‡1 Pan-Babylonianism Redivivus?

Fundamentalism[1] in Ivy League Garb

by David Dicks[2]

A The Integrity of Current History-of-Science Scholarship

A1 One of the few advantages of old age is the ability to take a more synoptic view of things than when one is actively engaged in the pursuance of a career. A certain detachedness can be cultivated by a retired academic; there is no longer any need to kow-tow to the pernicious doctrine of 'Publish or perish' which seems to be the sole motivation of much of what now passes for scholarship (particularly in the U.S.A.), and, provided one is still sufficiently interested to keep up with developments in one's chosen field of study (for which, in theory at any rate, there is now more time available), it should be possible to discern from a vista spanning, in my case, some 40 years, changes in the ways particular problems are envisaged and modifications in the methods used to approach them.

A2 It is gratifying to notice that, after my articles on Thales[3] and the Pre-Socratics,[4] there have been fewer attempts to foist anachronistic scientific knowledge on to famous names in the history of early Greek philosophy.[5] It is also a source of some satisfaction that the views I adumbrated in my book *Early Greek Astronomy to Aristotle* [*EGAA*], Thames & Hudson, 1970 (e.g., pp.60-61, 89-90, & *passim*) on the course of development of Greek astronomy have now become so commonly accepted that (regrettably) they are paraphrased by many writers on ancient science without any acknowledgement of their source. For example, the main ideas in an article by B.R.Goldstein and A.C.Bowen, misleadingly entitled 'A New View of Early Greek Astronomy'[6] — namely, that it was the desire to measure time that triggered off the development of Greek mathematical astronomy, that planetary theory came late (probably astrological doctrines acted as an incentive for it), and the importance of Eudoxus for initiating the scientific stage as distinct from the pre-scientific stage of empirical observations — are all fully developed in *EGAA*, some 13 years earlier than this derivative article [published by History of Science Soc]. Yet there is not a single reference

[1] [See also *DIO 1.1* ‡5 fn 12 and *DIO 1.2* §E4 & fn 129.]

[2] Long recognized internationally for his expertise in Greek astronomy and geography, David Dicks retired in 1988 as Senior Lecturer in Greek at Royal Holloway & Bedford New College (London University). His academic career has encompassed professorial posts on 3 continents, including a Visiting Professorship at Princeton University and work at the Princeton Institute for Advanced Study. His writings have appeared in numerous prominent professional journals, and he has contributed several entries to the *Dictionary of Scientific Biography*. He is author of two books: *The Geographical Fragments of Hipparchus* (Athlone Press, London, 1960) and *Early Greek Astronomy to Aristotle* (Thames and Hudson, 1970, and Cornell University, 1970).

[3] *Classical Quarterly 9* (1959) 294-309.

[4] *Journal of Hellenic Studies 86* (1966) 26-40.

[5] Although some old stagers continue to fight a losing battle, as witness the remarks of C.H.Kahn in *Science and Philosophy in Classical Greece*, ed. A.C.Bowen (Garland, New York, 1991), pp.2 & 8, who refers to an old controversy in the pages of the *Journal for Hellenic Studies*, but, not unnaturally, fails to mention *JHS 92* (1972), 175-177, where I administer the *coup de grâce*.

[6] *Isis 74* (1983) 330-340.

to *EGAA* in Goldstein & Bowen's work.[7] It is said that imitation is the sincerest form of flattery, but in reputable scholarly circles such imitation at least refers to the original.

A3 There is a fine dividing line between failure to acknowledge one's indebtedness to earlier writings because the subject matter is so uncontentious that detailed reference to the original chapter and verse is otiose, and a similar failure caused by (at best) slipshod scholarship and ignorance and (at worst) malicious intent or deliberate discourtesy. Most reputable scholars are aware of this line and instinctively stay on the right side of it — others do not. A good example of what I mean is afforded by some passages in [Brown & Harvard professor] G.J.Toomer's 'Hipparchus and Babylonian Astronomy'.[8] The last paragraph on p.360 and the first on p.361 merely restate in summary form the conclusions I had reached some 18 years earlier (see above: §A2); he even echoes my criticism (unprecedented at that time) of Schiaparelli's treatment of Eudoxus (cf. *EGAA* pp.179-180) but, by omitting to refer to this, conveys the impression that his pompous dismissal (*ipse dixit* . . . . . 'the Master has spoken') of Schiaparelli's work[9] is his own original insight. Similarly, his remarks on Hipparchus' rôle in the development of astrology[10] carefully ignore my discussion of this very topic some 28 years earlier in *The Geographical Fragments of Hipparchus* [*GFH*] (Athlone Press, London, 1960), pp.11-14. It might have been thought that simple academic courtesy would have dictated at least a brief reference to these sources; but the school to which Toomer belongs[11] disdains such niceties and sedulously avoids any appreciation of scholarship outside its own narrow confines.[12]

B The Central Error of R.Newton, D.Rawlins, & Others

B1 There is, however, another development which I have noticed increasingly in writings on ancient science in the last few decades and which should be unreservedly condemned — namely, a refusal to credit the plain evidence of ancient texts (which, if not ignored, are wilfully misinterpreted) if this goes against the particular far-fetched theory being promulgated by the writer at the time; not only this, but the ancient writer himself is implicitly (or even explicitly) criticised if his remarks do not support the modern commentator's fantasies. Out of the numerous examples of this type of misrepresentation, I select a few of the more blatant. The arch exponent is, of course, R.R.Newton [Johns Hopkins University Applied Physics Laboratory] who in his tendentiously entitled book, *The Crime of Claudius Ptolemy*,[13] by a mixture of an unhistorical approach, slipshod scholarship,[14] and a method-

[7] The fact that the diagrams on their p.335 bear a remarkable resemblance to mine in *EGAA* p.18 and *GFH* p.165 is no doubt coincidental . . . .

[8] In *A Scientific Humanist: Studies in Memory of Abraham Sachs*, ed. E.Leichty, M.de J.Ellis & P.Gerardi (Philadelphia, 1988), pp.353-362.

[9] *Ibid.* p.360 note 42.

[10] *Ibid.* p.362.

[11] For the uninitiated, adherents to this school (irreverently named the 'Muffia' by D. Rawlins in his samizdat publications — see note 27 below) can be readily identified by the frequent appearance of the letters *HAMA* in their writings — this is an acronym for *History of Ancient Mathematical Astronomy*, 3 vols. 1975, the *chef d'oeuvre* of the late O. Neugebauer of Brown University, Providence, R.I. Neugebauer is to American historians of science in the second half of the twentieth century what G. Sarton was in the first half.

[12] As I have had occasion to remark before, in my review of D.Pingree's Teubner edition of Vettius Valens in *Classical Review 39* (1989) p.24. [Similarly, see *J.Hysterical Astron. 1.2* fn 148 & §J2, and *DIO-J.HA* 1.3 ‡10, "Black Affidavit".]

[13] Johns Hopkins Univ. Press, Baltimore, 1977.

[14] As early as the Preface (p.xiii) we read, "In some cases, a topic has not seemed important enough to warrant the labor of locating a hard-to-get reference, and I have relied on secondary sources for these minor topics". Cf. p.42, "I have not seen an explicit reference to an ancient source which refers to the caravan method [for Eratosthenes' method — probably mythical (cf. J.Dutka in *Arch Hist Exact Sci 46*, 1993, p.58) — of estimating the distance from Syene to Alexandria], but I have not searched very hard"; p.136 [in a footnote], "I have not located this statement in the *Syntaxis* [Ptolemy], but I have not tried very hard to do so."

ologically unsound treatment of ancient observations,[15] managed to convince himself (if few others) that Ptolemy in the *Almagest* (our primary source for ancient Greek astronomy) consistently lied in his presentation of the evidence and that all the observations which he says he made were, in fact, 'fudged' to suit his argument. In subsequent publications attempting to substantiate this same theme of Ptolemy's fraudulence, he employs similar flawed techniques, and it is, perhaps, a measure of the mesmeric fascination of mathematics (as well as the gullibility of some modern writers on astronomy) that his erroneous views have been accorded more attention than they deserve.[16]

B2 However, Newton is by no means the only exponent of this art of misrepresentation; in fact, his severest critics reveal themselves as past masters at it. Swerdlow [University of Chicago], in an article[17] that contains one or two sensible remarks[18] amidst a plethora of baseless speculation, has no compunction in disbelieving what Ptolemy tells us about the development of solar theory in *Alm.* iii,1 and lunar theory in *Alm.* iv,2 because it does not fit in with Swerdlow's (and many others' — see below: §C1ff.) assumption that the Greek astronomers and especially Hipparchus took over most of the astronomical period relationships that they used from the Babylonians; when he can find no evidence for this unwarranted assumption — which itself arises from a conjecture of Kugler's, over-enthusiastically taken up by [Yale's] Aaboe and [Brown-Harvard's] Toomer (see below: §C2) — Swerdlow is driven to complain (p.296 footnote 7), "Ptolemy appears to be unaware of the Babylonian origin, or even the pre-Hipparchan origin, of this parameter and of the other period relations in Hipparchus' lunar theory"! He is apparently oblivious of the fact that this single sentence gives the game away completely, and shows that all his juggling with figures, and his belief that "Hipparchus need not have derived the value of the tropical year mentioned by Ptolemy, 365 1/4 − 1/300^{d}, directly from observation" (p.297), is a house of cards built on a complete disregard of what Hipparchus and Ptolemy actually say. Ironically enough, Swerdlow goes on to say, ". . . if Hipparchus had observations leading to one day in 300 years precisely, one would think that Ptolemy would have cited them"; but would Swerdlow have believed him? Hardly, to judge from the mode of 'reasoning' in this paper.[19]

B3 Even more perverse is his discussion of precession. Ptolemy first mentions this phenomenon, and ascribes its discovery to Hipparchus, in the long first chapter of the third book of the *Almagest* where he relies heavily on the latter's results and cites (sometimes verbatim) extracts from his lost works, *On the Displacement of the Tropical and Equinoctial Points* (which, in all probability, was where Hipparchus first announced his discovery), *On the Length of the Year*, and *On Intercalary Months and Days*; but Ptolemy chooses to

[15] It seems to me to be methodologically wrong to try to apply sophisticated mathematical techniques to the astronomical observations reported from antiquity. Such techniques are surely relevant only to modern-style observations carefully carried out with repect to possible sources of error, the observer's personal equation, statistical probabilities, mean values derived from hundreds of observations, etc., etc. None of this (except the occasional reference to and elementary discussion of very obvious sources of error, such as the shifting of the alignment of instruments and the effects of refraction on horizon phenomena) can properly be imputed to ancient observations, and it is therefore futile to treat them mathematically as though they were results emanating from a modern observatory. It is rather like insisting on using microscopes, pipettes and sensitive chemical balances in the preparation of farmhouse cookery recipes — and about as sensible.

[16] I am not alone in remarking this — cf. K.Okruhlik in *Proc. Philos. of Sci. Assoc. 1* (1978) 80-81. Other relevant publications of Newton are *Ancient Planetary Observations and the Validity of Ephemeris Time* (Johns Hopkins Univ. Press, Baltimore, 1976), reviewed with proper condemnation by N.T.Hamilton & N.M.Swerdlow [Newton replies at *DIO 1.1* ‡5] and by R.Mercier in *British Journal for the Hist. of Sci. 12* (1979) 211-217, and *The Origins of Ptolemy's Astronomical Parameters* (Centre for Archaeoastronomy, Technical Publication No.4, 1982), and *The Origins of Ptolemy's Astronomical Tables* (Centre for Archaeoastronomy, Technical Publication No.5, 1985), reviewed (in too kindly a fashion) by J.Evans in *Journal for the History of Astronomy [JHA] 24* (1993) 145-147.

[17] *Archive for History of Exact Sciences 21* (1980) 291-309.

[18] E.g., p.293 note b, "Some of these derivations [by modern commentators discussing ancient period relationships] seem a bit round-about . . . , and all seem more reliant upon a decimal calculator than upon the text of the *Almagest*." Unfortunately, he does not seem to realise that this applies with equal force to his own work. [See *DIO 1.1* ‡5 fn 7.]

[19] See his unsatisfactory treatment (p.299) of the order in which Hipparchus' treatises appeared — for a more satisfactory discussion see my *GFH* p.17.

postpone detailed discussion of precession until that part of his work that deals with the fixed stars[20] because, as he explains, the prior establishment of solar and lunar theory is indispensable for a proper study of the fixed stars, and such theory depends fundamentally on establishing an accurate value for the length of the tropical year, which forms the main topic for *Alm.* iii,1. Ptolemy's account is perfectly clear and logical — the only slight complication is that some observations of Spica (α Vir) and of lunar eclipses made by Hipparchus relative to the equinoctial points obviously played a dual rôle in helping to estimate both the length of the year and the rate of precession, which leads to a certain amount of repetition in *Alm.* iii,1 and *Alm.* vii,1-3 where Ptolemy treats precession at some length. He tells us that Hipparchus was led to his discovery by noting that, according to the observations made by Timocharis in Alexandria (about 150 years earlier), the position of Spica was estimated to be 8° from the autumnal equinoctial point, while his own observations gave a figure of 6°; and Ptolemy specifically tells us that Hipparchus found the same difference with other fixed stars, a difference that Ptolemy was able to confirm by comparing his own observations with those of Hipparchus — he even mentions the instrument he used (the armillary astrolabe, described in *Alm.* v,1).[21] Then in *Alm.* vii,3 further observations by Timocharis, Hipparchus, Menelaus, Agrippa, and Ptolemy himself are cited, from which a final figure of 1° in 100 years for the precessional movement (envisioned as a very slow rotation of the sphere of the fixed stars) is deduced.

B4 Now, what does Swerdlow make of all this? Very little, it seems. So obsessed is he with trying to 'prove' his absurdly speculative thesis that Hipparchus derived his rate of precession from prior knowledge (based, of course, on Babylonian sources) of the difference between the tropical and sidereal year and not (as we are plainly told) by comparing fixed star observations made by Timocharis and himself, that he can actually say (p.301), "It is **generally supposed** that Hipparchus derived his estimate of the precession from comparisons of observations of the fixed stars by Timocharis and himself separated by an interval of about 150 years. Indeed, **something of the sort may have played a role** in his qualitative recognition of precession and the distinction of the sidereal and tropical year . . ." [my emphases]. This travesty of interpretation is Swerdlow's apparent reaction to Ptolemy's clear and straightforward account!! One is entitled to ask what is the point of reading the ancient writers at all, if their explicit testimony is to be disregarded in favour of the far-fetched speculations of modern commentators?[22]

B5 Swerdlow goes on (p.305) to refer to Neugebauer's discussion of various Babylonian "years" in *HAMA*, 528-529 and quotes his mention of one parameter "attributed by Vettius Valens (2nd century) to the Babylonians, **although this parameter is not directly supported by any surviving cuneiform source"** [my emphasis]. Precisely! What Swerdlow significantly fails to quote is Neugebauer's remark on p.529, "All these 'years' are certainly to be taken as sidereal years, even if their derivation, from a modern viewpoint, would make them 'tropical' or 'anomalistic' years. [See *DIO 1.1* ‡5 fn 8.] Our sources leave no doubt that **such distinctions lie outside the framework of Babylonian astronomy**" [my emphasis]. Even Swerdlow feels obliged to say (*loc.cit.*), "Much of this reconstruction is obviously speculative"; such a statement of the obvious renders nugatory his optimistic claim on p.306, "The results of this study may be instructive . . . ."

[20] *Alm.* vii,1-3.

[21] Cf. my paper, 'Ancient Astronomical Instruments' in *J.Brit. Astron. Assoc. 64* (1954) 77-85.

[22] It is possible that acquaintance with the ancient texts only through translations plays a part in such misinterpretations. Certainly, Swerdlow thrice (pp.300, 306, & 307) quotes Toomer's erroneous translation of the Greek υπονενοηκως φαινεται as "seems to have suspected"; but this is to confuse the use of φαινομαι with the infinitive and its use with the participle — a distinction that I used to try to hammer home in my Beginners Greek classes! If Ptolemy had meant "seems to have suspected" he would have written υπονενοηκεναι and *not* υπονενοηκως which means "clearly [*or* plainly *or* evidently *or* obviously] has suspected". Manitius' [1912-1913 Teubner *Almagest*] translation (Bd.II, p.15, line 10), "Das is offenbar auch die mit Vorbehalt hingestellte Annahme Hipparchs gewesen" ["This is evidently also Hipparchus' opinion put forward with reservation"], is much more accurate; although even he 'nods' on occasion (see *JHS 103*, 1983, p.137, 'A Mistranslation in Manitius').

C The Muffia's Babylonian-Origin Idée Fixe

C1 Such wholesale disregard and distortion of Ptolemy's evidence has, unfortunately, proved irresistible for other members of the 'Muffia', who have vied with each other in selecting other parts of the Greek evidence to traduce in favour of their own misguided preconceptions. Thus, that well-known double act in American 'scholarship', Goldstein and Bowen, in their inimitable, long-winded and pompous style,[23] claim to "demonstrate that many of Hipparchus' reports and criticisms concerning Eudoxus and Aratus are, on occasion, anachronistic and even polemical" and "propose that Hipparchus does not describe faithfully the state of astronomy before his time but 'modernized' it, thereby providing Ptolemy with an astronomical history that was in some respects inaccurate and distorted".[24] One shudders to think what an "inaccurate and distorted" picture future historians of science might receive from papers such as this, which dresses up total misconceptions in pseudo-scientific language (à la R.R.Newton), demonstrates ignorance of the relevant scholarly literature (as in §1, which, e.g., fails to mention my *GFH* which deals with just the topics under discussion — cf. pp.174-175), and labours mightily to "show that the division of the day into equinoctial hours or into 360° of time was unknown in Greece at the time of Eudoxus" (cf. §§3-4), a statement of the obvious that nobody with any competence in the field would waste time on. At least, however, Bowen & Goldstein do not (yet — no doubt it will come . . .) lay claim to be clairvoyant about Hipparchus' methods, which is what Y.Maeyama does in an article[25] that not only follows the modern fashion of subjecting ancient data to inappropriately sophisticated mathematical techniques (on the illegitimacy of this see note 15 above) — and, naturally, praises Newton (p.307, "In his excellent study on fractions of degrees Newton has shown . . . . This would agree with Newton's highly interesting study on fractions of degrees") — but also actually claims to know just how Hipparchus worked (p.305, "Hipparchus left writings on only specific problems, *driven to their solutions by new discoveries which often visited him all of a sudden*") [my emphasis]!! A claim that is about as justified as his assumption "that there must have been abundant accurate observations of the fixed stars made at least at the epochs $-300 \sim -250$ in Alexandria. They must have disappeared in the fires which frequently raged there" (p.302). The cavalier manner in which ancient evidence is treated by modern commentators is well illustrated by this last remark; Maeyama is forced into making it because of his perverse refusal to accept Hipparchus' low opinion (reported by Ptolemy in *Alm.* vii,3) of Timocharis' stellar observations, a refusal which itself is a direct result of the application of inappropriate mathematical methods to the ancient data. One is tempted to quote Scott's "Oh, what a tangled web we weave . . . "! It would be tedious to cite all the recent effusions of commentators intent on (a) disbelieving and denigrating as much of the ancient sources as they can, and (b) insisting on the Babylonian origin of most of Greek astronomy [note by DR: it should be stated that the present work was independently completed & circulated before the author saw the DR & Thurston anti-(b) analyses cited below here, in the bracketted portion of fn 37]; but one particularly glaring example must be given. This is a paper by A.Jones[26] [University of Toronto & board of *Archive for History of Exact Sciences*], which as well as exemplifying both (a) and (b) above, invents the brand new concept of a "winter equinox" (p.119),[27] and contains the truly incredible sentence (p.122), "Ptolemy's organization of the theories of the Sun, Moon, and planets into an apparently rigorous logical progress from which every trace of Babylonian methodology was ruthlessly expunged, must be seen as a radical reform of the science." Shades of Newton's conspiracy theories!!

[23] Commented on even by fellow 'Muffia' member O.Gingerich [Harvard History of Science Department] in *JHA 22* (1991) 186-187.

[24] *Proc. Amer. Philos. Assoc*. vol.135 no.2 (1991), p.235.

[25] *Centaurus 27* (1984) 280-310. [See *J.Hysterical Astron. 1.2* fn 126.]

[26] *Journal for the History of Astronomy 22* (1991) 101-125. [*DIO 1.2-3* is devoted almost entirely to analysis of this bizarre *JHA* frontpage paper's math follies, which include several highschool-level foulups. See above at p.3. See also R.Newton at *DIO-J.HA 1.2* §F3.]

[27] On this, see Rawlins in *DIO*, vol.2, no.3 (1992 October), p.102ff. [Also *J.Hysterical Astron. 1.2* §B4.]

C2 Probably the most enthusiastic jumper on the Babylonian bandwagon is G.J.Toomer who, in a chapter entitled 'Hipparchus and Babylonian Astronomy' in a volume commemorating A.Sachs,[28] shows himself to be so obsessed with the idea that Hipparchus used various Babylonian values for period relationships of the moon and planets that not only does he not believe Ptolemy's clear account in *Alm.* iv,2,[29] but he actually invents (p.359) an entirely imaginary visit of Hipparchus to Babylon, which by the end of the chapter he has deluded himself into stating as fact!! No doubt 'evidence' for this mythical trip will be adduced in future publications (cf. note 32 below). Both in this chapter and in an earlier paper[30] Toomer makes great play with F.X.Kugler's work, *Die Babylonische Mondrechnung* (Freiburg im Breisgau, 1900).[31] In this book Kugler examines a cuneiform tablet (no.272 in his classification, no.122 in Neugebauer's *Astronomical Cuneiform Tablets* [*ACT*], 3 vols., 1955), which he describes (pp.9-10) as a "Rechnungstabelle des Kidinnu" and which lists new moons for the years 208 to 210 of the Seleucid Era = -103 to -100 or 104 to 101 B.C. (the epoch of the Seleucid Era being 311 B.C.);[32] the tablet comprises 18 columns of numbers, and by analysing 12 of these (the remaining 6 "were explained by Schaumberger in 1935" according to Neugebauer)[33] Kugler was able to show that they were based on System B so-called of Babylonian lunar theory.[34] Not only this, but they imply a series of astronomical parameters for mean lunar motions which are fundamental in Babylonian astronomy and which turn out to be exactly the same as those known to Hipparchus. Seizing on these coincidences (which are not all that surprising, given that both Babylonian and Greek astronomers, although using entirely different methods, were examining the same phenomena — 'the moon belongs to everyone', as a once popular song put it), Kugler leapt to the totally unwarranted conclusion that Hipparchus had simply taken his values for the various period relationships from Babylonian sources to which Kugler assigns the priority of discovery in each case. Thus he refuses to accept Ptolemy's account in *Alm.* iv,2 where it is clearly explained how Hipparchus obtained his results by comparing his own observations of eclipses with earlier ones.

C3 Now Toomer talks of "The enormous significance of this discovery", "the misleading impression which one derives from the *Almagest*",[35] "this truly astonishing revelation" and "how neglected it has been among classical scholars",[36] all in connexion with Kugler's work. Perhaps it is worth spelling out a few of the reasons why some of us have not been able to accord Kugler's conjectures the same uncritical acclaim that Toomer displays. To start with there is the date of the tablet; despite Toomer's clumsy attempt to backdate it (see

[28] See note 8 above.

[29] And it *is* a clear account despite the "inconsistencies" that A.Aaboe (in *Centaurus 4*, 1955, pp.122-125 — followed by Toomer, p.99 of note 30 below) claims to find in it. Of these "inconsistencies" the first is trivial (a discrepancy of 11 in the 4th sexagesimal place for the value of the mean synodic month, which amounts to less than 1/12th of a second!), and the second can readily be explained as the result of a Ptolemaic correction for the effect of precession (supposed to be 1° in 100 years) for the period between the early Babylonian eclipses used by Hipparchus to compare with his own observations and the dates of the latter. In fact, Aaboe himself suggests this perfectly feasible explanation, but perversely takes it as additional proof that Hipparchus, as well as making use of Babylonian observations for purposes of comparison (which nobody is going to deny — see below: §C2), knew and used the Babylonian figure for the sidereal year, which Aaboe describes (p.123) as "perhaps the most fundamental parameter in Babylonian astronomy", but which "*is nowhere attested in the Almagest*" [my emphasis].

[30] 'Hipparchus' Empirical Basis for his Lunar Mean Motions', *Centaurus 24* (1980), pp.97-109.

[31] The conjectures in Kugler's book were taken up equally uncritically in two publications by F.Cumont, in *Neue Jahrbücher für das Klassische Altertum* (Leipzig), *14*, 1911, pp.1-10, and in *Florilegium ou Receuil de travaux d'érudition dédiés à M. le Marquis Melchior de Vogüe* (Paris) 1909, pp.160-165.

[32] Incredibly, Toomer (in the Sachs volume p.354 — see above, note 8) dates this text as "computed for the years 185 to 188 of the Seleucid Era in Babylon", i.e. 126 to 123 B.C. — this presumably in order to bring it within the possible span of Hipparchus' lifetime (see my *GFH*, p.2ff.) and so buttress his own arguments.

[33] *ACT*, vol.1, p.145.

[34] Treated by Neugebauer in *HAMA*, II B, p.474ff., and elsewhere in this eccentrically organised work.

[35] Cf. note 30 above, pp.99 & 100.

[36] Cf. note 8 above p.354.

note 32 above), tablet 272 (Kugler) or 122 (*ACT*) is dated by both Kugler and Neugebauer to 104 B.C. at the earliest, i.e., some 20 years later than Hipparchus' probable life-span (see my *GFH* pp.2-3). So, as far as the date is concerned, the Babylonian scribe is more likely to have been copying Hipparchus than the other way round.[37] It is all very well for Toomer to say (p.98 in note 30 above) "Although he could not show that the tablets from which the above relationships were extracted predate Hipparchus, Kugler rightly concluded that the priority belonged to the Babylonians (as has been amply confirmed by subsequent investigations of the cuneiform material)", but not a single reference is given to substantiate this claim, and even in his later chapter in the Sachs volume (see note 8 above) all Toomer can do is to reiterate his misguided belief and weave a web of implausible inferences, still without adducing any hard evidence.

C4 Then there is the matter of the actual format of this type of Babylonian tablet; it consists entirely of lists of numbers, and detailed knowledge of the working of System B is necessary before these numbers can be interpreted in any meaningful way. Kugler himself, in a work[38] published 10 years after his *Die Babylonische Mondrechnung* in which he repeats the claims made in the earlier book about Greek borrowing from the Babylonians and the latters' priority, nevertheless introduces one welcome note of caution: on p.121 he says, "Zunächst hat man sich vor einer irrigen Vorstellung zu hüten. Die obigen Werte sind in den babylonischen Tafeln nicht etwa als einfache Beobachtungsergebnisse aufgeführt; sie sind vielmehr mit einer Reihe von andern Grószen zu einem höchst sinnreichen Rechnenmechanismus verbunden. So greifen in der Tafel SH.272 die numerischen Elemente von nicht weniger als 18 Kolumnen wie Räder einer Maschine ineinander." ["First of all one has to be on one's guard against an erroneous representation. The above values are not presented as simple observational results; rather they are connected with a series of other magnitudes to a highly ingenious calculating machine. Thus in tablet SH.272 the numerical elements are distributed into no fewer than 18 columns like wheels of a machine."] Quite obviously, without a comprehensive knowledge of how the 'Machine' works no one could derive the period relationships simply from the numbers in the columns. Yet even Kugler agrees that Hipparchus and Ptolemy did not know the **details** of the Babylonian System B[39] — but in that case, how could they possibly derive the exact periods which they are supposed to have copied? Kugler's very shaky reasoning in these pages not only involves denying Ptolemy's explicit testimony, but is logically inconsistent in itself. Equally suspect is Kugler's claim (*op.cit*. pp.85-86) that the Babylonians were the first to discover the inequality of the astronomical seasons, a claim he insists on despite saying (p.86) "Es ist angesichts dieser Verhaltnisse allerdings recht merkwürdig, dass **keine dieser babylonischen Jahrespunktbestimmungen auf uns gekommt ist**" ["It is in the face of these circumstances surely quite remarkable that **none of these Babylonian determinations of the seasonal points has come down to us**" — my emphasis], and actually agreeing that Hipparchus and Ptolemy relied on the Greek observations of Euctemon and Meton despite their inaccuracy, and not on Babylonian ones. These and other infelicities[40] afford good reason to doubt the wisdom of Toomer's apostolic fervour in promoting Kugler's conjectures.

[37] See Rawlins, *Vistas in Astronomy 28* (1985) p.256, for other instances of transmission from Greek sources to Babylonian. [See also *DIO 1.1* ‡6 §A7 & §B10-§B11, *J.Hysterical Astron. 1.2* §E1-§G4 & fn 73, *DIO 1.3* fn 266, and Hugh Thurston *Early Astronomy* (Springer, NYC, 1994) pp.123 & 128.]

[38] *Im Bannkreis Babels*, 1910.

[39] *Die Babylonische Mondrechnung*, pp.52-53. There is, in fact, no good evidence whatsoever that Hipparchus and Ptolemy knew any more about Babylonian astronomy than the occasional borrowing of some eclipse and planetary observations; certainly there is no hint in the *Almagest* or elsewhere that the Greeks knew anything about the structure of Babylonian lunar theory.

[40] E.g., his suggestion (p.51) that Hipparchus was not astronomically active before the mid 2nd century B.C. (on this, see my *GFH*, p.2) or that (pp.103-104) the Babylonians were aware of the phenomenon of precession (categorically denied by Neugebauer in *HAMA*, pp.369 & 543 note 13, who, incidentally, throws doubt on another of Kugler's cherished beliefs, namely that the author of System B was Kidinnu — in *HAMA*, p.611 we read "It is not at all evident that the colophons in question mean that Kidinnu is the architect of System B").

D Fits of Pan-Babylonianism

D1 The title of this paper refers to a phenomenon in German scholarship of the early years of this century, well described by Neugebauer:[41] "The main thesis of this school was built on wild theories about the great age of Babylonian astronomy, combined with an alleged Babylonian 'Weltanschauung' based on a parallelism between 'macrocosm and microcosm'. There was no phenomenon in classical cosmology, religion, literature which was not traced back to this hypothetical cosmic philosophy of the Babylonians. **A supreme disregard for textual evidence**, wide use of secondary sources and antiquated translations, combined with a preconceived chronology of Babylonian civilization, created a fantastic picture which exercised (and still exercises) a great influence on the literature concerning Babylonia" [my emphasis]. Ironically enough, Neugebauer goes on to praise Kugler's *Im Bannkreis Babels* for demonstrating the absurdities of this 'Pan-Babylonianism' (by collecting "17 pages of striking parallels between the history of Louis IX of France and Gilgamesh, showing that Louis IX was actually a Babylonian solar hero"), and suggests that his "example should be studied by every historian because it demonstrates far beyond its original purpose **how easy it is to fit a large body of evidence into whatever theory one has decided upon**" [my emphasis]. It seems to me that, at least partially in the restricted field of the history of ancient astronomy, 'Pan-Babylonianism' is in danger of being revived, and that certainly the two tendencies I have emphasized above are still very much in evidence. My paper is an attempt (probably foredoomed to failure, given the entrenched position of the American establishment[42] in the history of science) to protest against these tendencies and to enter a plea for a more balanced view of the relationship between Greek and Babylonian astronomy. Just because some parameters turn out to be the same in both (or, to put it more accurately, parameters that appear in Greek astronomy can be derived by modern methods from the Babylonian data preserved on selected cuneiform tablets), we are *not* justified in simply assuming that the Greeks copied the Babylonians (or *vice versa*), particularly as the textual evidence of the *Almagest* specifically tells us how the results were actually obtained. Over 20 years ago in my *EGAA* (pp.165-175) I discussed at some length possible borrowings by Greek astronomers from Babylonian sources and "the difficulties inherent in the utilization of Babylonian material" (p.171), and came to the conclusion that analysis of the evidence showed that "astronomical knowledge developed independently in accordance with the different aims of the Babylonian and Greek astronomers" (p.175). Given the totally different structure of the two systems (Greek astronomy, right from Pre-Socratic times, exhibited an essentially geometrical approach to an overall picture of an ordered cosmos, whereas the Babylonian astronomers used strictly arithmetical means to manipulate the sequence of the phenomena they were interested in — mostly horizon phenomena [see *DIO-J.HA 1.2* §E3 & §G3] such as first and last

[41] *The Exact Sciences in Antiquity*, 2nd ed., 1957, p.138.

[42] Unfortunately, Toomer is a very influential figure in this, and any idea he cares to float is eagerly taken up by other members (either singly or in pairs — see above: §C1) — even his erroneous ones. Thus, in a paper in *Centaurus 18* (1973) 6-28, he attempts to prove that Hipparchus' chord table was different from Ptolemy's by being based on a circle of radius $3438'$ ($= 57^\circ.3$), as found in Indian sine tables, instead of $3600'$ ($= 60^\circ$); his argumentation is largely circular (assume what you want to prove, and then use it to 'prove' your assumption), the figures do not support his thesis without some wildly speculative assumptions [see *DIO 1.3* eqs.19, 20, 23, 24 & especially §P2], and the comparison with Indian sources is far from convincing. Not only this, but Toomer himself, some ten years later in his translation of the *Almagest* p.215 note 75, actually repudiates his earlier suggestion by saying, "These calculations not only vindicate Hipparchus' computational abilities, but *cast doubt on my claim that he was operating with a chord table with base $R = 3438$*" [my emphasis]. Yet Neugebauer (in *HAMA* pp.299-300) accepts Toomer's fallacious thesis *in toto*, and such is the prestige of this unwieldy work (see the rave reviews when it first appeared, by Aaboe in *Isis 69*, 1978, by Chandrasekhar and Swerdlow in *Bull. Amer. Math. Soc. 84*, 1978, and by Hartner in *JHA 9*, 1978 — a slightly more judicious appraisal is given by Mercier in *Centaurus 22*, 1978) that it is likely that this error will continue to be repeated by future historians of science. [See *DIO 1.3* §P4.] In the same way, I fear that Toomer's uncritical acceptance of the false notion of wholesale and unacknowledged Greek copying of period relationships from the Babylonians is destined for similar repetition — as will be, no doubt, his invention of a Babylonian trip by Hipparchus.

visibilities of the moon and planets, but also including eclipses — to enable accurate predictions to be made for astrological and calendaric purposes), and in the light of further research, I see no reason to change this assessment. It does not seem at all strange to me that the two systems should arrive at identical results as regards lunar periods [e.g., *HAMA* p.310 eqs.2-4], independently and perhaps even at the same time (although on the available evidence I should give priority to the Greeks); rather, it would be far stranger if they differed to any significant degree, if one assumes an equal level of competence in the practitioners of both, working on exactly the same phenomena.[43] Of course, there was *some* borrowing from Babylonian astronomy. Ptolemy mentions Babylon, the Babylonians or the Chaldeans (synonymous with the Babylonians for Greek writers) about twenty times in the course of the *Almagest*, including the passage in *Alm.* iv,2 where he explains how Hipparchus arrived at his lunar periods by comparing his own observations with Chaldean ones; Ptolemy also uses three lunar eclipses of 721 and 720 B.C. in his calculations concerning the first lunar anomaly (*Alm.* iv,6), others of 491, 502 (*Alm.* iv,9), 383, 382 (*Alm.* iv,11), 621, 523 (*Alm.* v,14), and a few planetary observations (of Mercury in 237 and 245, *Alm.* ix,9, and Saturn in 229, *Alm.* xi,7), all from Babylonian sources.[44] The paucity of Babylonian planetary observations that might be of use to Hipparchus and Ptolemy is particularly noteworthy; even the three cited above are all from the Seleucid period (*and* Ptolemy is well aware of their shortcomings — cf. *Alm.* ix,2), and there are none at all for Venus, Mars, and Jupiter. The reason for this is that the planetary phenomena that interested the Babylonian astronomers were mostly horizon phenomena, which, as Neugebauer remarks,[45] were "least suited for [*sic*] Ptolemy's needs and furthermore subject to the greatest observational inaccuracy". The eclipse observations, too, that Hipparchus and Ptolemy borrowed from the Babylonians, obviously had to be selected with great care (*Alm.* iv,9), owing to the difficulties of accurate time measurement in ancient astronomy; Ptolemy himself draws attention to this as it affects Greek astronomy at the beginning of *Alm.* v,14, and a recent paper by Stephenson and Fatoohi[46] emphasizes the inaccuracy of Babylonian time measurement — according to this (p.266), "the mean discrepancy between measured and computed time-intervals is some 12 deg or almost 50 [time]minutes", and they go on to remark (p.267), "Typical errors of at least half an hour in measuring intervals of no more than six hours represents a poor performance by any reasonable standards."

D2 Other Greek borrowings from Babylonian astronomy are of a more general nature: the use of of the sexagesimal system itself is a certain example of such borrowing, and Herodotus may be right when he says that the Greeks learned about the gnomon and the division of the day into hours from the Babylonians;[47] but the idea (espoused by Kugler and many others) that the Greeks derived their stellar nomenclature (especially of the zodiacal

[43] I have never understood the aversion of scholars to recognizing coincidences; instead they go to enormous lengths to fashion hypothetical connexions in disparate systems on the flimsiest of 'evidence'. Thus, because he finds the ratio 3:2 for the longest and shortest daylight playing a rôle in Babylonian, Indian, and Chinese astronomy, Kugler (in *Im Bannkreis Babels* pp.119-120) jumps to the conclusion "*wenn wir nicht ein Spiel des blinden Zufalls bzw. menschlicher Willkür annehmen wohlen*" ["*if we do not want to assume an example of blind chance or else human arbitrariness*" — my emphasis] that "der Einflusz der babylonischen Astronomie bis nach China hinüber und bis ins Pandschab hinabgedrungen ist" ["the influence of Babylonian astronomy penetrated right to China and into the Punjab"] — which seems to me much more unlikely than simple coincidence.

[44] Because of Ptolemy's words in *Alm.* iv,2, and because he expressly states that Hipparchus also made use of the lunar eclipses in 502 (*Alm.* iv,9), 383 and 382 (*Alm.* iv,11), it is generally accepted that Hipparchus too had at his disposal all the Babylonian observations that Ptolemy mentions. [Ptolemy cites no Babylonian observations subsequent to Hipparchus.]

[45] *HAMA* p.145.

[46] 'Lunar Eclipse Times Recorded in Babylonian History', by F.R.Stephenson and L.J.Fatoohi, *JHA 24* pt.4 (1993), pp.255-267. [*DIO 1.3* fn 223 estimates the root-mean-square error of the −382-381 Babylonian eclipse trio (*Alm* 4.11) as about a half hour. (*Idem*: the rms error of the −200-199 Greek eclipse trio is ordmag 10^{m}.) All of which creates an obvious problem for 1994 Muffia speculation that 5th century BC Babylonians possessed a highly accurate lunar theory. (See also *DIO 1.2* fnn 81&87.)]
[Advice added 1997: Consult the perceptive conclusion of J.Steele & F.Stephenson at *JHA 28*:119 (1997) p.130.]

[47] Herod. ii, 109 — see my discussion in *EGAA*, pp.165-166.

constellations) from the Babylonians is almost certainly wrong — rather, recognition of the zodiac is yet another example of parallel but independent, and perhaps coincidental, development in the two astronomies.[48] What this paper is concerned to stress is that these borrowings **do not include** detailed knowledge of the various lunar periods derivable from Babylonian cuneiform tablets, as so many recent commentators assume; before this myth becomes established fact, a *caveat* should be entered about the shaky grounds on which it is being constructed, and the dubious 'reasoning' that seeks to justify it. It took the Great War of 1914-1918 to put an end to the first outbreak of Pan-Babylonianism — let us hope that another war is not required to stop the present irruption!

D3 Finally, I should like to draw attention to some wise words by W.R.Knorr,[49] which should be taken to heart by all historians of ancient science (p.163): "These three examples from the study of Euclid turn about a common methodological recommendation — that the historian of mathematics should give priority to the critical examination of the texts before undertaking a wider exploration of their philosophical and mathematical ramifications. This may sound too obvious to warrant special comment. But the combination of fragmentary evidence with a subject area readily associable with modern fields of mathematics and philosophy has made the study of ancient mathematics **an arena for ambitious interpretation, where reconstruction overwhelms textual criticism.** The result has been a striking use of intentionalist terminology in **accounts so heavily dependent on the critics' special predispositions (mathematical or philosophical), that the ancient authors could hardly have actually intended what is claimed for them.**" Substitute 'astronomy' for 'mathematics' and 'astronomical' for 'mathematical', and this describes the situation in the field of ancient astronomy with great accuracy — particularly the words I have emphasized.[50]

Publisher's Note:

Except for several bracketted informational references to *DIO* & *J.Hysterical Astron.*, inserted by DR at the author's suggestion and (as also the DR-inserted subtitle, section-titles, author-bio, & institutional identifications of scholars) approved by him at the proof-stage, the foregoing text is effectively identical to that offered by the author (1994/1/5) to the Editor-for-Life of the *Journal for the History of Astronomy*. The EfL, typically, **refused even to referee the paper**. (Further details of this incident will appear in *DIO 4.2*.) I.e., if Muffia control of apt history-of-science journaldum were still as inescapably ubiquitous as formerly, back in the medieval Era BD (Before *DIO*), then Dicks' revealing information & critical insights — and his right to publish them — might have been lost forever.

[48] Cf. *EGAA*, pp.163-165.

[49] In a chapter entitled 'What Euclid Meant: On the Use of Evidence in Studying Ancient Mathematics', pp.119-163 of the volume cited in note 5 above.

[50] I would commend these especially to the attention of Toomer, who, in his translation of the *Almagest* (Duckworth, 1984) on p.176 note 10, with breath-taking arrogance dismisses Ptolemy's account in *Alm.* iv,2 as "not historically accurate", and prefers instead his own wildly speculative views.