Claims of Originality and Innovation in Ptolemy's Almagest.

Introduction.

The fact that so little has survived of Greek astronomical literature from Ptolemy's time and the centuries preceding, together with great variation in the approaches historians take with respect to indirect testimony and filling in the gaps in our evidence, has made it possible for wildly diverse assessments to be put forward of what is new in Ptolemy's *Almagest*. At one extremity of the spectrum is R. R. Newton's thesis that essentially the whole of Ptolemy's system, encompassing the models, their parameters, and the tables, was the work of other authors whom Ptolemy plagiarized.[1] Somewhere towards the opposite end is B. R. Goldstein's "alternative story"—alternative not to Newton, but to what Goldstein describes as the "standard account of astronomy leading up to Ptolemy"—according to which Ptolemy was the first of the Greeks to try to account for planetary phenomena by direct appeal to quantitative geometrical modelling, so that even the concept of separation of apparent planetary motion into mean motion and correction for anomaly would have been original with him.[2] In between lies this "standard account," more accurately a plurality of hypothetical reconstructions of the

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[1] R. R. Newton, *The Crime of Claudius Ptolemy* (Baltimore: Johns Hopkins University Press, 1977); *The Origins of Ptolemy's Astronomical Parameters* (College Park: The Center for Archaeoastronomy, University of Maryland, 1982); *The Origins of Ptolemy's Astronomical Tables* (College Park: The Center for Archaeoastronomy, University of Maryland, 1985).

[2] B. R. Goldstein, "Saving the Phenomena: The Background to Ptolemy's Planetary Theory," *Journal for the History of Astronomy* 28 (1997), 1-12.

background to and innovations in Ptolemy's astronomy that more or less share the following suppositions drawn largely though not exclusively from Ptolemy's own statements scattered through the *Almagest*:

(1) that the model and parameters of Ptolemy's solar model of *Almagest* Book 3 together with the theory of precession of Book 7 were substantially due to Hipparchus;
(2) that the model and some of the parameters of the single-anomaly lunar model of Book 4 were due to Hipparchus whereas the two-anomaly model of Book 5 and the remaining parameters were original to Ptolemy; and
(3) that Ptolemy's epicycle-plus-eccenter-plus-equant planetary models and parameters are post-Hipparchian, though models employing at least an eccenter for the synodic anomaly and an eccentric deferent for the zodiacal anomaly (or a kinematic equivalent of this arrangement) were in use before Ptolemy.

The question of what was new or original in the *Almagest* is entangled with the question of Ptolemy's veracity. On the one hand, many observation reports in the *Almagest*, when examined in confrontation with modern astronomical theory and with the parameters that Ptolemy ostensibly derives from them, appear to have been significantly altered or even fabricated to fit prior theoretical assumptions. Newton maintained that Ptolemy employed fictitious observations to conceal his wholesale appropriation of predecessors' work, whereas others have suggested that Ptolemy's motivation was to provide the *Almagest*'s readers with a clear deductive path to results that he had himself obtained though through less direct or less didactically satisfying analyses of empirical data. And on the other hand, Ptolemy's references to his predecessors and contemporaries, most conspicuously his many discussions of

Hipparchus's theoretical work, are not mere historical asides but serve the *Almagest*'s argumentative goals. It is sometimes obvious where Ptolemy is being deliberately elliptic, but the absence of original sources to compare with Ptolemy's testimony makes it impossible to be certain how reliable and balanced the positive statements in the *Almagest* are.

My intention here is not to try to sort out what was new in the *Almagest*, but rather what Ptolemy *claims* was his original contribution, with some concluding thoughts on the place that the concepts of originality and personal credit had among the values he held as a scientific writer. These are matters that seem not to have been addressed with much thoroughness in the abundant scholarship on Ptolemy's astronomy, though they have obvious relevance for our efforts to understand why Ptolemy wrote the way he did.

Ptolemy's uses of the first person in observational contexts.

Let us begin with Ptolemy's use of the first person. As is common in Greek scientific literature, he employs the first person plural to designate himself as an individual. However, he also makes extensive use of the first person plural in the general timeless and impersonal sense also common in scientific writing, more or less meaning "the author, the reader, or indeed anyone." How can we tell the difference? First of all, first personal plural in the indicative past tenses obviously refers to Ptolemy himself. Secondly, the inclusion of a personal pronoun, ἡμεῖς (though only when in the nominative) asserts Ptolemy as the personal subject of a statement that could otherwise be construed as general. When neither of these conditions applies, the presumption should be that the first person is intended to have general force and is not asserting a historical or autobiographical fact.

We can show how these distinctions work through some examples. Dated observation reports such as the solstice and equinox reports from AD 132, 139, and 140 cited in *Almagest* 3.1, 3.4, and 3.7 are obvious cases of Ptolemy's asserting his agency through a first person plural aorist verb reinforced by the pronoun ἡμεῖς, for example "ἡμεῖς ἐτηρήσαμεν ἀσφαλέστατα," "we observed most securely" (ed. Heiberg, 1.204). More instructive is 1.12, the chapter in which Ptolemy describes instruments for observing the Sun's noon altitude and presents his value for the obliquity of the ecliptic. He begins by writing that a measurement such as that of the arc of the meridian circle cut off by the two tropic circles "is immediately determined by us instrumentally by a simple construction of the following kind" (αὐτόθεν δ' ἡμῖν τὸ τοιοῦτον ὀργανικῶς καταλαμβάνεται διὰ τοιαύτης τινὸς ἁπλῆς κατασκευῆς), where the present tense promises a general method that is open to anyone (ἡμῖν). He thereupon describes the construction of a graduated bronze meridian ring, using the first person plural throughout in the future tense (ποιήσομεν etc.) and without pronouns. Only when he has finished explaining how the instrument is to be made does he shift to the imperfect tense, "with such a setup having been made, we would observe (ἐτηροῦμεν) the Sun's lateral motion to the north and south...," thus telling us that he actually used this instrument.[3] The same tense (ἔτι δὲ εὐχρηστότερον ἐποιούμεθα τὴν τοιαύτην παρατήρησιν, "we made this kind of comparative observation still more conveniently") leads into the description of how to make a graduated meridian plinth. The instructions for constructing this second instrument are given entirely in two massive blocks of participial constructions, the first dependent on the clause just quoted, and the second on a resumptive ἐτηροῦμεν, "we would observe," but the finite verbs leave us in no doubt that Ptolemy means that he made observations of noon altitudes using this

[3] This point is missed by J. P. Britton, *Models and Precision: The Quality of Ptolemy's Observations and Parameters* (New York: Garland, 1992), 4, who infers that Ptolemy used the meridian plinth but not the meridian ring.

instrument too. Any vagueness arising from the imperfect tense is dispelled by the aorist with which he reports the actual results of the measurements: κατελαβόμεθα τὴν... περιφέρειαν... πάντοτε, "we always determined the arc to be...."

In 3.4, Ptolemy recounts that Hipparchus had established the parameters of an eccentric solar model from the assumption that the time intervals from vernal equinox to summer solstice and from summer solstice to autumnal equinox were respectively 94 1/2 and 92 1/2 days. He continues (ed. Heiberg 1.233), "we too find that the times corresponding to the aforesaid quadrants... are approximately the same as are now in effect" (καὶ ἡμεῖς δὲ τοὺς μὲν τῶν προκειμένων τεταρτημορίων χρόνους... τοὺς αὐτοὺς ἔγγιστα καὶ νῦν ὄντας εὑρίσκομεν). The pronoun ἡμεῖς, syntactically optional in the Greek, tells us (as the English rendering does not) that Ptolemy means that he has personally made observations of equinoxes and solstices that confirmed the time intervals.

Modelling decisions and their empirical justifications.

As Swerdlow has pointed out, Ptolemy justifies many of the structural elements in his models (as distinct from the parameters, which are derived from specific, usually dated, observations and measurements) as choices determined by generalized, ostensibly empirical statements.[4] These can be seen as a continuation of the broad empirical arguments that Ptolemy adduces in the opening chapters of *Almagest* Book 1 to establish the geocentric celestial-and-terrestrial-spheres framework within which his system of models subsists. Typically, Ptolemy expresses the empirical claims using a first person verb in the present tense, typically εὑρίσκομεν, "we

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[4] N. M. Swerdlow, "The Empirical Foundations of Ptolemy's Planetary Theory," *Journal for the History of Astronomy* 35 (2004), 249-271, esp. 250-254.

find," unaccompanied by the personal pronoun. Thus among the arguments for the sphericity of the Earth (1.4, ed. Heiberg, 1.15) is the statement, "we find that eclipses (ἐκλειπτικὰς φαντασίας), and particularly lunar ones, which take place at the same time, are not recorded by all people at the same hours, that is, equally separated from noon...," which of course was a well known fact of astronomical geography that Ptolemy would scarcely have put forward as his own discovery.

Swerdlow presents these statements in the *Almagest* as manifestations of the deeply empirical character of Ptolemy's astronomy. But it is a rather artificial empiricism, less removed than might appear on the surface from the hypotheses functioning as first principles in Hellenistic treatises of "applied mathematics" such as Aristarchus's *On the Sizes and Distances*, Archimedes's *On Floating Bodies*, and the Euclidean *Catoptrics*. Thus the statement from which Ptolemy deduces that the synodic anomaly of the five planets is produced by having each planet revolve on an epicycle in the same sense as the epicycle revolves around the Earth is:

> in the case of the five planets we find from the observations that are made at frequent intervals (συνεχῶν) of various (synodic) configurations (σχηματισμῶν) in the same parts of the zodiac that the time interval from greatest motion to mean is always greater than that from mean to least. (*Almagest* 9.5, ed. Heiberg 2.250)

In other words, we look in the observational record for a series of observations of a planet as it goes through different stages of its synodic cycle, all happening in roughly the same region of the zodiac in order to minimize the effect of the zodiacal anomaly, and somehow we tease out the fact that the planet takes a longer time to go from its greatest apparent speed to its mean apparent speed than from its mean to its least apparent speed. As Swerdlow points out, such a demonstration is really only viable for Venus, and even then one faces nontrivial complications in eliminating the zodiacal anomaly. At best it is disingenuous for Ptolemy to write as if his claim is empirically verifiable for all five planets.

The statements from which Ptolemy deduces that the Sun's anomaly is produced by its revolving on an eccenter (or the optically equivalent opposite-sense epicyclic model) and that the zodiacal anomaly of the five planets are likewise produced by having the epicycle's center revolve on an eccenter are still more problematic. For the Sun, Ptolemy asserts that its anomaly "makes the time interval from least motion to mean always greater than that from mean to greatest, for we find this to be in agreement with the phenomena" (3.4, ed. Heiberg 1.232). This assertion, as I have shown elsewhere, would simply not have been testable from any kinds of observations possible in antiquity. Likewise, when Ptolemy writes (9.5, ed. Heiberg 2.251) that:

> we find, by means of the arcs of the zodiac taken up (*scil.* between successive observations of a planet) at the same phases or the same configurations, that the time interval from least motion to mean is always greater than that from mean to greatest,

he is asserting a phenomenon that would have been too subtle to detect or test through observational records. In fact it is probably best to regard *all* Ptolemy's ostensibly empirical statements about the times between least, mean, and greatest speeds of heavenly bodies as a coherent system of notional phenomena that Ptolemy *deduced* from the models he had already chosen, and that he makes into a didactically coherent rationale for those very models without regard for whether they were truly observable. Hence his vague specifications of the kinds of observations that lead "us" to the time-interval statements are also artificial idealizations. What these passages are *not* is summaries of Ptolemy's own course of astronomical research, nor does he express them as such.)

We now come to a crucial case, the manner in which Ptolemy justifies the separation of the centers of the planets' deferents from the centers of their uniform motion (i.e. their equant points). Already in *Almagest* 9.2 (ed. Heiberg 2.212) Ptolemy has made an apologetic but very nebulous indication that he may adopt some principles in the planets' models, "having obtained the knowledge of them not from an apparent starting point but in accordance with continued trying-out and fitting (of observations to models)" (ὑποτίθεσθαί τινα πρῶτα μὴ ἀπὸ φαινομένης ἀρχῆς, ἀλλὰ κατὰ τὴν συνεχῆ διάπειραν καὶ ἐφαρμογὴν εἰληφότα τὴν κατάληψιν). He first brings up the planetary equants explicitly in the continuation of the passage in 9.5 that contains his pseudo-empirical arguments for the same-sense epicycle as cause of the synodic anomaly and the eccentric deferent as cause of the zodiacal anomaly:

> But in fact we find by means of application and continued comparison of the individually observed position to the methods arising from the combination of both the (epicyclic and eccentric) hypotheses that it will not work so simply...

Ptolemy turns out to have in mind *two* modifications to the straightforward combination of an epicycle with an eccentric deferent: the separation of the center of uniform motion from the deferent's center, and the sidereal alignment of the model's apsidal line. It must be stressed that nothing in these passages constitutes an assertion that Ptolemy was the first to introduce either equants or a sidereal frame of reference for the apsidal lines of the planets; throughout, he has been employing the impersonal present-tense "we" without pronoun.[5] And in fact the zodiacal anomaly had been modelled as sidereally fixed already in the Babylonian arithmetical

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[5] G. J. Toomer, *Ptolemy's Almagest* (London: Duckworth, 1984), 442, inserts a gloss that turns Ptolemy's statement into an assertion of novelty: "it will not work simply to assume [as one has hitherto]...."

models that had wide circulation in Ptolemy's time. Ptolemy's concern is not with novelty but with the lack of a clear and didactic deductive path, so that he is compelled to plead that "things hypothesized in a non-deductive manner, just so long as they are determined in agreement with the phenomena, cannot have been discovered without some method and knowledge, even if the manner of their determination is difficult to put into words" (ed. Heiberg 2.212).

By the time he comes to the superior planets, in 10.6, Ptolemy has determined the locations of the equants in the models for Mercury and Venus from an analysis of individual dated observations (9.9 and 10.3), obviating any necessity to revisit the empirical motivation for assuming a separation of equant from deferent center for these planets. His procedure for the superior planets is different, since he engages in the determination of these planets' eccentricities on the hypothesis that the deferent's center bisects the line from the Earth's center to the equant, so that the bisection calls for some prior justification. He provides this as follows (ed. Heiberg 2.316-317):

> In the case of the remaining three (planets) Mars, Jupiter, and Saturn, we find one and the same hypothesis for the motion as the one determined for Venus, that is, according to which the eccentric circle, on which the center of the epicycle always travels, is described such that its center is the point bisecting the (line) between the center of the zodiac and (the center) that causes the uniform revolution of the epicycle, since in the case of each of these (planets), roughly speaking (κατὰ τὸ ὁλοσχερέστερον τῆς ἐπιβολῆς), the (eccentricity) that is found by means of the maximum equation (τοῦ μεγίστου διαφόρου) of the zodiacal anomaly is found to be approximately double the eccentricity that arises from the magnitude of the retrogradations (ἐκ τῆς πηλικότητος

τῶν... προηγήσεων) around the greatest and least distances of the epicycle (from the Earth).

Like all the supposedly empirical justifications of Ptolemy's modelling decisions that we have considered up to this point, this one is expressed with the impersonal first person, and I find it difficult to agree with Swerdlow that the passage must be taken as Ptolemy's recounting the route by which he personally discovered the necessity of bisecting the eccentricity, in the first instance for Mars, and then by extension for the other superior planets and Venus.[6] According to Swerdlow's reconstruction of Ptolemy's approach, Ptolemy began by assuming a simple epicycle-with-eccenter model for Mars, and determined an eccentricity that accurately predicted the longitude of the epicycle's center. Since this model disastrously failed to reproduce Mars's observed retrograde arcs—the longitudinal intervals traversed between first and second station—Ptolemy realized that the epicycle's distance from the Earth must be different from the distance predicted by the simple model, and so, treating the deferent's center as a distinct point from the previously determined center (which remains as the equant), he found by trial and error that bisection yields accurate retrogradations. This reconstruction may be entirely correct—that would be a topic for a different paper—but it seems to me implausible that the passage quoted above was Ptolemy's effort to describe such an empirical path, however succinctly. Rather, it appears on the face of it to state that there exist two *independent* methods of estimating an eccentricity, one from the maximum equation of center, the other from the retrogradations, such that the first is *roughly* double the second. In fact this is not true, if by "retrogradations" Ptolemy meant retrograde arcs (as Toomer translates προηγήσεων, perhaps influenced by Swerdlow's interpretation since this is cited in a

[6] Swerdlow, "The Empirical Foundations" (cf. note 4), 254 and 262-263.

footnote *ad loc.*), but there are other ways in which a superior planet's observed motion around retrogradation can be made to yield an eccentricity about twice the eccentricity that best fits the equations of center.[7] But whatever Ptolemy meant, the function of the passage is not autobiographical but rather to provide some modicum of plausible empirical justification within the framework of the *Almagest* for extending the principle of bisection, previously demonstrated for Venus, to the superior planets.

If one wishes to see how Ptolemy would assert personal credit for innovation in modelling the motions of the heavenly bodies, and for discovering phenomena that necessitate this innovation, one merely has to turn to *Almagest* 5.1. In 4.5 (ed. Heiberg 1.294), the chapter that first sets out a provisional simple epicyclic model for the Moon, Ptolemy has already indicated that he will later offer something novel:

> at this point we will give an account of (the lunar anomaly) on the assumption that it is single, which is the sole (anomaly) that practically all our predecessors have manifestly given their attention to... but after this we shall demonstrate that the Moon exhibits a certain second anomaly too...

In 5.1 (ed. Heiberg 1.351) Ptolemy is even more explicit about his contribution:

> a second anomaly of the Moon has been established... We were drawn to such a realization and belief (ἐπίστασίν τε καὶ πίστιν) from the positions of the Moon observed and recorded by Hipparchus as well as those that were obtained by ourselves by means of the instrument that was constructed by us for such things...

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[7] For example, one can derive decent values for the minimum and maximum ratios of epicycle radius to distance from the Earth from estimates of the planet's apparent rate of daily motion at opposition around apogee and perigee.

Although one may wonder why in 4.5 he had written "practically (σχεδόν) all our predecessors," his wording leaves no doubt that he wishes to be recognized as the discoverer of the second anomaly, and hence by implication the deviser of the epicycle-and-eccenter model set out and quantified in 5.2-5. Strikingly, this is the *only* set of decisions at the level of model structures that Ptolemy personalizes explicitly in the *Almagest*.

Parameters.

The numerical parameters in the *Almagest* can be divided into two groups: (1) the mean motions, which represent the periodicities of the various circular revolutions in the models, together with the epoch values in effect at Ptolemy's epoch, Nabonassar 1 Thoth 1, noon at Alexandria, and (2) the static parameters such as eccentricities, epicycle radii, fixed or maximum inclinations of planes, and the sizes and distances of the Sun and Moon.

According to Ptolemy's theory, two periodicities are more or less directly measureable, namely the tropical year (obtained approximately as the time interval in days between two widely spaced observations of solstices or equinoxes of the same kind divided by the number of intervening tropical years) and the rate of precession (obtained by comparing observed tropical longitudes of fixed stars over as long an interval as possible). By refuting Hipparchus's arguments that the tropical year might exhibit fluctuations in length and claiming that his own observations of solstice and equinox dates showed no such fluctuations, Ptolemy effectively assumes responsibility for his fundamental assumption that the tropical year is a constant (3.1, ed. Heiberg 1.132-136); nevertheless, he goes out of his way to point out that the specific value he arrives at for the tropical year, 365 1/4 – 1/300 days, was either implied or explicitly given in several of Hipparchus's writings (ed. Heiberg 1.139). (He does not state that Hipparchus was the originator of this value.) Similarly, after obtaining a rate of 1° in 100 years for precession, he writes that Hipparchus "clearly suspected" (ὑπονενοηκὼς φαίνεται) this rate, which is the more noteworthy since the one quotation he adduces to support this actually gives 1° per century as a lower bound (7.2, ed. Heiberg 2.15-16).

In contrast to these solar-stellar periodicities, whose accuracy is only limited by the span of time between the most ancient and recent usable observations, Ptolemy believes that those for the Moon and planets are best determined by an iterative procedure: one hypothesizes first approximations drawn from the most trustworthy work of one's predecessors, then one works out the static parameters of the model and its configuration at some chosen moment from a set of observations made over a short time interval (such that the effect of any inaccuracy in the assumed periodicities will be minimal), and then one uses this worked out model to obtain from pairs of widely spaced observations corrections to the initially assumed periodicities.

In the case of the planets, Ptolemy starts with a set of fairly crude period relations that he says were computed (ἐπιλελογισμένας) by Hipparchus (7.3, ed. Heiberg 2.213), though he immediately provides them with the correction terms that are the basis for his own final mean motion tables.[8] In each of the chapters in which he conducts the "correction" of the Hipparchian period relations (9.10, 10.4, 10.9, 11.3, and 11.7), Ptolemy states, in almost the same words each time, that his corrected versions of the periodicities were obtained from the very observations cited in those chapters, so he clearly intends that we should consider the

[8] The periodicities in question were in fact of Babylonian origin, where they appear among the so-called Goal-Year periods used to forecast future planetary phenomena from past observational records.

final mean motions (and by implication the epoch values which depend on them) to be his own contribution.

The situation with the Moon's mean motions (in longitude, anomaly, and argument of latitude) is slightly more complicated. Again he begins (4.2) by adducing period relations that he ascribes to Hipparchus.[9] Again, he immediately provides his own corrected values for the mean motions in anomaly and latitude (4.3), while stating that the Hipparchian mean motion in longitude requires no correction. These corrections are derived in 4.7 and 4.9, following the determination of the static parameters in 4.6. But in 4.9, the chapter devoted to the correction of the mean motion in argument of latitude, Ptolemy also tells us that he had previously found a different correction using a procedure that relied on assuming Hipparchus's values for the apparent sizes of the Moon's disk and the Earth's shadow. Thus the final mean motion in the *Almagest* is not only presented as an original finding of Ptolemy's, but a replacement for an earlier, repudiated value that was also Ptolemy's.

For most of the static parameters, Ptolemy neither states that he was the first person to adopt whatever value he obtains nor ascribes it to a predecessor. The two exceptions are his values for the obliquity of the ecliptic and for the Moon's maximum latitude. In 1.12, he gives as the result of his own observations that the meridian arc between the tropic circles (i.e. the double obliquity) was consistently measured as between 47 2/3 ° and 47 3/4 °, to which he adds (ed. Heiberg 1.68):

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[9] Again, these are actually Babylonian parameters, but in this case refined ones from the so-called System B mathematical lunar theory.

> Through this there results more or less (σχεδόν) the same ratio as that of Eratosthenes, which Hipparchus also employed. For the (arc) between the tropics turns out as (γίνεται) approximately 11 of such (parts) as the meridian is 83.

This passage has given rise to much quibbling about whether Ptolemy means that Eratosthenes and Hipparchus used the specific ratio 11 : 83, or that 11 : 83 is a ratio obtained from his own limiting measurements, and which happens to be close to some other unstated ratio used by Eratosthenes and Hipparchus. (As it happens, 11 : 83 is a continued-fraction convergent for more than half the range between (47 2/3)/360 and 47 3/4)/360.) Either reading is possible. In any event, Ptolemy here is effectively saying that his obliquity was not a new parameter but had been anticipated by reputable authorities. Similarly, in 5.7 (ed. Heiberg 1.388) Ptolemy writes that Hipparchus also had Ptolemy's value 5° for the Moon's maximum latitude.

The Almagest in the sequence of Ptolemy's works on modelling.

Since Hamilton's discovery that the *Canobic Inscription* antedates the *Almagest*, interest in it has concentrated on its divergences from the system deduced in the treatise.[10] In the present context, its relevance arises from what it contains and what it does not. In its original lapidary form (transcribed in late antiquity and preserved through one branch of the manuscript tradition of the *Almagest*), it was a votive inscription to the "Savior God," and presumably it

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[10] N. T. Hamilton, N. M. Swerdlow, and G. J. Toomer, "The Canobic inscription: Ptolemy's earliest work," in J. L. Berggren and B. R. Goldstein, eds., *From ancient omens to statistical mechanics: Essays on the exact sciences presented to Asger Aaboe* (Acta historica scientiarum naturalium et medicinalium 39, Copenhagen: University Library, 1987), 55-73; A. Jones, "Ptolemy's Canobic Inscription and Heliodorus' Observation Reports," *SCIAMVS* 6 (2005) 53-97.

constitutes a kind of public statement of at least one aspect of what Ptolemy believed he had accomplished in attaining an understanding of the structure of the cosmos. What strikes us, then, is that it is no more nor less than a practically complete set of the numerical parameters of Ptolemy's models, coextensive with those that the *Almagest* contains (allowing that some were subsequently modified) except that the inscription concludes with a section of data relating to cosmic harmonies that found no place in the *Almagest*, though likely they were presented in the lost closing chapters of Book 3 of the *Harmonics*.[11] The structures of the models are not described at all, though the nomenclature of some of the parameters presumes knowledge of the models; for example, we can tell that Ptolemy already had the final lunar model of *Almagest* Book 5 because the names of the lunar mean motions refer to the revolving eccentric deferent. Does this tell us something about what Ptolemy thought was his main accomplishment in astronomy? Or does it reflect a traditional genre of astronomical inscriptions, like the extant fragment of the Hellenistic inscription from Keskintos, that tabulated parameters while leaving their theoretical meaning an enigma to most viewers?[12]

In the sequence *Canobic Inscription – Almagest – Handy Tables – Planetary Hypotheses*, we find a few changes in the parameters of the models (those of Mercury's model in particular seem to have been subject to repeated revisiting), but very little in their structures. In the *Planetary Hypotheses* Ptolemy appears to abandon the peculiar definition of the apogee of the

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[11] N. M. Swerdlow, "Ptolemy's *Harmonics* and the 'Tones of the Universe' in the Canobic Inscription," in C. Burnett *et al.*, eds., *Studies in the History of the Exact Sciences in Honour of David Pingree* (Leiden: Brill, 2004), 137-180.

[12] A. Jones, "The Keskintos Astronomical Inscription: Text and Interpretations," *SCIAMVS* 7 (2006), 3-41.

Moon's epicycle deduced in *Almagest* 5.5. Otherwise Ptolemy seems to have focused his revisions on the models for planetary latitude, which changed significantly between the *Almagest* and *Handy Tables* and again between that work and the *Planetary Hypotheses*.[13]

Concluding remarks.

Ptolemy names remarkably few predecessors in the *Almagest*. The majority were the observers associated with observation reports and measurements (Meton and Euctemon, Aristarchus, Timocharis and Aristyllus, Archimedes, Eratosthenes, Hipparchus, Agrippa, Menelaus, Theon the Mathematician).[14] The names of Callippus and Dionysius appear only in dating formulas. Apollonius of Perge is cited as one of the geometers who provided the theorem on stationary points that Ptolemy uses in Book 12 to calculate the planets' retrograde arcs and times, though Ptolemy does not expressly state that Apollonius was the theorem's author.

Hipparchus is, of course, the most often cited astronomer; he crops up in Books 1, 3, 4, 5, 6, 7, and 9, sometimes as an observer but primarily as a mathematical astronomer. Ptolemy credits Hipparchus with the discovery and basically correct modelling of precession, but this is the only structural element of the models of which Ptolemy explicitly says that Hipparchus was the discoverer; for example he does not say that Hipparchus was the first to assume eccentric or epicyclic models for the Sun and Moon. As we have seen, he does attribute a few

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[13] N. M. Swerdlow, "Ptolemy's Theories of the Latitude of the Planets in the *Almagest*, *Handy Tables*, and *Planetary Hypotheses*," in J. Z. Buchwald and A. Franklin, eds., *Wrong for the Right Reasons* (Archimedes 11, New York: Springer, 2005), 41-71.

[14] Ptolemy occasionally prefaces the names of observers with οἱ περὶ, "those about," as if to express uncertainty about whether these individuals were themselves the actual observers.

parameters to Hipparchus, some of which Ptolemy retains while others are corrected or replaced. But the principal reason for Hipparchus's prominence in the *Almagest* is Ptolemy's expressed admiration for (and occasional criticisms of) Hipparchus's methodological approaches to problems that Ptolemy also must deal with. Even Hipparchus's presumed choice *not* to attempt to construct models for the planets is contrasted favorably in 9.2 with the failure of unnamed later astronomers to work out such models in an appropriately demonstrative manner.

As Hippocrates was Galen's close-to-ideal of what a physician should be, so was Hipparchus the close-to-ideal astronomer for Ptolemy, and this probably tells us as much about what Ptolemy sought to accomplish in the *Almagest* as his assertions of personal credit. The deductive structure of the *Almagest* perhaps consciously resembles the logical cohesion and flow of a geometrical treatise by Archimedes or Apollonius, and Ptolemy has contrived to give the sequence of topics at every scale an impression of inevitability. Recognizing that deductive rigor was Ptolemy's paramount criterion for good astronomy—or, as he insists, good *mathematics*—offers, I believe, the most promising avenue for trying to make sense of puzzling and problematic aspects of the *Almagest*, including the altered and fabricated observations.