Book Reviews

On Ptolemy as the Greatest Astronomer of Antiquity

A History of Ancient Mathematical Astronomy. O. NEUGEBAUER. Springer-Verlag, New York, 1975. Three volumes. xxiv, 1458 pp., illus. $124.70. Studies in the History of Mathematics and Physical Sciences, 1.

Was Ptolemy a fraud? Are the observations reported by this famed Alexandrian astronomer a hoax?

Allegations that Claudius Ptolemy did not actually observe the celestial positions dating around A.D. 135 and described in his *Almagest* were made originally by the French astronomer Delambre at the beginning of the last century. They have recently been repeated with increasing insistence by R. R. Newton of Johns Hopkins University, who has concluded that "the science of astronomy would be further ahead if Ptolemy had never written the *Almagest*."

A totally different appreciation of Ptolemy is afforded by O. Neugebauer's new three-volume work on early astronomy. The inclusion of the word "mathematical" in the title is deliberate, for Neugebauer eschews the vague, speculative cosmologies of pre-Socratic philosophers. This is not the place to learn about Philolaos or even the Aristotelian spheres. But for Ptolemy, it is the source par excellence.

Divided into six "books," this compendium distills much of a lifetime of scientific research into its three volumes, and it is surely one of the landmark publications of this century in the history of astronomy. Book 1 opens with the *Almagest*, the standard against which both pre- and post-Ptolemaic astronomy of antiquity must be compared. Book 2 summarizes in massive detail Babylonian astronomy, a field that Neugebauer and a few close associates have made their own.

"Egypt has no place in a work on the history of mathematical astronomy," Neugebauer writes in introducing book 3; "Nevertheless, I devote a separate 'Book' on this subject in order to draw the reader's attention to its insignificance." Ten pages later he proceeds to book 4, on early Greek astronomy (but not before recounting a magnificently funny anecdote about the Jet Propulsion Laboratory and Egyptian astrology on p. 566), and 212 pages later he takes up the Roman imperial period and late antiquity to the 7th century A.D. At least half of book 5 is devoted to Ptolemy's minor works, ranging from the *Geography* and *Tetrabiblos* to his *Planetary Hypotheses* and *Handy Tables*. The final volume contains the book of appendices (on chronological, astronomical, and mathematical concepts), a bibliography with at least a thousand entries, over 600 figures, and nine plates.

Ptolemy's accomplishments loom large both at the beginning and at the end of this treatise. Although little is known of the man himself, we can easily imagine Ptolemy surrounded by assistants and graduate students at the famed Alexandrian library. Clearly he had at his command both computational assistance and a considerable library of earlier astronomical observations.

The magnitude of Ptolemy's astronomical accomplishment emerges in the rich fabric of Neugebauer's analysis. Among Ptolemy's greatest achievements were the introduction of the equant and his discovery that the tropical year was constant. The role of the equant in planetary theory (including Copernicus's abhorrence of it) is well known to students of early astronomy; suffice it to say that it is an elegantly simple device that permitted a notable increase in the accuracy of predicted longitudes.

Claudius Ptolemy. [From André Thevet, *Les vrais pourtraits et vies des hommes illustres* (Paris, 1584)]

The constancy of the tropical year—the time required for the sun to return to its same position with respect to the equator—is a more lasting discovery, ranking in subtlety with Hipparchus's discovery of precession (around 135 B.C.). The difficulty of discovering such an apparently elementary fact is revealed by Neugebauer's examination of the Hipparchian eclipse observations, which were sufficiently faulty to obscure this fundamental property of the sun's motion.

Ironically, after Ptolemy had established this constancy, he adopted Hipparchus's numbers for the length of the year and for the seasons. He claims to have observed the time of the equinox in Alexandria, but apparently he merely confirmed that the equinox came at least a day earlier than a strict 365¼-day year would require. Had he gone out two days earlier, he could hardly have missed the fact that the year was even shorter than Hipparchus had guessed.

Both Delambre and R. R. Newton (among others) have claimed that Ptolemy's equinox "observations" are simply extrapolations from Hipparchus. In Newton's estimation, this makes Ptolemy a fraud. Neugebauer, in contrast, passes over this circumstance in silence when discussing the *Almagest*. However, he examines the related problem of precession in some detail in the section on Hipparchus.

Delambre, who questioned whether Ptolemy made any observations at all, argued that the great catalog of over 1000 stars in the *Almagest* had been taken over from Hipparchus, but with the longitudes increased by an erroneous value for precession. (Precession is the slow change in the stellar coordinate system discovered by Hipparchus; Ptolemy set it at 1 degree per century compared to the correct value of 1 degree per 72 years.) Hence Ptolemy's stars have a systematic error that makes their longitudes about 1 degree too small.

Neugebauer brings together convincing evidence to show that Ptolemy's star catalog was quite independent of the earlier, smaller one of Hipparchus, and he further reports that (apart from the systematic error) Ptolemy was the more accurate observer. How was the value for precession bungled? In order to get the zero point of the stellar coordinate system, it is necessary to relate the daytime position of the sun to the nighttime position of the stars, no mean task. According to Neugebauer, the fault must lie largely in the observational technique, which involved measuring the star Spica with respect to the moon during a lunar eclipse. Clearly, the theories of the motions of the sun and moon are tightly bound up in this procedure. Since Ptolemy remarks that Hipparchus's eclipse data were seriously marred, Neugebauer points to this as the crux of the problem.

Neugebauer writes,

> In all ancient astronomy direct measurements and theoretical considerations are so inextricably intertwined that every correction at any one point affects in the most complex fashion countless other data, not to mention the ever present numerical inaccuracies and arbitrary roundings which repeatedly have the same order of magnitude as the effects under consideration. In the history of the most causal of all empirical sciences, in astronomy, the search for causes is as fruitless as in all other historical disciplines.

The difference in attitude between Neugebauer, a mathematician who has immersed himself in the languages and techniques of ancient science, and R. R. Newton, a physicist who is eager to extract specific results on the deceleration of the earth's rotation, is shown perhaps most clearly in their respective reexaminations of the lunar eclipse of A.D. 135.

According to Ptolemy's epicyclic model, the moon approached twice as close to the earth at quarter phase as when new or full, a situation clearly in conflict with the simplest observations. Ptolemy not only chose to ignore this untenable discrepancy, but in determining the lunar distance he picked the time of closest approach. The result was badly wrong, 40 earth radii instead of 60; nevertheless this apparently confirmed a model that had quite satisfactory distances for eclipses. The erroneous answer at quadrature, which fit so well with all the rest of the theory, was achieved by reporting a lunar position off by 2/3 of a degree.

Did Ptolemy forge this observation, as R. R. Newton would have us believe? Or do we just have here evidence of "uncontrollable" observational and "quite unnecessary" trigonometric inaccuracies yielding "one of the most unsatisfactory topics in the whole Almagest"?

In my own opinion, Newton's attack on Ptolemy is at the very least anachronistic. Ptolemy, using clumsy mathematics invented only a generation earlier, made possible for the first time calculations of the local circumstances of solar eclipses. His planetary theory allowed tolerably accurate predictions to be made for over a millennium. It is hard to imagine that such success rested on fabricated observations. It is nevertheless possible that, in those days before error theory was understood, selected observations were adjusted for pedagogic purposes and thus recorded in the *Almagest* in close agreement with the theory—a theory undoubtedly resting on far more data than Ptolemy specifically reports.

In summarizing the section on lunar parallaxes Neugebauer gives both an evaluation of Ptolemy and an effective appreciation of his own *History*:

> No ancient astronomer had any possibility of analyzing sources of errors in observations made long before his time or at far distant localities. It makes no sense to praise or to condemn the ancients for the accuracy or for the errors in their numerical results. What is really admirable in ancient astronomy is its theoretical structure, erected in spite of the enormous difficulties that beset the attempts to obtain reliable empirical data.

He goes on to say, about Ptolemy,

> Without the cinematic theories of the Almagest it would have been impossible to introduce, on the basis of better observational techniques, those improvements which found their explanation in Newton's celestial mechanics.

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Changes in Biology

Crystals, Fabrics, and Fields. Metaphors of Organicism in Twentieth-Century Developmental Biology. DONNA JEANNE HARAWAY. Yale University Press, New Haven, Conn., 1976. x, 232 pp. $15.

This is a curious and unusual book. The author argues that in biology a revolution, in Thomas Kuhn's sense, occurred in the early portion of this century when the mechanistic paradigm "and its associated metaphors" of Jacques Loeb and others were "successfully challenged" by a "nonvitalist organicism." What is meant by the latter is the idea that, however analytical and dissecting one's experiments, one should not forget the whole live beast. To substantiate her argument the author has chosen to discuss in detail the works and the views on the philosophy of science of Ross G. Harrison, Joseph Needham, and Paul Weiss.

A major part of the book contains excellent reviews of the biological life work of these three distinguished scientists. The case for the attribution of organicist views is clear with respect to Needham and Weiss, but less so with respect to Harrison. In fact, on p. 95 the author shows that Harrison rejected the "emergence" of Lloyd Morgan and the "holism" of Jan Christian Smuts. But these, by a subtle distinction that is discussed in some detail at various points in the book, are considered vitalistic forms of organicism. Harrison rates as an organicist because of his profound concern for problems of polarity and symmetry, the basis of many of his beautiful experiments.

My difficulty is that I am neither a historian nor a philosopher of science, and feel rather like a swine unable to appreciate the shower of pearls. As a working biologist and an amateur admirer of Thomas Kuhn's book, I would not have thought of the shift from Loeb's mechanism to the generally accepted organicism of the 1940's as a scientific revolution. In the modern history of biology I would have selected only three genuine revolutions: Darwin's conception of natural selection, Mendel's genetics and its marriage to cytology, and molecular genetics. My simple-minded interpretation of the whole cycle of events from the 19th century onward involving vitalism, mechanism, organicism, reductionism is that these changes are a rather crude reflection of the fact that biologists go through periods when they think they know everything (mechanism, reductionism) and others when they react against the simplicity of such views and stress that there is more to the problem than meets the eye (vitalism, organicism). All this is really an indication of our anxieties, or lack of them, about our progress.

At any one time, the experimentalist carries on regardless of the prevailing (or his own) optimism or pessimism about our ultimate understanding of living systems. A splendid example is given in *Crystals, Fabrics, and Fields*. When Hans Driesch discovered regulation in sea urchin embryos in 1891, he was so convinced that no machine could behave in such a way that he turned to vitalism. I do not share the author's view that this was the beginning of a new paradigm (at best it introduced a miniparadigm), but think instead that Driesch did not know so much about machines as we do today. It is true that because of the extraordinary success of molecular biology and genetics we are now in a period of confidence that sharp progress in the analysis of mechanisms of development is at hand. But I predict that in some years to come it will be clear to everyone that although we have made progress (in the manner of