest [81]. Archimedes' proof, even though the subject of his treatise restricts its application to the oceans, had been certainly used as an explanation of the shape of the earth as a whole, since the view that the earth had been initially fluid is reported by several sources and in particular by Diodorus Siculus, who clearly attributes the spherical shape of the earth to the action of gravity [82]. Archimedes' result is of great importance: it showed that the spherical shape of the earth had not to be accepted as the "natural" one for its perfection, but it could be obtained as a consequence of a few hypotheses about elementary forces. A feature of the actual constitution of the world had been thus deduced, for the first time, from laws of nature.

Once one has explained the spherical shape of the earth on the basis of gravity, another step can hardly be avoided: the extension of the same explanation to the sun and the moon; their evident spherical shape, indeed, necessarily appears, to readers of Archimedes' treatise, as an indirect proof of the gravity of these bodies: not toward the centre of the earth, of course, but toward their own centre. The first scientist who drew this consequence is unknown, but it is reasonable to guess that he might have been Archimedes himself. Archimedes, indeed, being very interested in astronomy and considering at least admissible Aristarchus' heliocentrism [83], had no reason for restricting to the earth the connection, so clearly demonstrated by him, between spherical shape and gravity. And undoubtedly this step was actually made, as it is documented by Plutarch, who writes:

> For as the sun attracts to itself the parts of which it consists, so the earth too .... [84]

At this point the aristotelian edifice (which shall be again the Ptolemaic one) has been destroyed from the inside. There are now two possibilities: either one assumes the existence of several independent centres of gravity, in particular in the centres of the earth, sun and moon, each of them having the power of attracting only bodies of "its own world", or attractions between different celestial bodies are also introduced. The first opinion was certainly proposed, because it is the one explicitly maintained in the *De facie...* by Lamprias (the aforesaid sentence about the sun belongs to his arguments on the subject).

We have now to ascertain whether the possibility of a gravitational interaction between different celestial bodies was also actually taken into account.

On this point the main information is given by Strabo. He reports, in particular, that Eratosthenes, on the basis of his research on tides, had asserted that the shape of the oceans was not exactly spherical, contesting the conclusion of the Book I of Archimedes'

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80 From this point of view Archimedean hydrostatics had certainly had some important precedents, due in particular to Democritus.

81 Archimedes, *On Floating Bodies*, Book I.

82 Diodorus Siculus, *Bibliotheca historica*, I, vii, 1-2.

83 Cf. Archimedes, *Arenarius* (ed. Mugler), pp. 135-136.

84 ὡς γὰρ ὁ ἥλιος εἰς ἑαυτὸν ἐπιστρέφει τὰ μέρη ἐξ ὧν συνέστηκε, καὶ ἡ γῆ... (Plutarch, *De facie quae in orbe lunae apparet*, 8 = Moralia, 924D).

treatise *On Floating Bodies* [85]. Here Strabo gives us a very interesting piece of information, since he puts a relation between research on tides and results on gravity obtained by hellenistic exact science.

Strabo (who is evidently incapable of following mathematical arguments) remarks that Eratosthenes, although he was a mathematician, was so naïve to contest a doctrine by Archimedes that is accepted by every one who has studied mathematics at all [86]. We, knowing Eratosthenes' scientific value and some of his mathematical results (for instance his mechanical method of extracting cubic roots) and being aware of the relationship between him and Archimedes [87], cannot share Strabo's opinion. Doubtless, Eratosthenes, before dissenting from an important work of the greatest scientist of his time, had carefully studied both hypotheses and demonstrations of the treatise *On Floating Bodies*. Since Archimedes' proof of the sphericity of the oceans is unexceptionable, we have to deduce that Eratosthenes knew very well that the spherical shape of the oceans at rest is a necessary consequence of the hypotheses on gravity assumed in Archimedes' work. He had therefore had the possibility of reaching different conclusions only by altering the hypotheses; more precisely the hypothesis of the spherical symmetry of the gravitational force had had to be eliminated [88]. Since, as we know from Strabo, Eratosthenes attributed tides to the moon's influence, we have to infer that he had altered Archimedes' hypotheses on gravity by taking into account the interaction with the moon. We can conclude that in the third century B.C. a gravitational interaction between different astronomical bodies had been assumed in at least one case: between earth and moon.

Attributing tides to the gravitational action of the moon necessarily implies a further step. The centre of the earth, considered by Aristotle the only source of gravity, cannot certainly be replaced by the moon in such a hegemonic role. Once one has realized that the moon's action is exerted on the earth, the only reasonable consequence is the hypothesis of a reciprocal action. Gravity must then be conceived no longer as an attraction toward one or more centres, but as an interaction between bodies. One could also think, of course, that reciprocity holds only between earth and moon, because of the "terrestrial" nature of the moon, without extending the idea to other celestial bodies, but such an extension seems a natural possibility. Was it actually accomplished?

According to Strabo's account, it seems that Eratosthenes had explained the tides as an effect of the action only of the moon. If, following the line of thought that we have tried to restore, somebody else had later discovered the influence of the sun on tides, the idea of a gravitational interaction between earth and sun should have become almost unavoidable. The idea of a "universal" gravitation should have then become within easy reach; an interaction between earth and sun should have been however sufficient for conceiving the idea, in which we are here interested, of a "dynamical heliocentrism".

Seleucus (as we know again from Strabo) was interested in particular in the tides of the "Erythraean Sea" (i.e., probably, of the modern Arabian Sea); he had not only studied their diurnal and monthly cycle, but also had related the variations of the differences between the two diurnal tides to astronomical phenomena: in particular he had realized

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85 Strabo, *Geography*, I, iii, 11.

86 In Strabo's words Eratosthenes had been so ἡδύς to contest Archimedes' statement.

87 As it is well known, Eratosthenes is the correspondent to which Archimedes sends, in particular, his important treatise *On Method*.

88 It seems unreasonable to suppose that Eratosthenes had denied homogeneity and isotropy to the water (apart, of course, from the existence of the "iced sea", which does not seem, however, too relevant in this context). Any other possible modification of Archimedes' hypotheses could not change the spherical symmetry of the problem (and of its unique solution, too).

that spring tides present the largest difference near the solstices and the least one near the equinoxes [89].

Had Seleucus limited himself to observe a correlation between the observed behaviour of tides and some astronomical phenomena, or had he given a theoretical explanation of it? The second possibility is supported by the following considerations.

First, the "theoretical" explanation of the effect described by Seleucus does not seem too hard. It is enough to attribute tides to the interaction with the moon and the sun, to admit that the effect of each of the two luminaries is maximum when it appears at the zenith or (equivalently) in the antipodal point and to take into account some delay of the actual tides. Once one has accepted the above hypotheses, it is easy to deduce that tides not too near to the Equator should behave according to the above description, if the earth should be a perfect solid sphere surrounded by a homogeneous layer of water (details are left to the reader). The really interesting point is that in the particular case of the Arabian Sea the actual behaviour of tides (which is often very far from the "theoretical" one) in fact is in agreement with both the above simple theoretical scheme and Seleucus' description. It is perhaps worth observing that the agreement between Seleucus' description and the actual behaviour of the tides observed by him was recognized, in 1898, by one of the founders of the modern theory of tides, G.H. Darwin[90].

Second, some evidence that in hellenistic times the above hypotheses had been actually accepted is given by Pliny. He talks explicitly about the moon's attraction and, just before that, he states that the cause of tides are the moon and the sun [91]. Pliny seems to refer implicitly to Seleucus when he mentions the differences between the two diurnal tides, a difference vanishing only at the equinoxes [92]; it is particularly significant that Pliny alludes also to the delay of the actual tides [93], since such a remark seems hardly understandable without some theoretical explanation of the phenomena.

Third, research on tides, as seen above, had been included in the tradition of hellenistic exact science (and in particular it had been related to gravity) since Eratosthenes' times. If, notwithstanding this (and the preceding considerations), Seleucus had taken back such research in a merely empirical setting, we could hardly understand why all extant testimonies should consider him a "mathematician" (or an astronomer) [94].

Since the essential contribution of Seleucus to the theory of tides, according to the information given by Strabo, was the study of the yearly cycle of the inequalities between the two diurnal tides, on the basis of the preceding considerations it seems likely that Seleucus had recognized the influence on tides of the sun [95]; it seems likely, in

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[89] Strabo, *Geography*, III, v, 9.

[90] G.H. Darwin, having read Strabo's passage about Seleucus in a collection of fragments by the Dutch scholar Bake, introduces his comment to the passage with the following words: "There is another very interesting passage in Strabo, the meaning of which was obviously unknown to the Dutch commentator Bake - and indeed must necessarily have been unintelligible to him at the time when he wrote, on account of the then prevailing ignorance of tidal phenomena in remoter parts of the world" ([Darwin], p. 76). Bake's book dates from 1810.

[91] Pliny, *Naturalis Historia*, II, 212.

[92] Pliny, *Naturalis Historia*, II, 213.

[93] Pliny, *Naturalis Historia*, II, 216.

[94] Cf., e.g., Strabo, *Geography*, XVI, i, 6, where Seleucus is listed among the "Chaldeans" famous among "mathematicians".

[95] The sun is of course essential for the monthly cycle, too; nevertheless, it seems not obvious at all to deduce the sun's influence from it. Cf., e.g., R. Descartes, *Le Monde... ou Le traité de la Lumière*, chap. 12, where the relation between tides and phases of the moon, even if clearly stated, is attributed to the moon alone. It seems that the yearly cycle, instead, could very hardly be explained ignoring the sun.

other words, that he had accepted or discovered (extending Eratosthenes' theory) the gravitational interaction with the sun.

Plutarch writes:

... Did [Timaeus] put the earth in motion ... and ought the earth, globed about the axis extended through all, be understood to have been devised not as confined and at rest but as revolving and turning, as Aristarchus and Seleucus afterwards maintained that it did, the former stating this as a hypothesis, the latter demonstrating it ? [96].

The verb used by Plutarch at the end of the passage, ἀποφαινόμενος, allows us different possible interpretations about the actual contribution by Seleucus, but the contrast to the Aristarchus' "hypothesis" makes it clear however that Seleucus had found some new argument supporting the motions of the earth.

As remarked in section 7, it seems possible to restore an argument supporting the heliocentrism on the basis of Seneca's testimony: such an argument, based on the idea of the gravitational interaction between the sun and the planets (in particular between sun and earth), amounts to observing that a heliocentric astronomical theory allows us to explain the motions of the planets on the basis of a balance between gravity and centrifugal force. Since no other argument supporting the heliocentrism has ever been found in the classical literature, the thesis that Seleucus had based both his tides theory and his proof of the heliocentrism on the hypothesis of a gravitational interaction with the sun seems reasonable.

If the previous thesis is refused, one must think that the two documented scientific interests of Seleucus were independent from each other and only by a mere chance were they the two subjects later connected with the idea of gravitation toward the sun. The last idea, being documented in Vitruvius and Pliny, should have been, however, proposed by some other unknown astronomer roughly contemporary with Seleucus [97], while another unknown astronomer (maybe coincident with the first one) should have elaborated the argument reported by Seneca.

Trying to follow the evolution of the ideas on gravity until Hipparchus, we have found a trail leading to Seleucus: a scientist probably contemporary but older than Hipparchus. One might perhaps conjecture that Hipparchus had accepted and developed the idea, first raised by Seleucus, of a gravitational interaction with the sun; such a conjecture cannot be proved, but it is consistent with Strabo's testimony that Seleucus was considered as an authority by Hipparchus on the subject of tides [98].

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[96] Plutarch, *Platonicae quaestiones*, VIII, i = Moralia, 1006C. Even though some scholars have maintained that Plutarch is here referring to Seleucus in connection with the only motion of rotation, Plutarch's passage seems clear enough. The motions of the earth are described with two different verbs (στρεφομένην καὶ ἀνειλουμένην). Each of the two verbs, it is true, might be interpreted, if separately considered, as referring to the motion of rotation (just as in English the verb "revolve" may be used, in a non-astronomical context, as a synonym of "rotate"). If both verbs were describing the same motion, we should not understand, on the other hand, why Plutarch should have used two different verbs instead of only one. Furthermore Plutarch specifies that he is talking about the motions first assumed by Aristarchus and after him by Seleucus. Since the motions attributed to the earth by Aristarchus are described unambiguously by Plutarch himself (*De facie...*, 6 = Moralia, 923A), I do not think that we could have too many doubts about the meaning of the sentence.

[97] The idea of an interaction between the sun and the earth seems indeed unknown to Eratosthenes and Archimedes (who, in the "Arenarius", reports the heliocentric idea in purely kinematic terms). On the other hand it seems very unlikely that such an idea could be later than Hipparchus.

[98] Strabo, *Geography*, I, i, 9. For an analysis of all testimonies on Seleucus and in particular of a passage of Aëtius ([DG], p. 383) which, in my opinion, misled many scholars (starting from Galileo), cf. [Russo5].

We may conclude that the ideas conjecturally restored on the basis of the passages of Plutarch, Strabo, Seneca, Pliny and Vitruvius go back to second century B.C. hellenistic astronomy, to which Hipparchus had certainly given an essential contribution. Separating Hipparchus' contribution from the one by Seleucus and other scientists, on the basis of the extant sources, does not seem a pursuable aim.

12. CELESTIAL GLOBES, FIXED STARS AND COMETS.

In this section we shall draw some clues about the hellenistic astronomy from the available information on three seemingly independent subjects (which shall appear actually related to each other if the theses here proposed are accepted): celestial globes, fixed stars and comets.

Celestial globes. It is well known that Aristarchus had maintained that the heliocentric hypothesis could "save the appearances" (φαινόμενα σώζειν). He had actually produced some ἀποδείξιας τῶν φαινομένων ("illustrations" or "demonstrations" of the "appearances"), as referred by Archimedes [99]. Since the description of the apparent motions of the sun, the moon and the fixed stars cannot get, of course, any benefit from the heliocentric hypothesis, the "appearances" to which Aristarchus was referring concerned evidently the planets: in particular he was clearly alluding to retrogradations and stations, since a heliocentric model actually can explain such complex "appearances" on the basis of simple regular motions. Aristarchus' "illustrations" could evidently become particularly impressive by means of a mechanical model (i.e. a mobile celestial globe).

We know that such a mechanical model was actually built by Archimedes. Many people have wondered how such a construction could have been accomplished, implicitly assuming that in Archimedes' model the sun and each of the planets could be moved according to some mechanisms connected, independently from each other, with a motionless earth [100]. Probably the above kind of mechanical model could be conceived only by people unaware of the possibility of "illustrating the appearances" following Aristarchus' proposal. Since we became aware of it (in the Renaissance) just by reading Archimedes' *Arenarius*, supposing that Archimedes' mechanical model was not "heliocentric" seems to me a little strange. The widespread idea that Archimedes had built a "geocentric" model arises probably from all testimonies telling us that the model represented the motion of the sun and the planets around the earth. This point has to be made clear. A mechanical model representing the motion of planets with respect to the sun does not reach the aim of explaining the "appearances" (of course for terrestrial observers), which is just the aim in building a moving celestial globe. Hence, even if one has built a model with earth and planets revolving around the sun, in order to show the actually observed motions, one has to set the mechanism in motion holding the earth at rest [101]. The point is that in a "heliocentric" model the planets are not directly connected with the earth, but the mechanical connection between each planet and the earth is

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[99] Archimedes, *Arenarius*, 136, 1-2 (ed. Mugler).

[100] Neugebauer, for example, remarks that in a spherical model (needless to say, implicitly supposed "geocentric") even the most characteristic features of planetary motions, stations and retrogradations, are necessarily omitted and the inner planets must be ignored completely ([Neugebauer], p. 652, note 7).

[101] Of course people who intend only to show the "true" motions, without any interest in what can actually be observed, would prefer a "pure heliocentric" model (i.e. a model with a fixed sun). This attitude seems however to me very far from Aristarchus' purpose of "saving the appearances".

obtained by means of a unique articulation point, coinciding with the sun. It is easy, in this way, to get relative motions of planets including stations and retrogradations. The conjecture that Archimedes' model was actually so conceived is consistent with the main testimony we have on the subject, namely the one by Cicero, who writes:

> The invention of Archimedes deserves special admiration just because he had thought out a way to represent by a single "conversio" those various orbits with their very different and divergent motions. [102]

Unfortunately, Latin is not lexically particularly rich on the subject of mechanical technology; the word "conversio" could mean a switch or a (mechanical device to get a) revolution or a reversal. It should have been certainly the right word to indicate an articulation point allowing the change from a direct to a retrograde motion. In any case, Cicero's emphasis on the uniqueness of the device on which all different motions depend should not be compatible with a "Ptolemaic" mechanical model. Let us observe that a "Ptolemaic" celestial globe, on the other hand, should be almost useless: nothing really interesting could be indeed shown by a complicated model repeating the combinations of deferents and epicycles of the theory; furthermore it should be extremely hard, from a mechanical point of view, to construct it.

The last astronomical mechanical model we know about was the one (about which Cicero writes, too), built by Posidonius in the first century B.C. [103]. We do not know about any later moving celestial globe until our "Copernican revolution", when we were again able to construct mechanical models ("heliocentric", of course) of the solar system. The history of celestial globes seems therefore to give a significant clue leading to the view that Aristarchus' heliocentric model had been given up not in the period between Aristarchus and Hipparchus, but, together with many other ideas, during the long interruption of the scientific activity that occurred between Hipparchus and Ptolemy.

<u>Fixed stars.</u> In the second century B.C. at least two astronomers had eliminated the sphere of the fixed stars, conceiving an infinite universe: Seleucus [104] and Hipparchus; we know that the latter had also conjectured that the seemingly "fixed" stars were actually moving [105]. Geminus, probably about 50 A.D., still refused the existence of the sphere of the fixed stars, assuming that the stars were at variable distances [106]. If one does not assume the rotation of the earth, it seems very hard to explain the diurnal motion of the stars, all keeping the same relative positions, without supposing some physical connection. It seems therefore not a chance that Ptolemy, who believes the earth to be motionless, also reestablishes the rigid sphere of the fixed stars. We have so gotten another indirect evidence supporting the conjecture that Hipparchus conceived a

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[102] "... in eo admirandum esse inventum Archimedi, quod excogitasset, quem ad modum in dissimillimis motibus inaequabiles et varios cursus servaret, una conversio" (Cicero, *De re publica*, I, xiv, 22). Cicero reports observations contained in a lost writing of Sulpicius Gallus, who had had the opportunity to see Archimedes' celestial globe in the house of his colleague in the consulship, Marcus Marcellus (who had inherited the globe from his grandfather Marcellus, the conqueror of Syracuse). Cicero explains the same idea (of a single "conversio" on which all motions depend) also in *Tusculanae disputationes*, I, xxv, 63.

[103] Cicero, *De natura deorum*, II, xxxiv, 88.

[104] Cf. Aëtius, [DG], p. 328.

[105] According to Pliny, Hipparchus had compiled his star catalogue just in order to allow posterity to observe the displacements (suspected by him) of the fixed stars (*Naturalis Historia*, II, 95). Hipparchus fully achieved his purpose. Motions of the "fixed" stars were indeed first observed in 1718 A.D. by Halley, who compared the coordinates measured by him of Sirius, Arcturus and Aldebaran with the ones of Ptolemy's star catalogue (coordinates which, as shown in [Grasshoff], came almost surely from Hipparchus' catalogue).

[106] Geminus, *Isagoge*, i, 23.

moving earth. The hypothesis of the rotation of the earth does not seem sufficient, however, for giving up the sphere of the fixed stars, as the examples of Aristarchus, Archimedes, Copernicus and Kepler show. We may remark that in modern times the idea of an infinite universe, with moving stars, arose only after a dynamics based on the law of inertia was applied to astronomy.

Comets. From a merely phenomenological point of view, comets seem to have little to do with planets: the appearance is quite different and it is not easy at all to realize their periodicity; furthermore, even if the periodicity of a given comet has been noticed, its motion should be in any case unobservable for most of the time and different from the one of any other comet. A purely "descriptive" astronomy, therefore, could hardly include a theory about comets. The question of the possible existence of other bodies, besides planets, revolving around the sun, whose orbits are more elongated, seems, on the contrary, a natural one within an astronomy based on "dynamics" and on "gravitation theory". It is not surprising that there is no theory about comets in the *Almagest* and the modern theory was elaborated (by Halley) only after the development of the gravitation theory.

Seneca in his *Naturales Quaestiones* writes:

> Apollonius [of Myndus] says that the Chaldeans place comets in the category of planets and have determined their orbits. [107] ...

> Apollonius of Myndos has a theory different from Epigenes. He says that a comet is not one body composed of many planets but that many comets are planets. A comet, he says, is not an illusion or fire extending from the edges of two planets but is a celestial body on its own, like the sun and the moon. It has a distinct shape thus: not limited to a disc, but extended and elongated lengthwise. On the other hand its orbit is not clearly visible. A comet cuts through the upper regions of the universe and then finally becomes visible when it reaches the lowest point of its orbit. [108]

In the source the statement about the "shape", even though Seneca might have not been aware of it, concerned the shape of the orbit. First, indeed, it seems very strange that an astronomical source could linger in remarking that a tail of a comet is "more elongated than a circle"; furthermore, only if the statement concerns the orbit, we can have a clear logical connection with the subsequent sentence.

Since no observable phenomenon can give, of course, any direct information about the origin of planets, we are led to think that the hypothesis, reported by Seneca, that planets might have been originated by an aggregation of many smaller bodies had been formulated on the basis of some theoretical argument. Let us observe that a theory could hardly suggest the possibility of a similar aggregation if it were not based on the idea of a reciprocal attraction between bodies.

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[107] "Hic [Apollonius Myndius] enim ait cometas in numero stellarum errantium poni a Chaldaeis tenerique cursus eorum." (Seneca, *Naturales Quaestiones*, VII, iv, 1). It is worth observing that one of the "Chaldeans" was Seleucus (cf. above, note 94).

[108] "Apollonius Myndius in diversa opinione est. Ait enim cometen non unum ex multis erraticis effici sed multos cometas erraticos esse. Non est, inquit, species falsa nec duarum stellarum confinio ignis extentus, sed proprium sidus cometae est, sicut solis ac lunae. Talis illi forma est, non in rotundum restricta sed procerior et in longum producta. Ceterum non est illi palam cursus: altiora mundi secat et tunc demum apparet cum in imum cursus sui venit". (Seneca, *Naturales quaestiones*, VII, xvii, 1-2). Here and in the previous passage I have used again the translation of Corcoran, with only one slight difference: where Corcoran writes "its orbit is not clear" I have preferred to translate "its orbit is not clearly visible".

Seneca, in another passage, reports again the idea that comets should follow the same kind of orbits as planets [109] and he tries to give several arguments supporting it [110]. The same idea is referred to also by Pliny, who, without attributing the opinion to anybody in particular, states:

> Some [comets] move like planets, some others remain motionless. [111]

We may imagine, perhaps, that the existence of motionless comets might have been an addition by Pliny. The view that comets move like planets, however, seems a widespread opinion in pre-Ptolemaic times, since it can be found, besides Pliny and Seneca, in Manilius, too. Manilius, in his astrological poem (written in the times of Augustus and Tiberius), alludes to three different theories on comets: according to the one most interesting to us, the sun should draw the comets periodically nearer to and farther from itself, in the same way as it does with the planets Mercury and Venus [112].

13. THE *ALMAGEST* AND SOME LATER SOURCES.

One of the main obstacles to accepting the picture of hellenistic astronomy suggested in the previous sections is certainly the distance between such a picture and the *Almagest*.

Ptolemy himself, on the other hand, seems to give some evidence about a break between the previous astronomy and himself, in particular when he tries to prove the immobility of the earth. He states indeed that no astronomical argument could be used in support of his own thesis. On this problem, faced at the beginning of the *Almagest*, Ptolemy, presumably lacking any support in his astronomical sources, has to refer to Aristotle, to which in particular he owes his ideas about space and motion. In a previous paper it was shown that Heron had probably behaved in the same way in beginning his exposition of geometry [113]. Concerning the astronomers referred to and contested by Ptolemy, we have to observe that they did not attribute simply a rotation motion to the earth, but maintained a clearly "relativistic" view. Ptolemy states, indeed, that according to some astronomers the motion could be attributed not only equivalently either to the earth or to the sky, but also to both, provided that both motions are around the same axis and their difference (i.e. the relative motion) is the observed one [114].

The idea of the relativity of the motion (an idea which not only had been clear to Heraclides of Pontus and Aristarchus, but which we have still found echoed by Lucretius and Seneca) seems to be completely extraneous to Ptolemy. He reports the above opinions just as possible astronomical fictions, without any bearing on the "true" physical motion, conceived according to Aristotle's concept of the absolute space. It seems unlikely that such a loss of basic ideas could have happened within a continuous high level scientific tradition as the one from Aristarchus to Hipparchus; it seems much more plausible that we are dealing with the result of a cultural break, like the one that actually happened between Hipparchus and Ptolemy, during centuries in which any true scientific teaching was probably lacking. Note that many other analogous ideas had

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[109] Seneca, *Naturales quaestiones*, VII, xix,2.

[110] Seneca, *Naturales quaestiones*, VII, xxii - xxiv.

[111] "Moventur autem aliae errantium modo, aliae immobiles haerent" (Pliny, *Naturalis Historia*, II, 91).

[112] Manilius, *Astronomica*, I, 867-875.

[113] Cf. [Russo4].

[114] Ptolemy, *Almagest*, I, vii, p. 24.

been forgotten in the same period. People had lost, for instance, the power of creating conventional terminologies [115].

Had Ptolemy actually recovered and improved the previous astronomy, we should infer that a few centuries of interruption could be very useful for the progress of science; unless one discovers the astronomers between Hipparchus and Ptolemy actually filling the gap and explains why Ptolemy ignores them completely.

The main Hipparchus' achievement referred to by Ptolemy is the discovery of the precession of the equinoxes [116]. The precession is so slow that observational data in which Hipparchus could trust had to concern displacements of at most a few degrees [117]. Nevertheless, Hipparchus (whose demand for agreement between theoretical and observational data was in general particularly high [118]) had dared extrapolate from a tiny arc the existence of a uniform circular motion with a period of 26,000 years. Note that to a scientist basing his (heliocentric) astronomy on dynamics, any top [119] could suggest searching observational data for existence and speed of a precession motion.

Let us observe that Hipparchus' treatise on gravity is never mentioned either in the *Almagest* or in any other of Ptolemy's works; we may think that it was probably one of the writings by Hipparchus unknown to Ptolemy.

Ptolemy, referring to the diurnal apparent motion of the fixed stars, reports (obviously contesting it) the bizarre opinion that they should move in a straight line with a constant speed [120]. We may suspect that Ptolemy's source might have referred to the motion supposed by Hipparchus as possible, on the basis of general principles, but whose actual existence he knew could only be verified with the help of posterity.

The "sling argument" reported by Plutarch in the case of the motion of the moon (i.e. the argument of the balance between gravity and centrifugal force) had become popular enough to be referred to by several latin writers. According to our conjecture, the same argument might have been extended also to the case of the motion of the earth and the planets around the sun. In any case we must expect that such kind of arguments were completely misunderstood in late antiquity and in the Middle Ages, when any memory of a "dynamical astronomy" was lost. The Byzantine prelate Eustathius states that, according to some authors, the earth should have the shape of a sling [121]. The authors mentioned by Eustathius are Dionysius Periegetes and, before him, Posidonius. We do not know what actually Posidonius had written on the subject, whereas Dionysius' statement is extant [122]. The idea that the earth should have the shape of a sling seems too strange to be conceived in an independent way by more than one mind;

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115 Herophylus had introduced into medicine a new and conventional terminology, whereas imperial age physicians not only did not follow his example, but were also unable to understand Herophylus' procedure. Caelius Aurelianus, for instance, censures Herophylus for having attributed different meanings to two terms without realizing that they were synonyms (Caelius Aurelianus, *Celeres vel acutae passiones*, I, praef. 4-5, reported in [von Staden], p. 377); Rufus of Ephesus criticizes some terms coined by Alexandrians as introduced by "Egyptian physicians", not acquainted enough with Greek (Rufus of Ephesus, *De nominatione partium hominum*, 151, 1 segg.).

116 Cf. Ptolemy, *Almagest*, III, i, p.192.

117 According to Ptolemy, Hipparchus had observed a displacement of less than 3 degrees (*Almagest*, VII, ii, p.15).

118 Cf., e.g., Ptolemy, *Almagest*, IX, ii, p. 211.

119 Tops were a very popular toy. Cf., e.g., Callimachus' epigram in *Anthologia Graeca*, VII, 89.

120 Ptolemy, *Almagest*, I, iii, p. 11.

121 Eustathius, *In Homeri Iliadem*, vi, 446.

122 Dionysius Periegetes writes that the earth is ὀξυτέρη βεβαυῖα πρὸς ἠελίοιο κελεύθους, σφενδόνῃ εἰοικυῖα (*Description of the earth*, 6-7; [GGM], pp. 104-105).

we may therefore suspect that Dionysius Periegetes had misunderstood the analogy, reported by Posidonius, between earth and sling.

The idea of a particular function of the sun, animating the universe, and the idea of a "cosmic sympathy" among celestial bodies survived for a long time and, associated to the idea of eternal laws, necessarily followed by the planets in their motion, are usually considered of a purely philosophical, more precisely "stoic", origin. Such views are reported, among others, also by Macrobius, who, however, attributes them to Pythagoras [123]. We are dealing with opinions which could well be interpretated as residues of a, no longer understood, "dynamical" astronomy, based on gravitation.

If one investigates the attribution of the above views to the "Stoics" it is easy to discover that it mainly depends on the circumstance that people looking for their origin were able to follow a trail leading to Posidonius (just as we have done in the case of the analogy between earth and sling). Let us note that Posidonius (who was also, as remarked above, the last celestial globe builder we know about) was the head of a school in Rhodes in the beginning of the first century B.C., i.e. not too many years after the end of Hipparchus' activity in the same city; he could hardly have ignored the whole of his astronomy.

Macrobius' mention of Pythagoras is not too surprising; it could have been originated not only by the general tendency of Neopythagoreans to date back to Pythagoras any kind of knowledge but, in particular, also by a confusion of Hipparchus with Hippasus of Metapontum, the Pythagorean who, according to the tradition, had divulged some secrets of the school [124].

14. SOME CONCLUDING REMARKS.

The previous analysis suggests a possible picture of second century B.C. hellenistic astronomy very far from the one usually accepted by science historians: it might have been an astronomy which, far from refusing Aristarchus' heliocentrism, could have also developed it on the basis of a dynamical theory, including the law of inertia and some form of gravitation theory. Even if the above picture may appear very surprising, it is consistent with the general superiority, first of all methodological, of hellenistic science to that of the imperial age. In the case of mathematics such superiority has become evident after the translation of cuneiform texts has shown that the algebraic procedures reported by Heron and Diophantus are not original to these authors, but go back to Mesopotamian mathematics; in many other fields, from propositional logic to medicine, it has been cleared when our information has become sufficient. As far as astronomy is concerned, the scanty and fragmentary extant sources have been always sufficient to date back certainly to the third and second centuries B.C. not only the main results incorporated in Ptolemaic astronomy (such as the idea of epicycles or the precession of

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[123] In particular in his commentary to Cicero's *Somnium Scipionis*.

[124] A fragment of a letter in which the Pythagorean Lysis blames Hippasus for having lectured publicly is preserved by Diogenes Laertius (*Lives of eminent philosophers*, VIII, 42). Following the same pattern, in late antiquity a *letter of Lysis to Hipparchus* was manufactured, a shortened version of which is reported by Iamblichus (*On the Pythagorean Life*, 75-78). Hippasus is named "Hipparchus" also by Clement of Alexandria (*Stromata*, V, 58). A scholium to Plato's *Phaedo* (reported in [Diels] as testimony 1a on Philolaus) relates Hipparchus to Philolaus, as the two only Pythagoreans who escaped Cylon's persecution (so putting Hipparchus in the role attributed by the tradition to Lysis). As a consequence of this confusion Hipparchus was considered a Pythagorean, up to modern times, by many people, for instance by Copernicus, who translated in Latin the letter of Lysis to Hipparchus, taking it as authentic, and reported it in the Book I of the *De revolutionibus...* (the letter was not included in the published version of the work, but Copernicus' translation was included by Koyré in his edition of the *De Revolutionibus...*).

the equinoxes) but also the heliocentric idea on which our "copernican revolution" was based and even the subsequent elimination of the sphere of the fixed stars (a sphere whose existence was still believed in by Kepler).

Some doubts certainly arise from the apparent modernity of the ideas that we have recognized in the examined sources. Is it really possible to find in Lucretius, Seneca and Plutarch, exponents of a civilization often considered far from us, concepts typical of Newtonian physics? Have we perhaps forced our interpretation of the passages, reading them in the light of our modern scientific concepts?

Another natural question arises: if the considered passages really have such an important scientific content, why did they fail to awake so far their due interest?

As a matter of fact, the classical passages considered in the preceding sections, even though certainly undervalued by science historians, attracted great attention from other categories of scholars. Let us consider a few examples, sufficient, in my opinion, to answer both the previous questions.

The first step of Galileo toward the formulation of the inertia law was made in his juvenile writing *De motu*, where Galileo writes:

> [In absence of friction] any body, on a horizontal plane, shall be moved by a least force, and even by a force less than any other force.[125]

A comparison between the above statement and Heron's sentence reported in sect. 6 arouses the suspicion that Galileo had not missed the importance of Heron's writing[126].

While the preceding one is only a suspicion, Kepler's interest in the *De facie quae in orbe lunae apparet* can be easily documented, since Kepler published a commented latin translation of it [127].

The same dialogue was carefully studied by Descartes, as results from a comparison between Plutarch's passage examined in sect. 5 and Descartes' discussion of the motion of a stone linked to a turning sling, contained in chap. 7 of *Le Monde... ou Le traité de la Lumière*. The main passages of the dialogue (i.e. those touching dynamics problems) were later transcribed and annotated by Newton [128]; some of them are also echoed in the published version of the *Principia Philosophiae Naturalis* [129]. Newton, among others, was also very interested in the passages on astronomical subjects of Seneca's *Naturales Quaestiones*. For instance it is evident that Seneca (even if Newton does not mention him explicitly) is the source of the following passage:

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125 "Quae omnia si ita disposita fuerint [i.e. in absence of friction], quodcumque mobile super planum horizonti aequidistans a minima vi movebitur, imo et a vi minori quam quaevis alia vis." (ed. naz. delle opere di Galileo Galilei, vol. I, p. 299).

126 Today we only have an arabic translation of Heron's work, but Carra de Vaux, in the introduction to his edition of the treatise ([Carra de Vaux]), tells us he has found references to Greek manuscripts of the "Mechanics" in several european libraries, in particular in some Roman libraries, in the Biblioteca Marciana in Venice and at El Escorial. The reference in Venice came out to be originated by a mistake (a copy of the *Pneumatics* classified under the title of *Mechanics*), whereas in the other cases it seems that the manuscripts were actually lost. Carra de Vaux emphasizes how difficult it is to follow the trail of lost manuscripts, in particular in the case of Roman libraries.

127 Johannis Kepleri Opera Omnia, ed. Ch. Frisch, Francofurti a.M., 1870, vol. viii.

128 In Newton's *Classical Scholia*, which lasted unpublished till the edition by P. Casini in "Giornale critico della filosofia italiana", 1, LX, 1981, pp. 7-53. Newton's interest in the *De facie quae in orbe lunae apparet* certainly goes back to the very beginning of his scientific career, since the first Newton's scientific work (in 1664) was just about the appearance of the face of the moon.

129 Cf. I. Newton, *Principia Philosophiae Naturalis*, *Definitions*. In particular definition 5 is similar to a passage of the *De facie...* which we have not quoted (*Moralia*, 923 E-F) and the subsequent explanation seems to be derived by Plutarch's passage about the sling.

> Comets, which formerly were included by many among celestial bodies, were considered wandering stars [i.e. planets] by Chaldeans, very expert in astronomical issues, as if they, coming down in a single revolution to the lowest part of strongly eccentric orbits, should become from time to time visible to us. [130]

One can also easily recognize Seneca in this other excerpt, which appears even more significant:

> Hence the earth, the sun and all planets of our solar system, according to the thought of the ancients, are reciprocally heavy and they should fall to each other, because of the reciprocal gravity, being joined in a unique mass, if they were not prevented from falling by circular motions. [131]

It is worth remarking that our interpretation of Seneca's passages, even if different from the one of many classicists (probably afraid of attributing to Seneca "Newtonian" concepts) coincides with Newton's own interpretation. It seems also of some interest Newton's attribution of the gravitation law to Pythagoras [132].

Having used essentially literary writings, we could of course recognize almost exclusively qualitative ideas and the problem of the possible quantitative developments, within second century B.C. astronomy, of the concepts so far examined must be considered open. Any conjecture on this subject should take into account, however, that a purely qualitative theory is generally considered by his author perfectly complete. Hence Seneca's statement that the motion of planets had been only "begun to be understood", a sentence of Pliny giving some evidence for the study of the orbit of Mars having been particularly hard [133] and Hipparchus' opinion that the agreement between theoretical and observational data was unsatisfactory [134] seem all clues pointing to the conjecture that the leading ideas which we have tried to restore in Hipparchus' astronomy had not remained at a merely qualitative stage, but on their basis a quantitative description of the planetary motions had been attempted.

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[130] I. Newton, *De mundi systemate liber*, chap.1. Cf. Seneca's passages about comets reported in sect. 12 (*Naturales Quaestiones*, VII, iv, 1; VII, xvii, 1-2).

[131] "Igitur Terra Sol et Planetae omnes qui in nostro systemate ex mente veterum graves sunt in se mutuo et vi gravitatis mutuae caderent in se invicem & in unam massam coirent nisi descensus ille a motibus circularibus impediretur". (I. Newton, *Classical Scholia*, op. cit., p. 46). Cf. Seneca' passage quoted in sect. 7 (*Naturales Quaestiones*, VII, xxv, 6-7).

[132] I. Newton, *Classical Scholia*, op. cit., pp. 41-42. Newton's justification of this attribution (based, in particular, on some passages in Macrobius) does not seem convincing. The Pythagorean origin of the gravitation law, however, is not Newton's own idea, since Boulliau, who first wrote the law in his *Astronomia philolaica* (Paris, 1645), seems to share the same opinion, as shown even by the title of his book.

[133] "Multa promi amplius circa haec possunt secreta naturae legesque, quibus ipsa serviat, exempli gratia in Martis sidere, cuius est maxime inobservabilis cursus, [...]" (Pliny, *Naturalis Historia*, II, 77). As is well known, Kepler, too, had particular troubles concerning Mars' orbit.

[134] Ptolemy, *Almagest*, IX, ii, p. 210.

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