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Celestial Spheres in fifteenth-Century Cracow Astronomy and Natural Philosophy

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Abstract

Medieval astronomers adopted the celestial spheres of Aristotelian cosmology, and combined them with Ptolemy's geometrical models, unaware directly that Ptolemy himself had interpreted the mathematical models of deferents and epicycles as spheres, and also as the entire physical orbs in which planets are moved. This essay focuses on discussions and developments of the tradition of celestial spheres and geometrical models at the University of Cracow in the fifteenth

century. After tracing the original contributions of astronomers and philosophers, the essay turns in particular to the commentary of Albert of Brudzewo on the most developed version of the spherical astronomy of the fifteenth century, Georg Peurbach's *Theoricae novae planetarum*. Albert's critique of that treatise along with his solutions to the problems that he identified set the stage for Nicholas Copernicus's adoption of celestial spheres and his innovative solutions for the reform of Ptolemaic astronomy.

Keywords: celestial spheres, Aristotle, Ptolemy, University of Cracow, Georg Peurbach, Albert of Brudzewo, Nicholas Copernicus

Introduction

"About what we cannot speak we must be silent." Ludwig Wittgenstein's famous dictum distinguished between what can be said and what can be shown. The aim of his book, *Tractatus Logico-Philosophicus*, was to set a limit to the expression of thoughts. Only in language can the limit be set, and what lies on the other side of the limit is nonsense.[1] Of course, exposing nonsense sometimes takes centuries. The assertion of the existence of something non-existent, however, is not nonsense; it is just false. Exposing what is false can also take centuries.

Like the eighteenth-century theory of phlogiston and the nineteenth-century theory of spatial ether, ancient and medieval beliefs about celestial spheres as the carriers and movers of the Sun, Moon, planets, and stars survived non-confirmation, and in the case of celestial spheres even the first serious challenge to geocentrism since antiquity. Among the philosophical explanations, Aristotle's accounts furnished authors formal structures that they interpreted in a variety of material and mechanical ways completely immune to falsification until the late sixteenth century. Questions about how the spheres carry or move the luminous celestial bodies had to be settled by dialectical speculation and philosophical critique. Logical coherence, not concrete physical description, guided authors as they adapted the Aristotelian account to their needs.

The problems, seen retrospectively, were fatal. They appeared early in the tradition, yet as with spatial ether or phlogiston, authors saw no insurmountable problems. After all, how else could one explain the regular and predictable circular motions of the heavens, and how could they do so if not attached to or embedded in spheres, all linked together in some coherent fashion?

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[1] Ludwig Wittgenstein, *Tractatus Logico-Philosophicus*, D. F. Pears and B. F. McGuinness (eds and trans.), London 1969. Compare Proposition 7 with the Preface, paragraphs 3 and 4. I have modified the English version. The German reads: "Wovon man nicht sprechen kann, darüber muß man schweigen."

Before I turn to problems of an astronomical sort, the kinds of problems that play a major role in Cracow developments, allow me to reconstruct the reasoning that persuaded Aristotle of both the concentrically spherical nature of the universe and its finiteness. Aristotle's argument depends on the obvious contrast between observed rectilinear natural terrestrial motions and the observed circular celestial motions. The prime motion of the stars in twenty-four hours could occur only if the stars are at a finite distance from Earth. If they were at an infinite distance, they would have to rotate at an infinite speed, a consequence that Aristotle considered to be impossible. Heavenly bodies, he reasoned, are made of a fifth essence, aether, which has nothing in common with the four sublunar elements of the world. The natural motion of aether is a circular motion, subject to neither generation nor corruption.[2] Pliny the Elder re-enforced these inferences in *Natural History*. Scholars relied routinely and extensively on Pliny's encyclopedia for many of their basic facts about celestial observations, especially in the editions available in print in the second half of the fifteenth century.[3] On the assumption, a commonsense belief that what we all see is what actually occurs, the circular motion of the starry vault seemed obvious.

As astronomers collected observations, anomalies required adjustments to preserve the 'axiom' of uniform, circular motions. Because of the unknown distance of the stars from Earth, the geocentric universe could be maintained as concentric as a whole. The motions of the Sun, Moon, and planets, however, usually required more sophisticated geometrical models, but we cannot completely overlook the efforts of those, including Regiomontanus, to

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[2] Aristotle, *On the Heavens*, I, 2-5, 10-12; II, 1, 3-6, 12-14; *Physics*, III, 5-8; *Metaphysics*, XII, 8. I have relied extensively on Michel-Pierre Lerner, *Le monde des sphères*, 2 vols. 2d rev. ed., Paris 2008, and Zofia Włodek, *Note sur le problème de la 'materia coeli' chez les scolastiques du moyen âge tardif a Cracovie*, in: "Actes du troisième Congres international de philosophie médiévale", Milan 1966, pp. 730-734.

[3] Pliny the Elder, *Natural History*, H. Rackham (ed. and trans.), in: "Loeb Classical Library", 330, Cambridge, Mass. 1997), II, I-IV. Pliny does not explain the motions of the planets as carried by spheres, but he seems to assume it, as at II, XIII, 64, where he says that the motions of the planets are eccentric yet converge on the center like the spokes of a wheel.

construct concentric-sphere models.[4] The astronomical context, in short, reveals more puzzles, but these are the kinds of problems that would lead eventually to the dissolution of the celestial spheres. Again, the problems appeared early in the tradition, but they too reveal the resiliency of the theory and the difficulties in resolving the details.

Ptolemy was not uncritical of Aristotle's views. He modified Aristotle's notions in ways suited to his effort to match geometrical models with observations as much as possible.[5] And therein lay the source of efforts to preserve a physically conceived concentric-sphere conception of the universe with one that fit actual observations. Although Latin authors remained ignorant apparently of Ptolemy's own cosmological views in *Planetary Hypotheses*, they were acquainted with them indirectly. We also cannot overlook the astrological context, for much of practical astronomy was devoted to predictions in other professional contexts, political, meteorological, and medical.

Ptolemy interpreted the mathematical models of deferents and epicycles as spheres, and also as the entire physical orbs in which the planets are moved. He eliminated Aristotle's compensatory spheres, and assumed that the spheres were nested. What emerged was a series of eccentric orbs up to a concentric starry sphere with no empty spaces except perhaps for the apparently hollow yet three-dimensional orbs in which an epicycle sphere moves. In this conception, then, the universe is finite, and the geometrical models could be interpreted as convenient mathematical descriptions of how the vital force in celestial bodies communicates its motion to the epicycle orb, and regulates the proper eastward motions of the planets. With all of the spheres contiguous, they also shared in the daily westward motions of the uppermost sphere.

Earlier historiography concerning developments at the universities in Cracow and Vienna claimed that they were peculiar for their teaching of mathematics and astronomy, and that Cracow was exceptional in devoting attention to the teaching of *De caelo*. More recent scholarship has challenged these claims. The level of instruction at Vienna and Cracow was higher, but several universities taught mathematics and astronomy, and other centers were

[4] Compare Michael Shank, *Regiomontanus and Homocentric Astronomy*, "Journal for the History of Astronomy", 29 (1998), pp. 157-166, and Noel Swerdlow, *Regiomontanus's Concentric-Sphere Models for the Sun and the Moon*, "Journal for the History of Astronomy", 30 (1999), pp. 1-23.

[5] Cf. Lerner, *Le monde*, pp. 63-81.

important for observation and publication of tables and for the construction of instruments. Likewise, scholars at many universities also taught *De caelo*, and produced commentaries that were copied, some of which were edited and printed in the second half of the fifteenth century.[6]

With the publication of Michel-Pierre Lerner's major study of celestial spheres, we have achieved a whole new plane of analysis, which serves as the basis for more exact studies of the fifteenth-century context. Lerner's extensive survey summarizes discussions about the nature and properties of celestial matter, the movers of the spheres, their number, and the 'place' of the universe. My essay focuses on the discussions at Cracow in the fifteenth century, especially the second half of that century. Although some have exaggerated the uniqueness of Cracow, there is a more important fact about Cracow that emerges from a critical reading of the scholarship, especially Polish scholarship, on fifteenth-century Cracow. The teaching of astronomy and astrology had a practical orientation that, combined with an eclectic philosophical context, tended to lend discussions a cross-disciplinary character that explains in part a pragmatic resolution of questions about celestial spheres and geometrical models. It may have been purely fortuitous, but it is significant that such pragmatism reaches a crescendo in the 1490s.

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[6] Compare the comments of Mieczysław Markowski, *Nauki wyzwolone i filozofia na Uniwersytet Krakówski w XV wieku*, "Studia mediewistyczne", 9 (1968), pp. 91-115; Ryszard Palacz, *Z badań nad filosofią przyrody w XV wieku*, "Studia mediewistyczne", 11 (1970), pp. 73-109; ibidem, 13 (1971), pp. 3-107; ibidem, 14 (1973), pp. 87-198; Richard Lemay, *The Late Medieval Astrological School at Cracow and the Copernican System*, in: "Science and History", "Studia copernicana", XVI (1978), pp. 327-354, at 346-350; Christe McMenomy, *The Discipline of Astronomy in the Middle Ages*, Ph. D. dissertation, University of California, Los Angeles 1984, pp. 98-99; Mieczysław Markowski, *Die Geschichte der Mathematik und Naturwissenschaft im 15. Jahrhundert an den mitteleuropäischen Universitäten*, "Studia mediewistyczne", 22 (1983), pp. 3-17; James Weisheipl, *The Interpretation of Aristotle's Physics and the Science of Motion*, in: "The Cambridge History of Later Medieval Philosophy", Norman Kretzmann et al. (eds), Cambridge 1988, pp. 521-536, at 522-523; Jerzy Dobrzycki, *Tablice astrologiczne Jana Regiomontana w Krakowie*, "Studia mediewistyczne", 26 (1988), pp. 85-92; Stefan Swieżawski, *L'Univers, La philosophie de la nature au XVe siècle en Europe*, "Studia copernicana", XXXVII, Jerzy Wolf (trans.), Warsaw 1999, pp. 63-75.

Polish students of these developments and their relation to Copernicus have adopted contrasting views. Some, like Jerzy Dobrzycki and Grażyna Rosińska, emphasized the advances in observation, production of tables, and mathematical astronomy, arguing for the prominence of technical problems or even the autonomy of the mathematical tradition from philosophical constraints.[7] Other scholars, such as Mieczysław Markowski, Ryszard Palacz, and Stefan Swieżawski, while acknowledging the mathematical developments, have interpreted them in philosophical terms, relying on their understanding of medieval and Renaissance developments in philosophy. They do not agree completely among themselves about the details, but they agree on the mutual dependence of mathematical developments and philosophical interpretations.[8] As will become clear, I regard the second approach as closer to an adequate explanation, but because of oversimplification and some dubious assumptions, its supporters have failed in their aim to construct a plausible narrative.

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[7] For example, Jerzy Dobrzycki, *The Astronomy of Copernicus*, in: "Nicholas Copernicus Quincentenary Celebrations Final Report", "Studia copernicana", XVII (1977), pp. 153-157, at 156: "On the cosmological plane the substance of the Copernican theory and the road to his discovery have been approached from many directions. The effects of the Neoplatonic and Pythagorean philosophy on Copernicus were a recurring subject of debates. The Copernican system was analyzed for its genetic links with the situation in natural philosophy (M. Markowski). . . . Historians of science are rather inclined towards an interpretation that would link the development of the heliocentric theory with the internal problems of science." See also Jerzy Dobrzycki, *Mikołaj Kopernik*, in: "Historia astronomii w Polsce", Vol. 1, Eugeniusz Rybka (ed.), Wrocław 1975, pp. 127-156; Grażyna Rosińska, *Mikołaj Kopernik i tradycje krakowskiej szkoły astronomicznej*, in: "Mikołaj Kopernik", Marian Kurdziałek, et al. (eds), Lublin 1973, pp. 33-56; eadem, *'Mathematics for Astronomy' at Universities in Copernicus's Time: Modern Attitudes Toward Ancient Problems*, in: "Universities and Science in the Early Modern Period", Mordechai Feingold and Victor Navarro-Brotons (eds), Dordrecht 2006, pp. 9-28.

[8] See, for example, Markowski, *Filozofia przyrody w drugiej połowie XV wieku*, in: "Dzieje filozofii średniowiecznej w Polsce", Vol. 10, Wrocław 1983; Ryszard Palacz, *Die krakauer Naturphilosophie und die Anfänge des heliozentrischen Systems von Nicolaus Copernicus*, "Studia mediewistyczne", 15 (1974), pp. 153-164; idem, *Nicolas Copernic comme philosophe*, in: "Colloquia copernicana" 4, "Studia copernicana", XIV (1975), pp. 27-40; and Stefan Swieżawski, *L'Univers*, chapter 2.

Dobrzycki and Rosińska had good reasons for their claims. The chairs of astronomy and astrology established at Cracow in the fifteenth century provided institutional support for the teaching of mathematics, the construction of observational instruments, and instruction in the use of tables. One problem with their view is that the *Canons* that always stand before the tables depended on the theory of celestial spheres. In some cases masters who engaged in these activities taught nothing else, but a closer look at the institutional setting suggests that their efforts were related to astrology and its application, especially in medical practice. It is also the case that until the last quarter of the fifteenth century, students trained in astronomy left the university, and nearly all of the better known ones left Poland altogether.[9] For these reasons I think it an oversimplification and exaggeration to maintain the view that mathematics became an autonomous discipline in fifteenth-century Cracow.[10] This is also why we need to examine the philosophical developments. Rather than critique the standard accounts, however, I will use them to construct a version that takes advantage of their scholarly insights, and interpret them in a way that does justice to the historical record.[11]

LERNER AND THE MATHEMATICAL-PHILOSOPHICAL TRADITION IN CRACOW

In his study Lerner reports the following results. For Aristotle the heavenly spheres are eternal, inalterable, perfect, and divine. They are not subject to generation and corruption;

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[9] See Markowski, *Kształtowanie się krakowskiej szkoły astronomicznej*, in: "Historia astronomii", I, ch. 4, pp. 57-86; Władysław Seńko, *La philosophie médiévale en Pologne: caractère, tendances et courants principaux*, "Mediaevalia philosophica polonorum, 14 (1970), pp. 5-21; Stefan Swieżawski, *L'Univers*, p. 70.

[10] Rosińska, *Mikołaj Kopernik i tradycje*, pp. 50-56, for instance, speaks of the "independence of astronomy from philosophy." Sometimes this claim is made in a more modest fashion, but the implication is that medieval scholars dealt with the discrepancy between adequate description and explanation by treating two kinds of inquiry "as different enterprises, to be dealt with, in effect, by people with different interests: philosophers whose goal was to understand the world and its workings and mathematical astronomers whose aim was the practical one of describing and predicting." For the quotation, see Ernan McMullin, *Kepler: Moving the Earth*, "HOPOS, The Journal of the International Society for the History and Philosophy of Science", 1 (2011), pp. 3-21, at 3-4.

[11] For a recent, brief survey of the literature, see Krzysztof Ożóg, *The Role of Poland in the Intellectual Development of Europe in the Middle Ages*, Cracow 2009.

their matter is subject only to movement from one place to another. As scholastic philosophers pondered the question, whether heaven possesses matter, they took into account the conception of matter as a substrate that is capable of receiving a contrary form. If contrariety is excluded from the heavens, then the supposed hylomorphic structure of heavenly bodies is doubtful.[12]

Avicenna maintained that a corporeal form is a form of continuity capable of receiving three dimensions. Following Plato's conception of the heavens as generated and corruptible, Avicenna concluded that all bodies in the universe are structurally identical; God alone assures the perpetuity of celestial bodies.

Averroes denied the hylomorphic structure of celestial spheres because they are simple bodies with no potentiality. Each celestial body has its own individual intelligence, and each constitutes a unique species. On celestial matter, some followed Averroes in denying any celestial matter, for the heavens are perfect, not susceptible to corruption, and, consequently, not composed of matter and form, nor of act and potency. Their only potentiality is to be in a certain place.[13]

Familiar with Avicebron's theory that all bodies possess the same material substrate, and that celestial and terrestrial bodies have specifically different forms with celestial forms not subject to corruption, Thomas Aquinas objected that celestial matter would in principle remain in potency to corruptible forms, and so would contradict the definition of celestial body. Thomas also objected, however, to Averroes's solution that celestial bodies are pure form and act, because celestial bodies are sensible. Thomas agreed with Avicebron about the hylomorphic structure of celestial bodies, and maintained the distinction between the celestial and terrestrial by claiming that celestial forms perfect their matter such that they are not in potency to being but only with respect to place, just as Aristotle had declared.

Giles of Rome followed Thomas on the hylomorphic character of celestial matter, but concluded that there are two genera of forms, one with contraries and one without. There is no form contrary to the form of celestial matter. Celestial forms inform celestial matter with

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[12] On the structure of material substance, see the extensive study of the medieval background by Anneliese Maier, *An der Grenze von Scholastik und Naturwissenschaft, Studien zur Naturphilosophie der Spätscholastik*, Vol. 3, 2d ed., Rome 1952, pp. 3-140.

[13] Lerner, *Le monde*, I, pp. 140-145; Włodek, *Note*, pp. 730-731.

their incorruptibility; sub-lunar forms, by contrast, can receive different forms and be deprived of a form, and so are corruptible.[14]

Perhaps following Robert Grosseteste, who was, however, influenced by light metaphysics to deny any difference between terrestrial and celestial matter, William of Ockham resolved the problem of corruptibility by assigning it to the perquisites of divine power. Celestial matter is in potency to other forms in principle, but only God can actually accomplish such a change.

Finally, John Buridan and his followers adopted the Averroist solution—celestial bodies are simple substances, not composed of specifically different parts, but subject only to magnitude, extension, motion, and other accidents.[15]

As Lerner remarks, all of these views, save that of Robert Grosseteste, were represented in fifteenth-century Cracow. He overlooks, however, a significant detail. While the nominalist and Averroist views prevailed in the first half of the fifteenth century, the contrary views prevailed in the second half.[16] The detail is significant because it reflects a pattern that holds for all of the other answers to questions about celestial spheres, particularly the anti-Averroist views of philosophers from 1475 to 1500. Lerner also neglects the institutional context, and in this regard the philosophical views are significant. Mathematicians, astrologers, and astronomers at Cracow in the last quarter of the century taught philosophy, and rejected Averroist homocentrism. They adopted the Ptolemaic models as necessary, leaving them, then, to discuss the reality of the models in their theoretical works.[17]

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[14] See Włodek, *Note*, p. 731, for a succinct description. See also Lerner, *Le monde*, I, 144.

[15] Lerner, *Le monde,* I, 144.

[16] Lerner, I, p. 328, n. 16, cites Włodek, but does not report the details. Włodek, while acknowledging differences of opinion held in the first half of the fifteenth century, reports that the views of Averroes and Buridan prevailed. She maintains that the situation changed in the second half of the century.

[17] Consider together the accounts of Dobrzycki, Rosińska, Markowski, Seńko, and Swieżawski already cited above. The idea is so commonplace that I cite only Seńko, *La philosophie*, p. 21, where he asserts that Averroism had no defenders in Poland. For claims about the positive reception of Averroes's ideas, see Ryszard Palacz, *Kopernikus und Averroes*, "Studia mediewistyczne", 22 (1983), pp. 105-110. In my view Palacz has not distinguished sufficiently between the acceptance of some Averroistic criticisms and the rejection of his exclusively homocentric assumptions.

What about the nature of stars and spheres, identical or specifically different? We need not enumerate the differences between the spheres and the visible, denser bodies that are embedded in them, but merely consider a spectrum of explanations.[18]

Avicenna's claim that celestial bodies are identical in nature required him to apply the distinction between genus and species. They are identical in genus, but specifically different.

In rejecting Avicenna's hierarchical ordering of celestial spheres and their intelligences according to their causal efficacy, Averroes concluded that the spheres are individuals of the same species like the individuals of the same animal species except that celestial species are constituted of one individual alone.[19]

The majority of Latin scholastics followed Avicenna, yet Thomas concluded that each celestial sphere and intelligence is an individual because its form actualizes all of the matter that can be actualized. He left questions about the relation between spheres and the bodies that they carry undecided. This indecisiveness seems consistent with Thomas's uncertainty about the relation between mathematical models and the motions of the planets.[20]

Many other authors such as Albert the Great and John Buridan concluded that there are three primary celestial substances: the Sun, the Moon and stars, and spheres.[21]

Pouncing on the Aristotelian principle that the solar and lunar spheres cannot be of the same nature because the orbs produce none of the effects of which the Sun and Moon are causes, Robert Grosseteste concluded that the heavenly bodies must be composed of terrestrial elements. This is a startling conclusion, but, aside from the fact that almost no one adopted this view, it contributed nothing to the discovery of the properties of the simple body and its relation to the visible celestial bodies.

Lerner claims that almost all astronomers who worked on mathematical models and commented on the *Theorica planetarum* neglected the properties of the celestial spheres. While the division between astronomy and natural philosophy, he asserts, was not absolute, it was sufficiently clear to leave such questions to philosophers and theologians. Here again,

[18] For these assertions and those in the following paragraph, see Lerner, *Le monde*, I, 145-146.

[19] Ibidem, 146-148. I pass over some apparent inconsistencies in Averoes's account.

[20] Ibidem, 148.

[21] Until the end of this section, consult Lerner, I, 150-159, 172-173, and 188-194.

the fact that at Cracow in the last quarter of the fifteenth century, philosopher-theologians practiced and taught mathematics, astrology, and astronomy eludes Lerner's reflections.

Aristotle and his followers affirmed a difference in density between spheres and the bodies in them, but this idea raised questions about how a rare body can carry and move a denser one, which led some to propose a fluid or air-like medium capable of moving denser bodies by its motion. Still, the idea that spheres move the bodies in them raised further difficulties. Homocentrists rejected eccentrics and epicycles because the models violate the perfection of the heavens and the fundamental principles of celestial physics (uniform, circular motions), yet astronomers needed the spheres to supply the substance that causes the bodies to move in eccentric circles or on epicycles. The typical solution was the three-orb system, two partial eccentric orbs and a concentric total sphere. Some authors describe the total sphere as divided into three partial orbs, one of which is concentric. Any hollow or intervening spaces must be filled with a body that is as rare as an orb, transparent, divisible yet inalterable. Albert the Great's conclusions about their properties seem to have been widely taken for granted: 1) Spheres are rare, transparent, indivisible, and inalterable. 2) Stars reflect light, are dense and opaque, indivisible, and inalterable, and in potency only to circular motion from one place on a circumference to another. 3) Spheres can be solid only in the sense of being indivisible. By following Aristotle, Latin astronomers retained spheres as the movers of the visible bodies, and most seem to have neglected the Stoic and Ptolemaic conception of the visible bodies communicating their motions to the spheres.

Aristotle's dual account of the nature of celestial bodies and their movers, and the questions that it generated are well known. Albert the Great rejected intelligences or angels as direct celestial movers, yet he conceived the motors as luminous forms produced by separated intelligences, and by emanation the forms impress rotational motion onto the spheres, like a potter's hand shaping clay on a wheel.

Thomas Aquinas resorted to a highly metaphysical and theological account that is thoroughly teleological. Leaving aside differences, we may say that Latin commentators agreed that God as prime mover and final cause is the source of all motion. From that agreement it was a relatively easy step for Buridan to reject intermediaries and conclude that God imparts an impetus to the celestial spheres and bodies.

The aetherial nature of spheres does not entail that aether causes circular motion, but rather entails that spheres are in potency to circular motion, a potentiality that intelligences, angels, or luminous forms actualize. It follows that the circular motions are natural, not violent. Adherents of impetus claimed that the heavens are immaterial; nevertheless, the spheres and celestial bodies possess volume and density with the impetus proportional to a sphere's velocity, volume, and density. Nicole Oresme, however, objected that impetus is by nature a temporary quality that causes bodies to accelerate, and so cannot be employed to save uniform and perpetual motions. Skeptical of purely natural accounts, Oresme, rejecting his own clockwork metaphor, retained the idea of angels as the movers of celestial spheres.[22]

Discussions of the continuity, contiguity, the number of spheres, and the place of the last sphere produced additional dialectical considerations that challenged the astronomical accounts. The diversity of motions argued against continuity, but the shared daily motion argued for their contiguity. The precession of the equinoxes, the theory of trepidation, and theological conceptions inspired the hypothesis of additional spheres as well as doubts about the finiteness of the universe.

The astronomical-philosophical tradition in Cracow, especially its practical orientation and anti-Averroism, reveals a number of developments that explain the acceptance of Ptolemaic models and variations on the three-orb system. The eclecticism of their philosophical theology, however, left a number of puzzles unresolved.

The spheres, as we noted, are luminous and transparent. The visible bodies are luminous and opaque. Other than these attributes and their motions, they lack qualities.[23] As we all know, the motions of spheres and visible bodies and the relation between the motions and geometrical models provoked the most serious disagreements among geocentrists. Before we examine the views that prevailed at Cracow in the 1490s, we must consider the achievements in astronomy earlier in the century. Here we have the benefit of the extensive research by Aleksander Birkenmajer, Jerzy Dobrzycki, Grażyna Rosińska, and Mieczysław Markowski.

ASTRONOMY AT CRACOW, 1400-1475

In the decade following the establishment of the Stobner chair in astronomy at Cracow, the University of Prague suffered a decline. Polish students, especially those from Silesia, began

[22] Note that Copernicus objected to impetus or force on similar grounds.

[23] See Swieżawski, *L'Univers*, pp. 216-219.

to attend classes in Cracow in larger numbers.[24] We do not know many names of early masters and students of astronomy for the first two decades, but the glosses in the manuscripts of the era inform us about their sources and about the training that they received in practical astronomy.[25]

In the 1420s several masters in the faculty of arts commented on standard mathematical treatises including the *Theorica planetarum* and on various versions of the *Alphonsine Tables*. There is also some evidence of acquaintance with the *Theorica* of Campanus of Novara, and even with Ptolemy's *Almagest*. Some commentators on the tables revised them for the Cracow meridian, versions that became known as the *Tabulae resolutae*.[26]

The first notable student in astronomy was from Czechel, a small town roughly halfway between Łódź and Wrocław. Sandivogius, also known as Sandko, enrolled at the university in 1423, received his B.A. in 1426 and M.A. in 1429. His service at the university was brief but significant.[27]

Grażyna Rosińska studied Sandivogius's work very carefully, and argued for the authenticity of one treatise, MS BJ 1929. Sandivogius read the works of Aristotle with Averroes's commentary, and rejected the existence of homocentric spheres, which I take to mean Averroes's homocentric critique of Ptolemy, because Sandivogius treated the celestial spheres as both mathematical constructions as well as bodies possessing density.[28] He clearly adopted, and taught his students, the models proposed by Ptolemy and the Arabic authors, Albohazen Haly, Geber ibn Afflah, and Thabit ibn Qurra. Sandivogius followed Aristotelian doctrine on the distinction between celestial and terrestrial bodies and the more perfect being of the former because of their unchangeableness.[29] Their circular motions are perfect, and

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[24] See Aleksander Birkenmajer, *Études d'histoire des sciences en Pologne,* in: "Studia copernicana", IV, Wrocław 1972, p. 455.

[25] See Markowski, *Historia*, pp. 75-86.

[26] Ibidem, pp. 79-83. See also Jadwiga Dianni, *Studium matematyki na uniwersytecie jagiellońskim do połowy XIX wieku*, Cracow 1963, pp. 11-12, 35, 199, and 217-219.

[27] Cf. Markowski, *Historia*, p. 59, and Rosińska, *Sandivogius de Czechel et l'école astronomique de Cracovie vers 1430*, "Organon", 9 (1973), pp. 218-229.

[28] See Rosińska, *Sandivogius*, p. 223, where she refers to fol. 150.

[29] Ibidem, p. 225, fols. 90-92.

influence the motions of terrestrial bodies. In his view “the science of astronomy is based on certain principles, including the whole world for all natural sciences are subject to them.”[30]

Sandivogius included astrology in the study of astronomy, but gave it less attention than most at Cracow. In dealing with the non-uniform motion of the Sun, Sandivogius adopted the eccentric model and interpreted the deferent as carrying the Sun and determining its motion. “The Sun moves on the eccentric not by itself [essentially], but rather with respect to the motion of the deferent. That motion is regulated finally with respect to the latitude of the solar body, as Aristotle intends in *Metaphysics* XII, because the Sun attached to the eccentric, the denser part of its orb, moves with the motion of the deferent.”[31] Citing different ancient opinions about the linear position of the Sun from Earth, he cites Haly that “the Sun is concentric and in the middle of the planets like a king who rules by the scepter in his hand and places his throne in the middle of his kingdom.”[32]

Sandivogius also dealt with the precession of the equinoxes and Thabit ibn Qurra’s theory of trepidation. Rosińska noted Sandivogius’s acquaintance with the works of Aristotle, and found evidence of Scotistic influence but no trace of Buridanism in his commentary. She concluded that instruction in astronomy at Cracow was influenced heavily by Arabic concepts, and in another important study reported Sandivogius’s proposal of a double-epicycle lunar model to save the observation of the spot on the Moon, not to replace the moving center of the deferent.[33]

In the texts cited by Rosińska, Sandivogius referred repeatedly to a celestial body attached to an orb. She quite rightly emphasized the centrality of practical astronomy, commentaries on tables and on the use of instruments, but Sandivogius seems otherwise to have adopted the standard view of orbs as the bearers and movers of the planets. Her claims about the independence of astronomy from philosophy seem exaggerated, and follow from the important recognition of anti-Averroest currents in Cracow and caution about the influence of

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[30] Ibidem.

[31] Ibidem, p. 226, fol. 94^{v}. Sandivogius was presumably referring to *Metaphysics* XII, 1073b18-22.

[32] Rosińska, *Sandivogius*, p. 226, fol. 112.

[33] Eadem, *Nasir al-Din al-Tusi and Ibn al-Shatir in Cracow?*, “Isis”, 65 (1974), pp. 239-243, at pp. 241-243.

Buridan. In my view her critique of Buridan's influence is correct, but this is far from demonstrating a complete neglect of natural philosophy.[34]

After a short period of decline, masters in the 1440s again commented on astronomical texts, but a reform inaugurated in the 1450s attached greater prominence to astrology. This fact re-enforces the emphasis at Cracow on practical astronomy, though one clearly dependent on natural philosophy.[35] In mid-century, Martin Król of Żurawica stands as one of the principal representatives of the Cracow school of astronomy. He wrote two mathematical treatises on tables and *Canons*, and a set of astronomical tables that he himself drew up. He also commented on the first six parts of the *Theorica planetarum*, stopping at the beginning of chapter 7, which deals with the motions of the planets.[36] According to Rosińska, Martin Król recognized clearly the disparity between natural philosophy and astronomy, and suggested that the astronomical solutions be interpreted as purely mathematical, a suggestion that perhaps influenced Albert of Brudzewo later in the century. In Rosińska's view this conclusion confirmed her claims about the independence of astronomy from philosophy, which she characterized as an atmosphere of freedom that was propitious for the search for new solutions. It is noteworthy, however, that authors discussed the principles of motion (natural vs. violent, circular vs. rectilinear, and uniform vs. non-uniform).[37] Rosińska's emphasis on mathematics comes at the cost of neglecting concerns about agreement between models and reality.[38]

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[34] These themes recur in several of Rosińska's indispensable works, for example, *'Mathematics for Astronomy*', pp. 10 and 21-24; *Instrumenty astronomiczne na uniwersytecie krakowskim w XV wieku,* in: "Studia copernicana", XI, Wrocław 1974; *L'École astronomique de Cracovie et la révolution copernicienne*, in: "Avant, avec, après Copernic", Paris 1975, pp. 89-92; *Tables of Decimal Trigonometric Functions from ca. 1450 to 1550*, in: "From Deferent to Equant", New York 1987, pp. 419-426.

[35] See Markowski, *Historia*, pp. 87-91.

[36] Rosińska, *Traité astronomique inconnu de Martin Rex de Żurawica*, "Mediaevalia philosophica polonorum", 18 (1973), pp. 159-166, at 159-160.

[37] See Swieżawski, *L'Univers*, ch. 5.

[38] Compare Rosińska, *L'École astronomique*, p. 91, with eadem, *Nicolas Copernic et l'école astronomique de Cracovie au XVe siècle*, "Mediaevalia philosophica polonorum", 19 (1974), pp. 149-157, at pp. 155-156.

Martin Król's students and successors continued the emphasis on practical astronomy, copying and preserving the *Tabulae resolutae*, copying John Bianchini's latitude tables, Regiomontanus's *Tabulae directionum et profectionumque*, and Peuerbach's *Theoricae novae planetarum* brought to Cracow around 1475.[39]

At this point I interject some reflections from Paweł Czartoryski that provide important contextual circumstances about developments at Cracow in the fifteenth century. Although nominalism gave way to earlier medieval traditions around the middle of the century, the empiricism of nominalist ontology,[40] especially the emphasis on observation and practical experience, played an influential role in the descriptive and exact sciences in which scholars in Cracow showed great interest. Now Czartoryski thought that nominalist physics, meaning the theory of impetus, led to Copernicus's theories, but Czartoryski and others have given it an emphasis that is not only lacking in Copernicus's own account but even conflicts with his explicit comments.[41] My point, however, in citing Czartoryski is that his comment about empiricism mediates between Rosińska's emphasis on the practice of astronomy and the relevance of natural philosophy for the achievements later in the century. In other words, a nominalist ontology and empiricism continued to influence scholars at Cracow until the end of the century, consequently natural philosophy did play a constructive role in the solutions that Cracow astronomers developed.

ASTRONOMY AND NATURAL PHILOSOPHY, 1475-1500

As we turn now to developments that culminate in the 1490s, I rely on the works of Johannes Versoris, Albert of Saxony, the anonymous *Quaestiones cracovienses*, and John of Glogovia.

Following the outline above, we begin with celestial matter.[42] Citing manuscripts and authors from the third-quarter of the fifteenth century, Zofia Włodek concluded that authors

[39] Rosińska, *L'École*, p. 90; Markowski, *Historia*, pp. 91-99.

[40] By 'empiricism' I mean an Aristotelian empiricism, not modern Humean empiricism.

[41] See Paweł Czartoryski, *La notion d'université et l'idée de la science à l'université de Cracovie dans la première moitié du XVe siècle*, "Mediaevalia philosophica polonorum", 14 (1970), pp. 23-39, at 28-35 and 38-39.

[42] Paralleling Maier's study of the medieval background is Markowski, *Filozofia przyrody*, chapter 4, pp. 124-172.

followed the views of Thomas Aquinas, sometimes as presented by Johannes Versoris, or the views of Giles of Rome (Aegidius Romanus), sometimes presented as reconcilable with the views of Thomas. Giles's views taught at Cracow constitute the most radical departure from those of Aristotle and especially Averroes. That fact led Aleksander Birkenmajer to speculate that Giles's theory played a role in works on natural science, especially astronomy, at Cracow near the end of the century. Włodek reminded readers that Copernicus insisted that the heavenly bodies are simple, without composition, contrariety, or change other than changes in position in relation to Earth. Birkenmajer remarked that without his deep convictions about the simplicity of celestial bodies and their uniform and circular motions, it would never have occurred to Copernicus to erect his own system.[43]

Such a consequence and transition, in my view, are too sudden. The significant and relevant facts are the anti-Averroism of fifteenth-century Cracow natural philosophy and the practical orientation of its astronomical tradition. I do not know of a single philosopher or astronomer at Cracow who rejected the Ptolemaic models or the doctrine of celestial spheres. Whatever differences of detail, Copernicus's teachers agreed on those fundamental points, leaving them, then, to adopt a spectrum of views about the reality of spheres and orbs.

The specific question here is the relation between celestial spheres and the visible celestial bodies. The anachronistic view that we must dismiss is the idea that celestial bodies possess natures and properties completely identical with sub-lunar bodies. We must also disentangle two other questions, namely, one ontological and the second about the relationship between observed motions and geometrical models. We shall treat the second after we address the ontological.

Even if they did not use the word, most Latin authors adopted Aristotle's conclusion about aether in its derivative sense as a substance that 'runs always' (αει θειν).[44] The heavenly bodies are eternal. The spheres move the visible bodies, which are also spherical, and hence possess the capacity to move in circles always. Because the visible bodies were also thought in most accounts to be constituted of aether, doubts about the relative rarity and density of

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[43] Włodek, *Note*, pp. 703-704, cites Aleksander Birkenmajer, *Kopernik jako filozof*, "Studia i materiały z dziejów nauki polskej", Warsaw 1963, p. 57. See also Birkenmajer, *Études*, pp. 563-578, 612-643, and 647-658.

[44] See E. J. Aiton, *Celestial Spheres and Circles*, "History of Science", 19 (1981), pp. 75-114, at 76.

spheres and visible bodies were to some extent misplaced. They are not contrary in nature but rather homogeneous so that their uniform circular motions, or, alternatively, their mean periodic motions, are regular and recur in a predictable way. Again, most authors adopted a clear distinction between the spheres of the aetherial region and those in the region of the elements.[45] Johannes Versoris makes the point in his commentary on *De caelo*: "Celestial rarity and density are not of the same nature as the rarity and density of the lower bodies here below."[46]

Albert of Saxony, whose views were well known and often cited by philosophers at Cracow in the fifteenth century, expressed similar conceptions about the celestial spheres and their relation to the terrestrial. On the relation of a celestial body to its orb, Albert maintained that the body is a part of the orb to which it is affixed, and therefore has the same simple motion as its orb.[47] Albert further concluded that it is not necessary for every spherical body to have a proper motion around its center. It suffices for many spherical bodies to move in a circle with the motion of the bodies to which they are affixed, so is it the case for planets that move with the motions of the orbs by which they are carried. It is furthermore not necessary that they have special motions around their proper centers.[48] These are startling concessions to

[45] For example, Campanus of Novara, *Theorica planetarum*. See *Campanus of Novara and Medieval Planetary Theory*, Francis Benjamin and G. J. Toomer (eds and trans.), Madison, Wisconsin 1971, p. 186, lines 376-377.

[46] *Quaestiones De coelo et mundo*, Biblioteca Jagellonica, Inc. 597, fol. 4^{rb-va}: "Ad secundum dicitur quod raritas et densitas in celo non sunt eiusdem rationis cum raritate et densitate istorum inferiorum."

[47] See André Goddu, *Sources of Natural Philosophy at Kraków in the Fifteenth Century*, "Mediaevalia philosophica polonorum", 35 (2006), pp. 85-114, at 100, quoting Albertus de Saxonia, *Questiones subtilissime in libros Aristotelis de celo et mundo*, Venice 1492; repr. Hildesheim 1986, II, q. 20, fol. G1^{ra}: "Ad rationes. Ad primam dico sicut iam dicebatur, quod stella est quedam pars orbis cui est infixa, ideo non oportet quod habeat motum simplicem alium a motu cuius est pars."

[48] Quoted in Goddu, *Sources*, p. 100; Albertus de Saxonia, *De celo*, II, q. 20, fol. G1^{rb}.

the Ptolemaic models, for Albert specifically rejected the Aristotelian principle that the spherical motion of a celestial body ought to be circular around its proper center.[49]

John of Glogovia's effort to reconcile the views of Thomas Aquinas and Giles of Rome should be read as supporting the idea that the celestial matter of the spheres and visible bodies is homogeneous but specifically different from sub-lunar matter.[50]

Finally, most authors at Cracow in the late fifteenth century applied the notion of potentiality to celestial bodies only with respect to place, not to being.[51] This leads us, then, into the next topic, the motions of celestial spheres and visible celestial bodies.

Although not a practicing astronomer, Versor did not ignore the basic observational facts and astronomers' efforts to explain them. Versor adopted the Ptolemaic models as the only way to account for the observed motions. He resolved the disagreements with Aristotelian principles by adopting the three-orb system.[52] As a celestial body moves on an eccentric or epicycle, it alternately withdraws from and approaches to Earth in the middle, but, Versor argued, the orb as a whole remains equally remote and near for its center remains the same distance from the middle.[53]

As for the non-uniform motions of celestial bodies, Versor provided an answer that suggests a more correct understanding of Ptolemy than most scholastics possessed. Versor interpreted

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[49] Goddu, *Sources*, p. 100, n. 35. Albert attributed the principle to Aristotle among the reasons cited at the beginning of *De celo*, II, Q. 20, fol. F4[vb], and proceeded to reject it or re-interpret it in scholastic dialectical fashion.

[50] See Markowski, *Filozofia przyrody*, pp. 144, n. 143. Compare with Włodek, *Note*, pp. 731-733.

[51] See Swieżawski, *L'Univers*, pp. 103-106.

[52] On this compromise system, see Edward Grant, *Eccentrics and Epicycles in Medieval Cosmology*, in: "Mathematics and its Applications to Science and Natural Philosophy in the Middle Ages", Edward Grant and John Murdoch (eds), Cambridge 1987, pp. 189-214; idem, *Planets, Stars, and Orbs*, Cambridge 1994, pp. 275-286.

[53] Johannes Versoris, *Quaestiones De celo*, BJ Inc. 597, I, q. 11, fol. 3[vb]: "Ad rationes. Ante oppositum. Ad primam dicitur quod licet aliqua pars unius orbis quandoque sit propinquior medio mundi et quandoque remotior tamen orbis secundum se totum est in eadem propinquitate et remotione a medio mundi, quia centrum eius equaliter distant a medio licet una pars sit quandoque proprinquior et a terra remotior."

'uniform' as meaning 'regular'. Where the motions of celestial bodies are not *perfectly* uniform, they are regular and periodic, following a regular pattern in some regular unit of time.[54] Because perfectly homocentric models do not work, the observational facts suggested the following inconvenient consequences. First, there is no unique center of heavenly motions. Second, there is no unique place downwards, but diverse centers. Third, the motion of a body on an epicycle would result in the penetration of dimensions or spheres, requiring them to posit the existence of void.[55] In response, Versor asserted that the diversity of circles in the heavens accounts for the observed irregularities in the following way. The farther a planet is on its epicycle, the slower it appears to move, and the closer it is, the more rapid it appears to move, but, in fact, the motion on its circle is uniform and direct, and the motion of the entire orb is also uniform and direct, so retrograde motion is merely an appearance generated by the direct motion of a planet on its epicycle as the orb moves uniformly and directly. The effect, of course, is that the planet appears to move backwards. As for the diversity of their proper motions, Versor reported a principle in such a fashion that it seems to have been a commonly held view, namely, the distance-period principle of the rotations of spheres from the center. He echoed the same principle already expressed in *Quaestiones cracovienses* on *De caelo*, namely, that the orbs nearer to Earth rotate more rapidly than those farther from Earth, indicating that the period of an orb is related to its distance from Earth.[56]

Albert of Saxony's commentary on *De caelo*, as reported, was well known in Cracow. Because he presented the views of John Buridan and Nicole Oresme, the more controversial views of fourteenth-century French authors survived in Cracow. Albert also adopted the three-orb solution that preserves both the eccentric-epicycle model with the aggregate of the

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[54] Ibidem, II, q. 8, fol. 17[rb-va]:"Ad rationes ante oppositum. Ad primam dicitur quod illud non impedit regularitatem motus, quia est equalis velocitas totius motus celi per totum tempus. Ad secundam dicitur quod licet in aliquo orbe inferior appareat irregularitas motuum propter pluralitatem motuum illi orbi convenientium, vel etiam in motu alicuius planete, tamen secundum veritatem ibi non est irregularitas, quia quilibet motus totalis alicuius orbis est regularis."

[55] Notice that Copernicus later adopted the first two explicitly, but whether he adopted the third remains unclear.

[56] For details and texts, see Goddu, *Sources*, pp. 91-92.

orbs concentric to Earth. He added that we must adopt either an eccentric or epicycle to solve the problem of the Sun's annual motion on the ecliptic, and we must adopt eccentrics and epicycles to save the appearances of planetary motions as well.[57] We have already noted above his comments on orbs and their motions, hence here we may remark on the significance of such scholastic discussions. While claiming to remain Aristotelian, they interpreted the Ptolemaic models as subordinate to the uniform motions of their orbs.[58]

Although written earlier in the century, masters at Cracow continued to use and comment on the *Quaestiones cracovienses* into the 1490s.[59] In considering the *Quaestiones*, we are restricted to comments on Aristotle's *Physics*, but the answers are consistent with other views in Cracow about celestial matter and the motions of the spheres. For instance, I, Q. 25 concludes that celestial and sub-lunar matter are different in species: "Ergo sequitur, quod materia caelestium et inferiorum different specie."[60] The only sense in which we can speak properly about privation and corruption of the heavens is with regard to the place of a part, not the whole nor with respect to its substantial form.[61] The commentator adopted intelligences as the movers of the planets (VIII, Q. 133) and the difference between the daily and proper motions of the Sun, Moon, and planets (VIII, Q 136, citing *De caelo* I). These

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[57] Ibidem, pp. 98-99

[58] Ibidem, p. 100. I have revised my reading of Albert's comments to reflect his meaning more clearly. Note that Copernicus's insistence on the uniform motions of celestial bodies and their epicycles around their proper centers amounts to a rejection of the scholastic compromises.

[59] See Ryszard Palacz, *Les 'Quaestiones cracovienses'—principale source pour la philosophie de la nature dans la seconde moitié du XVe siècle à l'université jagellone à Cracovie*, "Mediaevalia philosophica polonorum", 14 (1970), pp. 41-52; idem, *'Quaestiones' super libros Physicorum Aristotelis*, "Studia mediewistyczne", 10 (1969), entire issue.

[60] Palacz ed., *'Quaestiones'*, p. 49.

[61] Ibidem, p. 49, I, Q. 25: "Ex quo sequitur, quod in materia caeli non est potentia ad aliquid esse, loquendo de potentia distante ab actu, est tamen ibi potentia ad ubi." Ibidem, p. 63, I, Q. 32: "Dicitur igitur, quod eodem modo, sicut in corporibus caelestibus est privatio, sic etiam corruptio, modo ibi est privatio ad ubi et non formae substantialis, ergo est corruptio ipsius ubi et non formae."

positions are consistent with the compromise three-orb system, and how to reconcile it with Aristotle's homocentric system.

Unlike Versor and Albert of Saxony, John of Glogovia was not only a philosopher but also a mathematical astrologer who taught mathematical subjects and wrote treatises on astronomical and astrological topics.[62] He also produced his own version of the *Quaestiones*, where we find an even more comprehensive consideration of traditional sources and compromise views, all of which are consistent with the sort of pragmatic empiricism so dominant earlier in the century.[63] John's anti-Averroism, even where he adopted the standard subordination of the mathematical to the physical, indicates that he too accepted the three-orb system.[64] Whatever the theoretical motives for doing so, it is significant in his case that practice trumped a rigid acceptance of Aristotelian homocentrism.

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[62] There are numerous manuscripts in which John treated mathematical subjects, including almanachs, ephemerides including a treatise on Regiomontanus's *Tabulae directionum profectionumque*, introductions to astronomy, Canons for tables including the *Tabulae resolutae* for Cracow, a commentary on Ptolemy's astrological text, the *Quadripartitum*, a treatise on Sacrobosco's *De sphera*, a treatise on comets, a treatise on the theory of the Moon's motions, and an introduction to the Alfonsine Tables. See Mieczysław Markowski, *Repertorium bio-bibliographicum astronomorum cracoviensium medii aevi: Ioannes Schelling de Glogovia*, "Studia mediewistyczne", 26 (1990), pp. 103-162. For evidence of John's extensive output, see Stefan Swieżawski, *Materiały do studiów nad Janem z Głogowa*, "Studia mediewistyczne", 2 (1961), pp. 135-184; and Władisław Seńko, *Wstęp do studium nad Janem z Głogowa*, "Materiały i studia zakładu historii filzofii starożytnej i średniowiecznej", 1 (1961), pp. 9-59, and ibidem 3 (1964), pp. 30-38.

[63] Marian Zwiercan has edited the text cited below, but it remains unpublished: Johannes de Glogovia, *Quaestiones in octo libros Physicorum Aristotelis*, Cracow 1969). In BJ, MS 2017, John's Question 19 (fols. 70-75) corresponds to Q. 25 of *Quaestiones cracovienses*, and John's Question 23 (fols. 87-90) corresponds in part to Q. 32 of the *Quaestiones cracovienses*. We still lack modern editions as well of authors' questions on *De caelo* and *De generatione*.

[64] Aside from his treatise on *De sphera*, his treatise on the Moon is entitled *Theorica lunae et eius orbibus et eorum motibus*. See Markowski, *Repertorium*, p. 138, Item No. 106, referring to Cracow, BJ, cms 1840, ca. 1497.

The argument to this point has emphasized the eclecticism of late fifteenth-century Cracow philosophers. Their eclecticism renders the distinction between realists and nominalists almost useless for predicting answers to ontological questions. The ontology of celestial spheres is an especially contentious issue. Scholars have rejected Pierre Duhem's dichotomy between realist and instrumentalist interpretations of astronomical models. Some, however, have gone to the opposite extreme of denying instrumentalist/pragmatic interpretations of models altogether.[65]

As I am about to turn to Albert of Brudzewo, it is necessary to comment on the *Theorica* literature and especially Peuerbach's version. I have addressed these questions elsewhere as they pertain to Copernicus, but to appreciate the views held at Cracow in the 1490s requires a more systematic survey. What follows is a sketch that focuses on Peuerbach and the scholarship related to his *Theoricae novae planetarum* before turning to Albert of Brudzewo's take on Peuerbach's view of spheres and their relation to mathematical models.

THE *THEORICA PLANETARUM* IN MEDIEVAL ASTRONOMY AND NATURAL PHILOSOPHY

Before turning to Albert of Brudzewo and his commentary on Georg Peuerbach's *Theoricae novae planetarum*, I summarize briefly the tradition of texts that Peuerbach revised and corrected, focusing in particular on claims made by Edith Sylla in the paper that she presented at Łódź in September 2011. I have not seen the final version of Sylla's essay; hence I beg readers' indulgence for the following reconstruction.

The origins of the *Theorica* are obscure, but they served a primarily pedagogical purpose, namely, of providing a largely qualitative description and summary of the mathematical models and tables that accounted for the observations of celestial motions.[66] The structure of

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[65] In the session at the conference in Łódź, Edith Sylla challenged the accounts of Pedersen and Lerner for their interpretation of partial orbs, and indicated even some disagreement with that of Aiton on Peuerbach.

[66] See Olaf Pedersen, *The "Theorica Planetarum" and its Progeny*, in: *Filosofia, scienze e astrologia nel Trecento europeo*, Graziella Federici Vescovini and Francesco Barocelli (eds) Parma 1992, pp. 53-78; and *The Theory of the Planets*, O. Pedersen (trans.), in: *A Source Book in Medieval Science*, Edward Grant (ed.), Cambridge, Massachusetts 1974, pp. 451-465.

these works was fairly uniform, divided into eight chapters beginning with the theorica of the Sun, two chapters on the Moon, followed by a chapter on Mars, Jupiter, and Saturn, a fifth on Venus and Mercury, a sixth on retrograde motions and eclipses, the seventh, maybe a slightly later addition, on latitude theory, and an eighth, missing from several manuscripts, on the astrological 'aspects' of the planets.[67]

Initially, these summaries of mathematical astronomy said little about natural philosophy, not because they had no ontological commitments or made no assumptions about the natures of the celestial bodies, but because they were focused pedagogically on providing descriptions of astronomical models for students in the liberal arts and also, perhaps, for medical students for whom the context related to astrological practice. In other words, these texts provided an introduction and background. It is clear that all such texts adopted a geocentric perspective and various assumptions about the natures of celestial bodies.

According to Olaf Pedersen, who surveyed dozens of manuscripts dating from the early thirteenth to the fifteenth century and also printed editions of the fifteenth and sixteenth centuries, the *Theorica* features a "strictly geometrical presentation of the theoretical models" of planetary motions. Most manuscripts include figures composed of circles and straight lines "without reference to the celestial 'spheres' of Aristotelian cosmology." The author mentions only the sphere of the fixed stars. Pedersen surmises that "the author did not wish to trouble his students with the endless discussions on whether the 'mathematical' and the 'physical' account of the universe were compatible, at least not before they had been solidly grounded in its purely kinematic features." [68] Perhaps Pedersen imposed his own bias here, but he supports his conjecture with the additional observation that the author says nothing about moving forces, planetary 'souls' or 'separate intelligences'.

The commentaries of the fourteenth century, however, begin to introduce physical considerations and also their relevance for astrology. Pedersen sites Taddeo da Parma's dissatisfaction with the purely mathematical character of the *Theorica*. Pedersen sees this as evidence of the power that "the idea of a 'physical' astronomy had over the mind of astronomers of an Aristotelian bent."[69] Taddeo discusses the celestial spheres, and interprets the geometrical circles as 'a complete machinery' of spheres, yet the spheres play no role in

[67] Pedersen, *The "Theorica"*, pp. 56-59.

[68] Ibidem, p. 59.

[69] Ibidem, p. 64.

calculating planetary positions. That observation, however, does not diminish the importance of Taddeo's physical discussions. As a philosophical astronomer, he had to add the Aristotelian cosmology of spheres to his astronomical system, and even when his focus was purely on mathematical theory, he made it clear that the theory was merely a prerequisite to the art of astrology.[70]

By the late fourteenth century there is evidence that fourteenth-century developments in mechanics made an impact on theoretical astronomy. Discussions of the physical mechanisms of orbs became more sophisticated, and in treating the moving forces of a mechanism, some took up questions about separate intelligences attached to each sphere. The point is that in the fourteenth and fifteenth centuries, some commentaries on the *Theorica* are explicit about trying to integrate the mathematical models with physical mechanisms. Among the significant results of such discussions is the explicit recognition that "there is a period of precisely one solar year built into each and any of these [planetary] models."[71]

In Pedersen's opinion, Peuerbach's correction of the earlier manuals made "the physical system of spheres and the theory of trepidation of the equinoxes" an integral part of the exposition.[72]

Christe McMenomy's extensive survey of medieval astronomical literature, written in part to test hypotheses about disciplinary boundaries, tends to confirm the pattern that Pedersen laid out. The *Theorica planetarum*, she points out, explained the terms necessary for use of the Alfonsine tables, dealing "with the parts of the imaginary system of circles used to describe planetary motions for calculation."[73] After describing the various models, she concludes that the *Theorica*'s main value lay in describing the theorems and figures used in computational astronomy, and that it was intended for use with the tables of the *Almagest*. "Its author was

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[70] Ibidem, pp. 66-67.

[71] Ibidem, p. 76.

[72] Ibidem, p. 78. Pedersen's studies illustrate mistakes in the *Theorica* that were later corrected by Peuerbach. James Byrne, *The Mean Distances of the Sun and Commentaries on the Theorica Planetarum*, in: "Journal for the History of Astronomy", 42 (2011), pp. 205-221, shows, however, that commentaries on the *Theorica planetarum* from the fourteenth century already corrected some of these mistakes.

[73] McMenomy, *The Discipline*, p. 127.

not worried about justifying or defining the scope of the study of astronomy, or about relating his epicycles, deferents and equants to physical reality."[74]

Campanus of Novara's *Theorica planetarum*, also from the thirteenth century, does discuss celestial spheres and perhaps served as a transition to the physical interpretations of the geometrical models. A substantial part of the text describes instructions for the construction of an instrument that, it claimed, could determine the positions of the planets without having to make cumbersome arithmetical computations. McMenomy doubts that the instrument was ever built or used much because single-surface equatories were simpler to construct and more convenient to use. Citing Benjamin and Toomer, she agrees that the instrument did not match the accuracy of the tables in determining planetary positions. The book, however, was important for determining the *sizes* of every planet, epicycle, and deferent in the system. "The Ptolemaic theory was thus tied to a set of physical distances [linear distances] which related not mathematical abstractions to one another, but physical bodies" [brackets added]. The instrument itself was a physical machine that served as a compromise model of the universe, mathematical and physical. The compromise, however, could not account adequately for all of the motions, nor was the concept of center strictly Aristotelian. "Nevertheless," McMenomy concludes, "the *theorica* explanation raised the *possibility* of a system which satisfied both mathematical and physical constraints."[75]

There is, however, a sort of reversal. The more technically proficient astronomy became, the less it was required of all arts students by the end of the fourteenth century.[76] Instead, a text such as Sacrobosco's *Tractatus de sphaera* served as the introduction to astronomy for students in the arts. As McMenomy emphasizes, however, no medieval student using only Sacrobosco as a source would have been prepared to perform any of the functions of calculatory astronomy. The text, then, is almost exclusively descriptive.[77] There were many commentaries on the sphere from the thirteenth century that tend to give precedence to natural philosophy over astronomy. They do not question the reality of epicycles, deferents, and other devices used to describe planetary motions, but most are silent on the

[74] Ibidem, p. 133.

[75] Ibidem, pp. 127-139. Cf. *Campanus of Novara*, Benjamin and Toomer (eds), pp. 32-33.

[76] McMenomy, *Discipline*, p. 139.

[77] Ibidem, pp. 142-150.

incompatibility between physically concentric and mathematically eccentric planetary circles.[78]

Fourteenth-century commentaries take up specific questions about spheres concerning their contiguity and continuity, their relation to the varying speeds of planets, the existence of orbs and shells, the meaning of 'center', the sense in which spheres are 'solid', and the like.[79] Authors known mostly for their expertise in natural philosophy, not astronomy, discussed these issues, yet it is altogether likely that astronomers received such instruction. Aristotelian physical cosmology set the parameters. Celestial and elementary spheres are physical objects, yet there is disagreement about epicycles and eccentrics. Some treat them as real and others as abstractions or mental constructs, but most agree that there should be no contradiction between the physical features and mathematical descriptions.[80]

Fifteenth-century commentaries drive the discussion even more towards the definition of astronomy as a physical rather than a mathematical science. Commentaries on *De sphaera*, like those of the fourteenth century, are introductory scholastic manuals. A representative author is Johannes Baptista Capuanus de Manfredonia (c. 1475).[81]

Capuano, according to McMenomy, considered astronomy to be part of natural philosophy, not a mathematical science. In her twenty-page summary of Capuano's commentary, McMenomy describes the efforts of the author to lay the groundwork for astronomy as a *theoretical* science in the Aristotelian sense, locating astronomy as the science dealing with the motion of bodies around a center, that is, celestial bodies, the subject of the second book of *De caelo*. As McMenomy shows (Fig. 7, p. 233), Capuano left astronomy out of the list of the mathematical sciences belonging to the *quadrivium*.[82]

Capuano's treatment of the more mathematical subjects indicates that he was familiar with the more mathematical features of astronomy, referring the reader to Ptolemy's *Almagest* and

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[78] Ibidem, p. 179. Cf. Jürgen Sarnowsky, *The Defence of the Ptolemaic System in Late Mediaeval Commentaries on Johannes de Sacrobosco's De sphaera*, in: "Mechanics and Cosmology in the Medieval and Early Modern Period", M. Bucciantini, M. Camerota, and S. Roux (eds), Florence 2007, pp. 29-44.

[79] McMenomy, *Discipline*, pp. 180-226.

[80] Ibidem, p. 226.

[81] Ibidem, pp. 226-253.

[82] Ibidem, pp. 228-234.

works on the *Theoricas*, treating epicycles and eccentrics as mental constructs and not real circles. "Almost all references to the elements of the Ptolemaic system are phrased to show the circles are only 'posited' or 'imagined', not real."[83]

We have to interject some caution here.[84] What appears to be an exclusively mathematical consideration of circles in this context must be compared with the more realist account of spheres. Earlier in his commentary, as McMenomy points out, Capuano had cited both Euclid's definition of sphere imagined according to mathematical rules and Theodosius's definition of a sphere as a solid body. It is the latter sense that applies to the celestial spheres for they are three dimensional and corporeal.[85] In the context of the commentary on Sacrobosco, however, Capuano did not restrict his comments to the total or complete spheres, although he did not enter into details about the partial orbs. McMenomy leaves the impression that Capuano treated the geometrical models as only mathematical figures. In fact, Capuano made a distinction, cited by McMenomy, between the orbs used to account for

[83] Ibidem, pp. 246-247.

[84] In a re-assessment of Capuano's importance and originality, Michael Shank has questioned the adequacy of McMenomy's summary. See Shank, *Setting up Copernicus? Astronomy and Natural Philosophy in Giambattista Capuano da Manfredonia's Expositio on the Sphere*, "Early Science and Medicine", 14 (2009), pp. 290-315. Shank hypothesizes that some of Copernicus's arguments in Book I of *De revolutionibus* may have been responses to Capuano's *Expositio*. Shank, however, cites several other authors who held similar views. There are several editions of Capuano's work, but the one that Shank attaches to Copernicus is the 1518 edition, which, however, seems to have belonged to Rheticus. Shank believes that one annotation in that volume resembles Copernicus's hand. In my view, the annotation is clearly in Rheticus's hand, but note that the annotation does not appear in the *Expositio*, which is lacking in any annotation whatsoever. So, even if Copernicus saw this copy, it may have been in 1539 at the earliest, much too late for him to have used it for Book I. See André Goddu, *Copernicus's Annotations: Revisions of Czartoryski's "Copernicana"*, "Scriptorium", 58 (2004), pp. 202-226, especially pp. 207-208 and 225, and Plates 38, 39, and 40.

[85] McMenomy, *Discipline*, pp. 240-241, and notes 168-169.

the motions of the heavenly body and other circles, like the equant, that are only imagined.[86] Furthermore, in discussing the position of the planet within the sphere, he rejected the suggestion that the planet could lie on the surface of the sphere for this would require the penetration of the superior sphere or the existence of a void space. A planet also cannot move within the sphere for this would cause the sphere to break apart. The planet, then, is fixed in position within the sphere, and moves with the motion of the sphere. Here again, however, it is not clear whether Capuano was referring to the total sphere or specifically to a partial orb.

McMenomy does not discuss Capuano's commentary on the *Theorica*. In a recent brief consideration of Capuano's commentary, Peter Barker attributes to Capuano the principle that the *Theorica* intends to assign to each planet as many orbs as there are irregularities of motion.[87] It is unlikely, however, that Capuano also adopted this principle in referring to the equant. I suggest that Capuano interpreted the eccentric and epicycle orbs as real, and rejected some other circles as purely imaginary.

The main thirteenth-century scholastic commentaries on Aristole's *De caelo*, according to McMenomy, "reflect the hierarchical division of the sciences, maintaining a strict separation between astronomy and natural philosophy based on both methodology and subject matter."[88]

The so-called nominalist commentaries of the fourteenth century, by comparison with those of the thirteenth, tended to level the relationship of the sciences, allowing for demonstrations appropriate to the nature of the subject matter. John Buridan, for example, differentiated astronomy from natural philosophy, but accepted and used astronomical devices to supplement physical explanation.[89] Albert of Saxony seems to have taken Buridan's view further in the direction of recognizing mathematical astronomy as a science in its own right,

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[86] Ibidem, note 187, p. 423, citing Capuano, *Expositio*, f. 74[va]: "Agit de orbibus vnde dicit quod quilibet planeta propter Solem habet tres circulos eo modo quo declaratum est intelligendo; primus est deferens eccentricus simpliciter sicut eccentricus Solis. Secundus est circulus imaginatus equans nominatus; . . ."

[87] See Peter Barker, *The Reality of Peurbach's Orbs: Cosmological Continuity in Fifteenth and Sixteenth Century Astronomy*, in: "Change and Continuity in Early Modern Cosmology", Patrick Boner (ed.), "Archimedes" 27, Dordrecht 2011, pp. 7-32.

[88] McMenomy, *Discipline*, p. 270.

[89] Ibidem, pp. 271-277.

providing mechanisms necessary to 'save the appearances' that must be judged according to physical principles. That is to say the orbs are not just mathematical constructs but physical bodies that explain the mechanisms that account for the observed motions. We can see here that some fourteenth-century commentaries provided the impetus behind the construction of a unified mathematical and physical astronomy.[90]

As McMenomy concludes, "The lines of demarcation between astronomy and natural philosophy are not so clear cut."[91] It seems as well that by the early fifteenth century several approaches emerged that left students of astronomy to adopt a variety of viewpoints on the status of astronomy. The Averroist critique of Ptolemaic astronomy was very influential among those adopting homocentric assumptions and denying altogether the existence and even validity of the Ptolemaic mathematical models. At the opposite extreme were those who interpreted or, at least, wanted to interpret the mathematical models physically. In between were numerous compromise versions, those accepting the physical existence of spheres but differing on the physical existence of intermediate orbs. Among the latter are some who accepted the existence of intermediate orbs, others who denied them outright while acknowledging their usefulness, and still others who expressed agnosticism about their existence but without denying their existence in absolute terms.

The variety of options emerged primarily from two problems—the dominance of Aristotelian cosmology with all of its interlocking arguments and the success of the Ptolemaic models in accounting for observed motions and making predictions. Subsidiary to that problem-complex was how to reconcile the mathematical demonstrations with the ideal of Aristotelian demonstration in the strict sense.

PEUERBACH'S *THEORICAE NOVAE PLANETARUM*

We turn now to the innovations in Peuerbach's *Theoricae novae planetarum*. Even Pedersen acknowledges the significance of Peuerbach's emphasis on the reality of celestial spheres.[92]

Aiton's translation of Peuerbach's *Theoricae* provides the evidence for that interpretation, but he does not explain the plural form 'theoricae', nor why some versions of the treatise have a

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[90] Ibidem, pp. 277-281.

[91] Ibidem, p. 286.

[92] Pedersen, *The "Theoricae Planetarum"*, p. 78.

singular form.[93] One possible explanation, adduced by Sylla in her talk, is that the plural refers to the distinction between real spheres and mathematical models. Because this is relevant to Albert of Brudzewo's commentary, I will postpone further comment on Sylla to my summary of his treatise.

In one selective survey, Pedersen traces the evolution of the term 'theorica' from an adjective to a substantive, and from the singular to the plural, noting in particular that the plural often referred to instruments, that is, the illustrations or diagrams accompanying the text,[94] and even later to "a sort of astronomical computing machine," like the 'equatorium' described by Chaucer and *volvellae*, circular discs that represented the various circles in the planetary theories.[95]

The plural form has plainly many referents, but notice that the illustrations, diagrams, and machines served to help the student follow the text describing the 'theorica' for each celestial body. The point is that each body requires its own *theorica*. Why the plural seems to be so prominent in Peuerbach's version is unknown. Perhaps there is some significance in the explicit recognition of a plurality of models. Anyone familiar, for example, with the various eccentricities in the solar, lunar, and planetary models might well wonder about the coherence of the system—there is no single theory but a multiplicity of theories. Perhaps, it was this sort of incoherence that motivated even Regiomontanus to propose concentric models for the Sun and Moon.[96] This is a topic for further investigation. The point is that there is no single *theorica* but several, and I suggest that the plural form constitutes an explicit recognition of that fact.

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93 E. J. Aiton, *Peurbach's Theoricae novae planetarum*, "Osiris", second series, 3 (1987), pp. 5-44.

94 Olaf Pedersen, *Theorica, A Study in Language and Civilization*, "Classica et mediaevalia", 22 (1961), pp. 151-166, at 161.

95 Idem, pp. 164-165.

96 See Michael Shank, *Regiomontanus*, pp. 157-166. I should add, however, that there is a similar incoherence even in the Copernican system with respect to the varying positions of the mean Sun for each planetary model.

Now, to the ontology of the spheres about which there are many questions. Pedersen acknowledged and Aiton emphasized Peuerbach's commitment to the reality of spheres and orbs.[97] Unfortunately, further details are obscure. For example, Peuerbach maintained that the planets are 'attached' or 'fixed' to epicycle orbs. But exactly how is one celestial object 'attached' or 'fixed' to another? To my knowledge no author explained such nebulous attachments. Are these metaphors or category mistakes prompted by spurious questions? The mathematical models suggest mechanisms, but what machines or forces move aetherial bodies? Many philosophers supported the theory of celestial intelligences or angelic movers; others appealed to impetus; all of which derived from the unmoved mover. The fact remains that commentators on the *Theoricae* did not usually elaborate on such ideas, and the same holds for questions about the eccentric spheres carrying the epicycle. Some explicitly maintained the reality of both, but not all of the adjustments to the epicycle models in particular, such as the equant, the crank mechanisms for the Moon and Mercury, and reciprocation mechanisms.

ALBERT OF BRUDZEWO

With Albert of Brudzewo we encounter a figure who, in my view, moved the commentaries on Peuerbach in a decidedly new direction, setting up the sort of dialectical approach to Ptolemy and Averroes that we otherwise find only in Regiomontanus's *Epitome*, whatever Regiomontanus's genuine view may have been.[98]

In revising my brief comments on Albert at the conference in Łódź, I acknowledge that Edith Sylla's remarks provoked me to express my views more carefully, and introduce important distinctions that clarify my interpretation. It is also my hope that I can explicate clearly my comments about the agreement between what Sylla called 'critical realism' and what I called 'pragmatic empiricism' in a way that she will find acceptable.

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[97] Aiton, *Theoricae*, pp. 8-9, n. 14, also pp. 12, 14, 17-18. In some of these examples, the orb is said to be 'carrying' the epicycle.

[98] Albertus de Brudzewo, *Commentariolum super Theoricas novas planetarum Georgii Purbachii*, Ludwik Birkenmajer (ed.), Cracow 1900, pp. 14-15. I have provided a selective summary of the *Commentariolum* in André Goddu, *Copernicus and the Aristotelian Tradition*, Leiden 2010, pp. 162-166, with a somewhat different emphasis here regarding celestial spheres.

First, a general observation about Albert's commentary supports my comments about its significance. In almost every chapter of the *Commentariolum* Albert quotes Ptolemy from the *Almagest*,[99] and provides the data—from the observations that Ptolemy made using an armillary sphere—to interpret and explain Peuerbach's *Theoricae*. Albert is also explicit about the relationship between the planets and astrological effects following from qualitative characteristics.[100] Aside from its possible influence on Copernicus, the modern editor, Ludwik Birkenmajer, claims that later commentators borrowed extensively from Albert's commentary, referring specifically to Capuano de Manfredonia, Francesco Giuntini, and Erasmus Reinhold.[101]

In his introduction, Albert defends astronomical observations, and refutes Averroes's criticisms of astronomical models. He begins by acknowledging the efforts of astronomers who handed down ideas about the rotation of the first sphere carrying all of the orbs as well as rotations of other motions contrary to the first motion. The creator adorned the secondary orbs with stars and endowed them with diverse powers, leaving Earth immobile in the middle and influenced by them proportionally. The creator stretched them out like a cover where humans could contemplate them and deduce the nature of the heavens: free from all corruption and change, immense in size, amazingly beautiful with an indestructible union of motions, so related to one another that astronomers could construct certain rules for deducing their operations. The *Theoricae* accomplish these goals in an introductory and narrative way.

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[99] In a version, incidentally, that departed from the translation by Gerard of Cremona, according to Birkenmajer.

[100] The astrological influences of the six planets derive from and are mediated by their illumination by the Sun. Robert S. Westman's *Copernican Question*, Berkeley 2011, has developed and documented in unprecedented detail the astrological context of the Copernican revolution. Of relevance here, in particular, see Westman, pp. 53-56: Albert of Brudzewo's emphasis on the place of astrology and astrological influences in Cracow astronomy and natural philosophy suggests another way by means of which the Cracow tradition impressed connections with planetary order upon Copernicus as he began to immerse himself in the astrological culture of northern Italy.

[101] Ibidem, p. LVI. As Shank, *Setting up*, p. 294, n. 9, points out however, Birkenmajer offers little evidence of a connection between Brudzewo and Capuano.

What moves the wise to posit several celestial orbs? Because many stars rotate together from east to west, philosophers concluded that all of these stars are on a single sphere with the stars fixed in the sphere like a nail in a ship and a part in a whole, for the stars do not move except with the motion of the sphere.[102] They called this the eighth sphere or the starry vault. Besides these, experts observed seven stars with their own proper motions, and concluded that there are as many orbs, seven, as there are such stars.

As for the number of spheres, Averroes rejected the existence of a ninth sphere above the eighth because it has no star, and hence could not influence anything below it. Others, however, maintained that there are many more than eight or nine, for each star is a heavenly sphere, that is, a round celestial body, solid, in the middle of which is a point from which all lines extended to the circumference are equal. Astronomers in fact assign three orbs to the Sun and even more than three for the remaining planets, as is clear from the *Theoricae*.[103] Albert points out that there are a great variety of views held by philosophers and astronomers, but he announces his intention to examine the more probable truth, and restrict himself only to those that are more easily understood and that conform to the probable ones cited by Peuerbach in his *Theoricae novae*.[104]

To that end, Albert points out that a 'sphere' has three meanings: 1) a part of the spherical heaven, not separate from the whole, nor existing in itself subjectively; a star, and in this sense there are as many spheres or orbs as there are stars. 2) A sphere or orb that does exist in itself subjectively, whether concentric or not with Earth, and it is this sense that is meant when we say that the Sun has three orbs. 3) An orb or sphere that is concentric with Earth, that is, the aggregate of all orbs that are required and suffice for saving the motion of a planet in longitude and latitude. This aggregate sphere is concentric with Earth with respect to both its convex and concave surfaces, and this is the meaning appropriate here.[105]

Aristotle proved that some hypotheses in philosophy are true. First, heaven is a simple body (*De caelo* I). Second, a simple body has only a simple motion according to its proper nature

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[102] In other words, the stars do not appear to have individual proper motions.

[103] Albert uses the plural accusative here '*Theoricas*', suggesting that each planet has its own *theorica*.

[104] *Commentariolum*, pp. 3-6.

[105] Ibidem, pp. 6-7. Albert certainly describes the partial orbs later, but he seems here to take 'sphere' as referring primarily to the total or complete sphere.

(*De caelo* I). Third, the motion of a body contrary to its proper nature necessarily belongs to another according to its proper nature *(De caelo* I and II). Fourth, one sphere is not moved with several motions by the same intelligence, nor is a sphere moved by several intelligences equally proximate to the first (*Metaphysics* XII). Fifth, an inferior sphere does not influence a superior sphere, but rather the converse. Therefore, superior spheres can influence the motions of inferior ones.[106]

From these suppositions, Albert draws the following probable conclusions. If we adopt 'orb' in the third sense, there are ten orbs or mobile spheres: the seven of the planets, the eighth of the starry vault, the ninth of the second mover, and the tenth of the prime mover.

After discussing astrological influences from the perspective of natural philosophy, Albert turns to astronomy proper. Citing authorities, he accepts the division of astronomy into two parts, theoretical ('theorica') and practical, the second of which refers to astrology. 'Theorica' also called 'speculativa' is also treated both theoretically and practically. One sense of 'theoretical' is narrative and introductory, and such is the sense in the present 'Tractata Theoricarum'. The distinction here is between the demonstrative methods employed by Ptolemy, for example, in the *Almagest* and the descriptive or narrative, and so the present little book is 'theorica narrativa'. The practical part of theory involves the use of instruments and tables.[107]

The subject matter of astronomy is moving celestial body, considered both in itself and its parts, as that which is subject to imagination in a plane [that is, plane geometry]. Both the natural philosopher and the astronomer treat motion, but the natural philosopher considers the celestial motion of the whole sphere and of all of heaven as their motion is relative to the size of the orb. For example, the Moon's diurnal motion is less than Saturn's because the Moon's orb is smaller than Saturn's. The astronomer, however, considers not only the motion of all of heaven and of the whole spheres but also the motions of heaven and orbs, both total and

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[106] Ibidem, p. 8. The acceptance of uniform, circular motion and of an intelligence as moving the spheres is significant.

[107] Ibidem, pp. 16-18. In *Copernicus*, p. 164, I asserted that Albert seemed to propose the autonomy of astronomy, but that is dubious. Aside from consideration of philosophical objections, astronomers also discuss the properties of the visible celestial bodies, albeit as derived from their positions and motions, and some of these considerations are clearly qualitative. Cf. *Commentariolum*, Treatise II, pp. 128-141.

partial. The description, then, is more precise because the Moon moves the fastest for it completes a revolution more quickly than all of the other planets. The Moon moves about 13° each day while the Sun moves about 1° because 1° of the sphere of the Sun equals nearly 18° of the Moon's sphere, for the Sun's diameter is 18 times the diameter of the Moon's. This is what is meant above in speaking of plane geometry, which does not limit itself to the celestial bodies and their motions as they are in themselves, but also as those bodies and the diversity of their motions are perceived by the sense of sight and described in plane figures.[108]

The ancients, continues Albert, examined the diversity of celestial motions, how those most remote from our senses and only with difficulty understood by the intellect are the subject of sensual imagination. The senses help the intellect to explore, for the spheres can be projected onto a plane in two ways, only the first of which is relevant here. This is the one where a sphere is collapsed into or projected onto a circle; all of the 'Theoricae planetarum' are composed by means of this kind of projection. In this sort of projection the Sun is said to be on an eccentric circle, but this is not literally true, because they understand that spheres are projected onto a plane. The Sun is not on a circle, which is a plane figure contained by one surface, but it moves in an orb, which is a solid and spherical body.[109]

The 'Theoricae' is divided into three treatises; the first deals with the motions of the seven planets, the second with the phenomena and attributes that follow from their motions, and the third with the motion of the eighth sphere and the second mover. The first treatise is divided into five parts for the corresponding five 'theoricas': one for the Sun, one for the Moon, one for the superior planets, one for Venus, and one for Mercury. The Sun is treated first, not because it is the first in order, but because it has fewer and less diverse motions than the other planets, and also because the motions of the other planets have a natural connection to the motion of the Sun. In other words, we observe the motions of all of the planets by their relation to the Sun, by means of which they are measured, regulated, and studied.[110]

[108] Ibidem, p. 18.

[109] Ibidem, pp. 19-20.

[110] Ibidem, pp. 20-21. Albert is reflecting here in particular on the rules stipulated by Ptolemy that account for the bounded elongations of Mercury and Venus, the retrograde motions of all of the planets, and lunar phases and eclipses.

The intrinsic goal is the perfect understanding of heavenly motions. The extrinsic end is how the motions related to the operations and effects that celestial motions have on the terrestrial elements.

Albert concludes the introduction by indicating that where required he will sometimes introduce mathematical proofs but elsewhere natural ones, for the consideration of more capable students. In other words, he distinguishes the expertise of philosophers from that of astronomers, but he also acknowledges that the subject matter of the *Theoricae* requires natural proofs in addition to mathematical ones.[111]

Following the order in Peuerbach's treatise, Albert begins with the division of the solar sphere into three partial orbs. The commentary nature of Albert's treatise raises one interpretative difficulty. It is difficult to tell whether Albert is merely reporting Peuerbach's view or his own. In scholastic fashion, Albert divides Peuerbach's text into lemmata often followed by a brief summary of what Peuerbach says or does. Consider, for example, how he describes the division of the total solar sphere into three partial orbs and how in reducing these orbs to imaginary circles he defines and describes the eccentric circle.[112]

Because the stellar sphere is concentric and other orbs are eccentric, a question arises about whether the eccentric are whole orbs. If they were, then the result would be a rupture or penetration of spheres and the insertion of a void space. To avoid these consequences, then, yet account for the observations, astronomers were compelled to admit partial eccentric orbs. In the total sphere, they placed three orbs to save the motion of the Sun, and they also admitted orbs in the planetary spheres to account for the observed zodiacal motions.[113]

Most of the above texts seem to attribute reality to the partial orbs, and Albert seems to be following the standard way of distinguishing between real orbs and imaginary circles. Then comes the crucial passage about the reality of eccentrics and epicycles:

No mortal knows whether eccentrics truly exist in the spheres of the heavens, unless we grant (as some say) also with respect to epicycles that they are revealed by the revelation of spirits [illumination? divine revelation?]; otherwise, then, they are formed by the mathematical imagination alone, as Albeon testifies in the first part [of *Instrumentum*], chapter 10, where

[111] Ibidem, p. 21.

[112] Ibidem, p. 23.

[113] Ibidem, pp. 25-26.

he says: "No one instructed [in the discipline] could truly believe that eccentrics and epicycles exist in the heavens in such a manner as the mathematical imagination draws them. [They introduce them] because without mathematical figures of this kind the art cannot account for the regular motions of the stars, [for this art] determines their positions at any moment whatever in a way that does not disagree with our observations." So says Albeon. We ought, therefore, to be content concerning this since by means of them we understand the perfect art of the stars in their motions [brackets added].[114]

If Albert is referring to circles only, then the passage can be read, as Sylla maintains, as distinguishing between real orbs and imaginary circles. It is this passage and others like it that led Aiton to distinguish between orbs as real but circles as imaginary.[115] Nicholas Jardine claims that Albert adopted an agnostic and skeptical view about eccentrics and epicycles, although he later qualifies the comment about skepticism.[116] Michel-Pierre Lerner, for his part, questions whether even Peuerbach accepted the reality of partial orbs, citing commentators who affirmed their existence and others who denied them. Erasmus Reinhold later maintained, perhaps following Brudzewo, that Peuerbach affirmed their existence on physical grounds, namely, to counter the possibility of void and penetration of spheres. Capuano developed this argument against the Averroists. Lerner concludes that Brudzewo interpreted the orbs as products purely of the mathematical imagination.[117]

In my view, the passage calls for careful wording and distinctions, more careful than some of my comments at the conference. Albert, I believe, was referring to the reality of eccentric and epicycle orbs because the comments appear in the context of the discussion of spheres and orbs, total and partial, and in the context of Averroes's objections to eccentrics and epicycles (pp. 25-28). Second, a comment asserting the fictional character of abstract figures would be trivial. Perhaps, Richard of Wallingford and Albert were responding to some

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[114] Ibidem, pp. 26-27. The brackets are added, and Birkenmajer added the reference to Richard of Wallingford's *Instrumentem* with an explanation of "Albeon".

[115] Aiton, *Theoricae*, p. 8, n. 15, cites the 1495 edition, Sig. a^{vi}, which corresponds to Birkenmajer's edition, p. 19.

[116] Nicholas Jardine, *The Significance of the Copernican Orbs*, "Journal for the History of Astronomy", 13 (1982), pp. 171-172, and notes 15-29.

[117] Michel-Pierre Lerner, *Le monde*, I, pp. 128-130; on Brudzewo, p. 130 and n. 83; on Peuerbach, pp. 121-130.

Platonist asserting the existence of abstract geometrical figures, but which? Just as they distanced themselves from 'Platonic friendship', Renaissance Platonists tended to re-interpret Plato's robust mathematical realism.[118]

Albert's first comment citing Wallingford's *Albeon* is clearly agnostic, meaning that it is neither an affirmation nor a denial. The second comment quoting Wallingford does appear to be a stronger assertion, closer to skepticism and to denial, but the point, I claim, is a response to Averroist objections: the necessity of the mathematical models does not entail the real physical existence of partial orbs. Albert's concluding comment is a model of 'pragmatic compromise', the conclusion reached by Jardine, the aim of which is "to evade the potential conflict between mathematical astronomy and natural philosophy."[119]

Lerner's interpretation is stronger, namely, that Albert definitely rejected the reality of partial orbs, yet he too acknowledges the variety of interpretations held by commentators, also distinguished according to their disciplinary emphasis. Natural philosophers like Capuano supported the reality of orbs, while mathematicians interpreted them as products of the mathematical imagination alone.[120]

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[118] For example, *Bessarionis In calumniatorem Platonis libri IV*, in "Kardinal Bessarion", Ludwig Mohler (ed.), "Quellen und Forschung der Görres-Gesellschaft" 22, Paderborn 1927, II, pp. 205-207, where Bessarion doubts that Plato and the Pythagoreans maintained that the principles of natural things are really geometrical figures, adding that the models proposed by astronomers do not really exist in the heavens, for their purpose is to save the appearances. He concludes that the Platonic doctrine of plane figures should be understood in the same way. Bessarion wrote the defense of Plato around 1458-1469, according to Mohler, I, pp. 358-365. Bessarion's interpretation of both Plato and Aristotle supported the anti-Averroist attacks of humanist authors.

[119] Jardine, *Significance*, p. 172, who also raises sensible objections to attributing a modern instrumentalist theory of science to authors who were realists about geocentrism and total concentric spheres, but expressed doubts about the details of planetary models.

[120] Lerner, *Le monde*, I, p. 78 (n. 85: pp. 287-288); p. 121 (nn. 57-58: p. 314); p. 129 (n. 81: pp. 318-319); p. 130 (n. 83: pp. 319-320); pp. 150-164; p. 196 (n. 4: p. 361); II, pp. 3-4, 61 (n. 168: p. 265).

Additional details also urge us to be cautious. Where Albert uses agnostic language in speaking of eccentrics and epicycles, he is positively assertive in dealing with the equant—it is an imaginary circle, not a real orb.[121] This might be taken to suggest that the partial eccentric and epicycle orbs are real, that is, that there are motions of real orbs, but the point here is that the center of the epicycle is moving regularly relative to a point (the equant) that is *not* its center, hence it cannot be a real orb. The rejection of the equant orb, then, is consistent with *agnosticism* about partial eccentric and epicycle orbs.

Other complications in the lunar and planetary models, however, cast doubt on their reality. Consider the complicated mechanisms that generate an oval lunar orbit and that require a double-epicycle model.[122] The mechanisms are all circular, but not all of them can correspond to orbs. The epicycles of the double-epicycle model clearly penetrate one another, so the epicycles cannot be real orbs. The models for Mercury are notoriously complicated. Here we encounter reciprocation devices that work kinematically but also cannot be subordinated to a real orb.[123] Finally, consider Albert's tabular summaries of the eccentricities and planetary epicycles.[124]

The observations necessitated the multiplication of models. For example, each celestial body required a different eccentric point to account for non-uniform motion. In Ptolemy's system only Mercury and Venus have the same eccentricity; each of the other celestial bodies requires a different eccentric point. The points are extremely close to one another, yet they do not coincide. The epicycles are of different sizes, of course, to account for either bounded elongation or retrograde motion, but the equant points also do not coincide, meaning that each of the epicycle centers is rotating uniformly around its own equant point. In other words, the plural form of *theoricae* refers not to different mathematical and physical

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[121] *Commentariolum*, p. 86: "Notandum. Quantum est in se, ad motum orbium non est opus aequante. Nihil enim aequans facit ad motum orbis realis, cum sit circulum imaginarius, . . ." See also pp. 65-66.

[122] Ibidem, pp. 67-69 and 124. Albert probably borrowed the double-epicycle model from Sandivogius of Czechel.

[123] Ibidem, p. 120.

[124] Ibidem, p. 127.

theoricae, but rather to the fact that there are separate mathematical *theoricae* for the planets, Sun, and Moon based on the observations of each.[125]

Because none of the centers of the eccentricities coincide, requiring a model with a different center, it is wishful thinking to consider these as constituting a coherent system of real partial orbs. The models work reasonably well to predict future positions approximately, but all of these considerations taken together confirm, in my view, Albert's expressions of agnosticism as well as pragmatic compromise.[126]

To sum up, I agree that a fair reading of the evidence must acknowledge passages where Albert affirmed the existence of spheres and partial orbs. Leaving aside the possibility that he contradicted himself within the space of a few pages, I maintain that the most plausible explanation of these passages is that Albert was reporting Peuerbach's view, not his own, or that he was referring to the total or complete sphere, not real eccentric orbs and epicycles. The question about their reality, however, is ambiguous.

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[125] One of Copernicus's early insights was the recognition of multiple and even incompatible systems. But Copernicus's early optimism that he could produce a technically accurate and coherent system of models eventually gave way to the same incoherence as Ptolemy's models had—the centers of the various planetary models do not coincide, and some of them even place the mean Sun on a small circle.

[126] Barker, *Reality*, p. 14, also interprets Albert as a realist about partial orbs, and attributes to him the standard or principle that "there must be an orb for each separate circular motion performed by the sun, moon, or planets." Barker calls this article a sketch and explains that he lacked space for an exhaustive account. Still, as it stands, the account is unsatisfactory. As with his reading of Capuano above (n. 87), Barker ignores the passage from the *Commentariolum* in which Albert rejects the equant orb as real, and he ignores the passage in which Albert cites Richard of Wallingford and expresses agnosticism about partial orbs and epicycles. In the text that he does cite, p. 19, I suggest that Albert is referring to the total or complete sphere that carries the Sun around Earth or the restricted area of the sphere in which the visible body moves. In an earlier article, Barker, *Copernicus and the Critics of Ptolemy*, "Journal for the History of Astronomy", 30 (1999), pp. 343-358, esp. 346-348 and notes 9-12, cited the Wallingford reference, but merely to confirm Brudzewo's acceptance of eccentrics and epicycles to account for the observed motions.

The question has been taken in a narrow ontological sense to refer to the physical existence of partial orbs without consideration of their function. The visible bodies do not move just anywhere or in just anyway within the total concentric sphere, but rather in a defined area and in a particular way. An eccentric orb is not necessarily a physically existing thing; it may refer only to a description of the area within which the object moves, and the same holds for an epicyclic orb. In the latter sense, that is, understood as the restricted area of the total sphere in which the visible body moves, a partial orb is real. The cause of its motion within the sphere is the intelligence that guides it through the defined area of the total sphere. This explanation, I submit, accommodates the agnosticism that he expressed about eccentrics and epicycles in a context that relates to orbs not just circles. He absolutely rejected the equant orb because the equant circle could not possibly be grounded in an orb. The same judgment holds for the lunar double-epicycle model, the lunar crank mechanism, and the reciprocation mechanism for Mercury.

In the midst of all of this detail, I fear that the reader may get lost in the forest, so I return the discussion to the main point and the context of this essay. In carrying out his commentary, I claim, Albert expressed agnosticism about the real existence of eccentric orbs and epicycles, and concluded that while the motion of the Sun is non-uniform about the center of the world, it is uniform around the center of the eccentric.

The distinct motions of the spheres and the visible bodies dictated Albert's answer to the question about the number of total spheres. This principle guided him in proposing the number of orbs for each celestial body. The motion of the Sun requires only three, but the Moon requires three plus an epicycle, later called an 'epicycle orb'. Albert resolved the problem of uniform motion here not by arguing that the motion of the Moon's eccentric is uniform, but that the motion of the orb is regular, and that too refers probably to the complete sphere.[127] He similarly resolved the problem of the planetary equant models by reducing them to imaginary circles that are unrelated to the motion of a real orb.[128]

Albert of Brudzewo, the most competent and important Cracow astronomer of the second half of the fifteenth century, adopted a more agnostic stance on eccentric and epicycle orbs than his predecessors, although he seems to have leaned on them for some of his solutions. For example, he adopted Sandivogius's double-epicycle model for the Moon. To make my

[127] Ibidem, pp. 53-55.

[128] Ibidem, p. 86.

meaning here perfectly clear, however, I am not attributing to Albert a positive denial of the reality of epicycle or eccentric orbs but rather an agnostic view about their physical existence while acknowledging that the visible body moves within a defined area of the total sphere. Among the problems that contributed to his agnosticism is the penetration of orbs. Bi-epicyclic models apparently entail penetration of orbs, and so seem to be mathematical fictions. Models with partial orbs, including bi-epicyclic models, in separate shells avoid intersection, but how do segregated orbs interact with one another?

With Albert we have arrived at the most articulate anti-Averroist critique in Cracow and the most nuanced distinction between mathematical models and the reality of orbs. Albert was driven to this solution very likely by confronting the irresolvable problems deriving from the adoption of eccentric orbs as real. In other words, he expressed doubts about the philosophical rationalizations of Aristotelian natural philosophers but rejected Averroist-inspired homocentric solutions. To make my position clear, I depart from Peter Barker's and Edith Sylla's dichotomy between realism and fictionalism, and propose that the tradition at Cracow led to an intermediate or compromise view.[129]

This brings me, at last, to fold my account into the main theme of the essay. In confronting the dialectic in Polish scholarship, a mathematical tradition tending towards autonomy and a philosophical tradition towards mutual dependence, I suggest a somewhat more nuanced and complicated picture of fifteenth-century Cracow astronomy. There was a decidedly empiricist bent that combined with the emphasis on observation and mathematical modeling provoked a pragmatic distinction between fundamental assumptions and mathematical descriptions and models.[130] Because it was pragmatic, and not theoretically motivated

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[129] Framing these discussions in terms of such a dichotomy has, in my view, become something of a straw man. Even Olaf Pedersen, *A Survey of the Almagest*, Odense 1974, p. 395, acknowledged that Ptolemy's *Planetary Hypotheses* shows that the duty of the astronomer was not only to describe heavenly motions but also to give an account of the physical structure of the universe. He concludes, "that the often mentioned difference between a mathematical and a physical school of astronomers is smaller than we have been used to think." A smaller difference, however, is not the same as no difference at all.

[130] Again, by 'empiricism' I do not mean modern Humean empiricism but an Aristotelian empiricism interpreted in a late medieval nominalist way.

beyond the acceptance of a late-medieval empiricist theory of knowledge, however, it permitted the crossing of disciplinary boundaries, and while arguing for the truth of the fundamental assumptions, it set as a goal the construction of models in conformity with those assumptions. The observational data reveal that intelligences guide and move the visible bodies within restricted areas of the total sphere, hence the task of the astronomer is to construct models that account for such observations. This pragmatic approach combined the possibility of progress towards the goal of constructing a perfectly physical model with a willingness to settle for the best solution in the meantime.

CONCLUSION

Although these developments took a surprising turn in the early sixteenth century, they help us to clarify a number of details about Copernicus's retention of spheres. I have explained his decision elsewhere, but here I focus on a few specific details.[131]

Copernicus understood Albert of Brudzewo's pragmatic interpretation of orbs and Ptolemaic models.[132] Albert, however, did not provide a detailed version of Ptolemy's models, a lacuna that Copernicus needed to bridge. At this juncture we note another puzzle. Regiomontanus's summary of Ptolemaic astronomy appeared in Bologna near the center of homocentric geocentrism. With exception perhaps of some homocentric models, Copernicus seems to have completely ignored or rejected homocentric solutions. How did Copernicus position himself to make the breakthrough?

As early as the *Commentariolus* (ca. 1510), Copernicus expressed himself unequivocally about the inadequacy of Aristotelian and Averroist-inspired homocentric models.[133] He may

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[131] See Goddu, *Copernicus*, pp. 370-384. I add to some of those conclusions in what follows.

[132] Whether he knew the text directly is uncertain, but he certainly heard lectures on it from Albert's students.

[133] See Noel Swerdlow, *The Derivation and First Draft of Copernicus's Planetary Theory*, "Proceedings of the American Philosophical Society", 117, 6 (1973), pp. 423-512. Swerdlow, p. 434, distinguished between Copernicus's rejection of homocentric results, not the principle of homocentric spheres. But the only spheres that could be

have reached that conclusion in Italy, but it is likelier that he followed the tradition at Cracow where he also imbibed his teachers' anti-Averroism. His 'postulate' asserting the difference between the center of the Sun and the center of the universe and his adoption of double-epicycle models for the Moon and all of the planets confirmed his anti-Averroism, his rejection of Aristotelian homocentric solutions because of their inadequacy, and also his partial reliance on predecessors at Cracow. Yet, typical of Copernicus, in *Commentariolus* he proposed an eccentric circle for Earth's path around the Sun with the bi-epicyclic planetary models centered on the center of Earth's path, that is, the planets have a single, common center of motion, and so he retained another remnant of geocentrism, indeed a variant of homocentric spheres. We are also now in a position to be even more precise about his doctrine of spheres.[134]

The total encompassing spheres that move uniformly in circles and carry the planets are not just rarer than the visible bodies but of a different substance altogether. As ontologically superior, they possess the capacity to influence and move what is inferior, and so carry the bodies embedded in them.[135] Copernicus, it is thought, did not and could not have adopted aether as the essence of the invisible spheres, yet in the etymological sense of that word as 'that which runs always', his spheres are 'aether-like'. Alternatively, because of their capacity to carry and move denser bodies, they are analogous to a fire-like, air-like, or fluid substance. But the motions of celestial bodies are natural, meaning that they cannot be driven by some external force, so the relation here must be based on the similarity of forms—spheres are circular such that the circularly moving spheres can carry the visible spherical bodies in circles. It follows that the invisible celestial spheres are *sui generis*, substances that by nature move uniformly in a resistance-less medium and with the capacity to carry and

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homocentric in Copernicus's system are the starry sphere and the total encompassing planetary spheres, not the eccentric or epicycle orbs.

[134] To avoid any possible misunderstanding, his retention of spheres and reliance on predecessors at Cracow should be distinguished from Copernicus's path to a heliocentric cosmology. See André Goddu, *Reflections on the Origins of Copernicus's Cosmology*, "Journal for the History of Astronomy", 37 (2006), pp. 37-53.

[135] 'Ontological superiority' is ambiguous. In Copernicus's case I suggest that it involved the relation between the Sun and the planetary spheres, and between the spheres and the visible bodies.

move dense bodies. The visible celestial bodies are subject to gravity, and at least one of them, Earth, consists of elements clearly different essentially from the invisible spheres. The visible celestial bodies are also spherical, however, so they too possess a capacity for circular motion. While the details of these conceptions remain obscure, the ambiguities and dialectical polarities are analogous to those in the scholastic tradition known at Cracow.

Copernicus departed dramatically from the rationalizations about uniform, regular motions. In this respect his view falls between the supporters of Ptolemaic models and the standard defense of the Aristotelian axiom about uniform, circular motions. Copernicus saw his task as constructing a geometric solution that would surpass the efforts of his predecessors and produce a genuine solution.[136] He believed that by proposing Earth's motions and linking them with the observations that he could account for all of the observed irregularities. The problem turned out to be far more difficult than he realized. His solutions retain the ambiguity we found in Albert of Brudzewo about the reality of the total encompassing spheres as opposed to the geometrical construction of eccentric and epicycle models. Initially, he adopted a homocentric solution for planetary motions other than Earth. When that solution could no longer be sustained, he rejected homocentric models entirely. Copernicus needed to have the visible celestial bodies move on eccentrics and epicycles, yet he did not commit himself unambiguously to the physical reality of eccentric and epicycle orbs, but only to the areas described by the motions of the visible bodies.[137]

It was in the pragmatic tradition at Cracow that Copernicus acquainted himself with the problems that led eventually (which is to say about fifteen years after he left Cracow) to his new cosmological vision. Once he formulated the fundamental assumptions clearly, his goal was to construct the models that fit the best. By his own admission he did not always succeed in discovering a unique solution, developing his own non-Aristotelian version of homocentrism, rejecting that solution in the face of observational refutation, proposing several alternative models, ultimately leaving open the possibility for the emergence of one as the correct solution or the possibility for an entirely new solution.[138] This is not to say that

[136] Compare with Rosińska, *L'École*, pp. 89-92.

[137] Although I depart from some of his conclusions, see the excellent review of the problems in Nicholas Jardine, *The Significance*.

[138] In fact Copernicus assumed mistakenly that he had exhausted the alternatives, and that one of his solutions must be the correct one. Jerzy Dobrzycki, *Astronomiczna treść*

Copernicus was not committed to realism or truth, but rather to say that his achievement was partial, and so to that extent in conformity with the pragmatic empiricism of fifteenth-century Cracow astronomy and natural philosophy. After struggling with Copernicus's assumptions and models, I came to the conclusion that while relying on his teachers and predecessors, he constructed a compromise between a realist cosmology and a pragmatic mathematical adoption of models.[139]

Copernicus used his teachers' and predecessors' ideas, however, somewhat in the manner of Wittgenstein's ladder, as steps to climb up beyond them. Once he climbed up it and achieved his heliocentric vision, he threw the ladder away. Historians have been laboring to reconstruct it ever since.

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kopernikowskiego odkrycia, in: "Mikołaj Kopernik", Lublin 1973, pp. 171-176, also concludes from an examination of his two models for Mercury that Copernicus's ontological commitments were minimal.

[139] I would like to think that Edith Sylla and I agree to the extent that the texts she interprets in a realist way nonetheless play a role in a critical tradition as regards standards of demonstration. For that reason I believe that what she calls 'critical realism' is analogous to and compatible with what I term 'pragmatic empiricism'.