THE SIGNIFICANCE OF THE EROSION OF THE PROHIBITION AGAINST METABASIS TO THE SUCCESS AND LEGACY OF THE COPERNICAN REVOLUTION

Jason Aleksander
Department of Philosophy
Saint Xavier University, Illinois, USA
aleksander@sxu.edu

Abstract. *Although one would not wish to classify Copernicus' own intentions as belonging to the late-medieval and Renaissance tradition of nominalist philosophy, if we are to turn our consideration to what was responsible for the eventual success of the Copernican Revolution, we must also attend to other features of the dialectical context in relation to which the views of Copernicus and his followers were articulated, interpreted, and evaluated. Accordingly, this paper discusses the significance of the erosion of the Aristotelian prohibition against metabasis to the eventual success of the Copernican Revolution.*

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I would like to begin with a brief consideration of Copernicus' reasons for rejecting Ptolemaic astronomy. Copernicus writes in his dedicatory letter to Pope Paul III of the *De revolutionibus orbium caelestium*:

I was impelled to consider a different system of calculating the motions of the universe's spheres for no other reason than the realization that astronomers [*mathematici*] do not agree among themselves in their investigations of this subject. For, in the first place, they are so uncertain about the motion of the sun and moon that they cannot demonstrate and observe a constant magnitude even for the tropical year. Then, in establishing the motions not only of these bodies but also of the other five planets [literally: "wandering stars"], they do not use the same principles, assumptions, and explanations [*demonstrationes*] of the apparent revolutions and motions.… On the contrary, their experience was just like someone taking from various places hands, feet, a head, and other pieces, very well depicted, but not by comparison with the depiction of a single body; since these fragments would not answer each other in turn, a monster rather than a man would be put together from them. (p. 4)[1]

[1] Unless otherwise noted, all translations of *De revolutionibus orbium caelestium* are those of Edward Rosen in *Nicholas Copernicus on the Revolutions*. I will cite page numbers from the edition listed in the refrences.

This statement succinctly raises many of the most significant points on which Copernicus challenged the Ptolemaic astronomy. But above all, the statement shows that Copernicus' main objection to the Ptolemaic system was that it was monstrous, that it was neither an elegant nor unified system. This may appear to be merely an aesthetic consideration. However, in the following paragraph, Copernicus reveals a cosmo-theological principle at stake in the consideration:

I began to be annoyed that the movements of the world machine, *created for our sake* by the best and *most systematic Artisan* of all, were not understood with greater certainty by the philosophers, who otherwise examined so precisely the most insignificant trifles of this world. (p. 4, my emphasis)

Taking these two quotes seriously warrants a claim that may seem surprising in light of how the Copernican Revolution is commonly understood. My claim is this: the legacy of the Copernican Revolution cannot be reduced to the question of the confirmation of the heliocentric hypothesis; nor was heliocentrism what was most importantly revolutionary in Copernicus' cosmology; nor was empirical confirmation of heliocentrism what explains the initial success of the Copernican Revolution. Instead, the success of the Copernican Revolution seems to me to have been largely dependent upon other, more broadly significant scientific motivations, and understanding these underlying motivations is likely to be important to enhancing our understanding of both the success and legacy of the Copernican Revolution.

Even if we limit ourselves for the moment to a consideration of Copernicus' own stated intentions, it should be clear why there is more at stake in the Copernican Revolution than the success of the heliocentric hypothesis. For although it is true that Copernicus rejected the Ptolemaic astronomical techniques for calculating the apparent positions of heavenly objects, he explicitly claims that he did not do so for the same reasons that he rejected geocentrism as a cosmological doctrine. On purely astronomical grounds, Copernicus grants that "those who devised the eccentrics [e.g., Ptolemy] seem thereby in large measure to have solved the problem of the apparent motions with appropriate calculations" (p. 4). Thus, while the Ptolemaic astronomy was, from the standpoint of any sixteenth century astronomer, *at least* as well suited for saving the phenomena, Copernicus rejected it for two main reasons: first, because it involved a "monstrous" set of mathematical techniques; second, because it was inconsistent with cosmological principles held as necessary on *non-astronomical* grounds.

These two objections are importantly connected for Copernicus, but it is the second objection that will be the focus of my remarks below. With respect to this second point, what Copernicus specifically objected to was that, by employing the methodological device of the equant, the Ptolemaic astronomy "introduced a good many ideas which apparently contradict the first principles of uniform motion" (p. 4). In short, Copernicus rejected Ptolemaic *astronomical techniques* on the grounds that these techniques suggested inconsistencies with an even older and more entrenched cosmological principle of uniform, circular motion, a cosmological principle that Copernicus believed to be beyond doubt.[2] But this had nothing to

[2] Because Copernicus accepted the ancient principle of circle motion where Kepler did not, there may be something to Norwood Russell Hanson's view that Kepler is the greater innovator and more responsible for the developments of the scientific revolution: "The great difference which divides the work of Copernicus and that of Kepler is that the former never questioned, while the latter ultimately rejected, the hypothesis of circular celestial motion…. Copernicus might well be viewed as the last of the great orthodox planetary theorists, orthodox in that he never transgressed this principle.

do with Copernicus' reason for rejecting the cosmological doctrine of geocentrism. Rather, Copernicus rejected the *cosmological* doctrine of geocentrism because, although it allowed uniform motion, it could not provide a *unified and elegant* system of astronomical calculation that allowed adequate predictions of the motions of all the planets about the earth.

Copernicus wasn't the first thinker to see that the geocentric cosmology and what are called "Ptolemaic techniques" were to some degree incompatible—in fact, pretty much every competent astronomer since even prior to Ptolemy understood that this was the case. Moreover, as Bernard Goldstein explains, Muslim astronomers, beginning in the thirteenth century were able to produce many astronomical techniques that did not require the use of an equant for predictive purposes but which were also fully consistent with a geocentric cosmology. Thus, "In other words, the equant was an astronomical problem whose solution did not impinge on cosmological issues" (Goldstein, 2002, p. 220). However, Copernicus was clearly, in his own words, "annoyed" by the incompatibility between astronomy and cosmology and is also, in any case, the person whom the history of European science rightly or wrongly credits as the first to be successful (more or less) in the attempt to resolve this problem.

Consequently, in terms of what it reveals about both the success and the legacy of the Copernican Revolution, what I believe is in fact so historically significant about the Copernican hypothesis was its insistence that astronomy conform to cosmology—that is, what may be most significant about the Copernican hypothesis is its insistence that astronomy conform to the branch of physics that was understood to pertain to the motions of supralunar objects. Or, to put it precisely, Copernicus believed that the superiority of his astronomical system derived from the fact that, unlike prior astronomies (Ptolemaic or otherwise) that were merely sufficient for saving the phenomena, his system was a true description of the *real*, systematic order of the *cosmos*. Thus he writes:

We regard it as a certainty that the earth, enclosed between poles, is bounded by a spherical surface. Why then do we still hesitate to grant it the motion appropriate by nature to its form rather than attribute a movement to the entire universe, whose limit is unknown and unknowable? Why should we not admit, with regard to the daily rotation, that the appearance is in the heavens and the reality [*veritas*] in the earth? (1.8, p. 16)

At this point, then, I have made a claim that may seem surprising for the wrong reason. I have said that what is revolutionary about the Copernican hypothesis is not its

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He was heterodox, of course, within the framework of planetary thinking based on the principle, but this is not at issue. Kepler broke this conceptual commitment of orthodox astronomy and broke out of the framework of orthodox planetary thinking. He is much more than simply the mathematician who put the detailed arguments into Copernicus' theory. He did that; but he did very much more. It may even be didactically beneficial to suggest that the intellectual flight between Copernicus and Kepler is many times more steep and vertical than that between Ptolemy and Copernicus, and more steep also (perhaps) even than the flight between Kepler and Newton himself" (Hanson, 1961, p. 184). In my opinion, there is little sense in evaluating Kepler and Copernicus in these terms. Whatever innovations are to be credited to Kepler, the fact is that the question of which thinker was more tenacious or imaginative is unimportant with respect to the question of the significance of their impact in the history of science and philosophy. Indeed, in this sense, the significance of the individual contributions of each thinker may only be properly appreciated when their contributions are understood in relation to the dialectical context in relation to which they articulated their ideas and in relation to which these ideas were assimilated. And in this regard, let it be left at this: the individual contributions of both thinkers are enormously significant markers of the same basic and radical changes in the intellectual habits pertinent to both philosophy and science.

explicit and well-known heliocentric hypothesis but is, instead, that it asserted both that astronomy be a *true* description of the motions of the planets and that it is therefore possible to unify cosmology and astronomy under the same generic science. I am aware, however, that the only thing that might seem surprising about saying this is that I have insisted that it *should* be surprising. In other words, one might rightly wonder why I think this is a surprise. From a retrospective understanding of the history of early modern science, it may indeed seem not only inevitable but also self-evident that astronomy should tell the truth and that it should be *consistent* with any cosmology that also asserts the truth about a description of celestial motion.

Taking my cue from what I have said above about Copernicus' own awareness of the radicality of his hypothesis, in what follows I will offer a rationale for pursuing an examination of three main questions. First, to what degree is the revolutionary character of Copernicus' hypothesis a function of its integration of cosmology and astronomy? Second, to what degree did the success of this integration depend upon a prior erosion of the force of a prohibition against the integration of astronomy and cosmology? And third, in what ways might attending to the historical erosion of this prohibition reveal significant insights about the formation of modern science.

I will begin again, then, by discussing what some have called Aristotle's prohibition against *metabasis* and its relevance to the injunction against the integration of astronomy and cosmology.[3] In general, as Aristotle explains in *Posterior Analytics* A.7, because different sciences belong to different genera, one cannot use the principles or techniques of one science in demonstrations within another science.[4] What this means, then, is that the principles, demonstrations, or techniques pertinent to the science of arithmetic, for instance, simply cannot be applied to the science of physics. And from this also likely stems Aristotle's avowed conviction that demanding of any science whatsoever the precision provided by mathematical statements would be not merely unscientific but also perhaps imprudent.[5]

There are three ways in which this prohibition of *metabasis* is relevant to the success and legacy of the Copernican Revolution. First, the general prohibition against *metabasis* suggests that the actual motions of sensible substances are not entirely amenable to mathematical (either arithmetical or geometric) description, for, according to Aristotle, "arithmetic and geometry are not concerned with any substances" (*Metaphysics*, Λ.8, 1073*b*7-8), and those who posit "that physical bodies are composed of numbers and…that things

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[3] See especially the following works: Funkenstin, 1986; Livesey, 1982, 1985 and 1986 and O'Connor, 2006 and 2008.

[4] "It is not possible to prove something in one genus by passing over from another genus; for example, one cannot demonstrate a geometrical theorem by arithmetic" (*Posterior Analytics*, A.7, 75a38).

[5] "The accuracy which exists in mathematical statements should not be demanded in everything but only in whatever has no matter. Accordingly, the manner of proceeding in such cases is not that of physics" (*Metaphysics*, α.3, 995*a*15-17); and also: "So in discussing such matters [ethics and politics] and in using [premises] concerning them, we should be content to indicate the truth roughly and in outline, and when we deal this things which occur of the most part and use similar [premises] for them, [we should be content to draw] conclusions of a similar nature. The listener, too, should accept each of these statements in the same manner, for it is the mark of an educated man to seek as much precision in things of a given genus as their nature allows, and to accept persuasive arguments from a mathematician appears to be [as improper as] to demand demonstrations from a rhetorician" (*Nicomachean Ethics*, A.1, 1094*b*20-29, bracketed emendations are Apostle's).

which are heavy or light are composed of things which are neither heavy nor light…seem to be speaking of another heaven and of other bodies, but not of sensible ones" (*Metaphysics*, N.3, 1090*a*32-35). Accordingly, while there may be ways in which geometry can be used to assist astronomy in the prediction of the *appearances* of certain celestial objects, the *actual* motions of such objects *qua* eternal and perfect objects are not substantially equivalent to any circles described in geometry. Or, as Aristotle explains in *Physics* B.2 (where he also explicitly considers how astronomy may relate to supra-lunar physics): "on the one hand, geometry is concerned with physical lines, but not qua physical; on the other, optics is concerned with mathematical lines, but not qua mathematical but qua physical" (194*a*9-11). Thus, geometric circles are not sensible substances, and so geometrical investigation cannot be used in demonstrations concerning the motions of celestial phenomena insofar as such phenomena are understood to be the subjects of a science of sensible (though supra-lunar) substances (I will discuss below, however, Aristotle's allowance of a subalternation of mechanics to geometry and of his possible implicit allowance of a subalternation of cosmology to astronomy).

Second, and for similar reasons, the prohibition against *metabasis* prevents a unification of astronomy and cosmology. Accordingly, from Aristotle through at least the late Middle Ages, the consensus seems to have been that, while astronomy might be able to predict the apparent motions of celestial objects, it can have nothing to say about the causes of the motions of those objects. In fact, Alan Bowen has recently shown that Ptolemy himself regarded the science of astronomy to be both self-sufficient and of a higher degree of epistemic authority than cosmology (Bowen, 2007). But, in any case, throughout the Middle Ages and the Renaissance, although Ptolemy's techniques were regarded as effective for "saving the phenomena," these techniques were nevertheless generally understood to belong to a science different from or even inconsistent with cosmology. Consequently, as I will explain below, it is the erosion of this second aspect of the erosion of the prohibition that was most crucial for the *success* of the Copernican Revolution.

Third, even if the second aspect of the erosion of the prohibition against *metabasis* was most crucial for the *success* of the Copernican Revolution, another aspect of the prohibition and its eventual overturning is perhaps most important to the *legacy* of the Copernican Revolution. Beyond even the specific injunctions of Aristotle's prohibitions, a third, more general prohibition of Greek understanding held that demonstrations within the science of sub-lunar physics (such as those relevant to the science of mechanics) could not apply to the science of supra-lunar physics (i.e., cosmology). Of these three dimensions of the prohibition against *metabasis*, this view was also probably the most powerfully entrenched as a limit on the Greek and medieval European scientific imagination. That is, even for Platonists and ancient neo-Platonists who might otherwise have been less concerned with the specifically Aristotelian articulation of a prohibition against *metabasis* and who may have endorsed a cosmological understanding that the celestial realm is truly structured according to mathematical ratios, there remained a prohibition against the assumption that such ratios were directly relevant to scientific knowledge of matters in the sublunar realm. As Alexandre Kojève has put it:

[F]rom the point of view of classical pagan theology, we can find "mathematical laws," that is, precise and eternal ratios, only where there is no matter at all, or at the very least, where this matter is only a pure ether inaccessible to the senses. From the point of view of this theology it would be impious to search for such laws in the gross and vulgar matter of the sort which constitutes the living bodies which serve us temporarily as prisons. And this is why for convinced pagans such as Plato and Aristotle the search for a science such as modern mathematical physics would be not only great folly (as it would be for all the civilized Greeks, who because civilized, were predisposed to occupy themselves with the sciences) but also a great scandal, exactly as it was for the Hebrews. (Kojève, 1984, pp. 23-24)

Although I will explain in a moment a variety of medieval and Renaissance challenges to each of these aspects of the prohibition against *metabasis*, it will help to keep in mind that each of these prohibitions remained largely in effect throughout the Middle Ages and perhaps even until the end of the seventeenth century. It was only with the confluence of diverse challenges against the Aristotelian paradigm that this prohibition was overturned by the contrary enthusiasm for the reducibility of the sciences and, in some cases, attempts to generate a *mathesis universalis* valid for all scientific disciplines. But in terms of obstacles to the eventual success of the Copernican Revolution, the most important aspect of the prohibition against *metabasis* is surely a general insistence on the irreducibility of cosmology and astronomy. Nicholas Jardine explains this point as follows:

In medieval and Renaissance classifications of knowledge "mathematical" and "physical" study of the heavens are sharply separated. Mathematical astronomy forms one of the four mathematical arts of the *quadrivium*, whereas physical study of the heavens forms a specialized branch of natural philosophy. This gulf is reflected in *sixteenth-century* [my emphasis] teaching careers and university curricula. It is reflected also in the genres of literature of the period concerned with celestial matters. Physical astronomy [i.e., what I have been calling "cosmology"] is primarily represented in commentary on *De caelo* and *Metaphysics* Λ, and in specialized treatises on the substance and nature of the heavens. The primary texts of mathematical astronomy, Sacrobosco's *De sphaera* and Peurbach's *Theoricae novae planetarum*, are silent on natural philosophical issues. (Jardine, 1988, pp. 697-698)

However, as Jardine also cautions, "the isolation of the two disciplines should not be exaggerated" (p. 698). In the first place, it is now generally acknowledged by students of the history of Judeo-Arabic and Islamicate scientific traditions that, beginning in the tenth century, developments in the mathematical arts (especially that of astronomy) as well as detailed exegetical treatments of Aristotle's *corpus* (especially of the *Metaphysics*) gave rise to questions concerning not only the possible appropriateness of mathematical calculation to astronomy, but also of the applicability of astronomical techniques to the physical science of cosmology (as a science of the *supra-lunar* realm).[6] I will confine my own remarks below to the telling example of Averroes. Significantly, as Abdelhamid Sabra has shown, Averroes expressed a clear dissatisfaction in the *Long Commentary on Aristotle's Metaphysics* with the instrumentalism of the Ptolemaic system and encouraged attempts to produce astronomical techniques consistent with the principles of ancient cosmology. Specifically, even though Averroes accepted the capacity of Ptolemaic methods to save the phenomena, he firmly rejected these methods on the grounds that the use of equants for the calculation of apparent positions of celestial objects was "contrary to nature" (trans. and qtd. in Sabra, 1984, p. 141).[7] Moreover, according to Averroes, the success of the Ptolemaic mathematical calculations with

[6] For a detailed survey of recent scholarship on the significance of Islamicate astronomy to the Copernican Revolution, see Ragep, 2007. For a survey of recent scholarship concerned with the legacies of medieval Islamicate sciences more generally, also see Huff, 1993, esp. pp. 47-118. For more extensive discussion, see also Saliba, 1994 and 2007.

[7] For a translation of the entire section with which Sabra is concerned, see Averroes, pp. 176-79.

respect to saving the phenomena resulted in the loss of a prior ancient astronomy that, on the basis of his reading of Aristotle, had in fact been consistent with physical principles. Thus, Averroes concludes:

We should therefore embark on a new search for this ancient astronomy, for it is the true astronomy that is possible from the standpoint of physical principles…. In my youth I had hoped to accomplish this investigation, but now in my old age I have despaired of that, having been impeded by obstacles. But let this discourse spur someone else to inquire into these matters. For nothing of the [true] science of astronomy exists in our time, the astronomy of our time being only in agreement with calculations and not with what exists. (Sabra 142; bracketed emendation in Sabra's translation)

But even though Averroes remained committed to a geocentric cosmology, Copernicus, as I will explain in a moment, was partly motivated to endorse a heliocentric cosmology on precisely the grounds mentioned above—i.e., that the use of equants was inconsistent with accepted cosmological principles. And while Copernicus' own texts (including the *Commentariolus*) provide no basis for stating with any conviction that he was directly aware of Averroes' rationale, Rheticus, who was Copernicus' most devoted student, explicitly recognized the significance of Averroes' dissatisfaction with instrumental astronomy. In the *Narratio prima*, published three years before Copernicus' own *De revolutionibus*, Rheticus writes:

Averroes, who was in other respects a philosopher of the first rank, concluded that epicycles and eccentrics could not possibly exist in the realm of nature and that Ptolemy did not know why the ancients had posited motions of rotation. His final judgment is: "The Ptolemaic astronomy is nothing, so far as existence is concerned; but it is convenient for computing the nonexistent." (Rheticus, pp. 194-95)[8]

Leaving aside, however, the question of Averroes' *direct* influence on Copernicus and his followers,[9] it may seem that, beginning in the twelfth century, the erosion of the demarcation between astronomy and cosmology became to some extent inevitable. Moreover, with respect to the origins of modern science, it is an historical fact that the principles and methods of Aristotelian physics as regards the sublunar realm were not to be spared either. The development of the science of mechanics in the sixteenth and seventeenth centuries, for instance, necessarily would have followed a similar trend (though perhaps for different reasons) that I will describe below with respect to the questions of cosmology and astronomy. However, unlike the Aristotelian principles of sublunar physics, articulations of the principles of celestial, that is, supralunar cosmology were always more likely to involve mathematical methods, if only because such principles were avowedly about objects beyond direct experience in the sublunar realm.

To some extent, Aristotle's texts themselves encouraged these later developments. First, in the *Posterior Analytics* A.9, Aristotle discusses exceptions to the prohibition against *metabasis* such as the subalternation of mechanics and optics to geometry and of harmonics to arithematic. By this same reasoning, within the *quadrivium* of the Middle Ages, the science of

[8] It should be noted that although Rheticus is not directly quoting Averroes, his remark seems to refer to the last line of the Averroes quotation above.

[9] On the question of the *direct* influence of Averroes on Copernicus, see Goldstein, 1994 and Rose, 1975. More recent work has shown that, even if Copernicus was not directly familiar with Averroes' work, he must have been familiar with a wide range of contemporaneous Islamicate astronomy and that this Islamicate influence may have been decisive for Copernicus. See footnote 6 above.

music was understood to be subalternate to that of arithmetic and the science of astronomy as subalternate to that of geometry. Morevover, with respect to the question of the possible subalternation of cosmology to astronomy, as Averroes emphasized in his *Long Commentary on Aristotle's Metaphysics*, Aristotle himself had stated in *Metaphysics* that astronomy is "the mathematical science closest to philosophy" since, unlike arithmetic and geometry, which are 'are not concerned with any substances," "the science of astronomy is concerned with the investigation of sensible but eternal substances," i.e., planets and their perfect, eternal motions (*Metaphysics*, Λ.8, 1073*b*4-7).

But despite these points of encouragement in the Aristotelian *corpus*, Averroes was perhaps somewhat ahead of his time in expressing a wish to see the full integration (and not merely the subalternation) of astronomy and cosmology. And, in any case, Averroes' desire for the unification of cosmology and astronomy did not directly challenge the other two dimensions of the prohibition against *metabasis* mentioned earlier. That is, even had Averroes (or some other late-medieval astronomer) been able to find a means to unify astronomy and cosmology, there would still have remained not only a prohibition against the applicability of mathematics to the physical sciences but also against the unification of sub-lunar and supra-lunar physics. Indeed, Maimonides, so fundamentally in agreement with Averroes on so many other issues, seems simply to endorse the total *abandonment* of cosmology as a legitimate physical science in light of obstacles to there ever being a true description of celestial motion. He writes:

All that Aristotle states about that which is beneath the sphere of the moon is in accordance with reasoning; these are things that have a known cause, that follow one upon the other, and concerning which it is clear and manifest at what points wisdom and natural providence are effective. However, regarding all that is in the heavens, man grasps nothing but a small measure of what is mathematical…. For it is impossible for us to accede to the points starting from which conclusions may be drawn about the heavens; for the latter are too far away from us and too high in place and in rank. And even the general conclusion that may be drawn from them, namely, that they prove the existence of their mover, is a matter the knowledge of which cannot be reached by human intellects.… It is possible that someone else may find a demonstration by means of which the true reality of what is obscure for me will become clear to him. The extreme predilection that I have for investigating the truth is evidenced by the fact that I have explicitly stated and reported my perplexity regarding these matters as well as by the fact that I have not heard nor do I know a demonstration as to anything concerning them. (2.24, p. 327)

In any case, it seems likely that, even if the prohibition against *metabasis* began to face a number of challenges in the late Middle Ages, the success of the Copernican Revolution might be read as a sign that not only had the force and appropriateness of this prohibition been rejected by the middle of the sixteenth century, but also that the introduction of mathematical physics in the seventeenth century is in part the legacy of this rejection. It is in recognition of the possible significance of this legacy that I would like to offer two hypotheses concerning the process through which the prohibition against *metabasis* may have been rejected.

First, beginning with nominalists such as Ockham, one strategy of undermining the prohibition was to treat sciences as an aggregate by expanding the scope of subordination through the use of middle terms and analogies. Indeed, as Steven Livesey has shown, the consequence of this strategy was, for Ockham and his followers, to treat the prohibition of *metabasis* as an "extraordinary" prohibition rather than a general one. Specifically, Livesey claims:

Under Ockham and Albert's interpretation, the most distinct sciences are *eo ipso* autonomous and avoid *metabasis*, while other sciences which had been autonomous formerly are now elevated to the class of subalternating—or in Albert's words, "communicating"—sciences. This transformation was not accomplished by changing the theory of *metabasis*, but rather by changing the lenses under which the sciences were viewed. For Aristotle, the prohibitions of *metabasis* was the rule and subalternation the exception; for Ockham and his colleagues, subalternation became the rule and *metabasis* the exception. (Livesey, 1985, p. 145)

But there is a second and perhaps more important way in which thinkers who are classified as nominalists challenged the force of Aristotle's prohibition and thereby contributed to an intellectual climate favorable to the Copernican hypothesis. Specifically, nominalist philosophers, from the fourteenth century onwards, increasingly emphasized the epistemic significance of the most parsimonious, logically possible explanation capable of saving the phenomena independently of the question of existential predication. Indeed, Andreas Osiander's infamous, unsigned preface to the *De revolutionibus* suggests just how dominant this variety of nominalism had become by the mid-sixteenth century. In it, contrary to Copernicus' own intentions, Osiander baldly asserts that

Since he [the astronomer, or, literally, the mathematician] cannot in any way attain to the true causes, he will adopt whatever suppositions enable the motions to be computed correctly from the principles of geometry for the future as well as for the past. The present author has performed both these duties excellently. For these hypotheses need not be true nor even probable. On the contrary, if they provide a calculus consistent with the observations, that alone is enough…. Therefore alongside the ancient hypotheses, which are no more probable, let us permit these new hypotheses also to become known, especially since they are admirable as well as simple and bring with them a huge treasure of very skillful observations. So far as hypotheses are concerned, let no one expect anything certain from astronomy, which cannot furnish it, lest he accept as the truth ideas conceived for another purpose, and depart from this study a greater fool than when he entered it. (*De revolutionibus*, p. xvi, my emphasis)

It is probably an understatement to say that the insertion of these comments was overly-presumptive on Osiander's part, even though we should also acknowledge that this preface may have been partly responsible for keeping the *De revolutionibus* off the *Index of Prohibited Books* for more than seventy years after its first publication—i.e., until Galileo's outspoken reiterations of the tenets of Copernican cosmology encouraged its inclusion in a suspension decree of 1616 and a correction decree of 1620.[10] Be that as it may, and contrary though it may have been to the author's own explicitly stated view, Osiander's view—that the proper objective of a science is only to save the phenomena it investigates—probably represents the dominant attitude of era, and it is a view that, like the strategies for reduction mentioned above, is also probably rooted in fourteenth century nominalism's erosion of the prohibition against *metabasis*. As Edward Grant explains,

[10] The third edition (Amsterdam, 1617), however, escaped the 1616 decree only because it was printed before the decree itself. The next edition of the work (Warsaw, 1854) was published nineteen years after the work was finally removed from the Index in 1835. For details on the matter, see Rosen's notes on *De revolutionibus* in *Nicholas Copernicus on the Revolutions*, p. 342.

the typical [fourteenth century] attitude was reflected by Buridan when he said: "And they [astronomers] are satisfied to accept what is easier to imagine, for if it were true the celestial bodies would be moved by as many motions and such velocities by which they are now moved; but they do not care whether it is actually as imagined." (Grant, 1962, pp. 198-199)[11]

That is, because the relevant nominalist criterion for evaluating scientific propositions concerns their parsimony within a system of logically possible propositions, the erosion of Aristotle's prohibition was partly accomplished by deemphasizing the significance of the scientific context from which the propositions are initially generated. As Amos Funkenstein has put it "the same proposition [could] enter different proof-schemata in different 'sciences'" (Funkenstein, 1986, p. 307). In short, it may be that it is this aspect of the nominalist tradition that results in the abandonment of the Aristotelian insistence on the priority of actuality to possibility, and, as a consequence cultivates the regulative ideal that the "simpler" a given scientific explanation in terms of its systematic unification, the better. Accordingly, because the "the injunction against crossing methods eroded in the fourteenth century both in theory and in fact.… There was no need to challenge Aristotle's injunction head on: more and more disciplines could simply be declared to be *scientiae mediae*" (Funkenstein, 1986, p. 307).[12]

It is in the context of this trend towards *scientiae mediae* that the legacy of one variety of nominalism's emphasis on logical possibility helps to explain even the most wildly speculative attempts to bridge Aristotelian cosmology and Ptolemaic astronomy, whether by means of the mathematization of the *cosmos* or not. Nicholas Jardine summaraizes some of the typical (though often bizarre to the modern reader) strategies for doing this in the pre-Copernican Renaissance:

Though there was considerable variation on points of detail, the basic strategy was to realize Ptolemaic epicycles and eccentrics by substantial spheres and spherical shells, the so-called "partial orbs" (*orbes partiales* or *particulares*). The partial orbs of each planetary model were embedded in a substantial "total orb" (*orbis totus*), a spherical shell centred on the earth. The total orbs were supposed to form a plenum and were identified with the Aristotelian concentric orbs. This compromise proved resilient, providing as it did the framework for the planetary models of Peurbach's *Theoricae novae planetarum*, the main textbook of "advanced" mathematical astronomy in the Renaissance period. (Jardine, 1988, p. 698)[13]

But in Copernicus' case, it is not merely the possibility of a unification of astronomy and cosmology that is emphasized. Rather, what is emphasized by Copernicus and his followers is that the proper method for the unification of cosmology and astronomy is to be

[11] Grant derives the Buridan quote above from vol. 4 of Pierre Duhem's *Le système du monde: histoire des doctrines cosmologiques de Platon a Copernic*, pp. 138-139.

[12] Jardine reinforces this conclusion: "Following Thomas Aquinas, astronomy was often recognized as a *scientia media*, a branch of mathematics having special affinities with natural philosophy. In judicial astrology, widely taught alongside mathematical astronomy throughout the period, the two disciplines mingle freely. Further, though the primary textbooks of mathematical astronomy are devoid of natural philosophy, a considerable number of fifteenth- and early sixteenth-century commentaries on them discuss the substance and sources of motion of the celestial orbs. In these commentaries we find discussion of the potential conflict between mathematical astronomy and natural philosophy, a potential conflict that arises from the postulation by astronomers of epicycles and eccentrics, orbs whose centres of motion do not coincide with the centre of place in the universe, that is, the earth's centre" (Jardine, 1988, p. 698).

[13] For a further discussion of these cosmologies, see also Grant, 1981, § 11.

found in the capacity of mathematical description to provide a single, unified account of astronomical and cosmological phenomena.

If my hypotheses concerning Renaissance nominalism's role in the success and legacy of the Copernican Revolution are at all correct, there is clearly an historical irony in its having prepared the way for the success of a hypothesis that takes itself to be offering a true description of the real workings of the universe. Of course, one need not have been a nominalist to take advantage of the path that it helped pave in terms of a trend towards the modern enthusiasm for a reducibility of sciences—and, Copernicus's own realist commitments, not to mention his Platonist/neo-Pythagorean cosmological impulses, mean that he certainly should not be considered a nominalist. But Copernicus' own explicit views might simply conceal the underlying significance of nominalism's role in the success of the Revolution that bears his name. In fact, it is likely that Copernicus knew of prior discussions in the tradition of nominalist philosophy of the possibility of heliocentric astronomies that "saved the phenomena." And, by the same token, it would certainly have been possible for him explicitly to have presented his own system as just such an astronomy. However, when Copernicus addresses the precedents for his heliocentric system, he mentions only ancient cosmological sources rather than any of the more recent nominalist ones. What I think this means is that, the very fact that, in a work that is explicitly stated to be devoted to astronomy, Copernicus should refer to these sources as justification for pursuing a specific *astronomical* hypothesis must probably be understood as an indication of his intention to assert not merely the adequate predictive capacity of his astronomy but also to assert the *truth* of his description of the celestial motions. In other words, then, given Copernicus' explicitly stated intentions as well the textual sources to which he explicitly refers, it may not be an exaggeration to claim not only that, had Copernicus himself been a nominalist, there simply would not have been a Copernican Revolution, but also that Copernicus' own work is in part motivated to challenge aspects of the nominalist assumptions that paved the way for the success of his own hypothesis.

Be that as it may, I do believe that the *success* of the Copernican Revolution depended largely on Renaissance nominalism's impact on questions of scientific method and that there are traces of this impact in the modern legacy of the Copernican Revolution. Moreover, if these hypotheses regarding the significance of Renaissance nominalism to the success and legacy Copernican Revolution may be born out through future research, they might also provide direction for the study of other features in the historical development of modern science. This is important to note since I certainly do not wish to suggest either that the erosion of the prohibition against *metabasis* would be relevant only to a consideration of the success of the Copernican Revolution or that an attentive understanding of the development of modern science can be reduced to a question of the legacy of the Copernican Revolution. Indeed, it seems to me that a discussion of the significance of the erosion of the prohibition against *metabasis* to the success and legacy of Renaissance developments in the science of mechanics might also be especially promising.

In any case, I do think that I have provided reasons to think that the success and legacy of the Copernican Revolution in some crucial way involves the prior erosion of the prohibition against *metabasis* and that, moreover, this erosion might be understood to a large extent (though by no means exclusively) as a feature of the legacy of Renaissance nominalism. Perhaps one important question in this regard concerns whether the existential implications of the Copernican cosmological doctrines could have been entertained in the first place without a prior erosion of the prohibition against the reducibility of the sciences. This might also be linked with the question of the extent to which the legacy of the Copernican Revolution merges the nominalist tradition's emphasis on logical possibility with the realist commitments of Renaissance neo-Platonic cosmology and mathematics. I do not mean that an understanding of this legacy should be sought only by investigation of the text of the *De revolutionibus* itself. Rather, what I mean to propose is that by attending more carefully to the dialectical context from which the Copernican Revolution emerged, we will be in a better position to assess the extent to which the legacy of Renaissance philosophy and science is significant to the development of modern metaphysical realism conceived as logical possibility—precisely the commitment central both to the natural philosophies and to the rationalist and metaphysical philosophies of the seventeenth and eighteenth centuries.

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