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An Interesting Property of the Equant

A Database for the British Neptune-discovery Correspondence

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Galileo's Observations of Neptune

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This informal narrative will describe my discovery that Galileo had seen the planet Neptune in 1612, over two centuries before Neptune was "officially" discovered. I am not a historian, and my discovery had little or no astronomical importance. Nevertheless, this was one of the most exciting and rewarding experiences of my life. I found something obvious, that historians had overlooked for over three centuries! I hope you will find this story interesting and amusing

A little background

I spent most of my career as an observational astronomer. Mostly, I used the 48-inch Schmidt telescope at Mt. Palomar to perform various surveys. My main job was to search for supernovae, but my real passion was to search for unusual objects in the Solar System – comets, Apollo asteroids, and whatever else might be out there. The 48-inch telescope was ideal for Solar System surveys, although few people actually used it for that purpose. I recovered lost asteroids and comets, and found new ones. In 1974 I discovered the 13^{th} satellite of Jupiter, (**Leda**), and another one the following year. Encouraged by these successes, I started a full-scale survey of the ecliptic region in 1977. This promptly led to my discovery of the object **Chiron** in November of that year.

In the back of my mind during this survey, was the possibility of discovering a new planet. The only evidence that such a planet might exist were the unexplained residuals in the orbit of Neptune. Of particular interest to me was an article by Dennis Rawlins in the *Astronomical Journal.* [Astron. J. 75, 856−857 (1970)]. Rawlins described some pre-discovery observations of Neptune by Lalande in 1795. Lalande's residuals were small, but apparently significant. Clearly, more pre-discovery observations, especially even earlier ones, would be most interesting. I was on the lookout!

The occultations

The March 1979, issue of *Sky and Telescope* magazine contained an excellent article by Steve Albers, which listed mutual occultations of planets for the years 1557 to 2230. Among these occultations were two of Neptune by Jupiter, in January 1613, and September 1702. Aha! The telescope is in wide use by 1702, but who was watching Jupiter in 1613? Galileo, and no one else. Albers' article gave me the raw material I needed to search for a pre-discovery observation of Neptune. [In his article, Albers specifically mentioned that his computed occultations could be used to find pre-discovery observations of planets. At the time, I thought this was obvious, but I was subsequently criticized for not giving him credit for that insight. I scrupulously gave him credit for his occultation calculations, but *Sky and Telescope* implied that I gave him no credit at all! Let me make it plain that my subsequent work would not have been possible without the work of Steve Albers.]

The search

I did not know if Galileo's notebooks had ever been published in their entirety. Nor did I know how extensive they might be. To find out, I went to the Hale Observatories library, and talked with Dr. Alexander Pogo – one of the most fascinating people I ever met.

Dr. Pogo was an astronomer and classical scholar, born in Russia in 1893. He talked very little about himself, but rumors of his past exploits abounded. It was said that he had helped rebuild the Parthenon in Athens. At the time I did my search he was 87 years old. He looked exceedingly frail, but he delighted in climbing ladders while onlookers held their breath. (Years later, Dr. Pogo did fall down and break his hip. He died in 1988, at the age of 95.) Pogo knew every book in that library, as well as the publication history of many of them. When I asked Dr. Pogo about Galileo's notebooks, he gave me a long account of the various editions that had been published. As it happened, the Hale Observatories library had a copy of an edition published in Italy in 1909. (This book was on a top shelf, giving Dr. Pogo another opportunity to show off his ladder-climbing skills.)

It turned out that the notebooks were indeed extensive, and contained hundreds of drawings of Jupiter and its satellites, sometimes including background stars. It was a simple matter to look at the drawings for the days around the occultation.

The outcome

I was able to compute the positions of Jupiter and Neptune for each day in December 1612, and January 1613. I simply plotted those positions, and compared my plots with Galileo's drawings. The result was stunning. Galileo had seen Neptune at least three times.

The first candidate was on a drawing from December 28, 1612. Galileo had marked a "fixed star" near Jupiter. Upon checking the SAO star catalog, it became clear that there was no star in that position, but it did match my plotted position of Neptune. I was beginning to get excited.

The next candidate was another fixed star Galileo plotted five days later. This turned out to be SAO 119234. Finally, I found the Holy Grail on the drawing for January 28, 1613. On that night Galileo drew two stars near Jupiter. Star 'A' was SAO 119234 again. Star 'B' was Neptune! Galileo made a separate drawing showing just these two stars. He also wrote a comment in Latin, which I translated as: "After fixed star A, following in the same line, is star B, which I saw in the preceding night, but they then seemed farther apart from one another". Not only had Galileo spotted Neptune, he even noticed that it had moved from night to night!

My initial reaction was disbelief. It was all so easy, and it seemed impossible that historians had studied Galileo's notebooks for over three centuries and had never noticed these observations. Just to reassure myself, I contacted Stillman Drake, who was one of those Galileo historians. Drake became as excited at I was, and provided me with much information about Galileo's measurement techniques. He also confirmed my translation of the Latin note which indicated that Galileo had seen Neptune move from one night to the next. We agreed to write two papers about this discovery. I wrote one for *Nature,* [*Nature,* **287,** 311] and Drake wrote one for *Scientific American* [*SciAm*,**243**,52].

The aftermath

Galileo did not indicate the scale of his drawing of stars A and B, but I still had hope of using it for astrometric purposes. All I could do was to speculate that this drawing had the same scale as the main drawing of Jupiter and its satellites. Stillman Drake was particularly eager to move Neptune around like an old piece of furniture, and I was still optimistic, so we published our speculations about Neptune's position, particularly in the *Scientific American* article. Myles Standish and Dennis Rawlins quickly pointed out that our derived positions of Neptune were impossible, since they would have moved Neptune out of its known orbital plane. No surprise there, but it did make the *SciAm* article somewhat tainted.

The article in *Nature* was badly chopped-up in the editing, and it was preceded by a ridiculous and error-filled preface by the editor. Nevertheless, the article did, I think, prove that Galileo saw Neptune. Reaction in the popular news media tended to be on the order of: "Galileo May Have Seen Neptune", "Did Galileo See Neptune?", or even "Galileo's Mistaken Discovery"! By not accepting my work at face value, the media showed an admirable restraint which I can only wish they would show when reporting the latest medical "breakthrough".

Later, I continued to look through Galileo's notebooks for mentions of a "*stella fixa"*. I identified several cataloged stars, but there were a few objects that I could not identify. It is entirely possible that Galileo saw some of the brighter asteroids, but I did not pursue this. Those of you who are looking for something to do might look into this potential gold mine!

In 1982 I traveled to the Royal Greenwich Observatory, then at Herstmonceux Castle in East Sussex, England, to look at Flamsteed's notebooks from 1702. I found no evidence that Flamsteed or his colleagues had ever seen Neptune during that year's occultation by Jupiter. Of course, there were other observatories operating in Europe in 1702, and somebody, somewhere, may have seen Neptune in that year. This, too, is something that others might want to pursue.

Statistical Dating of the Phenomena of Eudoxus

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In about 275 B.C. Aratus wrote *Phenomena*, a poem describing, among other things, the arrangement of the constellations relative to each other and relative to the principal circles on the celestial sphere: the equator, the northern and southern tropics, the ecliptic, and the arctic and antarctic circles.[1] We know from the extensive *Commentary* of Hipparchus, *ca.* 130 B.C., that Aratus' main and possibly exclusive source was Eudoxus, who in about 370 B.C. wrote two books, *Phenomena* and *Mirror*, giving essentially the same descriptions that we find in Aratus, plus some additional material – principally lists of constellations on the colures and arctic circles – that we know only through Hipparchus.[2]

We therefore know that Eudoxus had a fully developed conception of the celestial sphere. He understood the importance of the celestial poles and the celestial equator, and that the path of the Sun – the ecliptic – is a circle inclined to the equator. He understood the tropics as the circles parallel to the equator that touch the ecliptic at its most northern and southern points – the solstices, and when the Sun was at a solsticial point, he knew the fraction of the circumference of a tropic above and below the horizon. He understood the colures as circles through the celestial poles and the solsticial points, and through the celestial poles and the equinoctial points, the points where the equator and ecliptic intersect. He understood that the solsticial and equinoctial points are precisely a quadrant apart on both the equator and the ecliptic, and that the two colure circles intersect at right angles at the poles. He understood that stars above the arctic circle are always above the horizon, and hence always visible on any night, and that stars below the antarctic circle are never above the horizon. Eudoxus describes the zodiac as a band bisected by the ecliptic, and he names the sequence of twelve zodiacal constellations that we still use today. Furthermore, he names constellations, and usually specific parts of

[1] D. Kidd, *Aratus Phenomena*, (Cambridge, 1997).
[2] I am using the English translation of Roger MacFarlane (private communication) with the assistance of Paul Mills. Until this is published, the interested reader must use Hipparchus, *In Arati et Eudoxi phaenomena commentariorium*, ed. and transl. by K. Manitius (Leipzig, 1894), which has an edited Greek text and an accompanying German translation.

constellations, that lie on the major celestial circles. Eudoxus is thus the earliest surviving source that describes the fully developed celestial sphere and, what is most important for our considerations, the relation of those circles to the constellations.

We may also infer, with at least some level of confidence, what Eudoxus did not know, or saw no reason to mention. It seems unlikely that he gave values for the height of the celestial north pole above the horizon or for his geographical latitude (which would be the same numbers, of course). Likewise, it is unlikely that he gave any information about the size of the arctic or antarctic circles. It also seems unlikely that he gave a value for the obliquity of the ecliptic, even a round number such as 24° or 1/15th of a full circle. It seems equally unlikely that he gave any actual numbers characterizing the position of constellations or stars relative to the principal celestial circles, or that he imagined any coordinate system of any kind beyond the circles already mentioned. Indeed, it seems unlikely that he gave any direct measures of position whatsoever, since while Hipparchus left us plenty of his own numbers in his *Commentary*, he does not mention any position numbers from Eudoxus.

Instead, what seems to have concerned Eudoxus more than quantitative spatial measurements are temporal relations, both daily and annual. For example, he gives two ratios for the length of longest day to shortest day (5/3 and 12/7). While Hipparchus knew that these can be used to specify geographical latitude, it is not clear that Eudoxus knew that. Eudoxus gives the constellations that rise and set simultaneously with the rising of each zodiacal constellation for the stated purpose of knowing when to expect sunrise. We know from the Geminus parapegma that Eudoxus tabulated the dates of heliacal rising and setting for several bright stars, and he gives the dates in that calendar for autumn equinox and winter solstice.[3] Indeed, it seems most likely that Eudoxus' interest in the tropics and equator was prompted mostly from observation of the annual north-south excursion of the rising and setting points of the Sun on an arc along the eastern and western horizons.

What we may know even less about is when and where Eudoxus' model of the celestial sphere was developed, and who developed it. Presumably some astronomical observations were made that underlie the information in Eudoxus' books, and if we could somehow assign a reliable date, or even a range of dates, to those observations, we would at least know that the celestial sphere was developed no earlier than the observation dates. It is therefore of some interest to use the information from Eudoxus to try and assign dates and possibly locations to the underlying observations.

[3] G. Aujac, *Geminus Introduction to the Phenomena* (Paris, 1975).

Dating a Star catalog

Before we perform a statistical analysis to date the phenomena of Eudoxus, it will be useful to review a simpler problem: the statistical analysis required to date a star catalog. For simplicity, let's assume we have a catalog listing a set of ecliptic longitudes and latitudes for known stars. Since the longitudes change with time due to precession, we can attempt to date the catalog by comparing the catalog longitudes L_i with the theoretically computed longitudes $\lambda_i(t)$. Assuming that the errors in the catalog longitudes,[4]

$$L_i - \lambda_i(t) = \varepsilon_i$$

are normally distributed with variance σ^2 and mean zero, *i.e.* $N(0,\sigma^2)$, then we can find the best fit time t_{min} by minimizing

$$\chi^2 = \sum_{i=1}^{N} \frac{(L_i - \lambda_i(t))^2}{\sigma^2}$$

Naively, and as we shall see incorrectly, the uncertainty σ_t in the determination of t_{min} can be determined from

$$\chi^2(t_{\min} \pm \sigma_t) - \chi^2(t_{\min}) = 1$$

and is approximately

$$\sigma_t^2 = \frac{\sigma^2}{p^2 N}$$

where p is the precession constant (about 1.4° per century). It is clear that the size of σ_t can be made smaller and smaller by using more stars N.

There is another easy, and equally naïve and incorrect, way to determine the uncertainty σ_t, and that is to use a Monte Carlo simulation. Having determined t_{min} as above, we simply construct a large number of new pseudo-catalogs, perhaps 1,000 or more,

[4] In practice, it is necessary to weight the errors by the cosine of the latitude of each star.

$$\{L_i' = \lambda_i(t_{\min}) + \varepsilon_i',\ i = 1...N\}$$

where the ε_i' are $N(0,\sigma^2)$. Then for each set we minimize χ^2 and determine $t_{\min}'$. The standard deviation of these $t_{\min}'$ values will be an estimate of σ_t.

Two final notes:

(a) All of the above analyses assume that the errors are *uncorrelated*, *i.e.* while $< \varepsilon_i^2 > = \sigma^2$, we must also have $< \varepsilon_i \varepsilon_j > = 0$ (where <..> is the usual statistical expectation value).

(b) In the Monte Carlo it is essential that each of the pseudo-catalogs be *possible* sets of observations, even though none is the same as the observed catalog. Another way of saying this is that each set of errors *could* have been the set given by the author, and has the same statistical distribution as the set the author did give.

As it happens, and as discussed below, both of these conditions lead to severe problems when computing the uncertainty in the date naively.

The Effect of Calibration Errors in the Star Catalog

Now let us suppose that the original observer had a calibration error in his measurements, *i.e.* he misplaced his zero-point in longitude with respect to the theoretically correct point that we are using for the λ_i*'s*. In practice, such an error is guaranteed to happen, of course. Thus we would have

$$L_i - \lambda_i(t) = \varepsilon_i + \eta$$

where we can assume that η is $N(0,\sigma_c^2)$, but uncorrelated with the ε_i, so $<\varepsilon_i\eta> = 0$. Now σ_c is the uncertainty in the observer's determination of the zero point in longitude, which for all practical purposes is equivalent to how well he knows the *position of the Sun relative to the stars* on the day of some cardinal event, *i.e.* an equinox or solstice. Since σ^2 is the variance in the positioning of the stars themselves, then it seems the most reasonable assumption is that σ_c should be at least as big as σ. In any event, it is clear that in the presence of a calibration error the errors in $L_i - \lambda_i(t)$ are *correlated*, and the analyses outlined above must be done differently.

We first compute the covariance matrix of the errors. The diagonal terms in this matrix are just

$$V_{ii} = <(\varepsilon_i + \eta)(\varepsilon_i + \eta)> = \sigma^2 + \sigma_c^2$$

while the off-diagonal terms, which are of course zero in the case of uncorrelated errors, are

$$V_{ij} = <(\varepsilon_i + \eta)(\varepsilon_j + \eta)> = \sigma_c^2$$

Now we have to minimize

$$\chi^2 = \sum_{i=1}^{N} (L_i - \lambda_i(t)) V_{ij}^{-1} (L_j - \lambda_j(t))$$

which clearly reduces to the familiar case when V is diagonal. The uncertainty σ_t in the determination of t_{min} can still be estimated from

$$\chi^2(t_{\min} \pm \sigma_t) - \chi^2(t_{\min}) = 1$$

and is now approximately

$$\sigma_t^2 = \frac{\sigma^2}{p^2 N} + \frac{\sigma_c^2}{p^2}$$

Contrary to what we found assuming uncorrelated errors, it is clear that now σ_t cannot be smaller than σ_c/p, and so can *no longer* be made arbitrarily small by using more stars N. In fact, if σ_c is about the same size as σ, then the final uncertainty in $p\sigma_t$ cannot be smaller than the uncertainty in the longitude of a *single* star.

Dating the Phenomena of Eudoxus

Now let us now suppose that instead of a star catalog, which of course gives in one way or another the author's determination of the positions of a collection of stars, we have the statements of Eudoxus, which have come down to us in two ways: first, through the collection of what are apparently direct quotes from Eudoxus by Hipparchus in his *Commentary to Aratus and Eudoxus*, and second, through the poem of Aratus, which according to Hipparchus is a fairly accurate paraphrase of Eudoxus' works. The relevant quotations from Eudoxus are given in the Appendix.

Instead of fitting ecliptic longitudes, it is just as easy to fit right ascensions and declinations, especially if we are using the first analysis outlined above, which treats the phenomena as if they are from a star catalog and ignores calibration errors. Indeed, one finds

for the colure data	1150 ± 130 B.C.
for the equator data	950 ± 230 B.C.
for the northern tropic data	1120 ± 280 B.C.
for the southern tropic data	1170 ± 300 B.C.
for all declination data	1070 ± 160 B.C.
for all the data	1130 ± 90 B.C.

in good agreement with previous results.[5] Note that the date 1130 B.C. is about 8σ away from Eudoxus' date 370 B.C.

It is the case, however, that the phenomena of Eudoxus should not be analyzed as if they come from a star catalog. In order to see this, let us initially consider the data for the solsticial colure. This is a line of constant ecliptic longitude (as well as constant right ascension). Thus the 'measured values' L_i are all either 90° or 270°, and we compute the various $\lambda_i(t)$ as before. For the moment let us ignore the calibration error, and consider just the statistical errors

$$L_i - \lambda_i(t) = \varepsilon_i$$

We might minimize χ^2 and determine t_{min} using

$$\chi^2 = \sum_{i=1}^{N} \frac{(L_i - \lambda_i(t))^2}{\sigma^2}$$

and proceed as described above, determining σ_t from $\chi^2(t_{\min} \pm \sigma_t) - \chi^2(t_{\min}) = 1$, and adjusting σ in each fit so that the χ^2 per degree of freedom is about unity.

On the other hand, we might try instead to use a Monte Carlo simulation to determine σ_t.
The first task is to generate a new set of errors

$$\{L_i' = \lambda_i(t_{\min}) + \varepsilon_i',\ i = 1 \ldots N\}$$

Suppose that the first star is supposed to be on the 90° colure. We can generate the first error, ε_1' , but then we must have that

[5] B. E. Schaefer, "The Latitude and Epoch for the Origin of the astronomical Lore of Eudoxus", *Journal for the History of Astronomy,* xxxv (2004) 161– 223, and references therein.

$$90^\circ - \lambda_1(t_{\min}) = \varepsilon_1'$$

Note, however, that once we know the error in the position of the first star with respect to the supposed solsticial colure, *we automatically know the value of t*: it is the time t_1' when the true colure is a distance (in longitude) ε_1' from the position of the star at time t_{min}. Suppose we try to generate the error in position for the second star, also said by Eudoxus to be on the 90° colure. Picking a random ε from $N(0,\sigma^2)$, we would get

$$90^\circ - \lambda_2(t_{\min}) = \varepsilon_2'$$

and from this a t_2' as before. However, it is in general statistically *impossible* that $t_1' = t_2'$, and so this set of errors is not a physically realizable set, and cannot be used in a valid Monte Carlo simulation.

In fact, it is easy to see in this case that once we know the error in the first star, which is after all just the distance from a specified circle of constant longitude, we can use the theoretically known position of the second, and all subsequent, stars to *compute* their positions relative to *that same circle*, and hence their errors *must* all be computed and not generated as random variables.

It is furthermore clear that this analysis generalizes to circles of constant right ascension and declination. Once we know the distance of the first star from such a circle, we can find the unique time t_1' when that error would be realized, and we must use exactly that same time to compute the positions of all the other stars, and hence their distances from any circle. To do otherwise would be to create a set of errors and star positions that is not physically possible, and would not be acceptable in a Monte Carlo simulation.

In practice, this all means that the uncertainty in the determination of the time is proportional to the uncertainty in position of a *single* star, so

$$\sigma_t^2 = \frac{\sigma^2}{p^2}$$

In the presence of a calibration error (which, of course, should not be ignored in any event) we then get

$$\sigma_t^2 = \frac{\sigma^2 + \sigma_c^2}{p^2}$$

Thus if σ_c is about the same size as σ, then the total uncertainty in $p\sigma_t$ is about $\sqrt{2}\,\sigma$. For $\sigma = 5°$, which is the correct average value for all the data, the uncertainty in t is then about $\sigma_t = 500$ y, and so the difference in the dates 1130 B.C. and 370 B.C. is about an 8σ effect when computed naively, but only a 1.5σ effect when computed correctly.

There is another consideration that should be taken into account in any statistical analysis of historical data that span many centuries. One justification for minimum χ^2 analyses comes from consideration of *likelihood*. In this case, there is only one model parameter to be determined, the time t, and the likelihood assumption is that at any time t, if the errors $\varepsilon_i = L_i - \lambda_i(t)$ are independently distributed according to some probability density $f(\varepsilon_i;t)$, then the likelihood of observing the values $\varepsilon = \{\varepsilon_i, i = 1,\ldots,N\}$ at that time t is

$$L(\varepsilon \mid t) = \prod_{i=1}^{N} f(\varepsilon_i;t),$$

and the most likely value of t is that which maximizes the likelihood. If $f(\varepsilon_i;t)$ is the normal distribution,

$$f(\varepsilon_i;t) = \frac{1}{\sigma_i\sqrt{2\pi}} e^{-\frac{\varepsilon_i^2}{2\sigma_i^2}},$$

then χ^2 is, up to irrelevant constants, just $-2\ln L(\varepsilon \mid t)$, so clearly the maximum likelihood occurs at the same value of t that minimizes χ^2.

More generally, however, we can ask what is the probability density $p(t \mid \varepsilon)$ of t given the observed error set $\varepsilon = \{\varepsilon_i, i = 1,\ldots,N\}$. That is given by Bayes' theorem as

$$p(t \mid \varepsilon) = \frac{L(\varepsilon \mid t)\pi(t)}{\int L(\varepsilon \mid t')\pi(t')dt'}$$

where $L(\varepsilon| t)$ is the likelihood defined above, and $\pi(t)$ is the prior probability density of t, reflecting whatever knowledge we might have of t before we consider the data set ε.[6]

Note that if we have no prior information on t then $\pi(t)$ is simply a constant, corresponding to a uniform probability distribution, and Bayes' theorem reduces to conventional maximum likelihood or minimum χ^2. In practice, of course, we must obviously admit that $\pi(t)$ can be constant only over some appropriate interval of time, so, for example, no one would accept an analysis indicating that Eudoxus' data was measured in, say, 4000 B.C. or A.D. 2000. What we learn from this is that if the only information we have about t in the case of Eudoxus' on-circle data is the data itself, then it is correct to perform a conventional minimum χ^2 analysis as we did above. But if we have any other independent information on t, then we should include it according to Bayes' theorem.

In fact, one thing we do know is that Eudoxus' lore is given in the context of a fully developed model of the celestial sphere. Thus, when Eudoxus says that a constellation is on a colure, he obviously must know what a colure is, and where it is located with respect to the visible stars. If we conclude that the lore date to a time much earlier than Eudoxus, then it must be the case that the celestial sphere was fully developed at that earlier time. So if we say that $\pi(t)$ is a constant over some interval of time, one of the things we are implicitly saying is that the knowledge of the celestial sphere and its associated cosmology was constant over that same interval. For the specific case of the on-circle data, we would be saying that *the model of the celestial sphere known to Eudoxus in, say, 370 B.C., was equally well known in, say, 1120 B.C.*

Now I strongly doubt that many historians would agree with that last statement. In disputing it, they would point out that, setting aside the on-circle data of Eudoxus, no evidence has come down to us suggesting that *any* culture prior to Eudoxus' time understood the cosmology of the celestial sphere. For example, the stories from Homer and Hesiod mention a few astronomical facts, but nothing approaching the celestial sphere. The same is true for all known Greek sources earlier than Eudoxus.[7] The cuneiform texts from Babylon and Uruk give no hint that the Babylonians understood the celestial sphere. Furthermore, the information in MUL.APIN, which is similar in some respects to the phenomena of

[6] this is all very standard material that is discussed in many places. See, e.g., Glen Cowan, *Statistical Data Analysis*, (Oxford, 1998) 93-94.
[7] See, e.g. David Dicks, *Early Greek Astronomy to Aristotle*, (Ithaca, 1970).

Eudoxus and which probably dates to late second millennium B.C., gives no hint of a celestial sphere.[8]

Considering all of these cases together, it strains credulity to the breaking point that each and every source we know from the time before Eudoxus might have known about the celestial sphere in all its details, but either chose not to write anything about it, or if they did, it has not reached us, even indirectly through intermediate sources such as Hipparchus. Therefore, it appears that if we invoke the knowledge we have independent of Eudoxus, we might tolerate a uniform prior for perhaps a century before Eudoxus, but we should most certainly not be assuming a uniform prior for the millennium or more predating Eudoxus.

The implementation of the prior knowledge of t in a statistical analysis is unavoidably somewhat subjective, but we can easily imagine reasonable approaches to the issue. One simple way to proceed is to assume that the celestial sphere was developed no later than some time t_0 and over some time interval τ, and use a function that approaches zero for times earlier than t_0, with the rate of approach controlled by the time interval τ, and is strictly zero for times later than t_0. One such probability distribution is the truncated normal,

$$\begin{aligned} \pi(t) &\simeq \exp\frac{-(t-t_0)^2}{2\tau^2}, \quad t \le t_0 \\ &= 0, \quad t > t_0 \end{aligned}$$

For the case of the cosmology of the celestial sphere, the choices $t_0 = 400$ B.C. and $\tau = 50$ yrs might be appropriate, although there is no way to know these parameters with any certainty. Adding to χ^2 the term $-2\ln\pi(t)$ and minimizing the resulting sum leads to an estimated date of 550 ± 50 B.C. Clearly, this estimate is effectively determined entirely by the assumed values of t_0 and τ, and so there is some truth in saying that we have essentially assumed the answer. In reality, though, what has been done is to simply enforce in the statistical analysis the very strong belief, founded on substantial historical data, that the celestial sphere did not precede Eudoxus by very long. Similarly, to *omit* the prior is to effectively assert an equally strong belief that invention of the concept of the celestial sphere is uniformly likely at any time over the millennium preceding 370 B.C.

[8] Herman Hunger and David Pingree, *Astral Sciences in Mesopotamia*, (Leiden, 1999); James Evans, *The History and Practice of Ancient Astronomy*, (Oxford, 1998) 5-8.

Of course, a more direct and for all practical purposes equivalent strategy is to go ahead and perform a standard χ^2 analysis ignoring the prior information, and if the result is found to conflict with the prior information, we simply discard the result as unreliable and say instead that Eudoxus, or some near contemporary, made errors sufficiently large to account for the observations he used.[9]

Altogether, we must conclude that statistical analysis provides no significant evidence that the phenomena of Eudoxus were not originated sometime near to 370 B.C.

Appendix

Hipparchus' quotations from Eudoxus list which constellations are on the colures, the equator, and the two tropics (omitting for now the two arctic circles).[10] For the equator and tropics Aratus gives similar lists, but whenever we have the direct quotations from Eudoxus, there is no reason to use the information from Aratus. There are several cases where it appears that Aratus is correcting Eudoxus, but we certainly do not want to use that information when attempting to date the observations underlying Eudoxus' statements.

The Solsticial Colure

> 1.11.9 Further, Eudoxus treats also the stars which lie upon the so-called colures, and says that the Great Bear's middle lies upon one of them, and also the Crab's middle, the Water-snake's neck, and the part of Argo between the prow and the mast; then it is drawn after the invisible pole through the tail of the Southern Fish, the Capricorn's middle, and the middle of Arrow; then through the Bird's neck, its right wing, and through Cepheus' left hand; and through the bend of the Snake and beside the Small Bear's tail.

[9] A. Gelman *et al.*, *Bayesian Data Analysis*, (Boca Raton, 2000) 259-262. It is perhaps interesting that this was exactly the conclusion of the discussion by Dicks (see ref. 7, p 162-3 and p 250, n 265) of R. Böker, 'Die Enstehung der Sternsphaere Arats', *Berichte über die Verhandlungen der sächsischen Akademie der Wissenschaften zu Leipzig,* **99** (1952) 3-68. Böker's statistical analysis puts the epoch of Eudoxus' phenomena as 1000 ± 30–40 B.C. and the latitude of the observer as between 32°30´ and 33°40´.

[10] The following quotations are all from the translation of Roger MacFarlane (ref. 2).

Solsticial Colure	DD	PK
the Great Bear's middle	bet Uma	25
the Crab's middle	M44	449
the Water-snake's neck	the Hya	900
the part of Argo between the prow and the mast		880
the tail of the Southern Fish	gam Gru	1022
the Capricorn's middle	eta Cap	618
the middle of Arrow	del Sge	283
the Bird's neck	eta Cyg	161
the Bird's right wing	kap Cyg	167
Cepheus' left hand	the Cep	80
the bend of the Snake	chi Dra	61
beside the Small Bear's tail	zet Umi	4

The Equinoctial Colure

> 1.11.17 In the other colure, he says that there lie first the left hand of Arctophylax and his middle, taken lengthwise; then the middle of the Claws, taken breadth-wise, and the right hand of the Centaur and his front knees; then after the invisible pole the bend of the River and the Sea-monster's head and the back of the Ram, taken breadth-wise, and the head of Perseus and his right hand.

Equinoctial Colure	DD	PK
the left hand of Arctophylax	kap Boo	88
the middle of Arctophylax, taken lengthwise	alp Boo	110
the middle of the Claws, taken breadth-wise	alp Lib	529
the right hand of the Centaur	kap Cen	951
the front knee's of the Centaur	alp Cen	969
the bend of the River	rho Eri	786
the Sea-monster's head	gam Cet	714
the back of the Ram, taken breadth-wise	alp Ari	375
the head of Perseus	the Per	194

the right hand of Perseus	CG869	191

The Equator

For most of the constellations, Hipparchus does not quote explicitly from Eudoxus, but from Aratus, who wrote [511-524]:

> As a guide the Ram and the knees of the Bull lie on it, the Ram as drawn lengthwise along the circle, but of the Bull only the widely visible bend of the legs. On it is the belt of the radiant Orion and the coil of the blazing Hydra, on it too are the faint Bowl, on it the Raven, on it the not very numerous stars of the Claws, and on it the knees of Ophiuchus ride. It is certainly not bereft of the Eagle: it has the great messenger of Zeus flying near by; and along it the Horse's head and neck move round.

Hipparchus then adds that Eudoxus gives the flowing additional information:

> 1.10.22 Eudoxus expressed the rest similarly; but, he says that the middle of the Claws lies on the equator, and that the left wing of the Eagle, the rump of the Horse, and also the northern of the Fishes do also.

Equator	DD	PK
the Ram as drawn lengthwise	alp Ari	375
the Bull, only the widely visible bend of the legs	mu Tau	386
the belt of Orion	eps Ori	760
the coil of Hydra	alp Hya	905
the Bowl	del Crt	923
the Raven	gam Crv	931
the Claws	alp Lib	529
the knees of Ophiuchus	zet Oph	252
the left wing of the Eagle	alp Aql	288
the rump of the Horse	gam Peg	316
the Horse's head (Aratus)	eps Peg	331
the Horse's neck (Aratus)	zet Peg	325
the northern of the Fishes	eta Psc	695

The Summer Tropic

> 1.2.18 Concerning the stars which are borne upon the summer and winter tropics, and also upon the equator, Eudoxus says this about the summer tropic:
> Upon it are: the middle of the Crab, the parts lengthwise through the middle of the Lion, the area a little above the Maiden, the neck of the held Snake, Engonasin's right hand, Ophiuchus' head, the Bird's neck and its left wing, the Horse's feet, but also Andromeda's right hand and the part between her feet, Perseus' left shoulder and left shin, and also the Charioteer's knees and the Twins' heads. It then concludes near the middle of the Crab.

Summer Tropic	DD	PK
the middle of the Crab	M44	449
lengthwise through the middle of the Lion	eta Leo	468
the area a little above the Maiden	eps Vir	509
the neck of the held Snake	del Ser	269
Engonasin's right hand	kap Her	122
Ophiuchus' head	alp Oph	234
the Bird's neck	eta Cyg	161
the Bird's left wing	eps Cyg	168
the Horse's feet	pi Peg	332
Andromeda's right hand	rho And	340
the part between her [Andromeda's] feet	gam And	349
Perseus' left shoulder	the Per	194
Perseus' left shin	xi Per	214
the Charioteer's knees	chi Aur	231
the Twins' heads	bet Gem	425

The Winter Tropic

1.2.20 About the winter tropic, Eudoxus says this:
Upon it are: the middle of the Capricorn, the feet of the Water-pourer, the Sea-monster's tail, the River's Bend, the Hare, the Dog's feet and tail, the Argo's prow and mast, the Centaur's back and chest, the Beast, and the Scorpion's stinger. Then proceeding through the Archer it concludes at the middle of the Capricorn.

Winter Tropic	DD	PK
the middle of the Capricorn	eta Cap	618
he feet of the Water-pourer	del Aqr	646
the Sea-monster's tail	iot Cet	732
the River's Bend	rho Eri	786
the Hare	alp Lep	812
the Dog's feet	zet Cma	834
the Dog's tail	eta Cma	835
the Argo's prow		879
the Argo's mast	alp Pyx	876
the Centaur's back	nu Cen	946
the Centaur's chest		
the Beast	del Lup	974
the Scorpion's stinger	lam Sco	565
the bow of the Archer (Aratus)	del Sgr	571

Hipparchus also writes:

2.1.20 Eudoxus makes it clear in the following statement that he places the tropic points at the middles of the zodiacal signs: "There is a second circle [the northern tropic], on which the summer solstices occur; and on this is the middle (parts) of the Crab." Again he says, "There is a third circle [the equator] on which the equinoxes occur; and on this are both the middle (parts) of the Ram and the Claws. And there is a fourth [the southern tropic] on which the winter solstices occur; on it is the middle (parts) of the Capricorn." He states it yet more conspicuously in the following, for the so-called colures, which

are drawn through the poles and the solstices and the equinoxes, he says: "There are two other circles through the poles of the cosmos, which cut one another in half, and at right angles. The constellations upon these lines are the following: first the ever-visible pole of the cosmos, then the middle of the Bear, reckoned breadth-wise, and the middle of the Crab." Then a little later he says, "Both the tail of the Southern Fish and the middle of the Capricorn." In later passages he says that in the other of the circles through the poles lie among others, which he enumerates, the middle (parts) of the Claws, reckoned breadth-wise, and the back (parts) of the Ram, reckoned breadth-wise.

The precise meaning of these passages is connected with how Eudoxus is treating the relationship of the zodiacal constellations and the zodiacal signs.[11] Hipparchus clearly thinks that when Eudoxus says 'the middle (parts) of the Claws" he is referring to the middle of the zodiacal sign. We know this because Hipparchus repeatedly tells us that Eudoxus has arranged his signs so that the solstices and equinoxes occur at the middle of the signs.

However, as discussed in detail by Bowen and Goldstein, it is not at all clear that Eudoxus was, as Hipparchus thought, referring to the signs of the zodiac when he mentions the middle of the Crab, the Claws, the Ram, and the Capricorn. Instead, by analogy with his coincident statements that the colure also goes through, e.g. the Great Bear's middle, Eudoxus might well have been referring not to the signs but to the constellations. Of course, it is also possible that Eudoxus was considering the zodiacal signs and constellations as equivalent in some sense.

In addition, it is also not at all clear that Eudoxus was referring to anything as specific as the *midpoint* of either the sign or the constellation, since his use of the plural (τά μέσα) implies simply 'in the interior', and nothing as specific as a central position.

Therefore, for our purposes we can safely assume that Eudoxus understood that

(a) the colures are great circles that intersect at right angles at the north celestial pole, so that one colure goes through the two solsticial points, the other through the two equinoctial points;

[11] The question of just what Eudoxus meant, as opposed to what Hipparchus says he meant, is dealt with in detail by Bowen, A.C., & Goldstein, B.R. "Hipparchus' Treatment of Early Greek Astronomy: The case of Eudoxus and the length of daytime". *Proceedings of the American Philosophical Society* 1991, 135: 233-254.

(b) the ecliptic and the tropics touch at the solstices, the ecliptic and the equator cross at the equinoxes, and neighboring cardinal points are exactly one quadrant apart on both the equator and the ecliptic;
(c) the solsticial colure goes through the middle parts of the Crab and the Capricorn, while the equinoctial colure goes through the middle parts of the Ram and the Claws
(d) the solsticial and equinoctial points mark a location where Eudoxus thought the Sun was on a particular day of the year, but he specifies the location no more precisely than the middle parts of the Crab on summer solstice, the middle parts of the Claws on autumn equinox, etc.
(e) various other specified constellations and constellation parts lie on or nearby the circles which define the celestial sphere.

So while we may be relatively sure that Eudoxus knew the *date* of, say, summer solstice, to an accuracy of a few days, perhaps by looking for the turnings of the Sun on the eastern and western horizons in summer and winter, we have no specific information about how he might have determined the sidereal location of the Sun on those dates, or on the equinoxes. One plausible explanation is that he observed the date of summer solstice by observing the most northerly setting of the Sun on the western horizon, and then observed which constellations rose at sunrise on nearby mornings.

An Interesting Property of the Equant

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Early astronomers were well aware of the fact that the speed of the planets varies as the planets move around the zodiac. This zodiacal anomaly, a departure from the perfect uniform motion expected of a celestial body according to many philosophers, was modeled by the early astronomers in several ways. For the Sun and the Moon (neglecting for now the second lunar anomaly), the simplest models used either an eccentric deferent with eccentricity e or an epicycle of radius r.

In the eccentric deferent model, the planet rotates around a deferent circle of radius R at a uniform speed as seen from the center of the circle. The Earth, which is of course taken as the center of the universe, is displaced a distance e from the center of the deferent. The effect of this shift is that as seen from the Earth the speed of the planet is slowest when the planet is on the extension of the line from Earth to the center of the deferent (apogee), and fastest in the opposite direction, when the planet is on the extension of the line from the center of the deferent to the Earth (perigee). The planet moves at its mean speed when it is normal to the apsidal line as seen from the Earth, which means that it is more than 90° from apogee as seen from the center of the deferent.

In the epicycle model, the planet rotates clockwise[1] around a (smaller) epicycle circle of radius r at a uniform speed as seen from the center of the epicycle, and the center of the epicycle moves at uniform speed around a deferent circle of radius R, which is centered on the Earth. In this case, the minimum speed of the planet occurs when the planet is at the apogee of the epicycle, i.e. when it is as far as possible from the Earth, and its maximum speed occurs when the planet is at the perigee of the epicycle, i.e. when it is as near as possible to the Earth. The planet once again moves at its mean speed when it is normal to the apsidal line as seen from the Earth, which of course means that the epicycle center is more than 90° from the direction of apogee as seen from Earth.

In *Almagest* 3.3, regarding the Sun, and again in *Almagest* 4.5 for the Moon, Ptolemy (*ca.* A.D. 150) explains in great detail the geometrical

[1] This clockwise motion on the epicycle when modeling the zodiacal anomaly should not, of course, be confused with the counterclockwise motion of the planets on their epicycles when modeling the solar anomaly, which is responsible for retrograde motion.

equivalence of these models.[2] This is seen most easily in Figure 1, which shows the two models superimposed, and the equivalence follows immediately from the elementary geometrical properties of the parallelogram. The equivalence of the models is further demonstrated in *Almagest* 3.5 by numerical examples, where Ptolemy essentially shows that the equation of center for the eccentric model is

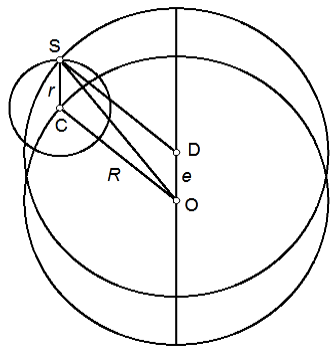

Figure 1. In the epicycle mode the Earth is at the center *O* of a deferent circle of radius *R*, an epicycle of radius *r* is centered at *C*, and the planet is at *S* on the epicycle. In the eccentric model the Earth is displaced a distance *e* from the center *D* of the eccentric deferent of radius *R*, and the planet is at *S* on the deferent. If $r = e$, then *ODSC* is a parallelogram and the position of *S* as seen from *O* is the same in both models.

$$\tan q = \frac{-e\sin\alpha}{R + e\cos\alpha}$$

and for the epicycle model it is

$$\tan q = \frac{r\sin\gamma}{R + r\cos\gamma},$$

so the equations of center are the same when $e = r$ and $\alpha = -\gamma$ (the sign arising from the opposite direction of increase for the angles). We know from Theon of Smyrna,[3] writing perhaps a generation earlier than Ptolemy, that the equivalence of eccentric and epicycle models was broadly understood among early astronomers, at least as far back as Hipparchus (*ca.*

[2] G. J. Toomer, *Ptolemy's Almagest* (1984).
[3] R. & D. Lawlor, *Theon of Smyrna: Mathematics Useful for Understanding Plato Or, Pythagorean Arithmetic, Music, Astronomy, Spiritual Disciplines* (1978).

130 B.C.), and we furthermore know from remarks by Ptolemy in *Almagest* 12.1 that the equivalence was very likely understood as far back as the time of Apollonius of Perge (*ca.* 200 B.C.).

Beyond these models, however, there is very good reason to believe that an additional model was used for the Sun and the Moon by Greco-Roman astronomers, most likely between the time of Hipparchus and Ptolemy.[4] The modern name for that model is the concentric equant, and it was used exclusively for the Sun and Moon in texts from the fifth through seventh centuries A.D. from ancient India.[5] For the concentric equant, the Earth is at the center of a deferent circle which is the orbit of the Sun or Moon, but the motion of the luminary on the deferent is seen as uniform not from the Earth, but from a point, the equant, displaced a distance *e* from the center of the deferent (see Figure 2). The speed of the planet as seen from the earth is slowest in the direction of the equant, fastest in the direction opposite the equant, and it has its mean value when the planet is 90° from the direction of apogee as seen from Earth.

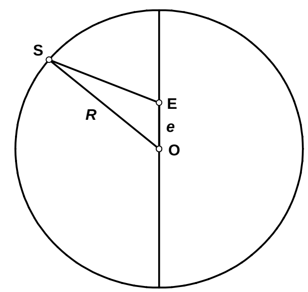

Figure 2. In the concentric equant model the Earth is at the center *O* of a deferent of radius *R* and the planet is at *S* on the deferent. The motion of *S* is uniform as seem from the equant point *E*, which is a distance *e* from *O*.

[4] David Pingree, "History of Mathematical astronomy in India", *Dictionary of Scientific Biography*, **15** (1978), 533-633.
[5] K. S. Shukla, "Use of Hypotenuse in the Computation of the Equation of Center under the Epicyclic Theory in the School of Aryabhata I ???", *Indian Journal of History of Science,* **8** (1973) 43-57.

The idea that these ancient Indian texts are ultimately of Greco-Roman origin, and from the time between Hipparchus and Ptolemy, dates from the very first investigations by Western scholars in the 1800's.[6] In the second half of the 20th century the most prominent champions of the idea, and the scholars who did the most to document and elaborate it, were Pingree[7] and van der Waerden,[8] and so it seems appropriate to refer to the idea as the Pingree – van der Waerden (PvdW) hypothesis. The principal basis of the argument is that almost all of the astronomical features in the early texts are significantly less developed than those we find in the *Almagest*.

The temporal coexistence of the concentric equant and the eccentric/epicycle models and the extensive surviving discussion of the equivalence of those models immediately begs the questions (a) can we extend the equivalence theorem to include all three models, and (b) is there any evidence that the ancient astronomers were aware of such an equivalence? The answer to (a) is a definite yes, but with an interesting twist, and the answer to (b) is also yes, at least in the framework of the PvdW hypothesis.

The geometrical equivalence of the concentric equant and eccentric models is illustrated in Figure 3.

[6] E. Burgess and W. D. Whitney, "Translation of the Surya Siddhanta", *Journal of the American Oriental Society*, (1858) 141-498, and references therein.
[7] David Pingree, "On the Greek Origin of the Indian Planetary Model Employing a Double Epicycle", *Journal for the history of astronomy*, ii (1971), 80-85; D. Pingree, "The Recovery of Early Greek astronomy from India", *Journal for the history of astronomy*, **7** (1976), 109-123; D. Pingree, *ibid.* (ref. 4), and references therein.
[8] B. L. van der Waerden, "The heliocentric system in greek, persian, and indian astronomy", in *From deferent to equant: a volume of studies in the history of science in the ancient and medieval near east in honor of E. S. Kennedy*, Annals of the new york academy of sciences, **500** (1987), 525-546, and references therein.

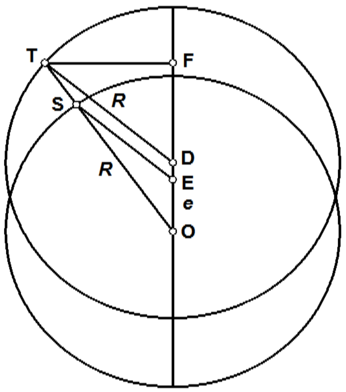

Figure 3. The concentric equant has the Earth at *O*, the planet at *S* on a deferent of radius *R*, and the equant at *E*. The equivalent simple eccentric has an eccentric deferent also of radius *R* but centered at *D*, and the planet is now at *T*. Since *ES* and *DT* are parallel both models have the same mean centrum (angles *FDT* and *DES*), and since *OST* is a straight line, the *S* and *T* have the same position as seen from the Earth and so both models have the same true centrum (angles *EOS* and *DOT*).

The models will be equivalent if and only if they predict the same value of the true centrum given the same value for the mean centrum. It is clear from the figure that this will in general be the case if and only if the eccentricity in the eccentric model is not constant but instead oscillates in a well-defined way about the value of the eccentricity in the equant model. This can be proved analytically by considering two cases:

(1) an eccentric model with eccentricity e', and
(2) a concentric equant model with eccentricity e.

For the eccentric the equation of center is given by

$$\tan q_1(e',\alpha) = \frac{-e'\sin\alpha}{R + e'\cos\alpha},$$

while for the concentric equant we have

$$\tan q_2(e,\alpha) = \frac{-e\sin\alpha}{\sqrt{R^2 - e^2\sin^2\alpha}} ,$$

where α is the mean centrum. These models are equivalent when

$$q_1(e'(\alpha),\alpha) = q_2(e,\alpha) ,$$

so the required function $e'(\alpha)$ is given by

$$\begin{aligned} e'(\alpha) &= \frac{-R\tan q_2(e,\alpha)}{\sin\alpha + \cos\alpha\tan q_2(e,\alpha)} \\ &= \frac{eR}{\sqrt{R - e^2\sin^2\alpha} - e\cos\alpha} \end{aligned}$$

It is clear that

$$\frac{1}{1+\frac{e}{R}} \le \frac{e'}{e} \le \frac{1}{1-\frac{e}{R}}$$

and that e'/e varies smoothly from its maximum at apogee to its minimum at perigee (see Figure 4). This then answers question (a) in the affirmative, and defines how the eccentricity of a simple eccentric model must vary in order to give the same equation of center as the concentric equant for a given mean centrum. Since the simple eccentric model is equivalent to a simple epicycle model, it immediately follows that the concentric equant is also equivalent to an epicycle model with a radius r' varying in the same way.

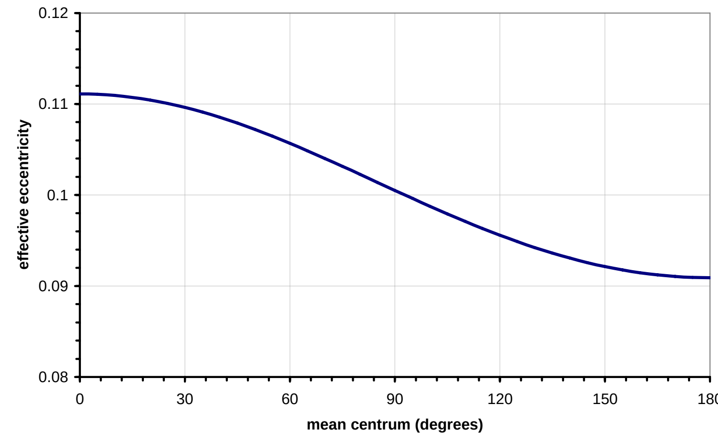

Figure 4. The effective eccentricity e'/R of the simple eccentric model that is equivalent to a concentric equant with $e/R = 0.1$. Clearly e' is maximum when the mean centrum α is zero, or at apogee, and e' is minimum at perigee.

Of course question (a) would be of only academic interest unless we can also answer affirmatively question (b), and clearly establish an historical background for believing that the extension of the equivalence to the concentric equant was known to ancient astronomers. In fact, the point is demonstrated explicitly in Bhaskara's *Mahabhaskariya*[9] (A.D. 629) which is a commentary on the *Aryabhatiya*[10] (A.D. 499), the primary text of Aryabhata. Bhaskara explains the equivalence of the concentric equant and an oscillating eccentric model by directly computing one from the other, as follows:[11]

> IV 19. Subtract (the Sine of) the final equation from (the Sine of) the koti or again add it, depending on the quadrant; the square-root of the sum of the square of that and the square of the bahu is the hypotenuse.

[9] K. S. Shukla, *Mahabhaskariya of Bhaskara I* (1960).
[10] K. S. Shukla, *Aryabhatiya of Aryabhata* (1976).
[11] David Pingree, "Concentric with Equant", *Archives Internationales d'Histoire des Sciences,* **24** (1974) 26-28.

IV 20. Multiply (the Sine of) the final equation by the hypotenuse (and) divide (the product) by the Radius; add (the quotient) to or subtract it from the previous koti (repeatedly) until the hypotenuse is equal (to the hypotenuse obtained in the immediately preceding computation).
IV 21. Multiply the Radius by the Sine of the bahu (and) divide (the product) by the (final) hypotenuse. Add the arc (corresponding to that quotient) to (the longitude of) the apogee according to the quadrant of the argument.

However terse this may appear to us, it is actually rather verbose in comparison to many early Indian astronomical texts. In any event, the execution of the algorithm is as follows (see again Figure 3):

Let $OS = DT = R$ and adjust point D so that DT is parallel to OS and triangles OES and ODT are similar. The mean centrum α (angles DES and FDT) and the eccentricity $EO = e$ of the concentric equant are given. The algorithm finds OD and $OT = h$ = 'the hypotenuse', and uses those to compute the true centrum (angle EOS). First, drop a perpendicular line from T to a point F on the apsidal line.

Step 1. (verse 19) assume $OD = e$

Step 2. then $OF = OD + R\cos(\alpha)$ and $TF = R\sin(\alpha)$. Here $R\cos(\alpha)$ and $R\sin(\alpha)$ are the Sine of the koti and the Sine of the bahu, respectively.

Step 3. then $OT = \sqrt{OF^2 + TF^2} = h$

Step 4. (verse 20) by similar triangles $\frac{OT}{OD} = \frac{R}{e}$, so we have a new estimate for $OD = eh/R$.

Step 5. go to step 2 with the new estimate of OD and recompute OF, TF, and h. When h stops changing, go to step 6.

Step 6. (verse 21) compute angle DOT = $c = \arcsin(R\sin\alpha / h)$ which, added to the longitude of apogee, gives the longitude of the planet.

The algorithm solves $h = \sqrt{(eh + R\cos\alpha)^2 + R^2\sin^2\alpha}$ by iteration beginning with $h = R$ as the first trial value. The angle computed in Step 6 will, of course, be the same value you could have gotten directly from $c = \alpha + q = \alpha + \arcsin(-e/R\sin\alpha)$ with a much simpler calculation (dropping a perpendicular from O to the extension of SE), so it is not clear why the iterative solution was used.

Verses IV 9-12 in Bhaskara's text give a similar solution employing an epicycle of varying radius. Invoking the PvdW hypothesis, we conclude that some Greco-Roman astronomers not only knew and used the concentric equant for the Sun and Moon, but they also understood that it was equivalent to an eccentric model with oscillating eccentricity and an epicycle model with oscillating radius.

The modern history of the concentric equant is interesting in itself. In 1952 van der Waerden noticed that the Tamil rules (*ca.* A.D. 1300) for computing solar longitude, based on the 248 day anomalistic cycle, were explained better by a concentric equant than by an Hipparchan eccentric model.[12] In 1956, van der Waerden's student Krishna Rav showed that the Tamil lunar longitudes computed using the same 248 day cycle were also explained better by a concentric equant model than by either an Hipparchan model or Babylonian System A or System B schemes for the lunar motion.[13]

However, in 1956 in the paper in *Centaurus* immediately following Krishna Rav's, van der Waerden changed his mind.[14] He claimed that since there was no known tradition of a concentric equant model in either Greek or Indian astronomy, it would be better to assume that the Indians were in fact using a conventional Hipparchan eccentric model, but were computing the equation of center by approximation. Thus, while $\sin q = -e/R\sin\alpha$ is exact for the concentric equant and is indeed used exclusively in Indian astronomy to compute the equation of center for the Sun and Moon, it is also a good approximation to the Hipparchan equation of center for small e, since

$$\sin q = -e/R\sin(\alpha + q) \simeq -e/R\sin\alpha + O(e^2/R^2).$$

[12] B. L. van der Waerden, "Die Bewegung der Sonne nach Griechischen und Indischen Tafeln", *Bayer. Akad. Wiss. Munchen* 1952, math.-nat. K1., 219.
[13] I. V. M. Krishna Rav, "The Motion of the Moon in Tamil Astronomy", *Centaurus*, **4** (1956) 198-220.
[14] B. L. van der Waerden, "Tamil Astronomy", *Centaurus*, **4** (1956) 221-234.

Thus van der Waerden concluded that the agreement of the approximation with the concentric equant was accidental.

However, in 1974 Pingree pointed out that there is indeed an explicit discussion of the concentric equant in Indian astronomy, the very commentary on Aryabhata by Bhaskara discussed above, thus contradicting the premise of van der Waerden's doubt.[15] Since Pingree, van der Waerden, and virtually all other western scholars agree that these Indian texts represent a tradition derived from much older Greco-Roman sources, it appears that van der Waerden's and Krishna Rav's original conclusions are indeed correct.

There is one other possible reflection of the concentric equant in ancient Greek astronomy. In Book 5 of the *Almagest* Ptolemy resolves the discrepancies between the simple Hipparchan lunar model and lunar elongations at quadrature by adding a complication to the model that bears a striking resemblance to the concentric equant. Indeed, Ptolemy's lunar model is a concentric equant as discussed above, with the modification that the Earth is positioned not at the center of the deferent but at the equant point. This is the earliest point in the *Almagest* that Ptolemy employs a deviation from uniform circular motion, and he does so here silently, so we have no information from him on the origin of the model. Ptolemy goes further by adjusting the position of the apsidal line of the lunar epicycle, but that has no bearing on the modified concentric equant construction he uses. It might, however, be worth noting that according to one plausible reading of Ptolemy's earliest commentator, the shift of the epicycle's apogee is in fact the *only* contribution by Ptolemy to the full lunar model.[16] Such an interpretation is, of course, not inconsistent with the present discussion.

It is also at least possible that the concentric equant was at some point used for not only the Sun and Moon, but that with an added epicycle it was also used for planetary motion. We have no textual evidence for this, but the textual evidence that the simple eccentric plus epicycle model was ever used for planetary motion is also very sparse, so both cases are essentially on the same footing. Such joint models exhibit both zodiacal and solar anomalies, the latter being responsible for retrograde motion. In addition, a concentric equant plus epicycle model has a great practical advantage over the eccentric plus epicycle model in that the two anomalies are not coupled, so computation is as easy as a couple of table look-ups, and no complicated decoupling interpolation, such as the scheme Ptolemy provides in the *Almagest*, is required for computation using tables. It turns out, however,

[15] David Pingree, *ibid*. (ref. 11).
[16] Alexander Jones, *Ptolemy's first commentator.* Philadelphia, 1990. Transactions of the American Philosophical Society, 80.7. 62 pp.

that such a model is no better or worse for explaining the observed phenomena than an eccentric plus epicycle model, although the models fail in virtually opposite ways, so for example when one model produces retrograde arcs that are too small, the other produces arcs too large, and *vice versa* (see Figure 5).

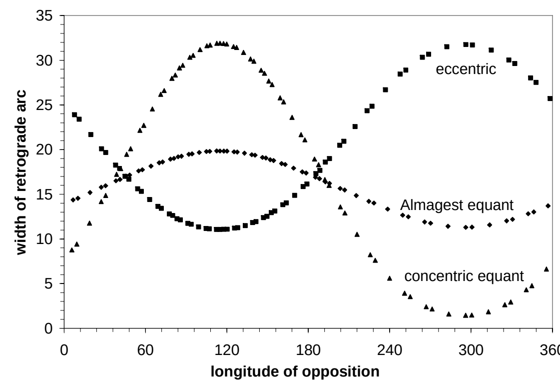

Figure 5. The width of retrograde arcs for Mars is shown as a function of zodiacal longitude. The apogee of Mars is around 115°, so the simple eccentric model gives arcs of maximum width at apogee and minimum width at perigee, while the concentric equant gives just the

At some point between the times of Hipparchus and Ptolemy astronomers realized that a more sophisticated model, the equant, was required to account for the observed phenomena. One way to think of the equant is to start from an eccentric deferent model, and to displace the center of uniform motion toward the apogee, and hence away from the Earth, by an amount equal to the distance *e* between Earth and the center of the deferent. But an equivalent view is to start with a concentric equant and displace the Earth away from the equant. Either way, if any ancient analyst had noticed the pattern of mirror image failing of the simplest models, then it is tempting to muse that at least one factor leading to the *Almagest* equant was a consideration of the sort shown schematically in Figure 6, where the full equant is seen as a sort of merging of an eccentric deferent and a concentric equant.

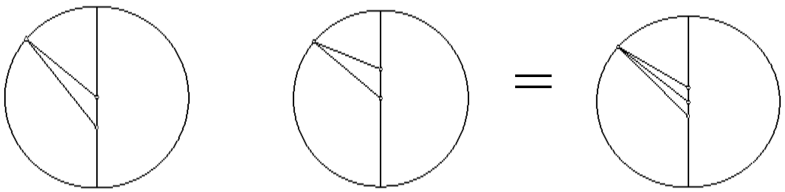

Figure 6. A schematic representation of how someone might have thought to combine the simple eccentric and concentric equant models into the *Almagest* equant.

Ptolemy, of course, uses the equant extensively in the *Almagest* to account for the zodiacal motion of Venus, Mars, Jupiter, and Saturn, and he gives detailed derivations of the equant model parameters based on empirical data for each of those planets, but he gives virtually no historical information on the development of the equant, and no other author even mentions its existence until Islamic astronomers began criticizing its philosophical shortcomings (nonuniform motion as seen from the center of the deferent and the impossibility of implementing such motion with rigid rotating celestial spheres). Several paths to the equant have been proposed, none of them mutually exclusive of the others, but the simple fact is that we cannot be sure when the equant was invented or how long it took, who invented it, or even what empirical data triggered the invention.[17] So whether or not anyone ever thought about planetary models along this line is of course a matter of speculation, but hardly more so than all other proposed histories of the equant.

In this context it is interesting to note that while all early Indian texts use the concentric equant to account for the zodiacal anomaly in both solar and lunar models, they use more complicated constructs to account for the planets, which exhibit both zodiacal and solar anomalies. Despite being effectively disguised by a remarkable series of approximations, the

[17] James Evans, "On the function and probable origin of Ptolemy's equant", *American journal of physics,* **52** (1984), 1080-9; Noel Swerdlow, "The empirical foundations of Ptolemy's planetary theory", *Journal for the history of astronomy*, **35** (2004), 249-71; Alexander Jones, "A Route to the ancient discovery of non-uniform planetary motion", *Journal for the history of astronomy*, **35** (2004); Dennis W. Duke, "Comment on the Origin of the Equant papers by Evans, Swerdlow, and Jones", *Journal for the History of Astronomy* **36** (2005) 1-6.

underlying mathematical basis of these Indian models is precisely the *Almagest* equant.[18] In particular, the longitude in the Indian models is computed by means of a sequence of steps involving the concentric equant for the zodiacal anomaly, using $\sin q = -e/R \sin\alpha$, and the usual epicycle equation

$$\tan p = \frac{r\sin\gamma}{R + r\cos\gamma}$$

for the solar anomaly. The development of this approximation scheme, itself an especially brilliant achievement in applied mathematics, might well be expected from someone familiar with the concentric equant, but of course by no means proves that case.

The equivalence of the concentric equant to an eccentric with oscillating eccentricity and an epicycle with oscillating radius can be easily extended to the *Almagest* equant. The discussion parallels that given above for the concentric equant, except for the *Almagest* equant one has

$$\tan q_2(e,\alpha) = \frac{-2e\sin\alpha}{\sqrt{R^2 - e^2\sin^2\alpha} + e\cos\alpha}$$

and so

$$e'(\alpha) = \frac{2eR}{\sqrt{R^2 - e^2\sin^2\alpha} - e\cos\alpha}.$$

In this case e'/e is bounded by

$$\frac{1}{1+\frac{e}{R}} \le \frac{e'}{2e} \le \frac{1}{1-\frac{e}{R}}$$

and so the variation is smaller than in the case of the concentric equant.

The fact that such an equivalence scheme for the equant is never mentioned in any Greco-Roman text is hardly surprising. Since only Ptolemy mentions

[18] B. L. van der Waerden, "Ausgleichspunkt, 'methode der perser', und indische planetenrechnung", *Archive for history of exact sciences*, 1 (1961), 107-121; Dennis W. Duke, "The equant in India: the mathematical basis of Indian planetary models", *Archive for History of Exact Sciences*, **59** (2005) 563-576.

the equant, and since we know that Ptolemy, and according to Theon of Smyrna, earlier astronomers, were developing a cosmology of the heavens in terms of physical spheres, it is certainly not unreasonable to suppose that even if he knew about this oscillating radius/eccentricity view of the equant, Ptolemy would see no reason to mention it since it conflicts so starkly with his view of cosmological reality.

However, one very curious feature of most of the Indian schemes is the use of pulsating values for e and r. One might suppose that these pulsating values somehow reflect an earlier knowledge of the relationship of the equant to oscillating e and r values, except for the facts that (a) the Indian schemes have *two* maxima and minima per rotation instead of the single maximum and minimum that we find in the equivalence with the equant, and (b) the Indian schemes already incorporate the equant, and the oscillating eccentricity arises from the equivalent simple eccentric model. Altogether this suggests that someone was tinkering, perhaps in order to improve some perceived fault in the model. Whether this tinkering was done originally in Greco-Roman times or later in India seems impossible to say in the absence of any further evidence.

A Database for the British Neptune-discovery Correspondence

Nick Kollerstrom

In my experience the discovery of Neptune is the one story which astronomers do really, really like to hear told. A few of the Neptune-discovery letters are already up at www.dioi.org/kn/neptune/corr.htm and previous articles in *DIO* 2.3 and 9.1 cover the story. But soon a much richer collection of documents will appear at www.dioi.org.

Summaries of around 440 letters concerning the discovery of the planet Neptune in 1846 are going into a database on the DIO website. These come mainly from the British libraries in Cambridge and London, and also Paris and a few from Washington. The discovery of Neptune was a huge international drama which involved France (where it was predicted), Germany (where it was found), England (where priority of prediction was claimed, retrospectively) and America (where the orbit theory seemed to show that the prediction had been a mere fluke).

The database will be searchable, so one can search by author, recipient, date of letter, archive or subject. It indicates where the original letter is stored and where copies exist. This integration of the correspondence enables a complete version of the story to be told, for the first time. For example, by collecting together the 22 letters by and to Leverrier a fuller version of his elusive personality becomes possible. This will appear in my forthcoming article ‘The Naming of Neptune’ in the next issue of the *Journal of Astronomical History and Heritage* (Australia). Leverrier sunk into a depression after the planet’s discovery and would not participate in the naming process, leading to an impasse in the decision-making. It is remarkable that there is no extant French correspondence of Leverrier around this period. It is all with his English colleagues chiefly Herschel and Airy.

The database also has a copy of the first line of each letter. Many of the letters are only readable because they have been transcribed around the beginning of the 20th century (the MacAlister collection at John’s College, the Herschel collection at the Royal Society). In Cornwall there is an archive with many family letters of John Couch Adams, and quite a few of these are of some relevance to the story. The web enables History of Astronomy to be researched and experienced by persons far from these few old libraries, or I suggest that it should do so. I hope we can eventually get scanned-in images of these actual letters, maybe with text enhanced to make the old handwriting more readable! John Herschel’s spidery scrawl is quite a nightmare to read, and nearly all of Airy’s letters in ‘The Neptune File’ exist only as blotting-paper copies of the letters he sent out.

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