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A LUNAR OCCULTATION OF MARS OBSERVED BY ARISTOTLE

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1. *Introduction*

In his *De caelo*, Aristotle cites an observation that he made of an occultation of the planet Mars by the Moon. He remarks that this provided direct evidence that the planets were further from the Earth than was the Moon. Reports of astronomical observations made by Aristotle are indeed rare, and it is of some interest to derive the date of this event and to investigate the reliability of his observation. His account (*De caelo*, 292a) may be translated as follows: "For we have seen the Moon, half full, pass beneath the planet Mars, which vanished on its shadow side and came forth by the bright and shining part."[1]

No hint of the date when this event occurred is given by Aristotle. Evidently its cosmological significance was of more direct concern to him. Aristotle's main biographical details are well known. Born in Stagira (Macedonia) in 384 B.C., he was sent to the Academy of Plato at Athens in 367 and remained there for twenty years. After the death of Plato in 348/7, Aristotle entered on a period of travel, lasting for twelve years. During this interval he resided in several Greek cities around the Aegean, before returning to Athens in 335 B.C. Here he remained until 323, dying in Chalcis the following year.

Attempts to date this observation by astronomical calculation originated with Kepler. In his *Astronomia nova*, published in 1609, Kepler deduced a date of 4 April 357 B.C.[2] This result does not seem to have been questioned until the present century. In 1927, Schoch amended this date by one month to 4 May 357 B.C.[3] In 1970, Longrigg[4] published a brief note stating that the present author had determined "by a computer analysis of all the conjunctions of the Moon with Mars when the planet was between 75 and 105 deg east of the Sun ... that only one such occultation took place between 367 and 322 B.C., which was visible in Greece or around the Aegean. This occurred on 4 May 357 B.C. between 19^h40^m and 20^h15^m Athens mean time". My derived date was identical with that deduced by Schoch.

2. *Computations*

Determination of the local circumstances of an ancient occultation requires fairly accurate knowledge of changes in the Earth's spin rate as well as an accurate ephemeris for the Moon and planet. Due to irregularities in the Earth's rate of spin (caused mainly by the tides), the Earth's rotational clock error, ΔT, amounted to several hours in the fourth century B.C. In recent years, there has been much development in our knowledge of Earth's past rotation,[5] and improved ephemerides[6] have become available. The aim of this note is to update my 1970 analysis and also

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to investigate the accuracy of Kepler's result.

I have adopted a slightly enlarged date range from that adopted in my previous investigation: from 370 B.C. (when Aristotle was aged 14) to his death (at age 62) in 322 B.C. I have made all the computations for Athens (37.98°N, 23.73°E). During the selected interval, Aristotle spent most of his time at Athens and in his period of travel he was never more than about 250km from the city. I have also widened the range of elongations of the Moon from the Sun (the range now being from 70° to 110° east of the Sun). Aristotle asserted that the Moon was half full when the occultation occurred. The fact that the planet Mars was seen to vanish on the shadow side of the Moon and reappear at the bright edge implies that the observation was made during the evening when the Moon was near first quarter.

The phase of the Moon (P) for a given elongation from the Sun (E) is given by:

$$P = 0.5\ (1 - \cos E).$$

Hence when the Moon is 70° from the Sun only 0.33 of the Moon's disk is illuminated, while for an elongation of 110° the corresponding fraction is 0.67. Phases below and above these limits could scarcely be described as "half full".

For my computations I have utilized the planetary ephemeris of Bretagnon,[7] and a version of the lunar ephemeris j=2, adapted for a lunar acceleration of –26 arcsec/cy/cy.[8] I have used the results of Stephenson and Morrison[9] for the Earth's rotational clock error ΔT. These yield values for ΔT ranging from 14900 sec (= 4.14 hours) at 370 B.C. to 14300 sec (= 3.97 hours) at 322 B.C. Both clock errors are substantial.

3. *Results*

Of some 70 conjunctions of the Moon with Mars that occurred during the selected interval from 370 to 322 B.C, and with the prescribed conditions, I have computed that only on a single occasion was an occultation visible in or near Athens. This event (see Figure 1) occurred on the evening of 4 May 357 B.C — the date derived by Schoch, as quoted above. My computed results are as follows:

As seen from Athens, Mars would be occulted at the dark limb of the Moon at a local time of close to 20^h00^m and emerge at the bright limb around 21^h15^m. The elongation was 83° east of the Sun, so that the Moon would be almost half full (phase 0.44). During the occultation, the Moon would be high in the sky (altitude at the start, 58°; at the end, 44°). The occultation began about 1^h15^m after sunset so that the sky would be fairly dark even when Mars reached the dark limb of the Moon (solar depression below the western horizon 13° at the start, 25° at the end). Mars, magnitude +1.0, should have been clearly visible. As shown in Figure 1, the planet would appear to pass a little below the centre of the Moon. The above circumstances fit the description by Aristotle extremely well and I have no hesitation in accepting 4 May 357 B.C. as the correct date.

On the date calculated by Kepler (4 April 357 B.C), no occultation of Mars

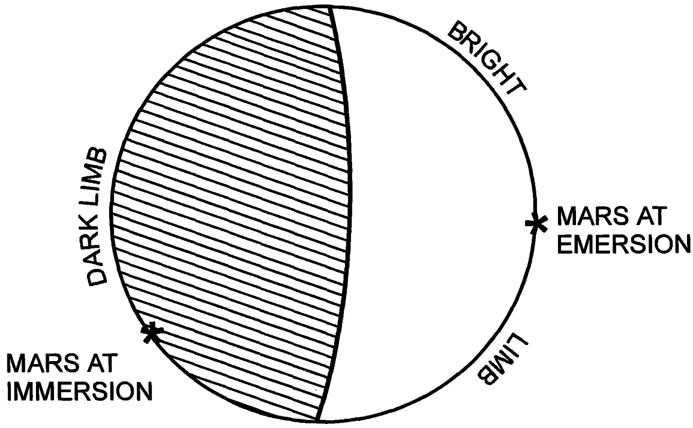

FIG. 1. Occultation of the planet Mars by the Moon as seen from Athens on the evening of 4 May 357 B.C.

would be visible (at least from Athens). On this occasion, closest approach between the Moon and planet occurred during daylight — at about 11^h. At dusk, at about 19^h (by which time the Sun was some 8° below the horizon), the Moon would have already passed approximately 3° to the east of the planet. Nevertheless, Kepler deserves commendation for deriving (albeit fortuitously) a date only one month in error.

Conclusion

Computation of the occultation of the planet Mars by the Moon recorded by Aristotle in his *De caelo* demonstrates that he gave a careful description of the event — enough to enable a unique date of 4 May 357 B.C. to be derived. Aristotle, then a young man aged 27, was midway through his first period at the Academy in Athens. Unfortunately, the above result does not help us to date the writing of *De caelo*.

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ESSAY REVIEW

WRITING, AND READING, *PRINCIPIA*

Reading the Principia*: The Debate on Newton's Mathematical Methods for Natural Philosophy from 1687 to 1736.* Niccolò Guicciardini (Cambridge University Press, Cambridge, 1999). Pp. v + 285. $80.

Despite what one might infer from its main title, this book does not undertake to help the modern reader with the task of following Newton's exposition. What it does do, richly, importantly, and usefully, is to trace the implications of Newton's approach in *Principia* and the debate among the mathematicians and natural philosophers of Newton's time about his methods and conclusions.

Guicciardini explores the plurality of mathematical methods used in *Principia* and traces how they were interpreted and understood by Newton, his contemporaries, and those immediately after him. "The combatants debated issues whose content is worth considering", Guicciardini says. "The cluster of problems addressed which concern the *Principia*'s mathematical methods is particularly interesting. How should natural philosophy be mathematized? Is it legitimate to use in this context uninterpreted symbols? Can we depart from the established Archimedian or Galilean/Huygenian tradition of geometrizing Nature? What is the value of elegance and conciseness? What is the relation between Newton's geometrical methods of the *Principia* and calculus? What is the advantage of using the latter?"

The book examines reactions and discussions from the publication of *Principia* (1687) to the publication of Euler's *Mechanica* (1736), after which the author considers Newton's geometrical methods of presentation "past and obsolete", fully replaced by analytical representations of Newton's results.

In Part 1, Guicciardini reviews the mathematical style and methods of *Principia*. He looks at how and why Newton chose to use a geometric calculus in *Principia* instead of an analytic (algebraic) calculus of the sort he himself had championed and helped develop earlier. The author examines the places where Newton refers to his calculus of quadratures as having provided the proof needed, and identifies the places where Newton actually uses his analytical calculus. In Part 2, he devotes a chapter each to Newton, Huygens, and Leibniz, tracing their responses to *Principia*. He shows how Newton's commitment to that approach to solving mathematical problems actually solidified in the years after publication of *Principia*, while Leibniz and his followers were following a different path with the differential calculus. In Part 3, he studies the two schools that emerged during the period of this study, the British Newtonian school and the Continental Leibnizian.

Guicciardini's discussions of the differences between the synthetic and the analytic approaches to solving problems of dynamics and his explanation of theory of proportions and its use by Newton are particularly illuminating. Newton's objections to symbolic notation were serious and deeply held. Guicciardini shows that in the 1670s Newton began moving away from his own early analytical calculus after reading works of Pappus and Apollonius and comparing algebraic and geometrical solutions to the same problems. He then began developing the geometrical method based on limits, which he used almost exclusively in *Principia*. This refusal to use his own analytical fluxional method (in Leibnizian terms, the differential and integral calculus) Guicciardini finds very striking, calling it "one of the most spectacular processes in the history of mathematics".

In rejecting the analytic, symbolic method, Newton did not align himself with the winning side. Almost immediately his demonstrations in *Principia* began being translated into symbolic notation, and, as Guicciardini points out, after 1736 almost no one even tried to read Newton's proofs as he wrote and presented them, reading instead analytic retellings in Leibnizian symbolism.

Why would Newton have done this? One might want to rule out stupidity, but perhaps one might want to accuse him of perversity, or of eccentric antiquarianism. Guicciardini refutes the belief that Newton used geometrical methods because he was unable to solve the physics of central forces using calculus. In fact, he could write differential equations of motion to handle central forces. (He could not solve all the problems; but neither could the Leibnizians; some problems could only be solved at that time using qualitative geometric techniques.)

Guicciardini backs us up to consider what Newton was thinking in choosing his geometric methods. He does cite Newton's belief in the ancient wisdom, but the epistemological arguments he presents are sufficiently persuasive without that consideration.

Newton's demonstrations in *Principia* were synthetic geometrical proofs (similar to those of the ancients but using his theory of limits to make the application to motion and forces) instead of proofs using his own analytic method of fluxions. He used the theory of proportions of the ancients as opposed to algebraic manipulations and the analytical geometry of Descartes. Geometrical proofs and proportion theory are not just more cumbersome ways to do the same thing. There are implications and consequences to the choices, Guicciardini shows, and these are the differences that led Newton and Leibniz, and their followers, to different approaches in their published works.

"It was easy, [Newton] claimed, to revert the synthetic demonstrations to the analytical method of fluxions. On the other hand, Newton did not want to define the synthetic methods of the *Principia* as equivalent to the analytical method of fluxions. In his opinion, the choice between the analytical method and the synthetic method of fluxions was not merely a question of language or of presentation. Newton was convinced that his geometrical mathematical way was superior to Leibniz's reliance on algorithm."

In using his synthetic style, Newton avoids blindly manipulating symbols. These symbols may have gone into the calculation as something with referential content, and may be again interpreted at the end as referring to something in the world, but during the algebraic manipulations they, and the terms incorporating them, have lost meaning and connection to any physical entities.

Using a geometrical demonstration allows one to see what is happening. This is true at a deeper level than the obvious one that a path is traced on paper analogous to the one traced by the body in the heavens. The lines representing magnitudes correspond to real distances in space, and so in following the proof we remain always clear what we are talking about.

Similarly, using proportion theory (which Guicciardini explains at length), one maintains clarity about the interpretation of each magnitude as it is set in ratio with another of the same kind, rather than reducing the relationships to numbers. Further, one does not move even farther away from interpretability by setting magnitudes of different sorts (distance and time, for example) into ratio and then turning that ratio into some sort of number (a quotient).

Guicciardini contrasts this with the approach of Leibniz and his followers. "The calculus, according to Leibniz, should be seen as an *ars inveniendi*: as such it should be valued by its fruitfulness, rather than by its referential content.... Newton could not agree: for him mathematics devoid of referential content could not be acceptable.... Newton required different standards of rigour: according to him, interpretability could not be abandoned in 'good geometry'. Newton didn't value mechanical algorithmic reasoning.... Newton understood his method as more than a mere heuristic tool: geometrical interpretability guaranteed ontological content."

These views were shared by Newton's followers in the English school. They insisted on representability of mathematical symbols and distrusted algorithmic techniques. This resistance to the algorithmic was present on the Continent too. "The calculus appeared to many [at the beginning of the eighteenth century] as unfounded, uninterpretable, not powerful enough."

Newton is not only at pains in *Principia* to avoid using algebra and analytic calculus and the making of numbers out of ratios, but he significantly and consistently formulates his proofs in terms of relationships: relationships between forces, between distances, between times, between velocities, between *latera recta,* or between quantities of the same sort as a limit is taken. He chooses not to set any force or distance or time or other quantity absolutely equal to some other magnitude or factors.

The conversion, now automatic, of Newton's presentation into equations abandons and loses a crucial element of his thinking about these matters. This is fine for a modern physics textbook, which is concerned only with the usefulness of the physical conclusions, but any such translation ought to be handled with the greatest care and sensitivity by any historian of science. Guicciardini's book helps the historian of science appreciate some of what is lost in reworking *Principia* symbolically.

It was not only Newton himself who adhered to proportions. "The fact is that in Newton's times mathematicians thought in terms of proportions, even when they wrote formulas which look very much like equations", Guicciardini points out. And, indeed, although there are some places in *Principia* in which Newton writes things that look like equations, one can go astray if they are taken to be something other than proportions in shorthand. That they are almost always the latter can be seen from a careful following of his unfolding of the argument over the sequence of propositions. It can also be seen from problems arising from the supposition that the proof has abandoned proportion theory and its relationships of forces, distances, etc., at different points on the orbit (or sometimes in different orbits). This can and sometimes does lead to absurdities or fallacies.

Guicciardini in his own presentation of Newton's demonstrations is careful to avoid changing Newton's casting of the proof in proportion theory into equations. "In my commentary I adhere almost entirely to Newton's notation." When he adopts an equation for simplicity, he reminds us in a footnote that this was a proportion for Newton. Although he generally uses the Cohen-Whitman translation of *Principia*, he makes a point of departing from some mathematical modernizations in that translation, "since in my book the exact form of the various mathematical styles must be rendered as closely as possible".

He points out that Propositions 6–13 of Book I, based on first and ultimate ratios, do not lend themselves to an easy translation into symbolic terms. Putting them in terms of the differential calculus was considered by Leibniz and his followers to be a major research project. Scholars who have treated them as easily convertible to algebra have often fallen into fallacious reasoning. This has also happened with Proposition 1 of Book I, which Guicciardini mentions as another example of a geometrical procedure not easily translated into analytical form.

When translation of Newton's presentation into symbolic calculus is done, even when done without introducing fallacies or misinterpretations, the calculus demonstrations are considerably different from the proof as Newton presented it. "This translation often spoils the evidence and informative content of Newton's geometric proofs", Guicciardini writes.

The author has organized this book very well. He is considerate in guiding the reader through what he is going to argue and what he has argued. The book is enjoyable reading. Guicciardini creates a sense of excitement as he follows the story. There is much here that is either unknown or ignored in too many discussions of *Principia* and its proofs. *Reading the Principia* provides an historical foundation for discussions of Newton's treatise on which Newtonian scholars and others who deal with Newton would do well to build their work.

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