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THE PHILOSOPHY OF HELIOCENTRISM IN PRE-NEWTONIAN ENGLISH SCIENCE

I. INDECISION

There exists a constant danger on the part of historians to read history backwards. For instance, since the time of Newton many modern thinkers have found it almost impossible to imagine how any honest intellectual coming to maturity after Copernicus and before Newton could doubt the truth of heliocentrism. However, as late as the 1670's it was in fact quite possible for a reasonable and educated person to still maintain the geocentric view. Around 1670 in England such a person would most likely have been a basically religious man reared on Scripture, familiar with at least the main conclusions of outstanding Dutch, Italian, and French savants, read in the poetry of Donne and Milton, and aware of the new currents in the methodology of natural philosophy. He would have realized that the acceptance or rejection of heliocentrism was not strictly a religious issue. Both those who accepted it (e.g., Digges, Gilbert, Kepler, Galileo, Descartes, Boyle, Hooke, an even Hobbes in his own way) and those who rejected it (e.g., Tycho Brahe, Bacon, and Isaac Barrow), despite whatever personal problems they may have had, remained staunchly devoted to their religious faiths. He may even have known that this was certainly true of Copernicus himself. Rather than religion, the key factor was the lack or presence of certain scientific evidence according to the individual's interpretation of exactly what was required in order to have a dependable piece of knowledge in natural philosophy.

This same person, listening to a non-Scriptual debate between those embracing the newer heliocentric view and representatives of that "immense body of conservatives" [1] endorsing the geocentric view, would

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[1] T. S. Kuhn, *The Copernican Revolution,* (New York, 1959), p. 205.

have heard several good arguments on each side. On the Copernican side he would have heard that the newer arrangement would make it very easy to explain the appearance of the retrograde motions of planets, that it could explain the apparent variations in the relative distances of Mercury, Venus, and Mars as seen from the earth,[2] that it might well account for the phenomenon of tides on earth,[3] that the ancient dichotomy between the corruptible area of the universe below the sphere of the moon and the incorruptible, unchanging (except for circular local motion) area above the sphere of the moon had been destroyed by Galileo's observations, that the universe was not a closed sphere demanding a heavy, fixed center but was perhaps infinite in extent with no privileged points,[4] that it was quite possible that some sort of Empedoclean, animate force pervaded the universe giving bodies in relatively close proximity a certain kinship with each other,[5] that it was natural and proper for a sphere to rotate and move around the sun, that the famous mathematician Christiaan Huyghens had recently (around 1663) shown that a rotating earth must needs be many times larger in radius than it actually is in order to develop enough centrifugal force to throw off a body resting on its surface,[6] and that the great Kepler believed in heliocentrism. He might also have heard it claimed that the Copernican system was "simpler" that the Ptolemaic or Tyconic.[7]

From the anti-Copernican camp he would have heard equally convincing arguments and rebuttals. In the first place, all arguments based upon inherent qualities of nature, such as kinship, sun force, the natural properties of spherical heavenly bodies, etc., must be rejected as being

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[2] See *On the Revolutions of the Heavenly Spheres* (tr. by C. G. Wallis), *Great Books of the Western World*, (Chicago, 1952), Vol. 16, Book I, chps. 9, 10.

[3] Galileo asserted that no cause other than the combination of the shape of the sea basins and the two motions of the earth was required to explain the tides. See *Dialogue on the Great World Systems* (de Santillana ed., U. of Chicago Press, 1964), Fourth Day. He rejected as being fanciful the notion that the moon could be the prime physical cause of tides. In contrast, and indicative of the paradoxes of the Sidereal Philosophy of the era, Hobbes was happy to call upon the moon for tides as well as upon sun power in order to explain the motions of the planets. See his *Concerning Body* (1655), Part 4, chp. 26, "Of the World and of the Stars".

[4] See *Revolutions, op. cit.*, pp. 516 ff. See also F. Grant, "Medieval and Seventeenth-Century Conceptions of an Infinite Void Space beyond the Cosmos", *Isis*, Vol. 60, (1969), pp. 39-60.

[5] See *Revolutions, op. cit.*, p. 519.

[6] Cf. R. S. Westfall, *Force in Newton's Physics*, (New York, 1971), pp. 171-2: "Once he had proceeded this far, Huygens inevitably applied his conclusion to the problem set by Copernican astronomy — if the earth turns on its axis, why are bodies not thrown off its surface? ... Huygens was now in a position definitively to solve it. ... Meanwhile, the formula for centrifugal force, even when applied to inaccurate data, had effectively demolished one objection to the heliocentric system — if indeed the objection was ever taken seriously by anyone except unregenerate geocentrists". Westfall greatly underestimates the rationality of those opposed to the heliocentric theory (not a proven system). The "unregenerate geocentrists" were precisely the people who never did take that particular objection seriously.

[7] See F. F. Centore, "Copernicus, Hooke and Simplicity", *Philosophical Studies*, Vol. 17, (1968), pp. 185-196.

equally capable of being used to support the geocentric view or as being out of step with the mechanistic philosophy of nature espoused by the atomists and Cartesians, i.e., with the world-view of most of the 17th c. Copernicans themselves. Any argument based upon an appeal to famous natural philosophers or mathematicians also had to be rejected since equally famous virtuosi could be cited in the other camp. As a variation on this last point it could have been argued that even though Kepler may have personally followed Copernicus there is no conclusive reason why Kepler's data could not be incorporated into a geocentric view simplified by the removal of epicyles.

The ancient arguments of Ptolemy himself were also omnipresent. Everyone in the mid-17th c. familiar with the debate knew how the great Alexandrian astronomer, largely following the case as outlined by Aristotle centuries before, was forced by his senses to restrain his mathematician's urge to treat the real bodies in the universe as merely mathematical points that could be moved about at will. Ptolemy reasoned that, although it would be much simpler to suppose that the earth turned on its axis once a day instead of the entire heavens, such a postulate could not be accepted as factually true since it contradicted certain overpowering sense experiences.[8] Although items actually in contact with the earth might continue to adhere to the surface even supposing that the earth rotated, it did not seem possible to imagine why a body which had broken all contact with the earth, such as one thrown high into the air, would still fall back to earth very near the stop at which contact was broken. If the large earth, the approximate size of which was well--known at the time, were imagined to turn once a day it must be moving very swiftly and thus should quickly move out from underneath anything disconnected from its surface thus leaving such things, e.g., an arrow shot straight up or clouds floating in the sky, far behind as it raced on its way. But could not the air be a sticky substance holding things in place once placed there? In that case, answered Ptolemy, nothing would ever come down at all! Not once had any of these occurrences been empirically verified.[9]

[8] See *The Almagest* (tr. by R. Taliaferro), *Great Books of the Western World*, (Chicago, 1952), Vol. 16, pp. 9 ff.

[9] In this regard Ptolemy spoke for the empiricists of all ages. Cf. Bacon's *New Organon*, II, 36: "... let it be examined whether any such motion be found in nature, or it be rather a fiction and hypothesis for abridging and facilitating calculation, and for promoting that fine notion of effecting the heavenly motions by perfect circles; ... And it is most certain, if we consider ourselves for a moment as part of the vulgar (setting aside the fictions of astronomers and the school, who are wont undeservedly to attack the senses in many respects, and to affect obscurity), that the apparent motion is such as we have said, a model of which we have sometimes caused to be represented by wires in a sort of machine". Unlike Bacon, however, who was no mathematician, Ptolemy could and did work out a geocentric theory mathematically.

Throughout this period several attempts were made to show experimentally that bodies do not fall back to the same place. All of them, however, were inconclusive.

With respect to the earth's yearly motion, again something Ptolemy was quite willing to consider rationally and calmly, there was also something that should be observable but which was not observed. If one holds up his finger about a foot in front of his nose and looks at it, first with one eye closed and then with the other eye closed, the finger will appear to move. If the earth moved in a great ring around the sun, should one not observe the same type of thing happening in the case of a star viewed from the earth six months apart? Geocentrists and heliocentrists alike knew that they should. They also knew that they in fact did not observe any parallactic angle for any fixed star. What does one do in the face of this lack of evidence for a necessary consequence of the heliocentric theory? Rather than being only "far simpler", as Cohen remarks, [10] to discard the Copernican hypothesis would it not be the only *scientific* thing to do? Consequently, there were still many good minds in the 17th c. attempting to explain the celestial phenomena based upon the ancient Ptolemaic model or upon some modified model such as that of Brahe.

The actual model used by Ptolemy is so well-known that it need not be repeated. For our purposes we need only emphasize that, true to the canons of ancient Greek thought, all motions, as the canons were interpreted by Ptolemy, must be reducible to combinations of perfect circles. In the perfect heavens above the moon this was the only possible kind of path for a perfect celestial body. However, in the process of setting up these motions, Ptolemy allowed into his theory two possibilities which were highly repugnant to any true Greek astronomer. It is often heard that Copernicus was a conservative in many ways. It is often overlooked, though, that in fact he was *more* conservative than Ptolemy himself.[11] Ptolemy was willing to allow that the actual, physical paths followed by the bodies circumventing the earth might not be perfect circles and that the timing of a body's period of revolution might not be uniform with respect to its actual center of revolution but with respect to another point, the *punctum equans*. Copernicus sought to do away with both of these deviations as far as it was possible.

II. SEEING AND BELIEVING

N. R. Hanson has stated that "Tycho and Simplicius see a mobile sun, Kepler and Galileo see a static sun." [12] "For Galileo and Kepler the

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[10] I. B. Cohen, *The Birth of a New Physics*, (Garden City, New York, 1960), p. 60.

[11] One of the few who has clearly recognized this point is E. J. Dijksterhuis, *The Mechanization of the World Picture* (tr. by C. Dikshoorn, Oxford U. Press, 1961), pp. 288-9.

[12] N. R. Hanson, *Patterns of Discovery*, (Cambridge U. Press, 1958), p. 17.

horizon drops; for Simplicius and Tycho the sun rises. This is the difference Price misses, and which is central to this essay." [13] If it is really necessary that Tycho and Galileo actually "see" different phenomena, not in the sense of understanding different things to occur but in the sense of witnessing different happenings, then the central thesis of Hanson's work is lost when it comes to the science of astronomy. Copernicans of the 17th c. no more saw the earth's horizon decline at dawn than we do today. Even granting subjective differences among individual observers, the over-all impression is the same. The reason is that, although what Hanson claims about gestalt impressions may be true on a small scale (he gives several well-known examples taken from gestalt psychology), it is not true on the large scale. Hanson criticized H. H. Price for maintaining that everyone, geocentrists and heliocentrists alike, see the sun move. Instead, Hanson insisted, it could just as well be the other way around.[14] Hanson, however, seems to have missed an important point, namely, the movement seen is always relative to what is most fixed and stationary as a background condition. This would be the surface of the earth and the clouds in the sky in the present case.

Copernicus would be the first to agree. In his own Preface to the *Revolutionibus* he informs us concerning the main alternatives in astronomy at the time. The pre-Ptolemaic concentric spheres theory must be rejected outright since it greatly disagrees with the observed facts. The Ptolemaic system, on the other hand, was not really that far off with respect to what had been observed and what could be predicted. It did, however, by deviating from the ancient principles which required that the heavenly movements of the celestial spheres be both circular and of *uniform* velocity along their paths with respect to the same center points, lead to a kind of mixed system in which at one time the ancient conditions were adhered to and in which at other times it seemed that other principles were being followed. Such inconsistencies troubled Copernicus a great deal.

Not only did he accept the ancient Pythagorean norms as the *only* principles upon which to build his astronomy but he also defended them. In Book 1, chapter 4 he explains why the principles must be physically true. In order to keep returning to their observed positions over and over again the paths of such bodies must be closed figures. In the area above the moon such figures could only be circles. What, now, must be the timing of such motions? Is there anything that would cause an irregularity? Copernicus could not find any. He could imagine no change

[13] *Ibid.*, p. 182, note 6.

[14] Hanson remained committed to his thesis to the end of his life. He also perpetuated the historically erroneous notion that Copernicus himself accepted heliocentrism because it was simpler than the geocentric view. See *Perception and Discovery* (ed. by W. C. Humphreys, San Francisco, 1969), chp. 14.

in the natural forces, if any, operating in the heavens. Neither could he believe that God would alter His creation after so many centuries of uniformity. Neither were the celestial bodies themselves about to alter their own shapes or to be so altered by God. What, then, justified Ptolemy's deviations? Nothing but a misguided empiricism unworthy of a mathematician. Copernicus found such an attitude abhorrent.

In answer to the empiricist's objections Copernicus called upon the natural affinity of the moon, other bodies separated from the earth's surface, and the air itself for the earth.[15] The problem concerning parallax was solved by claiming that indeed there was such an angle but that the nearest stars were so far away that the angle was too small to be observed from earth even by the best methods of observation. One sees how Copernicus was turning experience inside out. Rather than using experience to develop a theory he was using a theory to develop another theory (natural kinship) and to predict an experience which in fact could not be experienced. That his theory was rejected by many at the time should not seem surprising.

III. A CRUCIAL EXPERIMENT

In 1670 Robert Hooke (1635–1703) was one of the most respected figures in European natural philosophy. His opinion was valued by non-English as well as English savants. From humble beginnings, he went to Oxford where he made the acquaintance of Boyle and from where he later moved to London to become the first professional scientist in the Royal Society. In 1665, with the appearance of his *Micrographia*, a large collection of carefully reported observations and scientific theories on various subjects, his reputation was made. From then until his death, even after the publication of Newton's *Principia*, he maintained a high reputation.[16]

Hooke was a Copernican, even though his English mentor in scientific methodology, Bacon, was not. As a leader of his age who had his finger on the pulse of natural philosophy in the 17th c., and yet as someone who tended toward inductive empiricism rather than mathematics, one wonders why.

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15 Copernicus also put heavy emphasis upon the "naturalness" of motion to a sphere. Therefore, if one should expand upon the standard empirical objections by saying something like a rotating earth would throw off its outer surface and thus disperse itself into the air, it could be argued that such would not happen because the natural motion would not give rise to a violent tearing apart. Again, we have the truth of one theory (heliocentrism) being assumed in order to prove another theory (all spherical bodies must move as units).

16 For sympathetic treatments of Hooke's life and work see M. 'Espinasse, *Robert Hooke*, (London, 1956) and F. F. Centore, *Robert Hooke's Contributions to Mechanics*, (The Hague, 1970). Despite his central importance in English science in the 1670's it is not unusual to find some authors, even today, e.g., Westfall in his *Force in Newton's Physics*, giving Hooke rather short shrift.

In the recent past, M. Bunge has argued that any one of the half-dozen or so different kinds of simplicity cannot be counted as an important factor or weight in the acceptance of scientific theories. Taking the acceptance of the Copernican system as one case history, he has argued that the real reason why the heliocentric view was adopted was not due to its relative simplicity but due to its giving a truer image of the facts. [17] Although some of his points are well-taken, e.g., that simplification is not necessarily the same as clarification, that simplicity for the sake of simplicity can be very misleading, etc., his point concerning the actual historical situation with respect to the Copernican hypothesis must be disputed.

In 1670 Hooke composed and read to the Royal Society a work called "An Attempt to Prove the Annual Motion of the Earth." [18] His "attempt" was based upon a series of observations made between July 1669 and October 1669 inclusive in which, by use of what he claimed to be a highly improved telescope which could detect differences of up to one minute of arc, he discovered an angle of parallax for a bright star in the head of the Dragon. The angle he claimed to find, allowing for a margin of error, was between 27 and 30 seconds of arc. Needless to say, such an angle, as we know today, is far beyond the range of anything possible.

The important thing today, though, about Hooke's paper is not what he actually accomplished but the way he went about trying to do it and what it tells us about his era. He tells us, first of all, that as a Copernican he belongs to a select minority. Secondly, even though he and others are convinced Copernicans for many "plausible" reasons (which he does not state), what is needed is a definitive experiment to dispel the indecision. There is only one such experiment. Without it the indecision remains. With it, the objections of Brahe, Riccioli, and all other geocentrists will be destroyed. [19]

The plausible reasons referred to were undoubtedly the circumstantial evidences and corollary theories put forward by Copernicans such as Galileo. It seems, though, that there was something else at work in the mind of Hooke and those for whom he spoke. In a later work Hooke revealed what it was. In his "A Discourse of the Nature of Comets. Read at the Meetings of the Royal Society, soon after Michaelmas 1682" [20]

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[17] See his "The Weight of Simplicity in the Construction and Assaying of Scientific Theories", *Philosophy of Science,* Vol. 28, (April 1961), pp. 120-49, especially pp. 138-140. See also his *The Myth of Simplicity,* (Englewood Cliffs, N. J., 1963).

[18] Reprinted in R. W. T. Gunther, *Early Science in Oxford* (14 vols., Oxford, 1920-45), Vol. 8, Lecture I, pp. 1-28. His "attempt" was not published until 1674.

[19] The strength of Hooke's own convictions is indicated by his statement: "Though yet I confess had I fail'd in discovering a Parallax this way, as to my own thoughts and persuasion, the almost infinite extension of the Universe had not to me seem'd altogether so great an absurdity to be believed as the Generality do esteem it". (*Ibid.,* p. 6.)

[20] Contained in *The Posthumous Works of Robert Hooke* (ed. by R. Waller, London, 1705), pp. 149 (misnumbered as 194) and following.

Hooke had occasion to digress on several subjects other than comets. In the process of doing so he stated for the reader in a concise way what it was that made him and others into convinced Copernicans.

What are comets? Hooke answers that they must be explained by natural causes. And the most likely natural explanation is that they are burning material objects flying through the aether. He arrives at this conclusion by rejecting all other fanciful explanations and by supplying positive evidence of his own, the best of which is an analogy with a little combustible ball suspended by a wire and made to imitate the appearance of a comet. The effects, he claims, are due to fire and the gravitation of the air to the earth and so by analogy comets are due to fire and the gravitation of the aether to the sun. But, he admits, all similar things are not the same. This argument, however, he reasons further, could be used against any theory. In the case of celestial mechanics, for instance, if one merely wanted to list possibilities, all sorts of combinations of motions are possible. But which should be accepted as true? Every astronomer seems to have his own view, each one more confused than the other. The wise astronomer, though, cuts through this confusion, "Which is the reason why the Copernican has obtained with all the modern and best Astronomers, against all the other, as being the most simple, and the least incumber'd of any; especially as it is improved by the incomparable Kepler. All the Reason of which is from this Maxim, that *Natura nihil egit frustra, sed frustra fit per plura quod fieri potest per pauciora.*" [21]

When defending Copernicus in 1682, why does he not call directly upon his 1669 observations? At a later point in the Discourse, while discussing gravitation, be states that he has already shown that the earth moves around the sun. [22] This makes earth one of the celestial bodies and so, what is true about gravity on earth, may be extended by analogy to the other celestial bodies. Again, though, his parallax observations are not directly called upon. This indicates, I think, that Hooke himself did not put complete faith in the quality of his own eyes and instruments. [23] In the end, it seems, a non-empirical principle of discrimination among theories exercised the greatest influence upon him. It was a principle of simplicity that made it obvious to him that celestial motions did not require the paraphernalia of the older astronomers.

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[21] *Ibid.*, p. 167.

[22] See *ibid.*, p. 180.

[23] It is hard to judge what others may have thought about his work. In April of 1674 a letter from Hevelius was read to the Society saying that the French could find no parallax for the sun. The implication may have been that *a fortiori* one could not be found for a star. Even Newton, in the first and subsequent editions of his *Principia,* as part of his Hypothesis IV (1st ed.) or I (2nd and 3rd eds.), states that some still contend that the immobile center of the universe is the earth. How could this be if Hooke had experimentally disproven geocentrism for all times? See A. Koyré, *Newtonian Studies,* (Harvard U. Press, 1965), chp. 6.

IV. SIMPLICITY

What did simplicity mean for Hooke? It seems that what the Curator had in mind was the degree of physical complexity of one physical system relative to another one. Today, it would be something along the lines of saying that the gas turbine engine is simpler than the conventional kind because it has fewer parts. If such a machine can get the job done, Hooke would say, it is to be preferred to the other. Hooke did in fact reason this way with respect to several mechanical devices and techniques on which he himself worked. In the world of man-made artifacts and procedures, simplicity was a great virtue.[24]

Could this not be worked in the other direction? Is not man imitating God, or at least using a God-given power, as Bacon had taught, when he creates a new device or technique? Hooke reasoned that if simplicity works well for man it must be because God created nature so as not to do with more what could be done with less. When looking at the great machine of nature, then, it was safe to assume that it was designed according to a simple model.

In the same Discourse referred to above[25] Hoose gives his approach the status of a principle of inquiry. As part of his discussion of comets he comes to discuss at length the effects of gravitation on comets and then to discuss gravitation in general. He boils down the views of others in order to get out of them what is good before going on to summarize his own observations. Only then does he feel safe in stating his own conclusion concerning the cause of gravitation. Before giving his own data, though, he wants to emphasize something about his own procedure. He wants people to know that his proposed explanations are founded upon the phenomena of nature rather than being conjured up at random or by chance. Shortly thereafter he explicitly states his rules for doing the philosophy of nature.

In the first place, he supposes nothing that is obviously absurd or that cannot be supported by direct experience or by analogies based upon experiences. Secondly, since nature does not work in ways more complicated than necessary, he also will not work in vain by multiplying natural causes unnecessarily. In the third place, since a uniformity in natural processes must be supposed in order to make generalizations, he affirms that where similar effects are observed one can assume similar causes. Fourthly, he is careful to look for contrary evidence and when none is found one sees that his explanatory principles can be found

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[24] E.g., in May 1673 Hooke read a paper on arithmetical instruments in which he criticized Leibniz's calculating machine as being too cumbersome. Hooke's own was much smaller with only 1/10 the parts and therefore better. On another occasion, in July 1683, Hooke stated flatly that the simpler a machine is the better. See Gunther, *op. cit.*, Vol. 7 under dates mentioned.

[25] *Ibid.*, pp. 178-9.

operating in all bodies to one degree or another. Finally, he asserts, all natural phenomena can be adequately explained mechanistically by only two simple factors: matter (which includes the aether) and local motion.

Except for the second and third rules, Hooke's presentation is not especially clear and polished. In this he prefigures Newton's haphazard arrangement of "hypotheses" in the first edition of the *Principia*. Newton, however, went on to carefully redo his rules until, by the third edition, he had them in fairly good order.[26] It does not seem, though, that Hooke ever attempted doing the same type of thing. With respect to the heliocentric theory, however, what the Curator had at the very beginning was sufficient. What is nature really like in terms of its basic principles? Aristotle saw no reason to defend a countless number of principles (the atoms) in conjunction with a logical absurdity (the void) when the two principles of matter and form would suffice. Hooke thought in the same fashion. His simplification was to adopt the Cartesian dichotomy between extension and thought. In conjunction with local motion, extension (matter, space) would explain all natural events.[27] The same approach was also applied to the parts of nature, especially the large, overpowering, all-encompassing sweep of the universe itself. Copernicus had done away with equants. The "Incomparable Kepler" had done away with epicycles. Hooke, as well as the many for whom be claimed to speak, could not imagine any rational natural philosopher not applauding these moves in the right direction.

Copernicus was primarily a mathematician loyal to Pythagorean norms. This, however, did not prevent non-mathematicians from accepting what he said for reasons other than his. Aristotle cared little for Pythagoras and his school. The few times he does mention him it is never with approval. The reason is that Pythagoras attempted the foolish move of trying to make numbers into efficient causes. Even though the Copernican revolution was not one in cosmology it was a revolution with respect to the influence of mathematics in natural philosophy. Even though mathematics could not get at real physical causes it could describe in such a way as to give insights into structures and the motions of the

[26] Where did Newton obtain his *regulae philosophandi*? His commentators tend to treat them as entirely original to him. Could he have borrowed them from Hooke? From the first edition of the *Principia* onward Newton's first and second rules correspond in meaning with Hooke's second and third. With some rearrangement the others that Newton finally arrived at can be found in Hooke's first and fourth. Also, when Newton contemplated adding a fifth rule opposing the Cartesian Idealistic epistemology which had led to the aether, matter-mind dichotomy, etc., Hooke's fifth assertion may have been as much on his mind as the thought of his Continental Cartesian philosophical critics. See Koyré's work cited above in note 23.

[27] The great ontological difference between the two simple doctrines of hylomorphism and mechanism (whether with or without the void) gives credence to Bunge's critique of the dependability of simplicity as a principle of discrimination if the critique is taken as a statement of what *should* be the case rather than what was (and is) in fact the case.

parts of nature. The major dividing line between the Scholastic philosophy of nature and modern science is not to be found in the recognition of the value of sense knowledge nor in the postulation of unusual cosmological schemes. Neither is it in the failure of the older tradition to recognize that the philosophy of nature operates on the universe at a level different from that of mathematics. If all the fathers of modern science had done was to urge atomism against hylomorphism there would have been no advance beyond the Greeks. Rather, it is in their respective attitudes toward mathematics.

Copernicus was a message emanating from the Renaissance telling those who could hear that mathematics could be *practical.* [28] The massage was readily accepted by Galileo and Descartes; not so readily by Boyle and Hooke; not at all by Bacon. The differences appear to be due to the extent to which individual thinkers were willing to augment their senses by the use of quantified qualities which could be imaginatively rearranged with respect to the broader aspects of natural phenomena. Some more than others, therefore, tended to put their faith in ideal situations, mathematical formulas, and over-riding principles of investigation, especially that of causal, physical simplicity. The acceptance of the heliocentric theory by several notable 17th c. English natural philosophers is one example of this phenomenon.

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[28] *Mutatis mutandis,* a concluding statement by F. R. Johnson could well apply here: "Certainly, therefore, we must look to the sixteenth century, not the seventeenth, for the genesis of the first clear formulation of those ideas that have ever since been intimately associated with the development of modern science". *Astronomical Thought in Renaissance England,* (New York, 1968), p. 299. See also F. A. Yates, *Giordano Bruno and the Hermetic Tradition,* (London, 1964), on the neo-Platonic aspects of early Copernicanism.