MAX-PLANCK-INSTITUT FÜR WISSENSCHAFTSGESCHICHTE
Max Planck Institute for the History of Science

2012

PREPRINT 430

TOPOI – *Dahlem Seminar for the History of Ancient Sciences*

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Productive Errors: Scientific Concepts in Antiquity

TOPOI – Dahlem Seminar for the History of Ancient Sciences

The Dahlem Seminar for the History of Ancient Sciences is an initiative resulting from cooperation between the Max Planck Institute for the History of Science, Berlin, and the Topoi Excellence Cluster. Future events are intended to foster stronger links between scholars at the Max Planck Institute, Freie Universität and Humboldt Universität, under the overall aegis of the Topoi Excellence Cluster. The Dahlem Seminar for the History of Ancient Sciences, under the direction of Mark Geller and Klaus Geus, organises an annual colloquium series on various innovative themes in ancient scholarship and knowledge transfer.

PRODUCTIVE ERRORS: SCIENTIFIC CONCEPTS IN ANTIQUITY

CHAPTER 1

"IRRTUM": FALLACIES IN ANCIENT SCIENCES

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The first series of the Dahlem Seminar for the History of Ancient Sciences, convened in the Autumn and Winter of 2010 at the Freie Universität Berlin, was devoted to the theme of fallacies (Irrtum) in antiquity. The Seminar series aimed at approaching the subject of fallacies from both ancient and modern perspectives, i.e. what ancients considered to be fallacious and how modern scholarship views fallacies within ancient thought. Since all of the contributions to this study have in some way related their topics to fallacies, we will survey the range of topics without specific reference to Irrtum.

No less than three of the papers in the present collection (by Geus, Asper, and Brunke) deal with mathematics, and this allows us to compare different approaches to mathematical 'errors'. Mathematics has always represented a type of specialised training and in the pre-Classical world of Mesopotamia and Egypt comprised a standard part of the curriculum (see E. Robson and J. Stedall, The Oxford Handbook of the History of Mathematics, 2009, chapters 3.1 and 9.1). Nevertheless, at a more theoretical or advanced level, mathematics requires more than training but a special aptitude to numeracy in order to grasp more abstract mathematical concepts, and not every pupil (or even teacher) will possess this innate ability. Moreover, since we have little in the way of mathematical textbooks from pre-Classical antiquity, we often depend upon school exercises and mathematical riddles for knowledge of mathematical theory and how these theories may be applied to everyday situations. What we do not know, therefore, is who was actually responsible for mathematical theory and applications before we encounter Euclid´s Elements and Archimedes´ work, as well as first actual mathematical textbook, probably the Elements of Hippocrates of Chius, c. 400 BCE. Yet there is no specific profession associated with mathematics, as there is for medicine, magic, divination, liturgy or music. Who defined the weights and measures, designed the bookkeeping, and thought up the riddles? All this data from early antiquity is intriguingly anonymous and clouded in mystery.

Having taken these factors into consideration, Klaus Geus reviews cases in which mathematical calculations appear within Greek historical writings, and he justifiably asks whether historians were able to cope with complex maths, since there is little reason to assume any connection between historical writing and mathematical competence. In fact, the Geus concludes that chosen examples from Herodotus, Thucydides, and Polybius all show that Greek historians were surprisingly capable of calculating large numbers, although not necessarily in the way modern mathematics would tackle such problems. One of the intriguing systems adduced by Geus assigns to Greek alphabet characters different corresponding numerical values, either as a 1–24 consecutive sequence or in a 27-letter sequence with values of 1–900 (similar to Semitic alphabet numeration); a third system is acrophonic, in which the alphabetic character provides the first letter of a numerical term (e.g. pente, deka, etc.). The employing of one or another of these conventions not only produced typical computational errors but also introduced various kinds of unexpected mental images and means of expression. Another inference from Geus is that such mathematical calculations in historical works could only be intended for a reader and not for a listener, although it remains likely that these historical works, at least in part, were meant to be recited aloud.

Florentina Badalanova Geller's contribution ('The Poetics of Errors') follows closely upon that of Klaus Geus, since she deals with numerical values of alphabetic scripts (in this case Glagolitic vs. Cyrillic) and certain contradictions which arise from conflicting numbering systems associated with these alphabets. Considering the reluctance of most modern scholars to (re-)do calculating or even bother with numbers and ancient numbering system, it seems that much work can still be done here in terms of knowledge transfer and globalisation.

Markus Asper, in his paper, is interested in the social context of mathematical problems and how even complex mathematics was applied to everyday life. One of the issues is the value of π, which is approximated as 3 for practical purposes and as 22/7 in set mathematical problems, which is roughly the situation found in Babylonian mathematics a millennium earlier, as well as later in the Babylonian Talmud. Such approximations should not be considered as erroneous but standard, and in fact had practical advantages, such as for tax assessors who could officially over-estimate the size of a taxable field area. Moreover Asper contextualises the same mathematical problem from Polybius discussed by Geus, involving the length of a ladder required to scale the wall of a besieged city; Asper points out that the famous miscalculation meant that the city escaped capture.

Hagan Brunke has contributed two articles to Irrtum, both on mathematical themes. The first, 'On Mesopotamian Measure Theory', also defends the value of π used in Babylonian geometry as valid, despite being only an approximation of 3.141592..... Brunke provides various models for calculating π as 3 within Babylonian mathematics, all of which are clearly illustrated by diagrams. For a reader who remains baffled by complex mathematical formulae, the significant message is the level of sophisticated abstract thinking demonstrated by Babylonian mathematicians. Brunke's second contribution, on 'Embedded Structures' within Babylonian mathematics, is based upon diagrams found on a cuneiform tablet in the shape of knots and mazes. Brunke shows that these complex knots which can be compared to intertwined snakes (which is a frequent literary image or Bildsprache in Akkadian texts), represent drawings of a specific sort of geometric structure (with different complexity). They are collected together on the tablet in a similar way as a specific sort of objects (represented by their names) appears in Mesopotamian lexical lists. The analogy is attractive but not entirely apt, since lexical lists do not usually create a single collective whole produced by individual entries, unless one thinks of anatomical lists comprising the human body as an entity, or star lists describing the heavens. Nevertheless, the logic is persuasive, that the same type of thinking which produced Listenwissenschaften could have been responsible for the geometric diagrams on cuneiform tablets.

Mathematics plays a significant role in sundials, as explained by Irina Tupikova and Michael Soffel, which models several different types of sundials used in the ancient world, based on relative orientations in respect to the latitude of the sundial's position. In effect, the simplest type of sundial was oriented towards the equator and earth's axis, with the calculation of the inclination of the ecliptic to the equator being 23.5 degrees, already known to the Greeks. Nevertheless, this type of sundial is hardly attested in Greek but was popular in China. The interesting feature of the newly proposed mathematical model is that minor calculation errors are easily noticeable, which means that Tupikova´s formula can also be used to determine the correct latitude of the sundial's location, and the 'errors' in setting up the sundial or the true location of displaced sundials can now be readily identified. Another rather technically simple type of sundial was known from Egypt. The authors point out that measuring accurate astronomical time is not necessarily the essential goal of such an instrument, but that coordinated sundials giving the same approximate timings of events may have sufficed for ancient users, since ancient sundials were primarily used to synchronize social life. Nevertheless, it would be interesting to know to what extent simple trial-and-error may have played a role in positioning sundials, in addition to more sophisticated mathematical calculations mentioned in the article.

The use of calibrations is also essential for the manufacture of musical instruments, as explained by Graeme Lawson. The important feature of Lawson's descriptions of pre-modern musical instruments in general is the element of trial-and-error, since hordes of discarded incorrectly calibrated flutes were discovered by archaeologists, suggesting that the process of producing flutes with the required musical scales was somewhat hit-or-miss. This type of archaeological data is a salutary lesson for anyone working on ancient science, i. e. trying to determine the correct evaluation of and uses for medicinal drugs or mineral compounds in manufacturing processes. In the absence of laboratories, trial-and-error appears to be the only available means to get things right. Lawson describes a similar situation with stringed instruments, since one common problem with lyres was to balance the tension of the strings with the required thinness of the sound board. Lawson also draws attention to Wissenstransfer within music, particularly in the translation of Greek liturgical songs into Latin, a process fraught with difficulties.

We return to Egypt for another view of exact sciences and calculations, but this time related to astronomy and astrology, as explained by Alexandra von Lieven in a contribution which opens many new research questions. Von Lieven's discussion revolves around a Ptolemaic commentary on a much earlier astronomical text, and her text somewhat resembles Babylonian astronomical diaries; in fact the Egyptian word for 'commentary' (bl) is possibly a calque on the Akkadian for 'commentary', pishru (Hebrew pesher), essentially meaning 'resolution' or 'explanation' ('Auflösung'). The astronomical system used in Egypt divided the heavens into 10 distinct regions or Decans, consisting of 36 'weeks' of 10 days each, totalling an approximate 360-day solar year. This was later adapted into the Zodiac as 36 subdivisions of the zodiac (i. e. three per zodiac sign) of 10 degrees each. Within von Lieven's late commentary, the Decans refer to a group of 36 stars (although like in Babylonia, individual stars are not distinguished from constellations). The interesting feature of this text is the designation of the 'horizon' as the Duat, since in texts from earlier periods Duat designated both heaven and netherworld, whereas in this text it has generally shed its associations with the afterlife in a kind of secular cosmology.

The similarities between this Demotic astronomical commentary and its Babylonian counterparts brings us back to Mesopotamia and to the concept of fallacy within its vast omen literature. On one hand, ancient omens are often considered to embody the inherent fallacy of post hoc ergo propter hoc, i. e. confusing causation with sequential occurrences, although this matter has been hotly debated. Nevertheless, during the course of the Dahlem Seminars, Eva Cancik-Kirschbaum made the astute observation that there is no real concept of 'Irrtum' within Mesopotamian sciences. Within the Mesopotamian system, information derived through divination is always ipso facto correct, since gods always provide valid predictions, and problems only arise through human interpretation of divine messages. The omens may be ambiguous and difficult to interpret, or even contradictory, but none of this invalidates the divine message as fallacious. This theme has recently been taken up in relation to ancient dreams: according to J. Bilbija (The Dream in Antiquity, Aspects and Analyses, Ph.D dissertation, Vrije Universiteit Amsterdam, 2012), there is virtually no evidence for deceptive dreams in Akkadian sources (p. 41), while in Egypt no deceptive dreams can be found before the Ptolemaic period (p. 62). In contrast to Greek literature, where as early as Homer´s Iliad Zeus sends Agamemnon a false dream, the idea that gods could send false dreams is a relative latecomer and with it comes the notion that dreams – like omens – could be false and erroneous conveyers of portentous messages.

A similar theme has been addressed by Mark Geller, but from the unusual perspective of that adopted by Cicero in his De Divinatione, in which he set out to debunk the logic behind all omens and divination as faulty and fallacious. The argument is that Cicero was surprisingly well-informed about systems of divination which are best represented in Akkadian sources, and that previous assumptions that Cicero depended upon Etruscan or other local forms of divination are incorrect. Rome was the greatest cosmopolitan centre of the ancient world and Roman awareness of Babylonian sciences cannot be ruled out on a priori grounds; Babylonian tablets were still being actively studied and read in the first century BCE. In fact, Cicero reveals some of his sources, such as the seemingly well-known Diogenes of Babylon, and the question is whether this eminent scholar and philosopher may actually be known by a Babylonian name in local Akkadian sources.

We end where we began, with Greek science, but this time focusing on Colin King's discussion of logical fallacies and syllogisms in Aristotle. The interesting point here is how remote Aristotle's analyses were from any other discussions of ancient science from Greece and elsewhere in antiquity. No other sources in this collection of essays refer to syllogisms or logical fallacies as a technical subject or abstract theme in itself. In effect, the syllogism was the invention of Greek thought but was by no means universal in antiquity, and it hardly plays a role in discussions of error and fallacy in ancient writings. This in itself is surprising.

The concept of the Dahlem Seminars (which continue to be held each academic year) is that interdisciplinary approaches to ancient science allow for a more comprehensive view of differing systems of ancient thought, and how these attempted to explain the natural and social phenomena of their respective environments. The Dahlem Seminar in 2011–12 introduced the theme of Esoteric Knowledge in Ancient Sciences, and for 2012–13 the Dahlem Seminar will pursue the topic of Common Sense Science in Antiquity.

CHAPTER 2

ON MESOPOTAMIAN MEASURE THEORY

Hagan Brunke

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This paper is to illustrate how school knowledge of (modern) mathematics can lead the modern researcher to consider ancient mathematical practice erroneous (or merely approximative or even wrong); and how extended knowledge of it may force them to *re*consider. In particular, alleged error shall be confronted with possible definition. Our case study will be Mesopotamian methods of evaluating (the size of) certain geometric entities.

2.1 Measure and Error

Usually, the ancient Mesopotamian practice of computing the area of a circle as three times the square of the radius[1] which results in the value 3 for what we call "the number π" or of an irregular quadrilateral as the product of the mean values of opposite lengths is considered a more or less rough approximation. While this is possibly true for the latter case there is reason to assume the first case rather being the consequence of a particular *definition* of circular area measure.[2] What does it mean to say that the use of 3 instead of 3.1415... is "inaccurate" or even "wrong"?[3]

[1]Actually, this is not the way of computation explicitely found in the ancient Mesopotamian records, but the area of a circle was computed as $\frac{c^2}{12}$ where c is the circumference of the circle. This corresponds to the "modern" $\frac{c^2}{4\pi}$ with $\pi \to 3$. Even if the diameter d of the circle was given, they first computed the circumference as $c = 3d$ (corresponding to $c = \pi d$ with $\pi \to 3$) and then the area by means of $\frac{c^2}{12}$.

[2]Brunke (2011).

[3]Of course the statement "$\pi = 3$" is wrong because π is the name of a number *defined* differently by modern mathematicians. But *the use of the number 3 instead of the number π* for the computation of a circle's area is *a priori* not. It is merely inaccurate in terms of the modern definition of area computation, and may be correct in terms of a definition different from ours.

This is closely related to the question what a measure is and what properties it is required to have. To measure something means to assign a size value of some sort to it. How does one do that and how can one be wrong? To make the case more clear, let us start with a simple example. Suppose there is an ancient culture providing us with textual evidence for the assignment of size values to various plane figures (e.g. fields or parcels) as in figure 1.

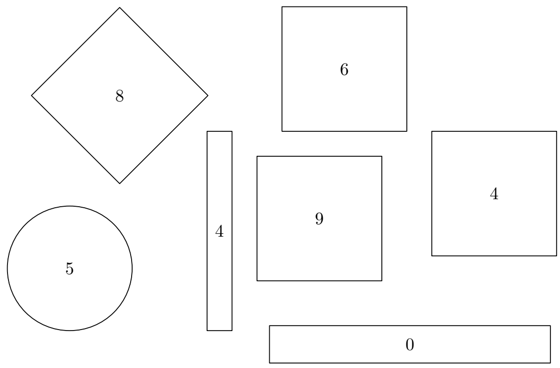

Figure 1: Example of size value assignment.

Let us not speculate about the modern scholar's verdict. Surely, it will be nothing like "obviously, these people have not yet developed a consistent concept of area measurement or have at least performed it in a very rough manner." Why not? Because a modern scholar is, of course, fully aware that neither assigning the value "zero" to an obviously non-empty field, nor assigning different values to fields of equal shape and length dimensions is anything to feel uneasy about. It's just that the concept underlying the measuring differs from our own. And to make a statement like the one just quoted would mean to make *one's own* measurement, by means of one's own (familiar) rules, and then call somebody else's an error or "wrong" or at least "rough" just because of this difference in concept.[4]

See below. Without a specific frame of reference, there is no meaning of "wrong".

[4]It sometimes seems, though, that the very same people who insist on seeing ancient mathematics not through the eyes of modern mathematics are doing exactly this, namely by judging

For example, the assignment of size values shown in Figure 1 could result from the following concept. The plane is furnished with a grid of points (which might, in practical terms, represent an array of date palms in a garden or of vegetable plants in some plantation), and the size of an area is defined to be the number of grid points contained in it (thus defining "size" by, say, produce or profit of a parcel), as is indicated in Figure 2.

Even though the example may look somewhat artificial, to connect pieces of land with the amount of their produce is quite natural and can be found in probably every agricultural society. Anyway: If a hypothetical scholar said, "obviously, these people have not yet developed a consistent concept of area measurement," then he'd be wrong. She might be right, of course, (even though unable to be sure of it) when omitting the word "obviously". After all, "these people" may just have been idiots; but that doesn't follow from the scarce evidence we might have from them.

Let's close this example with two remarks. First, it should be pointed out that this method of size value assignment is indeed an example of what is called a "measure" in modern measure theory, which means that the size value for each piece of land is non-negative and that, whenever in a collection of pieces no two of them overlap, the value assigned to their union is the sum of the values assigned to each of the pieces (additivity of measures). And second, the points whose number defines the size or value measure of a piece of land need not be arranged in a grid; the concept works as well with a set of arbitrarily distributed points (representing gold mines perhaps).

There is, however, one aspect of the above example one could nevertheless feel uneasy about: movement of a piece of land seems to result, in some cases, in a change of the size measure assigned to it. But then it is not the piece of land

ancient methods versus school geometric knowledge, and thus coming up with judgements such as "inaccurate", "false", "naive" etc.

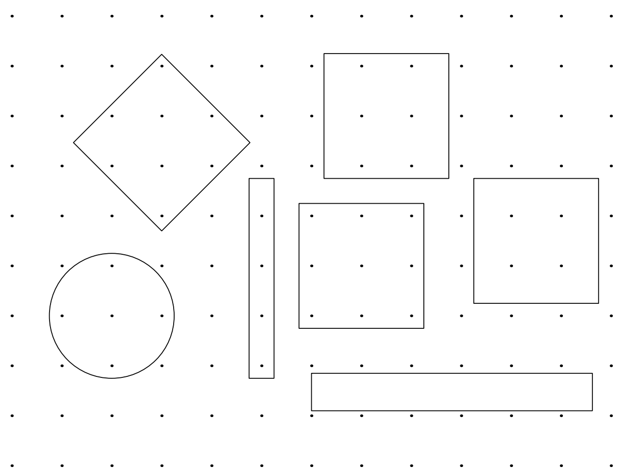

Figure 2: Possible origin of the size value assignments in Figure 1.

that you move, but an immaterial frame or shape that is laid over the land. And the uneasiness only results from this very idea of a "geometric figure as such", i.e. thinking of abstractions rather than real pieces of land[5] — abstractions that exist independent of a concrete localisation in space (or on the earth's surface). It is this independence of concrete localisation that enforces the size measure to be invariant under such operations as translations, rotations, or reflections. And it is — in very rough terms — this abstraction that defines the step from physical observation to mathematical thinking.

2.2 Circles

The desire to assign size values in a consistent way to such abstract geometric objects (planar or spacial)[6] generates the need for a concept of measure that ensures that the size values only depend on the shape and the lengths of characteristic linear elements (such as sides, diagonals, or diameters) of the geometric object in

[5]And thinking of the real pieces of land as of "material representations" of the abstract figures.
[6]And, in consequence, to measure real physical entities, like pieses of land, by measuring the abstract figure they are represented by, in the sense of note 5.

question. This more or less automatically amounts to using reference objects of defined shape and linear dimensions as base measure.[7]

A rather natural way is — for the case of planar geometric objects — to use *unit sqares*, i.e. squares of a defined side length, as base measures and to assign to a figure which is composed of non-overlapping[8] unit squares the number of these unit squares as size value or "area". For example, the figures shown in Figure 3 have the size value (area) "25 unit squares", or "25 lollies/square meters/. . ." when you decide to call the unit square "lolly/square meter/. . .", or just 25 when there is no confusion about the unit used.

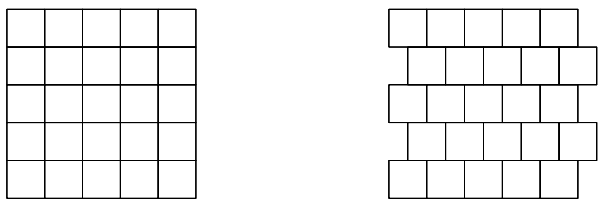

Figure 3: Figures composed of 25 non-overlapping unit squares.

Similarly, a rectangle composed of h rows each of which contains w unit squares (in such a way that no two squares overlap) has the size value "$w \cdot h$ unit squares", or — when there is no confusion about the unit used — just $w \cdot h$. When in addition the planar measurement is connected to length measurement in such a way that the length of the sides of the unit square is the *unit length* (defined correspondingly), then this amounts to "number of length units in width times number of lenght units in hight" or just "width times hight". When including fractions of the unit square (and assigning to them the corresponding fractions as size values), this rule extends to general rectangles. This concept of size value assignment is the Mesopotamian as well as ours.

[7]For the following see Brunke (2011).

[8]Note that the requirement of non-overlapping does not forbid the figures to *touch* each other.

In principle, this allows for the assignment of size values (areas) for arbitrary figures whose boundary consists of straight line segments (e.g. triangles, arbitrary quadrilaterals, etc.) by cutting and pasting, according to the above-mentioned understanding of additivity.[9] But what about curvilinearly bounded figures? Since they cannot be obtained from squares by cut and paste operations the size value assignement to them has to be explicitely *defined*.

The modern approach is — roughly speaking — to approximate the circle by increasing unions of squares (considered as fractions of the unit square), e.g. like in Figure 4, and to define the area of the circle as the *limit* of the sequence of these unions' areas. The quotient of this limit and the square of the circle's radius is then called π and has the value 3.141592... This amounts to the same concept as the Archimedean method of approximating the circle by an increasing sequence of inscribed respectively circumscribed regular polygons, as indicated in Figure 5, and thus obtaining lower and upper bounds of a limit that is considered the area of the circle itself.

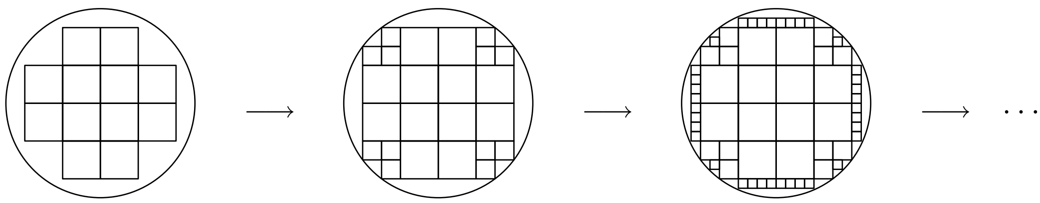

Figure 4: Exhausting a circle by more and more non-overlapping squares.

Now, 3 is (not too bad) an approximation of π with a relative deviation of about 4.5 per cent. And in view of the concept just described, *using 3 instead of* π as a ratio between the circle's area and the square of its radius is of course an approximation (corresponding to approximating the circle by an inscribed regular

[9]In the case of irregular quadrilaterals the Mesopotamian method differs from the one described here, see above.

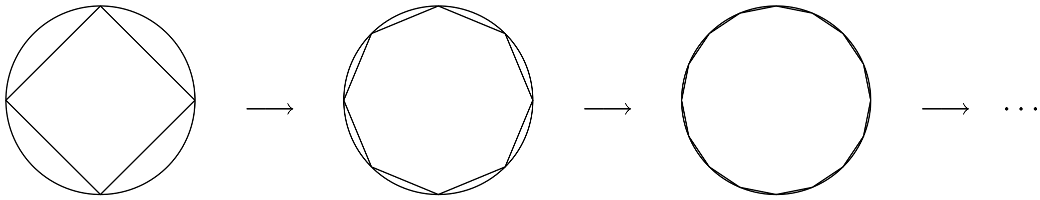

Figure 5: Exhausting a circle by a sequence of inscribed regular $4n$-gons with increasing n.

12-gon, see Figure 6). But this statement about approximation (and together with it every verdict on the approximation's quality) becomes invalid when dealing with a different concept of size value assignment to the circle. And there must have been a different concept in Mesopotamia since there was no idea of "limits" or suchlike.

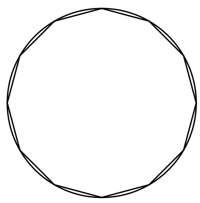

Figure 6: A circle with an inscribed regular 12-gon the area of which is $3r^2$.

In Brunke (2011), I have suggested that the Mesopotamian equivalent of π, namely the coefficient 3, is the result of defining the area of a circle as the mean value of the areas of an inscribed and a circumscribed square (cf. Figure 7),[10] mainly based on considerations on the old-Babylonian geometric problem text BM 15285[11] and

[10]The area of the inscribed square is half the area of the circumscribed one. Damerow (2001, 240-43) convincingly argues that a sequence of some of the problems of BM 15285 (see note 11) could have served as a step by step deduction of this fact. It follows that the mean value of the two squares' areas is three quaters of the area of the circumscribed square. But the latter is just d^2, if d denotes the circle's diameter. So we end up with $\frac{3}{4}d^2 = 3r^2$ with r the radius of the circle.

[11]BM 15285 is a collection of problems, each of which contains a drawing of a planar figure and a

the fact that expressing size values of planar as well as spacial geometric objects by means of mean values was a central method in Mesopotamian mathematics, for another example of which see below.

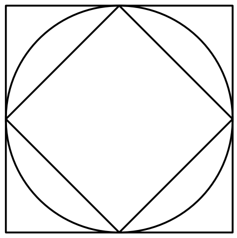

Figure 7: A circle with an inscribed and a circumscribed square. The mean value of the two squares' areas is three times the square over the circle's radius.

Another possible "modern error" in connection with circle measurement concerns the interpretation of computational practice. The fact that Mesopotamian problem texts regularly compute the area of a circle from the circumference rather than from the diameter or radius (cf. note 1 above) has led modern scholars to consider the use of the circumference as *fundamental concept* instead of just *standard practice* of circle computation.[12] Of course, Robson is correct in saying that "... the circumference is the starting point from which the circle is conceptualized" (Robson, 1999, 37), if one is talking about the *circle itself*, as a geometric entity (especially in view of Akkadian terminology, see Robson, *loc. cit.*). But the circumference would have hardly been the point of origin for the *computation of the circle's area*, since it is completely unnatural and unintuitive to erect the square over a bent line. This point of origin is more likely to be comparison to other

verbal description of its construction. The text asks for the areas of the figures' constituents, but does not give the answers. For a full treatment of the completely collated text with handcopy and publication history see Robson (1999, 208-17). A colour photograph of the obverse can be found in Walker (1991, 250 bottom), and a photograph (obverse and reverse) of the then known fragment in Neugebauer (1935b, plates 3-4).

[12]E.g., Høyrup (2002, 372); Robson (1999, 37). Cf. Brunke (2011, 123[16]) for this.

elementary figures, maybe the way suggested above. The use of the circumference as the basic *computational tool* may have developed for practical reasons: "If the circle is the cross-section of a massive cylinder, the entity which is most easily measured *is* evidently the thread stretched around it" (Høyrup, 2002, 372[453]). While textual evidence is and must be the principal starting point for all considerations concerning ancient mathematics, it has to be taken into account that in our case at hand this evidence does by no means offer a direct view on the origins of the mathematical ideas. The collections of mathematical problems, partly offering solutions, and of technical or geometric coefficients represent the practical computational standards of the ending third and beginning second millennium BC, and it is not their intention to inform about how these computational techniques and methods originally arose and developed long before. Similarly, in a modern collection of problems with solutions one won't find the original ideas of, say, Gauss but just a presentation of today's methods that result from them.

There remains the question why the value 3 has also been used to compute the circumference from the diameter. The use of the same value for the computation of area (in disguise of the coefficient $\frac{1}{4 \cdot 3}$, see above) and circumference suggests that the one computational method has been derived from the other. Maybe one started vith an *annulus* rather than a whole circle, and conceptualized this annulus as sort of bent trapezoid, as indicated in Figure 8.

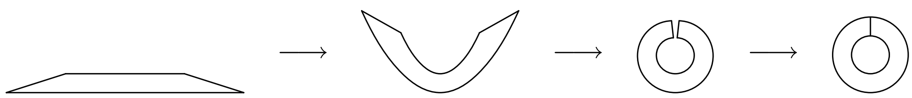

Figure 8: An annulus obtained as a "bent trapezoid".

If we denote the radii and the circumferences of the outer and the inner circle (bounding the annulus) by R and r, C and c, respectively, the parallel sides of the underlying trapezoid have the lengths C und c, and the trapezoid's hight is $R - r$, whence its area is computed as[13]

$$A = \frac{C+c}{2} \cdot (R-r)\,.$$

On the other side the area of the annulus naturally is the difference of the two circles' areas, $A = 3(R^2 - r^2) = 3(R-r)(R+r)$ (the latter identity reflecting one of the binomial formulae which were well known and play an important role in the old-Babylonian problem texts). Thus we have $\frac{C+c}{2} \cdot (R-r) = 3(R-r)(R+r)$, whence $\frac{C+c}{2} = 3(R+r)$ or $C+c = 3(D+d)$, when D and d denote the respective diameters. From this one might have found the relation circumference $= 3\cdot$ diameter.[14,15] For another possibility how the relation between circumference and diameter might have been obtained, see Brunke (2011, 124).

2.3 One more definition?

The following is of rather speculative nature and is meant as a question rather than a statement. The old-Babylonian[16] tablet BM 85194 contains a collection of thirty-five solved problems, one of which we shall consider here: obv i 1-12 deals with a "quasi-prismatic"[17] ramp with trapezoidal cross sections. By this is meant

[13]The computation of the area of a trapezoid as the product of its hight and the mean value of its parallel sides is attested from as early as the end of fourth millennium BC; see Friberg (1997/98).

[14]This possibility may be reflected in, e.g., the problem text Böhl 1821 (Leemans, 1951) which gives the difference of the radii, $R-r$, and the area A of an annulus and asks for the perimeters of its bounding circles. The solution starts by computing $3(R-r)$ (line 8) and from this $A \cdot \frac{1}{3(R-r)}$ (line 9). From what follows (lines 10-13) in order to compute the circumferences, it is clear that $A \cdot \frac{1}{3(R-r)}$ has been recognized as equalling $R+r$, so the relation $A = 3(R-r)(R+r)$ was used in order to obtain circumferences from the radii. (There is a mistake in line 13, though, since the scribe forgot to multiply $2R$ and $2r$ by 3 again to get the circumferences; see M. Bruins in (Leemans, 1951, 34-35)).

[15]Note that the transformation of bending the trapezoid into an annulus doesnot preserve angles. Nevertheless, the resulting formula for the area of the annulus is "by accident" correct also in the sense of today's geometry (with $3 \to \pi$).

[16]Robson (2008, 94): "It is very likely that they come from the city of Sippar, and their spelling conventions suggest a date in the late seventeenth century", based on Høyrup (2002, 329-332). For transliteration, translation and comment of the tablet see Neugebauer (1935a, 142-193), for photographs Neugebauer (1935b, plates 5-6).

[17]Friberg (1987–90, 567).

that at each point along the length of the ramp, the cross section is a trapezoid, but that this trapezoid changes not only in size but also in proportion. This means that we are not dealing with a prism (constant cross section) or a truncated pyramid (cross section changing in size but proportionally) but with a body that has *curved surfaces* as left and right sides; cf. Figure 9.

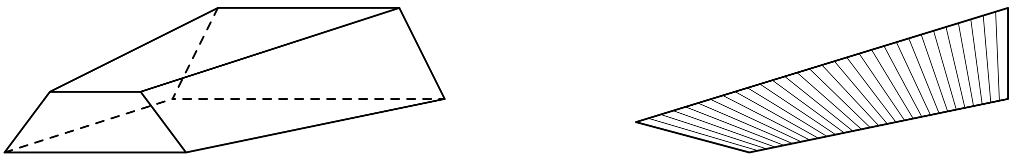

Figure 9: Schematics of the "quasi-prismatic" ramp from the tablet BM 85194 and an (exaggerated) illustration of its curved right side.

Whereas the volumes of prisms and cylinders were computed the same way we do it (product of base area and height), truncated pyramids and cones were treated by means of an averaging process similar to that for computing the area of the trapezoid and, as suggested above, possibly the circle, which is not in accordance with our modern definition of "3-dimensional" size value assignment.[18]

The method used for the quasi-prism in the text amounts to

$$V_{\text{text}} = \frac{1}{2}\left(\frac{A+B}{2} + \frac{a+b}{2}\right) \cdot \frac{H+h}{2} \cdot l$$

where a and b denote top and bottom whidth and h the hight of the small (front) trapezoid, A, B, H accordingly for the big (back) trapezoid, and l the length of the ramp. This is equivalent to

$$V_{\text{text}} = \frac{1}{2}\left(\frac{A+a}{2} + \frac{B+b}{2}\right) \cdot \frac{H+h}{2} \cdot l\,,$$

[18]For which reason Friberg (1987–90, 567) calls it "false volume formula", an expression reflecting the fixation to modern school geometry (as are expressions like "naive approximation" in connection with our quasi-prism (Friberg, *loc. cit.*)). For a possible use of the (in our modern understanding) "correct" formula for the volume of a truncated pyramid (Friberg, 1987–90, 567) see the discussion in Neugebauer (1935a, 187-188).

i.e. the lenght is multiplied with the area of the cross sectional trapezoid half way between front and back.[19] This corresponds to one of the two averaging procedures found for the computation of truncated pyramids and cones.[20] Nevertheless, this is not the way the text puts it. Is this a reflection of the knowledge about the curved sides? If so, here too we might be dealing with a *definition* (rather than a result of mere analogy). Note that an explicit definition of the size value to be assigned to such a body would in principle be necessary, since its curved surfaces present the same difficulty as does the curved boundary in the case of the circle.

One final remark: The volume formula is also equivalent to

$$V_{\text{text}} = \frac{1}{2}\left[\frac{1}{2}\left(\frac{A+B}{2}H + \frac{a+b}{2}h\right)\cdot l\right] + \frac{1}{2}\left[\frac{1}{2}\left(\frac{A+B}{2}h + \frac{a+b}{2}H\right)\cdot l\right] .$$

It is interesting to observe that the modern way of evaluating this object's volume (i.e., the modern size value assignment) by means of

$$V = \int_0^l \frac{\left(A + \frac{x}{l}(a-A)\right) + \left(B + \frac{x}{l}(b-B)\right)}{2}\left(H + \frac{x}{l}(h-H)\right)\,\mathrm{d}x$$

results in

$$V = \frac{2}{3}\left[\frac{1}{2}\left(\frac{A+B}{2}H + \frac{a+b}{2}h\right)\cdot l\right] + \frac{1}{3}\left[\frac{1}{2}\left(\frac{A+B}{2}h + \frac{a+b}{2}H\right)\cdot l\right]$$

and thus both V and V_{text} can be brought into the shape of a weighted mean value of the terms $\left[\frac{1}{2}\left(\frac{A+B}{2}H + \frac{a+b}{2}h\right)\cdot l\right]$ and $\left[\frac{1}{2}\left(\frac{A+B}{2}h + \frac{a+b}{2}H\right)\cdot l\right]$, just with different weights (1 and 1, respectively 2 and 1). Here, $\frac{1}{2}\left(\frac{A+B}{2}H + \frac{a+b}{2}h\right)$

[19]Note also that

$$\frac{1}{2}\left(\frac{A+B}{2} + \frac{a+b}{2}\right) = \frac{1}{4}(a+b+A+B) ,$$

i.e., the size value assigned to the quasi-prism is the same as the value assigned to a rectangular block whose width is the avarage of the four widths of the bounding (front and back) trapezoids, whose height is the average of the two trapezoids' heights and whose length is the length of the quasi-prism.

[20]The other procedure being to average the sizes of the top and bottom (corresponding to front and back here) surfaces, and to multiply this average with the height (corresponding to lenght here); see Friberg (1987–90, 567).

is the mean value of the areas of the front and back trapezoids, whereas $\frac{1}{2}\left(\frac{A+B}{2}\,h + \frac{a+b}{2}\,H\right)$ is the same with the heights exchanged.

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CHAPTER 3

KONNTEN GRIECHISCHE HISTORIKER RECHNEN? ANMERKUNGEN ZU EINIGEN MATHEMATISCHEN STELLEN BEI HERODOT, THUKYDIDES UND POLYBIOS.*

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0. Einleitung

In der schöngeistigen Literatur der Griechen finden sich auffällig häufig mathematische Einlagen. Aristophanes lässt in den Vögeln einen Sprecher über die Quadratur des Kreises philosophieren. Einer der alten Tragödiendichter brachte das Problem der Würfelverdoppelung auf die Bühne. In der Anthologia Graeca ist eine ganze Reihe von mathematischen Aufgaben in metrischer Form überliefert. Auf die mathematischen Stellen im Corpus Platonicum muss wohl nicht besonders verwiesen werden.[1] Auf den ersten Blick scheint also die Mathematik bei den Griechen einen höheren Stellenwert als bei uns heute gehabt zu haben.

Diese Vermutung scheint dadurch Bestätigung finden, dass auch bei den griechischen Historikern mathematische Passagen vorkommen. Eine möglicherweise sich daran anschließende Folgerung, dass diese Historiker „besser“ rechnen konnten als wir, möchte ich einer näheren Prüfung unterziehen.

1. Problemstellung

Da ich an dieser Stelle unmöglich Dutzende von Autoren und Hunderte von Stellen vorstellen kann, beschränke ich mich auf drei der berühmtesten und wohl auch scharfsinnigsten Historiker, auf Herodot, Thukydides und Polybios. Ziel meines Beitrages soll sein, paradigmatisch zu untersuchen, ob sie überhaupt (und gegebenenfalls) wie sie rechneten und welche Einstellung sie insgesamt zur Mathematik hatten.

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* Für Hinweise danke ich Reinhold Bichler, Anca Dan, Edgar Reich, Rainer Streng – der sich erneut um die technische Seite des Preprints verdient gemacht hat – und last but not least Irina Tupikova.

[1] Aristoph. av. 656–64, 1001–3; Eutoc. comm. in Archim. sphaer. cyl. p. 88, 4ff. = Anon. fr. 166 (TrGF); Anth. Graec. XIV.

Der Historiker Thukydides[2] vergleicht zu Beginn seines Werkes den Trojanischen Krieg, den Homer beschrieben hat, und den Peloponnesischen Krieg, den er beschreiben möchte.[3] Sein uns heute merkwürdig anmutendes[4] Argument, dass sein Gegenstand der bedeutendere sei, beruht vor allem auf der Behauptung, dass am Peloponnesischen Krieg mehr Soldaten beteiligt waren als am Trojanischen Krieg.[5] Wörtlich schreibt Thukydides (1, 10, 4–5):[6]

> „[Homer] lässt nämlich von 1200 Schiffen die der Boioter 120, die des Philoktet 50 Mann fassen, womit er, wie mir scheint, die größten und kleinsten bezeichnet ... Dass sie selbst an den Rudern saßen und alle Kämpfer waren, gibt er bei den Schiffen des Philoktet an [Hom. Il. 2, 510 u. 719]: alle Ruderer lässt er dort nämlich auch Bogenschützen sein. Bloße Mitfahrer sind auf den Schiffen kaum viel gewesen, außer den Königen und den höchsten Würdenträgern, zumal sie mit Kriegsgerät über See wollten, und zwar auf Schiffen, die ohne Verdeck nach alter Art mehr zur Seeräuberei gebaut waren. Nimmt man die Mitte zwischen den größten und kleinsten Schiffen, so waren es offensichtlich nicht viele, die mitkamen – jedenfalls für Leute, die aus ganz Griechenland gemeinschaftlich geschickt wurden.“
> πεποίηκε γὰρ χιλίων καὶ διακοσίων νεῶν τὰς μὲν Βοιωτῶν εἴκοσι καὶ ἑκατὸν ἀνδῶν, τὰς δὲ Φιλοκτήτου πεντήκοντα, δηλῶν, ὡς ἐμοὶ δοκεῖ, τὰς μεγίστας καὶ ἐλαχίστας … αὐτερέται δὲ ὅτι ἦσαν καὶ μάχιμοι πάντες, ἐν ταῖς Φιλοκτήτου ναυσὶ δεδήλωκεν· τόξότας γὰρ πάντες πεποίηκε τοὺς προσκώπους. περίνεως δὲ οὐκ εἰκὸς πολλοὺς ξυμπλεῖν ἔξω τῶν βασιλέων καὶ τῶν μάλιστα ἐν τέλει, ἄλλως τε καὶ μέλλοντας πέλαγος περαιώσεσθαι μετὰ σκευῶν πολεμικῶν οὐδ᾽ αὖ τὰ πλοῖα κατάφαρκτα ἔχοντας, ἀλλὰ τῷ παλαιῷ τρόπῳ ληστικώτερον παρεσκευασμένα. πρὸς τὰς μεγίστας δ᾽ οὖν καὶ ἐλαχίστας ναῦς τὸ μέσον σκοποῦντι οὐ πολλοὶ φαίνονται ἐλθόντες, ὡς ἀπὸ πάσης τῆς Ἑλλάδος κοινῇ πεμπόμενοι.

Nach diesen Worten wendet sich Thukydides der nachtrojanischen Zeit zu und lässt einen irritierten Leser zurück. Thukydides nennt sämtliche Terme einer mathematischen Gleichung:

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[2] Thukydides geht es in seinem Proömium vor allem um zwei Dinge: „die Behauptung der Größe seines Gegenstandes und die Garantie dieser Behauptung“ (Schadewaldt 1982: 320).

[3] In 1, 23 sagt allerdings Thukydides: „Von allen früheren Taten war also die bedeutendste der Perserkrieg …“ Vgl. 7, 20: „Unter den Feldzügen, die wir kennen, war der des Xerxes der bei weitem größte, in einem Ausmaß, dass sich weder der Zug des Dareios gegen die Skythen daneben sehen lassen kann, noch der der Skythen gegen die Kimmerier, noch – nach dem, was darüber berichtet wird – der Zug der Atriden gegen Ilion, noch der der Myer und Teukrer, der vorher geschah ... alle diese und andere Feldzüge kommen nicht auf gegen diesen einen, den Xerxeszug.“

[4] Der Topos von der Größe des Krieges war bei den griechischen Historikern beliebt. Vgl. z. B. Polyb. 5, 33: „Und doch, wer ist so unkundig, um nicht zu wissen, dass um jene Zeit in Iberien und Sizilien und Italien die zahlreichsten und größten Taten vollbracht wurden, und dass der Hannibalische Krieg der bedeutendste und längste war, mit Ausnahme dessen, der um Sizilien geführt wurde, und dass wir alle, bei der Größe desselben, unsere Blicke auf ihn richten mussten, angstvoll dem erwarteten Ausgang entgegensehend?“). Obwohl die Bemerkung des Polybios auf hellenistische Historiker wie Timaios anspielt, ist sie auch und vor allem eine Auseinandersetzung mit Thukydides´ Behauptung.

[5] Dieser Punkt war Thukydides so wichtig, dass er ihn gleich im 2. Satz seines Werkes herausstrich (I 1): „Er begann damit gleich beim Ausbruch, in der Erwartung, der Krieg werde bedeutender werden und denkwürdiger als alle früheren …“ Vgl. auch 1, 21: „Und obgleich die Menschen den Krieg, den sie gegenwärtig gerade führen, immer für den größten halten, um nach seinem Ende wieder das Frühere höher zu bewundern, so wird doch dieser Krieg sich dem, der auf das wirklich Geschehene merkt, als das größte aller bisherigen Ereignisse erweisen“; außerdem 1, 23.

[6] Vgl. dazu z. B. Howie 1984. Hornblower 1991: 35 nennt es „An over-rational argument.“

die Gesamtzahl der Schiffe, das Maximum und Minimum einer Kampfeinheit pro Schiff,[7] zuletzt eine numerisch begrenzte und daher vernachlässigbare Variable wie eine zusätzliche Besatzung. Er scheint durch die Bildung eines Mittelwerts zwischen den größten und kleinsten Schiffen sogar den ersten Schritt in Richtung Lösung dieser Gleichung machen zu wollen – versäumt es dann aber zum Schluss, die einfache Rechnung durchzuführen.[8] Thukydides bringt sein zentrales Argument, dass der Trojanische Krieg weniger bedeutend sei, nicht zum Abschluss.[9] Es verpufft.[10]

Kommen wir zu Herodot.[11]

Der Pater historiae schreibt in seinen Historien (7, 186–7) über das persische Heer, das Griechenland im Jahre 480 v. Chr. angegriffen hat, das Folgende:

> „So hat 5 283 220 Mann Xerxes, der Sohn des Dareios, bis nach Sepias und den Thermopylen geführt. Dies ist die Zahl der gesamten Heeresmacht des Xerxes, die Zahl der Köchinnen, der Nebenfrauen und der Eunuchen aber kann wohl niemand genau nennen ... Daher kommt es mir gar nicht wunderbar vor, dass das Bett von manchen Flüssen austrocknete, viel eher kommt mir wunderbar vor, dass für so viele Zehntausende die Lebensmittel ausreichten. Denn beim Zusammenzählen komme ich zu folgendem Ergebnis: Wenn jeder einen Choinix [1 Choinix ≈ 1,1 l] Weizen täglich erhielt und nicht mehr, haben sie für jeden Tag 110 340 Medimnen [1 Medimnos ≈ 52 l) verbraucht.“
> οὕτω πεντακοσίας τε μυριάδας καὶ εἴκοσι καὶ ὀκτὼ καὶ χιλιάδας τρεῖς καὶ ἑκατοντάδας δύο καὶ δεκάδας δύο ἀνδρῶν ἤγαγε Ξέρξης ὁ Δαρείου μέχρι Σηπιάδος καὶ Θερμοπυλέων. οὗτος μὲν δὴ τοῦ συνάπαντος τοῦ Ξέρξεω στρατεύματος ἀριθμός, γυναικῶν δὲ σιτοποιῶν καὶ παλλακέων καὶ εὐνούχων οὐδεὶς ἂν εἴποι ἀτρεκέα ἀριθμόν·... ὥστε οὐδέν μοι θῶμα παρίσταται προδοῦναι τὰ ῥέεθρα τῶν ποταμῶν ἔστι ὧν, ἀλλὰ μᾶλλον ὅκως τὰ σιτία ἀντέχρησε θῶμά μοι μυριάσι τοσαύτῃσι. εὑρίσκω γὰρ συμβαλλόμενος, εἰ χοίνικα πυρῶν ἕκαστος τῆς ἡμέρης ἐλάμβανε καὶ μηδὲν πλέον, ἕνδεκα μυριάδας μεδίμνων τελεομένας ἐπ᾽ ἡμέρῃ ἑκάστῃ καὶ πρὸς τριηκοσίους τε ἄλλους μεδίμνους καὶ τεσσεράκοντα.

Auch hier ist man verblüfft; nicht darüber, dass Herodot die Zahl des persischen Invasionsheeres so genau angeben konnte – sie ist natürlich viel zu hoch –, sondern über den Rechenfehler, den Herodot hier begangen hat. Eine Choinix ist der 48. Teil eines Medimnos. Der Ta-

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[7] Zu der hier nur am Rande relevanten Frage, ob tatsächlich die Besatzung mit den Kombattanten weitgehend identisch ist, vgl. einerseits Morrison/Williams: 1968: 46, 68, andererseits Casson 1973: 63, Anm. 103.

[8] Es fehlt bei Thukydides auch eine Beschreibung der Stärken während des Peloponnesischen Krieges, vielleicht wegen der wechselnden Verhältnisse. Vgl. zu den Zahlen Morpeth 2006.

[9] Wenn wir die Rechnung des Thukydides vollenden, kommen wir auf ca. 1200 Schiffe x 90 (Mittelwert von 120 und 50 zuzüglich 5 Mitfahrer), folglich auf ca. 108 000 Mann allein aus Griechenland. Möglicherweise unterstellt Thukydides auch deswegen Homer poetische Übertreibung (1, 10), weil ansonsten dessen Krieg tatsächlich ein größeres Aufgebot an Schiffen und Soldaten gehabt hätte!

[10] Etwas anders resümiert Gomme 1945: 114: „Thucydides cannot in fact be acquitted of a certain inconsequence; this excursus, like most of the others, has not been fully thought out.“ Hornblower 1991 hat zu diesem Sachverhalt nichts zu sagen.

[11] Für das Thema „Die Rechenfehler Herodots“ stütze ich mich vor allem auf den vorzüglichen Aufsatz von Keyser 1985/86.

gesbedarf war also 5 283 220 : 48, was 110 067,08 Medimnen[12] und nicht 110 340 entspricht, wie Herodot schreibt.[13]

Kommen zu einem dritten Beispiel.

Es findet sich bei Polybios[14] in einem poliorketischen Zusammenhang.[15] Für den Sturm auf eine Stadt sei die Kenntnis der Mauerhöhe von größter Wichtigkeit, damit man in der Länge passende Leitern herstellen könne.[16] Konkret schreibt Polybios (9, 19, 5–7):

> „Die Art und Weise, die passende Länge der Leitern zu bestimmen, ist die folgende: Wenn durch irgendeinen Mitkämpfer [in der Stadt] die Höhe der Mauer verraten wurde, ist die passende Länge der Leitern klar. Wenn nämlich die Höhe der Mauern 10 Einheiten beträgt, müssen die Leitern ein wenig mehr als 12 Einheiten lang sein. Der Abstand der Leiter muss, wenn er für die Hochsteigenden richtig bemessen sein soll,[17] halb so groß sein wie die Leiter, damit sie [die Leitern], wenn sie weiter entfernt hingestellt werden, infolge der Menge derjenigen, die darauf treten, weder leicht brechen noch umgekehrt, wenn sie in geraderer Richtung aufgestellt werden, für die Angreifer die Gefahr des Überkippens haben."[18]
>
> περὶ δὲ τῆς τῶν κλιμάκων συμμετρίας τοιοῦτός τίς ἐστιν ὁ τρόπος τῆς θεωρίας.[19] ἐὰν μὲν γὰρ διά τινος τῶν συμπραττόντων δοθῇ τὸ τοῦ τείχους ὕψος, πρόδηλος ἡ τῶν κλιμάκων γίνεται συμμετρία· οἵων γὰρ ἂν δέκα τινῶν εἶναι συμβαίνῃ τὸ τοῦ τείχους ὕψος, τοιούτων δώδεκα δεήσει τὰς κλίμακας δαψιλῶν ὑπάρχειν. τὴν δ᾽ ἀπόβασιν τῆς κλίμακος πρὸς τὴν τῶν ἀναβαινόντων συμμετρίαν ἡμίσειαν εἶναι δεήσει τῆς κλίμακος, ἵνα μήτε πλεῖον ἀφιστάμεναι διὰ τὸ πλῆθος τῶν ἐπιβαινόντων εὐσύντριπτοι γίνωνται, μήτε πάλιν ὀρθότεραι προσερειδόμεναι λίαν ἀκροσφαλεῖς ὦσι τοῖς προσβαίνουσιν.

Polybios rät also an dieser Stelle, die Sturmleitern in einem bestimmten Winkel anzulegen, steil genug, dass sie unter der Last der aufsteigenden Truppen nicht zusammenbrechen, flach genug, daß die Gefahr des Überkippens gering gehalten wird. Die Länge der Leitern wird nach dem Satz des Pythagoras im rechtwinkligen Dreieck berechnet.

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[12] How/Wells 1912: 213 schreiben fälschlich „110,067½" (recte: 110 067 1/12).

[13] Weitere Rechenfehler Herodots listen Flower/Marincola 2002: 161 auf. Insgesamt hat sich Herodot nach Keyser 1985/86 an sieben Stellen seiner Historien verrechnet.

[14] Polybios ist an Naturwissenschaft und Technik sehr interessiert. Vgl. bes. 9, 2: „Für die Darstellung des Geschehens in der Gegenwart habe ich mich entschieden, erstens, weil sich immerfort Neues ereignet und dies infolgedessen fortlaufend einen neuen Bericht verlangt – denn natürlich konnten die Früheren uns nicht von Dingen erzählen, die erst später passierten –, zweitens, weil dies das Allernützlichste schon immer war, vollends aber jetzt, weil Wissenschaft und Technik in unserer Zeit einen solchen Aufschwung genommen haben. dass man alles, was in jeder Lage an uns herantritt, gleichsam methodisch zu bewältigen in der Lage ist, sofern man sich nur um Erkenntnis und Wissen bemüht."

[15] Polybios betont in 9, 14 die Wichtigkeit der mathematischen Kenntnisse für den Feldherrn. Er berichtet in 9, 19, dass im J. 217 v. Chr. ein makedonischer Angriff auf Meliteia wegen zu kleiner Leitern scheiterte.

[16] Die benötigte Information beschaffte man sich entweder durch Agenten, mittels eines Fadens, der an einem zur Mauerhöhe hochgeschossenen Pfeils befestigt war (vgl. Veget. IV 20, 3), auf Grund trigonometrischer Berechnungen oder durch Zählen der Ziegelsteinschichten. Vgl. dazu Geus 2012: 115–6 (mit weiterer Literatur).

[17] Walbank/Habicht 2011: 51: „so as to achieve a proper relationship to those ascending"; anders in der älteren Übersetzung Patons (1925) zur Stelle (mit fraglicher Bedeutung von συμμετρία): „in order to suit the convenience of those ascending it".

[18] Vgl. auch Polyb. 5, 97–8. Der Leiterangriff spielt im militärischen Denken des Polybios eine wichtige Rolle. Umso pikanter sein Fehler. Vgl. aber jetzt die Interpretation von Markus Asper in diesem Band.

[19] Die gewählte Ausdrucksweise mag – worauf mich Anca Dan hinweist – auf eine Vorlage des Polybios hindeuten. Vielleicht hat diese das pythagoreische Standarddreieck (3:4:5) einfach mit dem Faktor 2,5 multipliziert.

In seinem Beispiel nennt Polybios eine Mauerhöhe von 10 Einheiten und eine Länge der Leiter von „ein wenig mehr als 12“ Einheiten. Aus diesen Angaben berechnet er, dass der Abstand der Leitern „unten“ halb so lang wie die Leiter selbst, also ca. 6 Einheiten beträgt. Und das ist evident falsch, wie wir leicht nachrechnen können.

Die Länge der kleineren Kathete in einem rechtwinkligen Dreieck erhalten wir, indem wir die Wurzel aus der Differenz der Produkte von Hypotenuse und der anderen Kathete ziehen. In unserem Fall also: Wurzel aus (12^2 - 10^2) bzw. Wurzel aus 44, was ca. 6,63 Einheiten entspricht. Polybios hätte also besser gesagt, dass der Abstand der Leiter zur Mauer 7 Einheiten beträgt. Sein Rechenfehler wird noch deutlicher, wenn, wie Polybios ja explizit schreibt, die Leiter etwas länger als 12 Einheiten beträgt. Bei einer Leiterlänge von 12,21 Meter betrüge der Abstand exakt 7 Meter.

Wir haben nun drei verschiedene Fälle kennen gelernt, aus denen sich auf Unsicherheiten und Rechenfehler seitens der griechischen Historiker schließen lässt. Einen Zufall in den Quellen dürfen wir ausschließen. Die gewählten Beispiele stellen nämlich nur eine kleine Auswahl aus. Wir werden heute einige noch weitere Fälle für fehlerhaftes Rechnen bei den Historikern kennen lernen. Begäben wir uns außerdem noch auf das Feld der Epigraphik und Papyrologie, würden wir unseren Eindruck noch bestärkt finden. Selbst in offiziellen Inschriften wie den „Athenian Tribute Lists“ lassen sich fehlerhafte Rechnungen nachweisen. Dies verdient deswegen besondere Erwähnung, weil es in Athen ein eigenes Gremium gab, die logistai, die zusammen mit den euthynoi die Abrechnungen und den Finanzhaushalt in regelmäßigen Abständen zu kontrollieren hatten.[20] Und das Beherrschen der Grundrechnungsarten gehörte sicherlich zu den Voraussetzungen, um eine solche Position ausüben zu können.

Auch der Ausweg, dass es sich bei unseren Beispielen um Textverderbnisse handelt, ist wegen der Fülle des Materials nicht gangbar. Zudem wäre eher der umgekehrte Fall zu erwarten, dass im Laufe der Überlieferung die Fehler durch einen aufmerksamen Schreiber korrigiert worden, als dass umgekehrt Fehler hinzugekommen wären. Die falschen Zahlen in unseren Textausgaben sind also eine Art lectio difficilior.

Unser vorläufiger Befund lautet daher: Offenbar bewegten sich die griechischen Historiker, aber vielleicht nicht nur die, auf dünnem Eis, wenn sie kompliziertere Rechenoperationen durchführten. Wie erklärt sich das?

Man wird wohl kaum behaupten wollen, dass die gebildete Griechen wie Herodot,

[20] Im 4. Jh. wurden aus den Mitgliedern des Rats zehn Logistai ausgelost, die in jeder Prytanie der Beamten prüften. Vgl. [Aristot.] pol. 48, 3; Lys. 30, 5. Die Schlussrechnungen kamen nach Ablauf des Amtsjahres an zehn durch das Los aus der Bürgerschaft bestellte Logistai und ihre Synegoroi. Vgl. [Aristot.] pol. 54, 2.

Thukydides und Polybios mit den elementaren Grundrechnungsarten nicht gut vertraut waren oder nicht richtig rechnen konnten. Sie waren gewiss nicht dümmer als ihre modernen Historikerkollegen. Das Fehlen von Computern und Taschenrechnern wird sogar eher dazu geführt haben, dass sie im Kopfrechnen geübter als wir heute waren.

Nach diesen Vorüberlegungen stelle ich die eingangs aufgeworfene Frage erneut, füge aber das Interrogativpronomen „Wie“ hinzu: Sie lautet also: „Wie konnten griechische Historiker rechnen?“

2. Griechische Zahlen und die Anderson-Methode

Die Griechen hatten mehrere Möglichkeiten, um Zahlen auszudrücken. Sie konnten sie einfach als Wörter ausschreiben oder sie konnten sie abkürzen. Bei den Abkürzungen gab es eine Fülle von lokalen Varianten. In der Regel kürzten die Griechen aber nach einem der alphabetischen Systeme oder nach dem so genannten akrophonischen System ab.

α	1	ι	9	ρ	17
β	2	κ	10	σ	18
γ	3	λ	11	τ	19
δ	4	μ	12	υ	20
ε	5	ν	13	φ	21
ζ	6	ξ	14	χ	22
η	7	ο	15	ψ	23
θ	8	π	16	ω	24

Im alphabetischen System entspricht ein Buchstabe einem Zahlwert, also Alpha = 1, Beta = 2, Gamma = 3. Man unterscheidet bei dem alphabetischen System zwei Varianten: Das erste ist das Thesis-System aus den bekannten 24 griechischen Buchstaben, bei dem in aufsteigender Weise einfach „durchgezählt“ wird (das also mit Omega gleich 24 endet). Dieses Thesis-System ist uns vor allem als Buchnummerierung bekannt: beispielsweise sind die 24 Bücher von Ilias und Odyssee nach diesem System bezeichnet. Größere Zahlen als 24 gibt es in diesem System nicht, weshalb es für Rechnungen so gut wie nie verwendet wird.

Α	1	Ι	10	Ρ	100
Β	2	Κ	20	Σ	200
Γ	3	Λ	30	Τ	300
Δ	4	Μ	40	Υ	400

Ε	5	Ν	50	Φ	500
ϛ	6 (auch ϝ)	Ξ	60	Χ	600
Ζ	7	Ο	70	Ψ	700
Η	8	Π	80	Ω	800
Θ	9	ϙ	90 (auch ϟ)	↑	900 (auch ϡ)

Das zweite alphabetische System besteht nicht nur aus 24, sondern sogar aus 27 Buchstaben – einschließlich der drei Sonderzeichen Stigma bzw. Digamma für 6, Koppa für 90 und Sampi für 900. Im Unterschied zum Thesis-System „springt“ es ab „Zehn“ (Iota) gleich zu den anderen Zehnern. Kappa entspricht also der „Zwanzig“, Lambda der „Dreißig“ und mit Omega wird nun nicht der Zahlwert 24, sondern auch 800 ausgedrückt.

Die Tausender werden dann wieder von vorne gezählt, d. h.: ‚α = 1000, ‚β = 2000, ‚ι = 10 000, ‚κ = 20 000. Zur Unterscheidung tragen in unseren Textausgaben die Zahlen bis 999 „rechts oben“ einen Strich („Apostroph“)[21], ab 1000 einen „links unten“.[22] Dieses System ist auch als das Milesische System oder nach dem Grammatiker Herodian als Herodianische Zahlen bekannt.[23]

Neben den beiden alphabetischen Systemen gibt es außerdem noch das so genannte akrophonische System.[24] Beim akrophonischen System werden die Anfangsbuchstaben der Zahlwörter zur Schreibung der entsprechenden Zahlwerte benutzt: Π = 5 (von πέντε), Δ = 10 (von δέκα), Η = 100 (von ἑκατόν), Χ = 1000 (von χίλιοι), Μ = 10 000 (von μύριοι).[25] Außerdem gab es zusätzlich 5er-Bündelungen, um die Sache übersichtlicher zu machen. Denn es ist auf einem Blick oft schwer auszumachen, ob z. B. sieben, acht oder neun „Striche“ nebeneinander stehen.

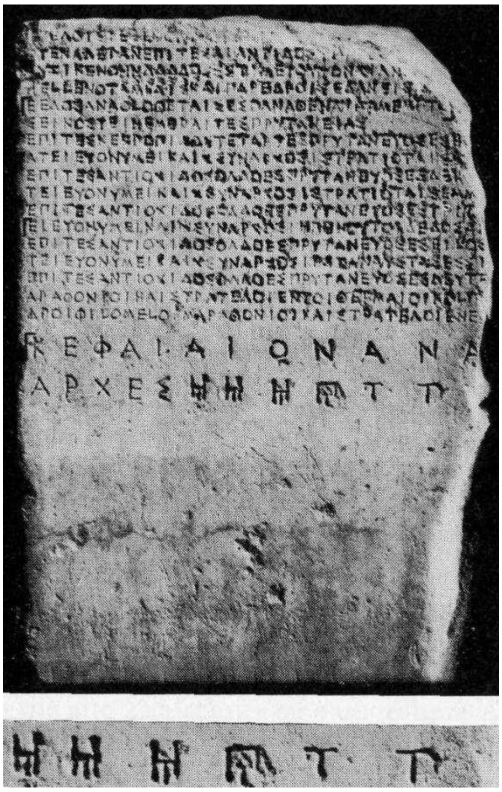
[21] Alternativ werden sie auch von einem Strich überschrieben.
[22] Theoretisch konnte man mit den Apostrophen Zahlen bis 1 Million darstellen. Da das Myriadensystem in Griechenland aber sprachlich fest verankert war (und größere Zahlen ohnehin selten waren), verzichtete man darauf, über 9999 hinauszugehen. Selbst im Neugriechischen ist der Gebrauch des Zahlwortes Million (das in Italien im 14. Jh. entstand) unüblich.
[23] Durch additive Verbindungen lässt sich jede beliebige Zahl schreiben, z. B. 318 = λιη (Lamba + Iota + Eta = 300 + 10 + 8).
[24] Von Menninger 1958: 77 als „Reihenschrift“ bezeichnet.
[25] Ausführlicher (mit Beispiel) bei Hankel 1894: 37*.

Ι = 1	𐅃Ι = 6	Η = 100
ΙΙ = 2	Δ = 10	ΗΔ = 110
ΙΙΙ = 3	ΔΔ = 20	Η𐅄 = 150
ΙΙΙΙ = 4	𐅄 = 50	ΗΗ = 200
𐅃 = 5	𐅄Δ = 60	𐅅 = 500
𐅅Η = 600	𐅇 = 50,000	
Χ = 1,000	𐅇ΜΜ𐅆ΧΧΧ𐅅Η𐅄ΔΔΔ𐅃Ι = 78, 696	
ΧΗ = 1,100		
𐅆 = 5,000		
Μ = 10,000		

Man sieht sofort, dass die Verwechslungsgefahr durch diese verschiedenen Systeme groß ist. Die griechische Buchstabenform „Eta“ (H) entspricht im Thesis-System der Zahl 7, im milesischen System der Zahl 8 und im akrophonen System der Zahl 100 (und konnte natürlich auch in Texten als Wort ἤ [„oder“] oder als einzelner Buchstabe verstanden werden). In der Praxis ist aber meist klar, welches System vorliegt, vor allem wenn mehrere Zahlen genannt oder wenn Rechenoperationen ausgeführt worden sind. Für das Rechnen haben ohnehin nur das milesische und das akrophone System Bedeutung. Das milesische System wurde häufig in privaten attischen Inschriften sowie in den Dokumenten in der Zeit ab 100 v. Chr. sowie in den meisten anderen griechischen Städten, außerdem in den Papyri verwendet. Das akrophone System war fast nur in Athen und überwiegend nur in den offiziellen Inschriften bis etwa 100 v. Chr. im Gebrauch.[26]

Dort sind die Zahlen oft mit Einheiten wie Obolen oder Talenten zu Kompendien oder Ligaturen zusammengestellt. Beispielsweise „verschmilzt“ der einfache Strich für eine Einheit mit einem T für Talent zu einem T. Oder das H trägt eine dritte Haste. Das ist aus dem beiliegenden Bild unschwer zu ersehen:

[26] Vgl. Tod 1911–1912; Tod 1913; Tod 1926–1927.



Diese bekannte Inschrift[27] aus den Athenian Tribute Lists handelt von den Ausgaben Athens in den Jahren 418–14, die aus dem Tempelschatz der Athena bezahlt wurden. Der Text ist fragmentarisch, aber mit Sicherheit zu ergänzen. Für das letzte Jahr (415/14 v. Chr.) wird zum Schluss der Gesamtbetrag für die Ausgaben (κεφάλαιον ἀναλόματος) in diesem Amtsjahr angegeben.

Die Zahl, d. h. die Gesamtsumme wird in der Literatur merkwürdigerweise unterschiedlich gelesen. Karl Menninger (1958: 75) in seinem Werk Zahlwort und Ziffer (übrigens auch in der englischen Ausgabe des Buches) liest 327 Talente, Brodersen, Günther und Schmitt (1992: 108) vermuten im 1. Band der Historischen Griechischen Inschriften in Übersetzung 153 Talente, Meiggs/Lewis in A Selection of Greek Historical Inscriptions (1984: 233) haben in ihrer Transkription anscheinend 353 Talente, schreiben aber im Kommentar (1984: 235) „between 353 and 355“ Talente.

Ich kann in der Zahl drei „Hunderter“ (H mit dem dritten Unterstrich für Talent), einen „Fünfziger“ (eine Kombination aus Π, kleinem δ und einer Haste für Talent) und nur zwei T (also zwei Talente) erkennen. Es last sich nicht gänzlich ausschließen, dass rechts von dem 2. Tau noch etwas stand, das weggebrochen ist. Aber selbst in einem solchem Fall müsste man

[27] IG I^2 302 = GHI 77 = IG I^3 370 = HGIÜ I 128.

ein drittes oder gar viertes Tau in der Transkription als Ergänzung durch Klammern kenntlich machen.

Schlussendlich kann die Summe aber nur maximal 354 Talente betragen haben, weil die letzte Fünf in 355 als Π (für πέντε geschrieben worden wäre. Ich lese also hier nicht 327 oder 153 oder 353, sondern 352 Talente.[28]

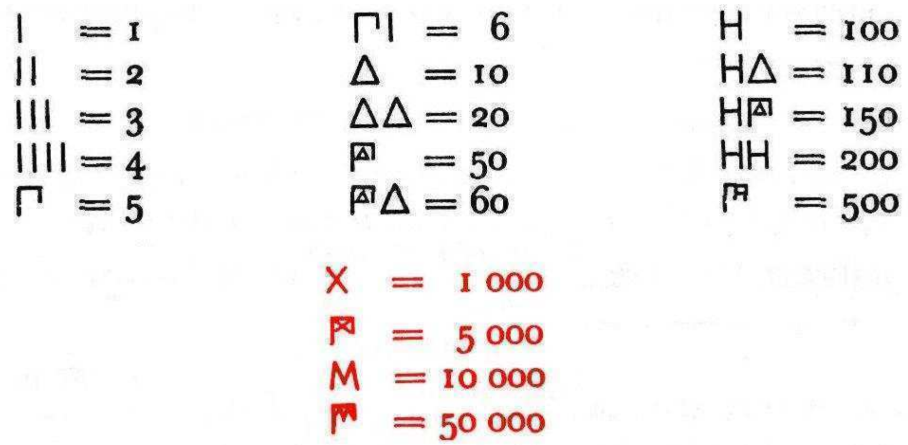
Kommen wir zu unseren historiographischen Beispielen zurück. Welches dieser Systeme verwendeten nun unsere drei Historiker?[29]

Da in den Codices und erhaltenen Papyri die Zahlen ausgeschrieben sind, könnte man zunächst vermuten, dass die Historiker weder nach dem alphabetischen noch nach dem akrophonen System gerechnet haben.[30] Wir haben aber auf der anderen Seite auch gute Hinweise, dass zumindest Herodot das akrophone System kannte. Er lässt nämlich in 7, 103 den Perserkönig Xerxes zu Demaratos das Folgende sagen:

> „Erwäge doch nur, was möglich und wahrscheinlich ist! Wie sollen 1000 oder 10 000 oder 50 000 Menschen, die auch alle gleichermaßen frei sind und nicht von einem Einzelnen befehligt werden, diesem gewaltigen Heer Stand halten können? Es kommen ja, wenn jene [Spartaner] 5000 Mann stark sind, mehr als 1000 auf einen Einzigen."
> ἐπεὶ φέρε ἴδω παντὶ τῷ οἰκότι· κῶς ἂν δυναίατο χίλιοι ἢ καὶ μύριοι ἢ καὶ πεντακισμύριοι, ἐόντες γε ἐλεύθεροι πάντες ὁμοίως καὶ μὴ ὑπ' ἑνὸς ἀρχόμενοι, στρατῷ τοσῷδε ἀντιστῆναι; ἐπεί τοι πλεῦνες περὶ ἕνα ἕκαστον γινόμεθα ἢ χίλιοι, ἐόντων ἐκείνων πέντε χιλιάδων.

Es fällt an dieser Stelle zunächst auf, dass Herodot im ersten Teil des Textes nicht, wie es natürlich wäre, sagt, 1000, 10 000 und 100 000 (oder gar 1 000 000), sondern die „krumme" Zahl 50 000 nennt. Überdies passt das genannte Verhältnis von 5 zu 1 gar nicht zu dem, was er kurz vorher und kurz nachher (7, 60; 7, 184) über die Größe des Perserheeres sagt. Dort ist nämlich von 1 7000 000 Persern die Rede. Wohl aber fügt sich alles wunderbar in das „Fünfer-System" der akrophonen Zahlen.

[28] Das mag als Illustration dafür dienen, dass die griechischen Zahlen auch heute noch selbst geübten und ausgezeichneten Epigraphikern Schwierigkeiten machen können.
[29] Für unsere Fragestellung ist es wichtig zu wissen, dass in den beiden alphabetischen Systemen und im akrophonen System die Werte in absteigender Reihenfolge angegeben werden (100 + 50 + 3). Herodot, bei dem die Zahlen ausgeschrieben sind (jedenfalls in unseren Textausgaben, aber auch in den erhaltenen Papyri), schreibt dagegen die Zahlen in der Regel in aufsteigender Reihenfolge, also 3 + 50 + 100.
[30] Nach griechischer Schulgrammatik (vgl. z. B. Bornemann/Risch 1978; 70, § 73, 2) muss bei aufsteigender Reihenfolge *καί* zwischen den Zahlen stehen, bei absteigender Reihenfolge kann *καί* entfallen. Bei Herodot sind die meisten Zahlen Ordinalzahlen.



Herodot hat in seinem Beispiel – und ich denke, ganz bewusst – die vier größten Symbole des akrophonen Systems zitiert, nämlich X (χίλιοι) = 1000, IxI (Pentakischilioi) = 5000, M (μύριοι) = 10 000 und ImI (Pentakismyrioi) = 50 000. Eine solche Ausdrucksweise ist für jemanden typisch, der im akrophonen, nicht im alpabetischen Zahlsystem „zuhause“ ist.

Soweit zur Darstellung der Zahlen. Wie aber rechneten die Griechen damit? Es ist auf dem ersten Blick klar, dass das Rechnen mit diesen Zeichen wesentlich komplizierter ist als mit unseren arabischen Ziffern.

Nehmen wir wieder ein Beispiel aus Herodot: Um die Jahre der Herrschaft der Meder auszurechnen, würden wir einfach die vier einzelnen Summanden (also die vier Regierungszeiten der Mederkönige) untereinander schreiben und von „rechts nach links“ die Einser und Zehner, gegebenenfalls auch die Hunderter und Tausender zusammenzählen.

35	ΔΔΔΠ
+ 22	ΔΔII
+ 40	ΔΔΔΔ
+ 25	ΔΔΠ
-------	-------
= 122	HΔΔII

Dies funktioniert bei den griechischen Zahlen offenkundig nicht. Hier gilt dasselbe wie für die römischen Zahlen, dass ein „Positionsrechnen“[31] nur schwer möglich ist.

[31] Das Positionsrechnen statt Numerationsrechnen kam nach der communis opinio erst ab 12. Jh. auf. Edgar Reich weist mich allerdings darauf hin, dass bei Eutokios und Theon eine ganze Reihe von Rechenbeispielen vorkommen, bei denen sowohl bei Multiplikator und Multiplikand als auch beim Produktion und den Zwischenergebenissen Einer, Zehner, Hunderter usw. so untereinander angeordnet waren, dass sich ein Positionssystem schwerlich leugnen lässt.

Wie die Griechen in der Praxis diese Schwierigkeit meisterten, wissen wir nicht mit Sicherheit. Sieht man vom Kopf- oder Fingerrechnen ab, gibt es prinzipiell zwei Möglichkeiten. Entweder rechneten sie mit einem Rechengerät wie dem Abakus oder dem Rechenbrett oder nach einer uns leider in den Quellen nicht überlieferten Rechenmethode.

Der überzeugendeste Rekonstruktionsversuch stammt von dem Amerikaner French Andersen. Er hat für die mykenischen und römischen Zahlen eine einfache Rechenmethode entwickelt, die sich problemlos auch auf die griechischen Zahlen übertragen lässt. Die Methode erinnert in vielen Dingen an das Rechnen mit einem Abakus. Bei Additionen im akrophonen System zählt man einfach die einzelnen Summanden zusammen und erhöht, wenn man bei fünf angekommen ist, das nächsthöhere Symbol um 1.

Wie das genau funktioniert, können wir uns anhand eines Beispiels aus Herodot (7, 89–95) klar machen:[32]

> „Die Zahl der Trieren betrug 1207. Folgende Stämme hatten Schiffe gestellt. Die Phöniker samt den Syriern in Palästina stellten 300 Schiffe ... Die Ägypter stellten 200 Schiffe ... Die Kyprier stellten 150 Schiffe ... Die Kiliker stellten 100 Schiffe ... Die Pamphyler stellten 30 Schiffe ... Die Lyker stellten 50 Schiffe ... Die Dorier aus Kleinasien stellten 30 Schiffe ... Die Karer stellten 70 Schiffe ... Die Ioner stellten 100 Schiffe ... Die Inselbewohner stellten 17 Schiffe ... Die Aioler stellten 60 Schiffe ... Die übrigen aus dem Pontos ... stellten 100 Schiffe.“
> Τῶν δὲ τριηρέων ἀριθμὸς μὲν ἐγένετο ἑπτὰ καὶ διηκόσιαι καὶ χίλιαι, παρείχοντο δὲ αὐτὰς οἵδε· Φοίνικες μὲν σὺν Συρίοισι τοῖσι ἐν τῇ Παλαιστίνῃ τριηκοσίας … Αἰγύπτιοι δὲ νέας παρείχοντο διηκοσίας … Κύπριοι δὲ παρείχοντο νέας πεντήκονα καὶ … Κίλικες δὲ ἑκατὸν παρείχοντο νέας … Πάμφυλοι δὲ τριήκοντα παρείχοντο νέας … Λύκιοι δὲ παρείχοντο νέας πεντήκοναν … Δωριέες δὲ οἱ ἐκ τῆς ᾿Ασίας τριήκοντα παρείχοντο νέας … Κᾶρες δὲ ἑβδομήκοντα παρείχοντο νέας … Ἴωνες δὲ ἑκατὸν νέας παρείχοντο … νησιῶται δὲ ἑπτακαίδεκα παρείχοντο νέας … Αἰολέες δ᾿ ἑξήκοντα νέας παρείχοντο … οἱ δὲ λοιποὶ ἐκ τοῦ Πόντου … παρείχοντο μὲν ἑκατὸν νέας.

Wir schreiben die zwölf Summanden einfach untereinander und fangen mit der kleinsten Einheit zu zählen an.

HHH	300
HH	200
H iΔi	150
H	100
ΔΔΔ	30
iΔi	50
ΔΔΔ	30
iΔi ΔΔ	70
H	100
ΔPII	17
iΔi Δ	60
H	100
---------	-----
XHHPII	1207

[32] Interessant ist zu sehen, dass Herodot bei schwierigeren Rechenoperationen im Gegensatz zu seinem üblichen Verfahren die Zahlen in absteigender Reihenfolge (100 + 50 + 3) darstellt.

Wir haben zwei Einer. Wir notieren 2. Die nächst höhere Einheit ist die Fünf (P). Davon haben wir nur eins. Wir notieren 5. Von den Zehnern haben wir 10 Stück. Wir tauschen die zehn Zehner in zwei Fünfziger um. Von den Fünfzigern haben wir außer den beiden „neuen“ noch vier „alte“, also insgesamt sechs. Wir tauschen die sechs Fünfziger in drei Hunderter um. Von den Hundertern (H) haben wir außer den drei neuen noch „neun“ alte, also insgesamt zwölf. Zehn der Hunderter tauschen wir in einen Tausender um und notieren bei den Hundertern die verbliebenen zwei, bei den Tausendern den einen neuen. Zum Schuss zählen wir zusammen, was wir notiert haben: wir haben 1 Tausender, keinen Fünfhunderter mehr, 2 Hunderter, keinen Fünfziger, keinen Zehner, 1 Fünfer und 2 Einer, also insgesamt 1207.

Ungewohnt ist vielleicht für uns, dass wir statt der Zehnersprünge auch Fünfersprünge haben. Die Anderson-Methode funktioniert auch für die anderen Grundrechnungsarten wie Subtraktionen, Multiplikationen, Divisionen, ja sogar für das Wurzelziehen[33].[34]

Versuchen wir als nächstes, die Anderson-Methode auf unsere eingangs zitierten Beispiele mit den Fehlern anzuwenden. Rechnen wir zunächst die mathematisch leichtere Polybios-Stelle aus. Polybios gab an, wie weit die Leiter unten von der Mauer entfernt ist, wenn die Mauerhöhe zehn Einheiten und die Leiter zwölf Einheiten beträgt. Um den Satz des Pythagoras ($a^2 + b^2 = c^2$) anwenden zu können, müssen wir zuerst 12 und 10 potenzieren, also mit sich selbst multiplizieren.

Multiplikationen in der Anderson-Methode funktionieren so:

12	10
12	10
----	---
100 20 20 4	100 0 0 0
-----------------	--------------
144	100
(Schritte:	Schritte:
10 x 10 = 100	10 x 10 = 100
2 x 10 = 20	0 x 1 = 0
10 x 2 = 20	1 x 0 = 0
2 x 2 = 4	0 x 0 = 0
Addition der vier Schritte)	

[33] Die Regel beim Wurzelziehen lautet: Finde eine Zahl, die mit sich selbst multipliziert, vom Dividenden abgezogen werden kann.

[34] Multiplikationen werden von der größten Einheit zur kleinsten unternommen – als entgegengesetzt zu unserer Methode. Bei der Positionierung der Zahlen dient die Zahl mit den wenigsten Stellen als Multiplikand. Divisionen verlaufen ähnlich wie in unserem System, haben aber den Vorteil, dass man nicht wie in unserem System genau wissen muss, „wie oft“ der Divisor in den Dividenden geht.

In einem nächsten Schritt subtrahieren wir 100 von 144. Subtraktionen sind die Umkehrungen von Additionen. Wir schreiben die beiden Zahlen untereinander und löschen gleiche Zahlen aus (in unserem Fall also Eta für 100. Es verbleiben 44).

144	HΔΔΔΔIIII
-100	H
--------	------------
44	ΔΔΔΔIIII

Im letzten Schritt ziehen wir die Wurzel aus 44.

Die Regel beim Wurzel-Ziehen in der Anderson-Methode lautet: Finde in einem ersten Schritt die Zahl bzw. den Divisor, der mit sich selbst multipliziert, vom Dividenden abgezogen werden kann. Das klingt kompliziert, ist aber im Grunde sehr simpel. Wir erhalten durch „Probieren" die Potenz, die am nächsten an 44 heranreicht. Diese Zahl ist in unserem Beispiel 6. Wir multiplizieren 6 mal 6 = 36 und ziehen das Resultat von der Ausgangszahl aus. Bleiben als Rest (neben der 6) 8.

44
36
---
8 (Rest)

Das Ergebnis lautet also 6, Rest 8. Wahrscheinlich hat Polybios diesen Rest 8 einfach unter den Tisch fallen lassen bzw. einfach nach unten abgerundet. Daher kommt er auf die Zahl 6 und nicht auf die Zahl 7, die korrekter wäre. Die Griechen haben soweit wie möglich das Rechnen mit Brüchen vermieden. Wir haben eine ganze Reihe von Beispielen mit Divisionen und Wurzelziehen, wo dies der Fall ist.

Der Fehler des Polybios ist also nicht als echter Rechenfehler, sondern eher als Rundungsfehler anzusprechen. Er ist dadurch induziert, dass die Griechen in der alltäglichen Praxis kein Positionsrechnen benutzten und daher die Stellen „hinter dem Komma" nicht mehr ausrechneten und daher meistens ignorierten.

Die „Anderson-Methode" bzw. ihr antikes Pendant ist aber auch noch in anderer Hinsicht fehleranfällig.

Kommen wir auf das Herodot-Beispiel mit der Armee des Xerxes zurück und rechnen das Ganze jetzt in der Anderson-Methode durch. Ich erinnere noch mal: Herodot nennt 5 283 220 Mann, die eine Choinix täglich verbrauchen, was Herodot falsch mit 110 340 Medimnen (statt 110 067,08) ausrechnet.

Herodot splittet die lange Zahl auf und berechnet in einem ersten Rechenschritt korrekt die Myriaden. 11 Myriaden (= 110 000) mal 48 ergibt 5 280 000. Es verbleibt von 5 283 220 noch ein Rest von 3 220, der nun wiederum durch 48 zu teilen ist. Es folgt die komplizierteste Division bei Herodot.

Divisionen werden in der Anderson-Methode als vervielfältigte Subtraktionen durchgeführt. Es bedarf sechs Schritte, um zu einem Ende zu kommen.[35] Statt mit 48 rechnete Herodot mit 480 (also dem Zehnfachen), damit er nicht so viele Einzelschritte machen musste.

3220 – 480 = 2740 (1. Schritt) / 10 „gemerkt“
2740 – 480 = 2260 (2. Schritt) / 10 „gemerkt“
2260 – 480 = 1780 (3. Schritt) / 10 „gemerkt“
1780 – 480 = 1300 (4. Schritt) / 10 „gemerkt“
1300 – 480 = 820 (5. Schritt) / 10 „gemerkt“
820 – 480 = 340 (6. Schritt) / 10 „gemerkt“

An dieser Stelle – nach dem sechsten und letzten Schritt – begeht Herodot seinen Fehler. Er hält an dieser Stelle inne, weil er die letzte Zahl 340 nicht mehr durch 480 teilen kann (die Griechen vermeiden das Bruchrechnen). Aber statt nun die sechs Zehner aus den Zwischenschritten plus dem überzähligen Rest zusammenzuzählen und auf die korrekte Zahl von 67 zu kommen, hält er irrtümlich den letzten Rest (340) für den gesuchten Quotienten für die Division 3220 durch 48. Er schreibt also fälschlich 11 Myriaden und 340 statt 11 Myriaden 6 Zehner 7 Einer plus „vernachlässigbarem“ Rest.

Wir können also als Zwischenfazit festhalten: Der Hauptgrund für die erwähnten arithmetischen Fehler (und wahrscheinlich vieler anderer) ist zuallererst in den unhandlichen griechischen Zahlen zu sehen, mit denen sich nur schwer rechnen lässt. Insbesondere das Positionsrechnen ist nicht möglich, weshalb die Brüche oft nicht ausgerechnet sind. Wenn mit der Anderson-Methode gerechnet wurde, was trotz fehlender Belege wahrscheinlich ist, waren für manche komplizierten Rechenoperationen viele Schritte vonnöten, was natürlich die Fehleranfälligkeit erhöhte.

Neben den Zahlen und der Rechenmethode ist vielleicht noch eine weitere Fehlerquelle anzunehmen, nämlich die mangelnde Kenntnis oder doch zumindest mangelnde Praxis der Griechen im Rechnen. Ich kann an dieser Stelle nicht mehr über den Mathematikunterricht in der Antike und über die Verbreitung mathematischer Kenntnisse bei den Griechen sprechen. Die verstreuten Aussagen in den antiken Quellen dazu sind sehr diffus und müssen nach Ort, Zeit, Beruf und vor allem sozialer Schichtung sehr unterschiedlich bewertet werden.

[35] Die Anderson-Regel bei der Division lautet: „For the first number of the quotient use any number that, multiplied by the divisor, can be subtracted.”

Ich möchte aber in einem letzten Schritt zumindest noch ein paar Bemerkungen zu dem Kenntnisstand unserer drei Historiker und zu ihrem Verhältnis zur Mathematik machen.

3. Die Rolle der Mathematik bei Herodot, Thukydides und Polybios

Leider finden wir bei Herodot, Thukydides und Polybios kaum explizite Aussagen über mathematische Dinge. Am ausführlichsten äußert sich Polybios im 9. Buch (9, 20), allerdings nur in Bezug auf die Geometrie. Grundwissen in der Geometrie gehöre seiner Meinung nach wie Grundwissen in der Astronomie zu den notwendigen Kenntnissen eines Feldherrn.

> „Daher wird an diesen Dingen [gemeint sind die Fehler von früheren Feldherren] wiederum deutlich, dass jeder, der sich bei seinen Plänen und Unternehmungen vor Fehlern in Acht nehmen will, Geometrie getrieben haben muss – nicht bis zur Vollkommenheit, aber doch so weit, dass er von der Proportion und der Theorie der Ähnlichkeiten einen Begriff hat. Denn diese Betrachtung ist nicht nur für diese Zwecke, sondern auch für die wechselnden Formen des Heerlagers notwendig ...“[36]
> Διὸ πάλιν ἐν τούτοις φανερὸν ὅτι δεήσει τοὺς βουλομένους εὐστοχεῖν ἐν ταῖς ἐπιβολαῖς καὶ πράξεσι γεγεωμετρηκέναι μὴ τελείως, ἀλλ᾽ ἐπὶ τοσοῦτον ἐφ᾽ ὅσον ἀναλογίας ἔννοιαν ἔχειν καὶ τῆς περὶ τὰς ὁμοιότητας θεωρίας. οὐ γὰρ περὶ ταῦτα μόνον, ἀλλὰ καὶ περὶ τὰς τῶν σχημάτων μεταλήψεις ἐν ταῖς στρατοπεδείαις ἀναγκαῖός ἐστιν ὁ τρόπος ...

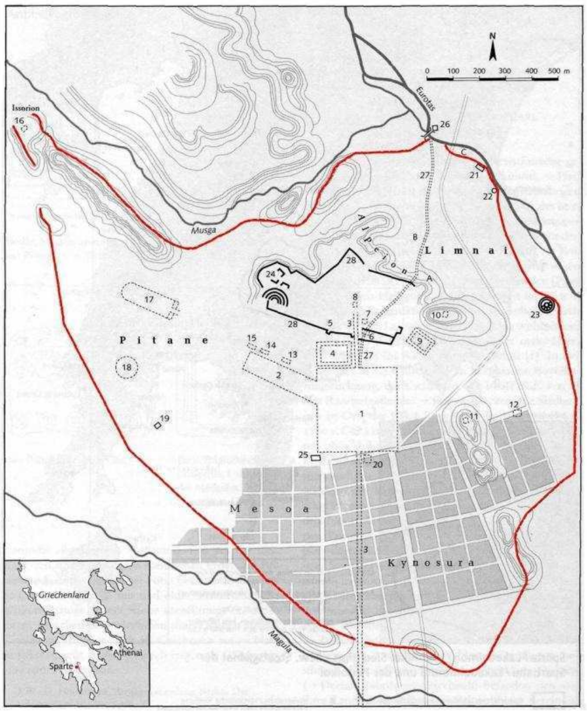
Polybios spricht an dieser Stelle von einem mathematischen Sachverhalt, der gelegentlich auch noch heute für Verwirrung sorgt. Ich meine damit den Sachverhalt, dass geometrische Figuren bei gleichem Umfang nicht unbedingt die gleiche Fläche oder umgekehrt bei gleicher Fläche nicht den gleichen Umfang haben müssen. Und es ist der Kreis, der bei gegebenem Umfang die größte Fläche einnimmt. Dieser Sachverhalt ist auch als isoperimetrische Ungleichung oder als das „Problem der Dido“ in die Mathematikgeschichte eingegangen.[37]

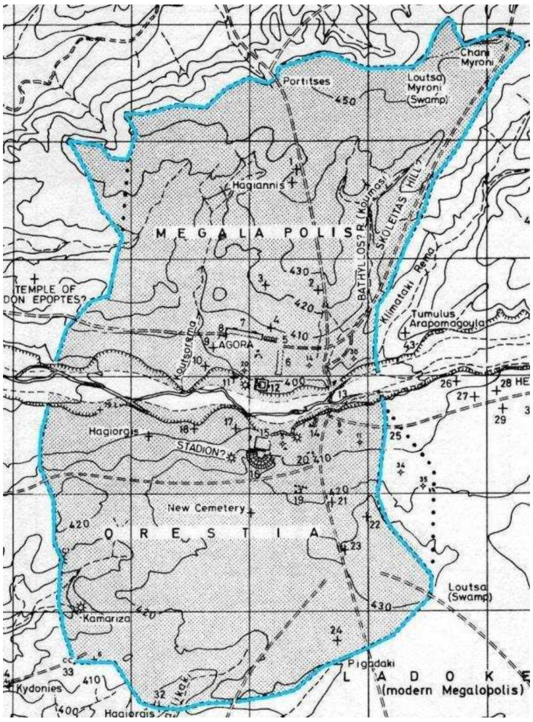
Polybios gibt sogar ein konkretes Beispiel für die Anwendung einer solchen isoperimetrischen Gleichung in der militärischen Praxis. Er schreibt nämlich im 9. Buch seiner Historien (9, 26a) noch das Folgende:

> „Die meisten Leute glauben aber aus dem Umfang einen Schluss auf die Größe ziehen zu dürfen. Wenn ihnen daher jemand sagt, dass die Stadt Megalopolis 50 und Sparta 48 Stadien Umfang habe, dass aber Sparta doppelt so groß wie Megalopolis sei, so scheint ihnen diese Aussage unglaubhaft. Wenn aber einer, der das Paradoxe auf die Spitze treiben will, sagt, es sei möglich, dass eine Stadt oder ein Heerlager von 40 Stadien Umfang doppelt so groß sei wie eine Stadt oder ein Heerlager von 100 Stadien Umfang, so findet man, dass diese Aussage sie völlig außer Fassung bringt. Der Grund davon ist, dass wir uns nicht mehr an das erinnern, was wir als Kinder in der Schule aus der Geometrie gelernt haben. Hierüber jetzt zu sprechen werde ich dadurch veranlasst, dass nicht nur

[36] „damit dieses bei Änderung der Form den gleichen Flächeninhalt behält, um die Insassen aufnehmen zu können, oder umgekehrt, wenn die Form die gleiche bleibt, dass man den Raum, den das Lager einnimmt, vergrößern oder verkleinern kann, je nachdem ob Truppen hinzukommen oder ausscheiden. Hierüber habe ich in meiner Schrift über Taktik eingehender gehandelt.“
[37] Vgl. dazu Geus 2012b (dort auch die ältere Literatur).

die große Masse, sondern auch manche der Staatsmänner und der Befehlshaber außer Fassung gebracht werden und sich bald wundern, wie es möglich sei, dass Sparta größer, und zwar viel größer, sei als Megalopolis, weil es doch einen kleineren Umfang hat, bald auf die Menge der Männer nur aus dem Umfang des Lagers schließen.“
Οἱ δὲ πλεῖστοι τῶν ἀνθρώπων ἐξ αὐτῆς τῆς περιμέτρου τεκμαίρονται τὰ μεγέθη τῶν προειρημένων. λοιπὸν ὅταν εἴπῃ τις τὴν μὲν τῶν Μεγαλοπολιτῶν πόλιν πεντήκοντα σταδίων ἔχειν τὸν περίβολον, τὴν δὲ τῶν Λακεδαιμονίων ὀκτὼ καὶ τετταράκοντα, τῷ δὲ μεγέθει διπλῆν εἶναι τὴν Λακεδαίμονα τῆς Μεγάλης πόλεως, ἄπιστον αὐτοῖς εἶναι δοκεῖ τὸ λεγόμενον. ἂν δὲ καὶ συναυξῆσαί τις βουλόμενος τὴν ἀπορίαν εἴπῃ διότι δυνατόν ἐστι τετταράκοντα σταδίων πόλιν ἢ στρατοπεδείαν ἔχουσαν τὴν περιγραφὴν διπλασίαν γίνεσθαι τῆς ἑκατὸν σταδίων ἐχούσης τὴν περίμετρον, τελέως ἐκπληκτικὸν αὐτοῖς φαίνεται τὸ λεγόμενον. τοῦτο δ᾽ ἐστὶν αἴτιον ὅτι τῶν ἐν τοῖς παιδικοῖς μαθήμασι παραδιδομένων ἡμῖν διὰ τῆς γεωμετρίας οὐ μνημονεύομεν. περὶ μὲν οὖν τούτων προήχθην εἰπεῖν διὰ τὸ μὴ μόνον τοὺς πολλοὺς ἀλλὰ καὶ τῶν πολιτευομένων καὶ τῶν ἐν ταῖς ἡγεμονίαις ἀναστρεφομένων τινὰς ἐκπλήττεσθαι, θαυμάζοντας ποτὲ μὲν εἰ δυνατόν ἐστι μείζω τὴν τῶν Λακεδαιμονίων πόλιν εἶναι, καὶ πολλῷ μείζω, τῆς τῶν Μεγαλοπολιτῶν, τὸν περίβολον ἔχουσαν ἐλάττω, ποτὲ δὲ τὸ πλῆθος τῶν ἀνδρῶν τεκμαίρεσθαι, στοχαζομένους ἐξ αὐτῆς τῆς περιμέτρου τῶν στρατοπεδειῶν.





Zur Verdeutlichung der Aussage des Polybios seien zwei Karten – links die von Sparta,[38] rechts die von Megalopolis – gegenüber gestellt.

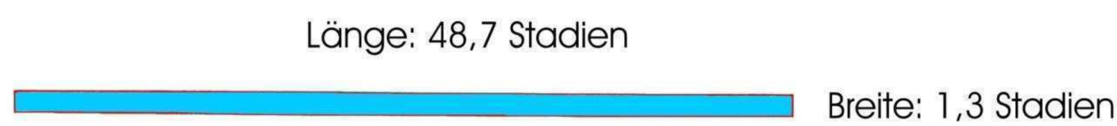
[38] Welwei 2001: 787–8. Bis Ende des 4. Jh. v. Chr. war Sparta ohne Mauern (Agesilaos soll nach Plutarch [apophthegmata Laconica 29] gesagt haben, „die Spartiaten seien selbst die Mauern Spartas“). In den Jahren vor 218 v. Chr., als der Makedonenkönig Philipp V. Sparta angriff, wurde – nachdem wahrscheinlich bereits eine Holzmauer existierte – eine Steinmauer errichtet. Später hat sie Nabis verstärkt. Die Mauer scheint aber noch nicht fertig gewesen zu sein, als Flaminius 195 Sparta belagerte. Philopoimen zerstörte im Jahr 188 die Mauern. Sie wurden aber kurze Zeit später wieder errichtet und standen noch in der römischen Zeit. Vgl. Wace 1905/06; Waywell 1999.

Es lässt sich erahnen, was Polybios mit seinem ersten Beispiel meinte. Spartas Grundriss weist – abgesehen von der Nordostecke (um den Issorion-Hügel)[39] – eine annähernd runde Form auf. An anderer Stelle bezeichnet Polybios Sparta auch explizit als „rund".[40] Dagegen hat das durch den Fluss Helisson geteilte Megalopolis einen annähernd rechteckigen Grundriss mit langen Längsseiten.[41]

Archäologische Ausgrabungen haben allerdings den Größenvergleich des Polybios nur teilweise bestätigen können. Annähernd richtig sind die Umfänge der beiden Städte angegeben. Die 48 Stadien für Sparta entsprechen etwa 9 km, was etwa auf einen halben Kilometer genau stimmt. Die Länge des Mauerrings von Megalopolis haben die Archäologen auf 8, 4 km bestimmt, was ebenfalls ziemlich genau den 50 Stadien bei Polybios entspricht.[42]

Die Aussage des Polybios, dass bei annähernd gleichem Umfang die Fläche von Sparta doppelt so groß wie die von Megalopolis sei, ist allerdings, wie ein Blick auf die Karte zeigt, falsch. In Wirklichkeit ist Megalopolis etwas größer als Sparta.[43] Der Grund liegt vor allem darin, dass Sparta nicht wirklich kreisförmig ist. Verliefe die Mauer im Norden nicht konkav, sondern konvex, wäre tatsächlich Sparta bei annähernd gleichem Umfang die flächenmäßig größere Stadt – allerdings immer noch nicht doppelt so groß wie Megalopolis. Hier übertreibt Polybios gewaltig.

Die weiteren Ausführungen des Polybios sind ebenfalls nicht ganz stimmig: dass eine Stadt oder ein Heerlager von 40 Stadien Umfang doppelt so groß sein kann wie eine Stadt oder ein Heerlager von 100 Stadien Umfang, ist zwar mathematisch korrekt gesagt, ergäbe aber in Praxis einen völlig widersinnigen architektonischen Plan. Ein Kreis mit 40 Stadien Umfang hat eine Fläche von ca. 128 Quadratstadien. Die Hälfte davon sind 64 Quadratstadien. Das gesuchte Rechteck mit 100 Stadien Umfang und 64 Quadratstadien Fläche sähe dann folgendermaßen aus:



[39] Dort stand das Heiligtum der Artemis Issoria. Vgl. Nep. Ages. 6, 2; Plut. Ages. 32, 3; Polyain. strat. 2, 1, 14. Die Identifizierung ist nicht ganz sicher. Vgl. Olshausen/Lienau 1998: 1145: „evtl. die h. Klaraki gen. Anhöhe".

[40] Vgl. Polyb. 5, 22: „Sparta hat im Ganzen eine runde Gestalt und ist in ebener Gegend gelegen, umfasst aber auch verschiedene unebene und hügelige Teile."

[41] Meyer/Lafond 1999: 1135: „Die Mauer ist den das Stadtgebiet umgebenden Hügeln angepaßt."

[42] Auf eine Diskussion nach der Länge des Stadions kann an dieser Stelle nicht eingegangen werden.

[43] Bury 1898: 20 beziffert das nördliche Areal auf 1 977 486, das südliche auf 2 113 238 square yards, also auf insgesamt 4 090 724 square yards. Walbank 1967: 156 schätzt die Größe von Sparta auf „2,500,000 sq. yds. at the most".

Die Längen wären ca. 48, 7 Stadien, die Breiten dagegen nur 1, 3 Stadien.

Wahrlich ein absurder Grundriss für eine Stadt oder ein Heerlager![44]

Eine restlos überzeugende Erklärung für diese verwirrende Passage des Polybios kann ich leider nicht anbieten. Zweifellos ist Polybios das mathematische Prinzip bekannt, dass der Kreis die maximale Fläche einnimmt, eine polygonale Fläche dagegen entsprechend weniger. Seine beiden Beispiele jedoch zeigen, dass er sich aber offenkundig nicht klar gemacht hat, was dieses Prinzip in der Praxis bedeutet. Er hat zumindest beim zweiten Beispiel nicht oder nicht richtig nachgerechnet. Polybios erweist sich also auch an dieser Stelle als wenig sattelfest in der Mathematik. Und das, obwohl er doch die Wichtigkeit der Geometrie an dieser Stelle herausstreichen möchte.[45]

Bei den beiden anderen Historikern, Herodot und Thukydides, finden wir keine metamathematischen Aussagen, sehen wir davon ab, dass Herodot den Ursprung der Geometrie nicht bei den Ägyptern, sondern bei den Babyloniern sucht. Trotzdem lässt sich ein signifikanter Unterschied zwischen beiden in ihrer Haltung zur Mathematik ausmachen.

Wir haben eingangs festgestellt, dass es Thukydides vermieden hat, eine mathematische Textaufgabe zu lösen. Mustert man die Stellen seiner Historien durch, an denen Thukydides Zahlenangaben macht – insbesondere wo er Truppen- und Flottenstärken nennt –, stellt man fest, dass dies beleibe kein Einzelfall ist. Thukydides gibt nur sehr selten Lösungen für einfache Rechenaufgaben an. Vergleichen wir etwa eine weitere Stelle aus dem 1. Buch (1, 27, 2):

> „Sie [die Korinther] baten auch die Megarer um Schiffe zum Geleit, falls sie von den Kerkyraiern an der Fahrt gehindern werden sollten; sie rüsteten acht Schiffe zum Geleitschutz aus und Pale auf Kephallenia vier. Auch die Epidaurier gingen sie an und diese stellten fünf, Hermione eines, Troizen zwei, Leukas zehn, Amprakia acht; von Theben erbaten sie Geld, ebenso von Phleius, und von Elis leere Schiffe und Geld. Von den Korinthern selbst wurden 30 Schiffe ausgerüstet und 3000 Hopliten."
> ἐδεήθησαν δὲ καὶ τῶν Μεγαρέων ναυσὶ σφᾶς ξυμπροπέμψαι, εἰ ἄρα κωλύοιντο ὑπὸ Κερκυραίων πλεῖν· οἱ δὲ παρεσκευάζοντο αὐτοῖς ὀκτὼ ναυσὶ ξυμπλεῖν, καὶ Παλῆς Κεφαλλήνων τέσσαρσιν. καὶ Ἐπιδαυρίων ἐδεήθησαν, οἳ παρέσχον πέντε, Ἑρμιονῆς δὲ μίαν καὶ Τροιζήνιοι δύο, Λευκάδιοι δὲ δέκα καὶ Ἀμπρακιῶται ὀκτώ. Θηβαίους δὲ χρήματα ᾔτησαν καὶ Φλειασίους, Ἠλείους δὲ ναῦς τε κενὰς καὶ χρήματα. αὐτῶν δὲ Κορινθίων νῆες

[44] Warum er überhaupt diesen Größenvergleich zwischen Sparta und Megalopolis zieht, liegt zum einen in seinem Lokalpatriotismus begründet: Polybios stammte aus der Stadt Megalopolis. Zum anderen konnte er es sich vielleicht nicht verkneifen, ein Wortspiel auf die Spitze zu treiben. Denn wörtlich übersetzt bedeutet ja Megalopolis „große Stadt". Es ist aber in Wirklichkeit „kleiner" als Sparta oder anders gesagt: „groß" nur in einem ganz bestimmten geografischen Sinn.

[45] Aus diesen Gründen lässt sich kaum abschätzen, wie verbreitet die Kenntnis dieses mathematischen Satzes war. Vielleicht darf man aber aus der Stelle zumindest zwei Dinge erschließen: erstens wurde der Satz zu Polybios´ Zeiten von den Kindern in der Schule gelernt und gehörte somit zum allgemeinen Bildungsgut. Zum anderen lehren die beiden Größenvergleiche des Polybios, dass die isoperimetrische Ungleichung gerade in der Topografie und der Geografie Anwendung gefunden hat. Weitere Überlegungen bei Geus 2012b.

παρεσκευάζοντο τριάκοντα καὶ τρισχίλιοι ὁπλῖται.

Vermutlich hätten die allermeisten Historiker ihrem Leser das Zusammenzählen der einzelnen Posten erspart und zum Schluss gesagt, dass es abgesehen von den Sondereinheiten wie Geld, Hopliten und leere Schiffe insgesamt 68 Schiffe waren, die auf Seiten der Korinther in den Peloponnesischen Krieg eintraten.[46] Thukydides führt aber, genau wie in unserem eingangs erwähnten Beispiel mit homerischen Schiffen, eine solche Rechnung nicht durch.

Wie schon angedeutet, tut er dies auch bei vielen anderen Gelegenheiten nur sehr selten. An den 25 Stellen, an denen Thukydides drei oder mehr Zahlen von einer Einheit aufzählt (wo es also mindestens drei Summanden bei einer Addition gibt und wo wir zur Erleichterung unserer Leser und Zuhörer immer ein Gesamtergebnis nennen würden), wird zwanzig Mal keine Gesamtsumme genannt. Zweimal wird nur eine Teil oder Zwischensumme gebildet, und nur dreimal werden die Summanden addiert, wird also eine echte Addition mit Nennung des Endergebnisses durchgeführt. Und selbst an diesen drei Stellen ist es so, dass die Gesamtsumme nicht unmittelbar im Anschluss auf die Aufzählung der Einheiten folgt, sondern entweder vor der Aufzählung vorweg genommen wird oder weit entfernt davon im Text nachgetragen wird.[47]

Ich kann mir diese überraschende Beobachtung nur damit erklären, dass Thukydides keinen Zuhörer, sondern einen Leser voraussetzt. Die Tatsache, dass Thukydides sich in dieser Hinsicht so sehr von Herodot unterscheidet (der nun tatsächlich auch bei einfachsten Additionen die Gesamtsumme regelmäßig angibt), spricht für mich entschieden dafür, dass er mit einem ganz anderen Rezipienten, nämlich einen Leser und keinen Zuhörer rechnet. „Zum bloßen Anhören" sei sein Werk nicht geeignet, schreibt Thukydides ja selbst in seiner Einleitung.[48] Dass Thukydides seinem Leser oft das Nachrechnen nicht erspart, ja offenbar gar nicht ersparen will, erweist sich in diesem Sinne als komplementär zu seinem anspielungsreichen Darstellungsstil. Das Rechnen gehört bei Thukydides zu den mentalen Aufgaben, die der Leser selbst zu leisten hat.

Genau umgekehrt dürfte es bei Herodot gewesen sein. Es scheint sicher, dass Herodot zumindest Teile seines Werkes öffentlich vorgelesen hat. Die Annahme, dass er sogar mathe-

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[46] In 1, 29, 1 spricht Thukydides von 75 Schiffen, was man wohl so erklären muss, dass die Zahl der von Elis gestellten „leeren" Schiffe sieben war.

[47] Man könnte – da Herodot es anders macht – vielleicht vermuten, dass dies eine Eigentümlichkeit des Thukydides ist. Rihll 2002: 50 behauptet, dass in athenischen Inschriften nur selten Gesamtsummen angegeben waren, was mir eine starke Verzerrung des Befundes zu sein scheint.

[48] Ob man deshalb von einem „esoterischen Verzicht auf ein zeitgenössisches Publikum" sprechen kann (Malitz, 1982: 270), lasse ich dahingestellt. Vgl. auch Meier 2006: 141.

matische Passagen wie etwa unsere diskutierte Passage zur Größe des persischen Heeres vorgelesen hätte, mag auf den ersten Blick absurd erscheinen. Ich halte sie trotzdem für richtig.

Untersucht man nämlich die Stellen, an denen Herodot Rechenoperationen vor dem Auge des Zuhörers vorführt, stellt man mehrere Auffälligkeiten fest. Zum einen sind alle längeren Rechenaufgaben nicht zufällig über sein Werk verstreut, sondern finden sich an exponierten Stellen. Die Berechnung zur Größe des persischen Heeres findet sich unmittelbar vor den ersten größeren Verlusten bei Sepias und den Thermopylen, also quasi auf dem Höhepunkt der persischen Macht. Einen überaus langen Bericht über die persischen Satrapien (einschließlich einer umfangreichen Rechenaufgabe über die jeweiligen Abgaben) baut er an der Stelle in sein Werk ein, wo er über die Größe der Welt und die äußersten Länder der Erde spricht. Das Vorrechnen der riesigen Steuererträge diente natürlich auch dazu, die Größe des persischen Reiches „auszumalen". Herodot benutzt eine Rechenoperation, um staunende Zuhörer zu unterhalten und die Hauptaussage seines Werkes, dass die Griechen trotz ihrer zahlenmäßigen Unterlegenheit den Persern mehr als gewachsen waren, quasi auch in „mathematischer" Form an den Mann zu bringen.

Zum anderen dienen die vielen Rechnungen und Zahlen bei Herodot auch dazu, die Reputation des Herodot als exakten Rechner und damit als seriösen Historiker herauszustreichen. Zahlenangaben sind für Herodot eben nicht nur wie für Thukydides und die meisten anderen Historiker ein Mittel, um Einheiten zu quantifizieren. Detlev Fehling hat, überzeugend wie ich glaube, nachgewiesen, dass viele Zahlenangaben bei Herodot frei erfunden sind, oder vorsichtig ausgedrückt: bestimmten literarischen Zwecken dienen. Dahinter steckt natürlich eine ganz bestimmte Wirkungsabsicht. Zahlen machen viele Dinge erst fass- und begreifbar. Durch Zahlenangaben wirken historische Aussagen exakter. Und Herodot begnügt sich an manchen Stellen offenbar nicht nur damit, Zahlen zu nennen. Er gibt seinen Lesern durch das Aufzeigen des Rechenwegs einen Schlüssel an die Hand, mit dem er in der Lage ist, gewisse Aussagen wie zur Größe des persischen Heeres „nachzurechnen" und damit seine Glaubwürdigkeit zu überprüfen. Dass natürlich die von Herodot zugrunde gelegten Zahlen oft fiktiv sind,[49] dass also trotz richtigen „Rechnens" historisch falsche Aussagen herauskommen, steht natürlich auf einem anderen Blatt.

Nicht zuletzt dient die Mathematik dem Herodot auch dazu, seinen Aussagen besonderen Nachdruck zu verleihen. Ich zitiere ein letztes Beispiel, diesmal ohne Rechenfehler (Hdt. 1, 32, 2–3):

[49] Dies hat – trotz im Detail berechtigter Kritik – Detlev Fehling (1971) m. E. überzeugend gezeigt.

„Auf 70 Jahre setze ich für einen Menschen die Grenze seines Lebens an. Diese 70 Jahre machen 25 200 Tage – ohne einen Schaltmonat. Will man jedes zweite Jahr noch einen Monat länger machen, damit die Jahreszeiten an der richtigen Stelle im Jahr eintreffen, kommen zu den 70 Jahren noch 35 Schaltmonate hinzu, das sind 1050 Tage. Und von allen diesen Tagen der 70 Jahre, also von den 26 250 Tagen, bringt kein einziger Tag völlig das Gleiche wie ein anderer Tag.“
ἐς γὰρ ἑβδομήκοντα ἔτεα οὖρον τῆς ζόης ἀνθρώπῳ προτίθημι. οὗτοι ἐόντες ἐνιαυτοὶ ἑβδομήκοντα παρέχονται ἡμέρας διηκοσίας καὶ πεντακισχιλίας καὶ δισμυρίας, ἐμβολίμου μηνὸς μὴ γινομένου· εἰ δὲ δὴ ἐθελήσει τοὔτερον τῶν ἐτέων μηνὶ μακρότερον γίνεσθαι, ἵνα δὴ αἱ ὧραι συμβαίνωσι παραγινόμεναι ἐς τὸ δέον, μῆνες μὲν παρὰ τὰ ἑβδομήκοντα ἔτεα οἱ ἐμβόλιμοι γίνονται τριήκοντα πέντε, ἡμέραι δὲ ἐκ τῶν μηνῶν τούτων χίλιαι πεντήκοντα. τουτέων τῶν ἁπασέων ἡμερέων τῶν ἐς τὰ ἑβδομήκοντα ἔτεα, ἐουσέων πεντήκοντα καὶ διηκοσιέων καὶ ἑξακισχιλιέων καὶ δισμυριέων, ἡ ἑτέρη αὐτέων τῇ ἑτέρῃ ἡμέρῃ τὸ παράπαν οὐδὲν ὅμοιον προσάγει πρῆγμα.

Es ist hier schön zu sehen, dass es Herodot überhaupt nicht darauf ankommt, eine bestimmte Zahl zu errechnen. Dazu hätte er dazu auch mit 90 Jahren rechnen oder auch einfach die Endsumme nennen können. Und schon gar nicht kommt es ihm bei dieser Rechnung auf die Schalttage an. Die 1050 zusätzlichen Tage fallen bei einer Gesamtsumme von 26 250 Tagen kaum ins Gewicht. Sehr viel wichtiger ist für Herodot der Weg zu seinem Ergebnis, das Rechnen selbst. An dieser Stelle ist die Mathematik zu einem Stilmittel geworden. Die an sich banale Feststellung, dass im menschlichen Leben kein Tag dem anderen gleicht, wird durch ein virtuoses Zahlenspiel dem Zuhörer gleichsam eingehämmert. Rechnen ist bei Herodot auch und vor allem ein artistisches Spiel.[50] Gerade in diesem letzten Punkt sehe ich den entscheidenden Unterschied zwischen Herodot und den anderen Historikern bezüglich ihrer Einstellung zur Mathematik.

Die aufgeführten Beispiele dürften hinreichend gezeigt haben, dass selbst für eine solch zahlenmäßig kleine Gruppe wie die griechischen Historiker kaum Verallgemeinerungen möglich sind. Jeder der drei untersuchten Historiker hat eine unterschiedliche Einstellung zur Mathematik. Vielleicht ist mit aber zum Schluss doch eine generalisierende Aussage erlaubt. Sie beantwortet meine in der Überschrift aufgeworfene Frage ganz kurz auf die folgende Weise: Selbstverständlich konnten die griechischen Historiker „rechnen“. Sie rechneten aber „anders“ als wir heute. Und dieses „anders“ bedingte auch „andere“ Einstellungen zur Mathematik und letztlich auch „andere“ Fehler.

[50] Vgl. auch Hdt. 2, 142: „Sie [die ägyptischen Priester] haben mir nachgewiesen, dass zwischen dem ersten König von Ägypten und jenem letztgenannten Priester des Hephaistos 341 Menschenalter liegen. denn so viele Oberpriester und Könige hat es im Laufe dieser Zeit gegeben. Nun machen aber 300 Generationen einen Zeitraum von 10 000 Jahren aus. Denn drei Menschenalter sind gleich 100 Jahren. Zu den 300 kommen noch die 41 Menschenalter, das macht 1340 Jahren. Das heißt also: in einem Zeitraum von 11 340 Jahren haben nur menschliche Könige, nicht Götter in Menschengestalt, in Ägypten geherrscht.“

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CHAPTER 4

'TRUE' AND 'FALSE' ERRORS IN ANCIENT (GREEK) COMPUTATION[1]

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In life, many are the ways to be wrong and, accordingly, to differentiate types of error: e. g., linguistic, moral and economic ones, errors of taste, permissible and unforgivable ones. In discourses about explicit knowledge, however, errors have usually gone undifferentiated, especially where science and practices that concern quantifiable data are concerned. Common knowledge knows only two modes of giving, say, the measure of a distance between two points. One number is correct, all other ones are equally wrong (although theorists may insist on definitions of 'distance' and 'point').

This paper attempts to further qualify these different ways of being wrong from a historical perspective. The two main ways of being wrong that I am interested in here, I have labeled 'true' and 'false' errors.

What do we gain by such a paradoxical distinction? How can in applied mathematics, which is the field I will be looking at (and within which I include procedures of measuring distances, surfaces, and volumes), where the line between correct and incorrect seems to be so clear that one could even arrive at the truth by counting,[2] how can there exist in such a field any uncertainty regarding the status of error? I will introduce some examples that show, I hope, how for historians of science the line mentioned is less obvious, and how an unqualified concept of 'error' will not do. At least in passing I would also like to consider the possible typologies and terminologies of being wrong.

Since the days of Kuhn and Feyerabend, the topic of 'error and science' has developed into a productive sub-field of Science and Technology Studies, especially in the United States.[3] At the time of writing, Deborah Mayo (at Virginia Tech) and Douglas Allchin (at the

[1] Thanks to Mark Geller and Klaus Geus for making me think about error; to Anna-Maria Kanthak, Stephen E. Kidd, Saskia Lingthaler, Maria Börno and Sebastian Luft for their help in avoiding it; special thanks to Hagan Brunke who read this paper with an especially keen eye.

[2] See, e. g., Plato's Meno 82 E 14 – 83 C 2.

[3] For Kuhn 1962, the concepts of truth and error are relative to the ruling paradigm, which follows, I think, on his idea of the incommensurability of paradigms (see esp. 77–79 on falsification); Feyerabend 1983, 271 f.

University of Minnesota) seem to be the leading figures in the field.[4] For these historians of science, 'error' is a heuristic concept: "[...] past error, properly documented, is a form of negative knowledge."[5] Allchin structures his typology of scientific error as a spectrum ranging from "material" to "social", that is, from the least severe error which is easily corrected to the somehow skewed perspective the scientist adopts from his social environment and that one can diagnose and correct only in hindsight. In the cases of Allchin and Mayo, error studies are steeped in a quasi-Thucydidean form of optimism that claims modern scientists could learn from past errors and optimize their practices accordingly.[6] It is evident that such an essentially practical perspective cannot be interested in thinking about the status of error itself. For the retrospective glance, therefore, scientific errors are just that: errors.[7] When the historian's glance, however, falls back far into the past, everything becomes blurred and not even error is what it once was. In order to show this, I have chosen a heterogeneous sample of ancient computations and procedures, all aiming at establishing firm measures of lengths and surfaces.

1. Circles, Co-Efficients, and Canonicity

One classical topic of the newly emerging sub-discipline 'error and science' should be the long history of attempts to calculate the area or perimeter of the circle. Today we use the number π, an irrational number, which means that it has both an infinite number of positions after the decimal point and is non-periodical. In practice, therefore, even the most accurate calculations can only approximate. Some modern mathematicians, however, have made it a pastime to come up with ever more accurate approximations of Pi. After Fabrice Bellard had calculated the number π in December 2009 to the extent of approx. 2.7 trillion positions, in August 2010 Alexander J. Yee and Shigeru Kondo took the record with five trillion positions.[8] In terms of practical considerations, such carnivals of calculation[9] do not matter: 39

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[4] Allchin lists his work on error in science on his homepage at the University of Minnesota (http://www.tc.umn.edu/~allch001/papers/themes.htm, last accessed on Sept. 27th, 2011). I have found Allchin 2001 and Allchin (forthcoming) especially useful (special thanks to Anna-Maria Kanthak).

[5] Allchin 2001, 38 (similar gist already in Feyerabend 1983, 21–33); Allchin (forthcoming) 2 f.; Mayo 2010, passim.

[6] Allchin's "program of error analytics" (2001, 40). In a more general form, Darden 1987, 38 f. claims the same for all history of science. A similar perspective prevails when error is thought of as 'negative knowledge'.

[7] Which is the perspective usually adopted by traditional history of science: cf. Lindley 1987.

[8] For Bellard see http://bellard.org./pi/pi2700e9/ (last accessed on Sept. 28th, 2011); for Yee & Kondo http://www.numberworld.org/misc_runs/pi-5t/announce_en.html (last accessed on Sept. 28th, 2011). As Brunke

decimal positions are sufficient in order to compute a circle which corresponds to the size of the whole visible universe, approx. 15 billion light-years, and still deviates from a perfectly circular form by no more than a proton's width.[10]

The question with which I will begin my investigation of computational error is, obvious by now, whether there is a firm line between approximation and error. Thus, I will have to move the discussion from Nagano, where Kondo's computer is located, to King Solomon's court. In the first book of Kings we read a detailed account of how King Solomon had a spleendid temple built for him. The account includes also a description of a large body of holy equipment, all fabricated by Hiram of Tyre, among which there is also a vessel so large that it is called the 'sea' (jām, I Kings 7.23):[11]

> And he made the molten sea of ten cubits from brim to brim, round in compass, and the height thereof was five cubits; and a line of thirty cubits did compass it round about.

This ‚sea' was a famous, awe-inspiring vessel that is mentioned also in 2 Chron. 4.2–5 and by Josephus as a triumph of craftsmanship, which later served as a reservoir for the temple. Already Spinoza wondered, however, how a circular body could have a diameter of ten and a perimeter of thirty units.[12] The easiest approach to the problem is to assume that error is involved, or, phrased more mildly, that the perimeter's number results from a practitioners' approximation of Pi.[13] There is, indeed, some evidence of 3 as the accepted co-efficient for computing both the perimeter and area of a circle in the ancient Near East.[14] (Of course, there are other ways of solving the problem, e. g., one could assume that the vessel's brim, with the main body being cylindrical, curved outwards bell-like. Then, the diameter at the brim could be ten units, and still the perimeter of the vessel's main body could be less than ten times Pi.) Zuidhof has tried to demonstrate that the measurements given in Kings are based on the number $\pi = 3.136$, which would be a good approximation, even better than other contemporary co-efficients in use, namely a Babylonian one with $25/8 = 3.125$ and an Egyptian one

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points out, these results are impossible to verify in practice, which actually conflicts with the notion of a 'record'.

9 The term is Netz's (2009, 17 ff.), used for Archimedes' Stomachion and similar works in ancient Greek mathematics.

10 I take this highly poetic visualization from Scinexx 13.3.2008 "Wieviel Pi braucht der Mensch?" (http://www.g-o.de/dossier-detail-389-7.html, last accessed on Sept. 28th, 2011).

11 Translation of the Jewish Publication Society, 1917. In what follows, all translations are my own, unless noted otherwise.

12 B. Spinoza, Tractatus Theologico-Politicus, Hamburg 1670, II 22 (quoted by Mulder 1998, ad locum). Interestingly, the Septuagint's text has 33 cubits, which is even farther removed from a 'correct' solution.

13 Noth 1968, ad locum.

14 See the igi-gub in Friberg 1981, 61 (diameter is 1/3 of perimeter), and the discussion in Brunke 2011, esp. 121 who treats 3 not as an approximation but as result of a definition. Compare now Muroi 2011 (non vidi).

with 256/81 = 3.1605).[15] The description of the 'molten sea' provides us, perhaps, with the first type of error: These cannot possibly have been exact measurements and thus the numbers are wrong and their relation is a case of error. One should, however, also pay attention to the context's intention in assigning the numbers given.

For the historiographer who wrote this passage of Kings it probably did not matter whether 30 units were an accurate or a less-than-accurate computation of the 'molten sea's' perimeter. As the modern reader can see from other numbers found in the text, the author wants to impress us by giving a rhetorically accurate description of gigantic or extremely valuable artifacts. The 'molten sea' was huge, almost unimaginably so for something consisting of cast bronze. The author has chosen his numbers in order to function as a rhetorical, one could even say: 'poetic-mimetic', device that will convey to the distant reader the awe and fascination of the original observer. Catchy numbers will probably do the trick better than more accurate measurements that are less easy to grasp (and that are difficult to express in a language written to be listened to, something which applies to modern fractions as well).

If, however, the author's intention were mainly rhetorical-poetical, that is, if he aimed for a mimetic impression on the recipient, and if he actually managed to accomplish such an aesthetic intention, it would not make sense anymore to speak of 'error'. At least, modern readers have to admit that it does not compromise the ancient text and the discourse of which it is a part to use 'wrong' numbers. Therefore, a diagnosis given by a modern historian of science, e.g. 'This is an erroneous computation' misunderstands the text's function and becomes, thus, itself erroneous, which I call a case of 'false' error. This is not a problem of insufficient computation and thus not a real case of error ('true' error). Rhetorically motivated vagueness works, to a certain extent and in this context, better than any string of number-words, be it accurate or not. Many numbers in ancient texts, especially in historiography,[16] including those that are not presented as results of computation, belong to the same class of rhetorically motivated 'false' error.

The problem of how to compute circles brings me to the next type of error. Let me start from Ps.-Hero's Geometrika, now read rather rarely, a useful and comprehensive collection of geometrical problems that are often solved more than once in different ways for the didactic benefit of the reader. The manuscript tradition that varies quite substantially in terms of problems included, attests to the lively interest readers had in this text in late antique and By-

[15] Zuidhof 1982, 180–183.
[16] Notorious Herodotus comes to mind who, e. g., gives the number of 1,700,000 for Xerxes' army (VII 60.1).

zantine times. This interest was almost certainly practical; accordingly, the mode of exposition is not the Euclidian one, but the one of 'recipes' that one finds in papyri since the 1st cent. AD.[17] Towards the end, in chapter 24, one finds computations that have to do with circles. Many of these have the character of mathematical riddles, that is, they transcend purely practical problems for the sake of theoretical interest,[18] yet stay within a practical frame of form and reference. For a historical typology of computational 'error', it suffices to look briefly at problems 44 and 45.[19]

> Εἰ δὲ θέλεις εὑρεῖν τὴν περίμετρον ἀπὸ τῆς διαμέτρου, ἐὰν ἔχῃ ἡ διάμετρος πόδας ιδ, ποιεῖς πάντοτε τὴν διαμέτρον ἐπὶ τὰ κβ· γίνονται πόδες τη. ἄρτι μερίζω· ὧν ζ'· γίνονται πόδες μδ· ἔστω ἡ περίμετρος ποδῶν μδ.
> If you wish to find the perimeter, when the diameter is given, if the diameter has 14 feet, you multiply every time the diameter with 22. The result is 308 feet. Instantly I divide: of these a seventh. The result is 44 feet. The perimeter shall be 44 feet.

Such texts are providing methods which the reader, by going through several variants with exemplary numbers, is meant to learn by heart (exactly in the way Babylonian and Egyptian mathematics are represented in the problem texts still extant).[20] In order to make sure that his readers are grasping the idea, Ps.-Hero provides a second recipe:

> Ἄλλως δὲ πάλιν· ἐὰν ἔχῃ ἡ διάμετρος πόδας ιδ, πάντοτε ποίει τὴν διάμετρον τριπλασίονα· γίνονται μβ· καὶ τὸ ζ' τῆς διαμέτρου· γίνονται πόδες β. ταῦτα πρόσθες τοῖς μβ· ὁμοῦ γίνονται μδ· ἔστω ἡ περίμετρος ποδῶν μδ.
> Again, in another way: If the diameter has 14 feet, make the diameter every time three times as big: The result is 42. And a seventh of the diameter: the result is 2 feet. Add these to the 42. (Taken) together, the result is 44 feet. The perimeter shall be 44 feet.

Quite obviously, Hero wants his reader to use a co-efficient of 22/7 or 3 1/7 (= 3.142857...) in order to determine the relation of perimeter and diameter. 22/7 is a decent approximation, comparatively easy to express and handle. On the other hand, it is of course not the number π itself and so, according to modern standards, insufficient in theory. We would treat it as an error, although, strictly speaking, every fixed number for the number π is no more than an approximation (in theory, the number π calls for a re-definition of 'error' or a re-evaluation of approximation, respectively). What begins as decent approximation, however, becomes more problematic, when Hero's co-efficient becomes part of an equation, e. g., of the following remarkable type:[21]

> Ἐὰν μίξω τὴν διάμετρον καὶ τὴν περίμετρον καὶ τὸ ἐμβαδὸν τοῦ κύκλου καὶ μίξας εὕρω τὰς

[17] On these texts see Asper 2003, 1–5, and 2009, 108–114.
[18] Asper 2007, 166 f., quoting Høyrup, e. g., 2002, 362–374.
[19] Hero Alexandrinus Bd. 4, S. 445 ed. Heiberg.
[20] See, e. g., Ritter 1998, esp. 79-97, and the texts quoted in Robson 2008, e. g., 89, 108, 186.
[21] Geom. 24.46, p. 445.20–446.3 Heiberg. Cf. Høyrup 2002, 371, 405 and more often.

ἀμφοτέρας φωνὰς ποδῶν ἀριθμόν σιβ, ἀποδιαστήσομεν ἕκαστον ἀριθμὸν ἀπ‘ ἀλλήλων.
If I unite (= add) the diameter and the perimeter and the surface of the circle and, when I have united them, find that all measures? taken together[22] have the size of 212 feet, we will each number separate from the others (lit. ‚from each other‘).

Hero 'solves' the problem for a 14-feet diameter and a 44-feet perimeter, i. e. exactly corresponding to his co-efficient of 22/7. Strictly speaking, from an un-historical perspective, the solution does not work: instead of 212 a value of approx. 211.91 results. What seems to be simple error, however, becomes something else when one approaches the problem through the history of such computations. In an Old-Babylonian list of mathematical problems (BM 80209, mostly quadratic equations) that was discussed by Friberg, one finds some problems that concern areas of circles. In the typical manner of this mathematical tradition, the list varies certain basic types of solutions by exchanging known and unknown values. This is Friberg‘s transliteration and translation of his basic type Nr. 7 which appears in four different variants:

a-šà gúdtr dal gúr ù sí-hi-ir-ti gúr UL-gar-ma A
Circle area, circle transversal, and circle perimeter added: A[23]

The problem type A + d + p = α, that gives the sum of surface, perimeter and diameter, provides an exact parallel to Hero. This text, however, precedes Hero by about 2000 years. One can find exactly the same problem with the same solution even much later than Hero, e. g., in Ibn Thabats so-called *Reckoner's Wealth*, a collection of conventional computations just like Ps.-Hero, which was written in the 12th cent. AD. In all these instances, the equation cannot be solved with whole numbers; instead, an inaccurate co-efficient provides a pseudo-solution. Høyrup has discovered how many such constellations actually exist, i. e., certain types of mathematical problems, same or similar co-efficients or algorithms, and a very similar rhetoric of these texts, that are observable in the 3000 years from Old Babylonian Algebra to medieval Arabic al-gabr, via, among other traditions, Greek practical mathematics. He has postulated powerful continuities of practitioners‘ traditions that, now and then, emerge whenever we have some texts, but have existed over thousands of years mainly as a background to the emergence of mathematical 'theory'.[24] For the conventional modern perspective, theoretical mathematics of the Euclidean-Archimedean type has eclipsed these traditions almost comple-

[22] LSJ s. v. ἀμφότερος II: "later, of more than two, 'all together'".
[23] Friberg 1981, 61 f.
[24] Cf. Høyrup 2002, 362–374.

tely; even an expert such as Høyrup has judged these traditions in earlier publications as 'sub-scientific'.[25]

Back to error. Approximations for the number π of 3 in practical contexts and of 22/7 among sub-scientific mathematicians are traditional co-efficients the status of which must be expert knowledge. With Hiram of Tyre, Hero of Alexandria, and Ibn Thabat we have three specimens of such experts. To label these practices instances of computational error, however, seems dubious for the following reason: In cultures in which (i) there is no competing group of experts who could criticize these methods and procedures, and, above all, in cultures that have (ii) no reason to criticize this body of knowledge because its experts are well-established in practical (problem-solution) and social (public acceptance of problem-solution) respects,[26] such mathematical practices cannot be 'wrong' in the same sense as they would be when judged from the perspective of modern Western European high-school mathematics. In the cases of Hero and Ibn Thabat one could perhaps doubt (i), but certainly not (ii), and in the context of Old Babylonian scribal culture it is evident that these practices were the only ones available and were part of scribal education precisely because they were regarded as canonical. From a historian's point of view, to label them as 'wrong' is a meaningless act within the field of historiography (by calling them 'meaningless' I want to say that such a statement says something about our knowledge systems, not about theirs). Here, the perspectives of mathematicians and historians clash: for the first, truth in mathematics is timeless, for the second, truth is social agreement and thus contextually defined. I am not saying, though, that mathematical truth is under all circumstances socially constructed (modern axiomatic mathematics and its ancient Greek predecessors have worked hard on being independent of social contexts). Often, the co-existing perspectives can even be complementary.[27] Nonetheless, only certain social groups develop an interest in and thus an instrument for having abstract concepts of the number π or $\sqrt{2}$ that would more or less satisfy what we call mathematical standards. For groups that do not have such an interest, the ethnologist-historian must refrain from the etic judgment of 'error'.[28] Thus, in my ad-hoc terminology, this is a typical case of 'false' error.

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[25] The term is Høyrup's (first in 1989).

[26] The two aspects might well turn out to be identical.

[27] See my comparison of the two Greek mathematics in Asper 2003, 30 f. (slightly different in Asper 2009, 128 f.).

[28] For the distinction of 'etic' and 'emic' perspectives, which coincides more or less with observer's and actor's categories, among ethnologists, see Goodenough 1970; Harris 1976. This is also the reason of why it is futile to discuss whether or not the Babylonians 'had', e. g., the theorem of Pythagoras. They did not have it, because

What I am suggesting here is to look at and describe error from a social rather than from an epistemological perspective (I will come back to that). For the time being, I claim that in a field of knowledge where a ruling group of experts lays out a problem and univocally adopts a certain solution that becomes canonical, this solution can never be 'wrong' in the same sense as the solution could be in a different context.

The argument will become much clearer when one compares a treatment of the same problem at more or less the same time, but in a completely different social context: I am talking of the famous approximation of Archimedes who, in his Dimensio circuli, by circumscription and inscription of polygonals that approximate the circle's perimeter, defines the number π as a number between 3 1/7 and 3 10/71.[29] This is true and thus certainly not an error. On the other hand, it would be very difficult to solve an equation with such a definition and, above all, to compute a given surface practically. Under social circumstances that accept and transmit expert knowledge exclusively under the condition of applicability and problem-solving, Archimedes' definition of the number π would be next to useless. One could even doubt that the practitioners of the traditions sketched out above would accept this notion of mathematical truth that is so fundamental in the Greek theoretical tradition. It is certainly doubtful what they could possibly have done with it.[30] If they did not have that notion, however, modern historians should refrain from basing their accounts of these knowledge traditions on it.

The error is a 'false' one, because it is socially established as acceptable in those groups that decide ex officio about truth, or rather, about successful problem-solution. The decision follows criteria that result from the social context of the knowledge, for which our notions of error and truth are, presumably, irrelevant. As a consequence, such a perspective on error has to understand error as an act of socially obvious failure, not theoretical (= logical) inconsistence.

So far, I have described two forms of error, at first sight obvious ones, that have turned out to be 'false' errors, namely a poetical simplification that enhances a text's function precisely by the quality that makes it appear erroneous, and a body of practitioners' knowledge which contains canonical approximations traditionally accepted as the correct solution.

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they had no use for it. This is a historical statement the reasons for which one can discuss. There can be, I think, no doubt, that, if they had had use for it, they would have had it. See Damerow 2001.

[29] See Geus 2007, 324 f., for the different theoretical approaches to π by Apollonius and Philon.

[30] This is not true for Hero who also did some work in the theoretical tradition. It is, however, unclear how close Hero is to Ps.-Hero, Geom., regarding both time and milieu.

2. Girths, Grounds, and Geometers

Klaus Geus has discussed the so-called 'Dido's Problem', quoting, among several examples, the case of Sardinia which Herodotus takes to be the largest island in the Mediterranean, apparently because he thinks Sardinia's circumference is especially large.[31] One finds the same argument in Thucydides who infers the size of Sicily from the number of days a merchant-ship takes to sail around it ("not much less than eight").[32]

When employed by these fathers of history, renowned for their analytic powers, this method may seem to be oddly inadequate to a modern reader. One has, however, to realize, first, the complete lack of a possible alternative and, second, that the method of computing a surface by measuring its circumference is the established one among expert practitioners (the so-called 'geometers', i. e., the agrimensores), preferred even when more precise approaches would be available. I will present some examples for and discussion of this striking fact, which in our perspective, again leads to computational error.

Professionals who are paid to calculate the size of grounds regularly use this method.[33] We do not have any reason to assume that common knowledge would have had an alternative way of computation. Apparently, geometrical knowledge in the Euclidean sense is in fourth-century Greece, apart from the realms of 'theory' and philosophy, not yet accessible enough to understand such questions as mere applications of abstract geometry. Court disputes provide especially striking glimpses of popular mathematical knowledge when the size and the value of a given piece of land is at stake, as, e. g., in the case of the antidosis of Phaenippus (Ps.-Demosthenes, or. 42.5).[34] The plaintiff claims that Phaenippus' estate measures more than 'forty stades' in circumference:

> ἐν ταύταις ἐκάλεσα κατὰ τὸν νόμον Φαίνιππον τουτονί. καλέσας δὲ καὶ παραλαβὼν τῶν οἰκείων τινὰς καὶ φίλων, ἐπορευόμην Κύθηρόνδε εἰς τὴν ἐσχατιὰν αὐτοῦ. καὶ πρῶτον μὲν περιαγαγὼν τὴν ἐσχατιὰν πλέον ἢ σταδίων οὖσαν τετταράκοντα κύκλῳ, ἔδειξα καὶ διεμαρτυράμην ἐναντίον Φαινίππου, ὅτι οὐδεὶς ὅρος ἔπεστιν ἐπὶ τῇ ἐσχατιᾷ·
> Within the period prescribed, I have called to court this man here, Phaenippus, according to the law. I arranged for some friends and family to come with me and with them I walked down to Kytherus to his boundary estate (i. e. an estate limited by the shore or mountains). And after I had first walked around his estate which had a size of more than 40 stadia all around, I demonstrated (this?) and had, while Phaenippus was present, it established before witnesses that the property was free of mortgages.

[31] Herodotus I 170; V 106; VI 2 (see Rowlands 1975, 438 f.); Geus, forthcoming.
[32] Thukydides VI 1.2: Σικελίας γὰρ περίπλους μέν ἐστιν ὁλκάδι οὐ πολλῶι τινὶ ἔλασσον ἢ ὀκτὼ ἡμερῶν.
[33] For the following remarks, see Netz 1999, 300.
[34] The contested issue is that the plaintiff wanted Phaenippus to be a member of the 'Three Hundred' who all had to pay a special tax called proeisphora, instead of becoming a member himself. He thus had to prove that Phaenippus was wealthier than he; if Phaenippus is proven wrong, he has to change property with the plaintiff. Apparently Phaenippus had claimed that this large piece of land was heavily mortgaged.

De Ste Croix has shown convincingly that it must be in the plaintiff's interest to let the property of Phaenippus appear as large as possible before the jurors, since the plaintiff wishes to ascertain that Phaenippus is wealthier than he is himself. In all other cases, actually very few, that discuss the sizes of estates, all that is discussed are numbers of areas, usually in plethra, unfortunately without a hint of how these were established.[35] According to de Ste Croix, the plaintiff wants to fool the jurors into believing that Phaenippus' estate is bigger than it actually was (ironically, the trick has worked with almost all historians of ancient Athenian economy). Quite amusing are the conversions that one finds in modern historians, from Wallon and Boeckh down to Finley, most of who assume that the estate was rectangular. Since it was an eskhatia, that is an estate that was limited by either the shore or the mountains, it might be that its shape was very irregular, and thus impossible to calculate by the usual methods (which we do not know). Today, it is impossible to ascertain the actual size of the estate in question. I believe, however, that the established method, geometrically erroneous or not, was precisely the one applied in this case by the plaintiff. At the very least, the method itself must have been familiar enough to the jurors that the plaintiff thought they could be convinced. If it had not been an established procedure, the jurors certainly would have been puzzled by the plaintiff's unorthodox choice of computation.

This passage can show, I believe, how elusive, fuzzy, and potentially misleading, the label of 'error' can be when we try to apply it to historical contexts. Only in the most simple, and least probable, of several possible constellations, that is, in the case of mathematical incompetence on the side of the plaintiff, can we justly label his account as 'truly' erroneous. In that case, one has to assume that the correct approach would have been within reach and that the erroneous procedure was not chosen because it delivered a more desirable result. It is, to me, far more probable that the plaintiff uses (a) an established method (for which see below) or (b) chooses among several accepted methods the one that fits him best. In both cases, the error is a 'false' one.

We do not know much about fifth- and fourth-century BC surveyors in Greece. There is a group of such people mentioned in Ps.-Democritus, the ἁρπεδονάπται, but we can only guess at what exactly they were doing.[36] At least, the author mentions them with respect to theoretical mathematics, which might point towards surveyors with an interest in theory. Perhaps one can imagine them along the lines of the authors of the imperial and late antique

[35] Finley 1951, 58 lists five cases of where sizes of estates are known from the 5th-4th centuries.
[36] Ps.-Democritus fr. 68 B 299 Diels & Kranz; see esp. Gandz 1930, 256.

Roman Corpus agrimensorum who, as far as I can see, operate on firmly Euclidean grounds when facing problems like the one discussed. Evidence that supports the priority of circumference in calculations of areas comes from Roman Egypt: Fowler has, in his study of the mathematics in Plato's day, discussed some Roman-Egyptian surveyors‘ papyri. These show a procedure that averages the lengths of the opposite sides of a given estate and then multiplies the two resulting averages. Packaged in a formula, handy but anachronistic, [37] the following equation is a modern abstract 'translation' of the procedure applied (given that ABCD is a quadrilateral area, and a and c, b and d are the sides opposite to each other):

$$\tfrac{1}{2}(a+c) \times \tfrac{1}{2}(b+d) = \tfrac{1}{4}(ab + cb + ad + cd),$$

which means that the surface of a given irregular tetragon is treated as the average of the four rectangular areas resulting from a multiplication of all sides sharing an angle. As Fowler correctly remarks, the procedure is, from the perspective of the mathematician, inadequate. The surveyor will always overestimate the size of the ground in question, unless it is rectangular (then $\tfrac{1}{4}(ab + cb + ad + cd) = ab$).[38] (This last fact, however, makes me wonder whether the imprecise procedure was tolerated in favor of its welcome outcome. The surveyors worked for tax collectors, and thus had perhaps an interest in slightly increasing the taxable areas, under the veil of an accepted procedure.)

Just as was the case with Phaenippus' estate, these experts compute areas by means of their circumference. These conventions all disregard 'Dido's problem' and the obvious fact that shapes with identical areas can have different circumferences. From a theorist's point of view, the error as such is easily diagnosed. One can understand such procedures, thus, as an indication of a sociological fact, namely wide-spread mathematical incompetence, as Netz does.[39] Already in antiquity, theoretical mathematicians thus despised experts of computation: In his commentary on Euclid (In Eucl. 403.4–14 Friedlein), Proclus mentions,[40] certainly in a disparaging voice,

> χωρογράφοι τὰ μεγέθη τῶν πόλεων ἐκ τῶν περιμέτρων συλλογιζόμενοι
> surveyors who infer the size of cities from their perimeters.

[37] For this discussion, see Unguru 1979.
[38] Fowler has extrapolated the procedures with much-to-be admired astuteness from texts that are very difficult to understand (1987/1999, 231-234).
[39] Netz is, however, interested in establishing his important point that theoretical mathematics is a very rare bird in classical Greece (Netz 2002, 209–210 n. 52).
[40] The text begins after a lacuna in the manuscript tradition. Proclus is discussing here the fact that areas of surfaces are different, even if certain parameters are the same (the opposite of what the Euclidian proposition I 37 claims, i.e. that triangles between the same parallels that have the same base, have the same area).

Perhaps there is even a rhetorical agenda behind Proclus' refusal to call these people not geō-metrai but khōrographoi. Besides being wrong according to us and to theoretical mathematics, such a disregard of Dido's problem has also a sociological component: if expert knowledge is, by definition, the knowledge that is in charge of calculating these things and if the experts proceed via the circumference, then this is, again, an instance of 'false' error, because the solution fulfills the criteria of competence and traditionally accepted appropriateness. In the world of the khōrographos, his method leads to correct results, as long as it is communally accepted.[41] This is precisely why such traditions put such a great weight on the tradition of established procedures: they avoid error, dispute, and external competition by way of establishing canonicity[42]—something which physicians or philosophers, for example, in ancient Greece and Rome were never able to achieve.

These remarks have demonstrated, I hope, that error as a meaningful category becomes relevant—or perhaps more radically—that the category of absolute error emerges when transitions are concerned, especially transitions from practice to theory. Such transitions will be at the center of the next and last sample of texts.

3. Ladders, Lengths (and Laughter)

Polybius was, already in his life-time, famous as an author of Taktika, i. e., a strategic manual or an 'art of war'.[43] Perhaps this is one of the reasons of why leading Romans were so interested in him. The Taktika are lost, but occasionally the Histories provide glimpses into what Polybius might have recommended, e. g., when he develops a program of strategic learning that is both very broad and very far from reality. In these respects, Polybius' didactic ambition provides a perfect parallel to Cicero's sketch of the ideal speaker, Vitruvius' of the ideal architect, and Galen's of the ideal physician.

In Hist. IX 12 ff. Polybius describes the tasks of the general and the kinds of knowledge that he has to employ therein in order to be successful. Among this body of knowledge there is also competence in geometry and computation, exemplified by the need to know the

[41] Which is not always the case. See Fowler 1987/1999, 233.
[42] It would be interesting to compare the Vedic Sulbasutras, treatises that within a religious context transmit knowledge about the geometrical equivalence of certain areas (see Asper 2007, 157). In this tradition, 'proof' is simply an established and accepted practice (Michaels 1978, 58–82).
[43] Mentioned by Polybius himself in Hist. IX 20.4; apparently a classic for later writers of Taktika (Arrianus, Techne tact. I 1, Aelianus Tacticus, Tact. theoria I 2, both vol. 2, p. 242 ed. Köchly & Rüstow).

length of scaling ladders used in sieges (IX 19.5–7). The following is the gist of what Polybius has to say:

> ἐὰν μὲν γὰρ διά τινος τῶν συμπραττόντων δοθῇ τὸ τοῦ τείχους ὕψος, πρόδηλος ἡ τῶν κλιμάκων γίνεται συμμετρία· οἵων γὰρ ἂν δέκα τινῶν εἶναι συμβαίνῃ τὸ τοῦ τείχους ὕψος, τοιούτων δώδεκα δεήσει τὰς κλίμακας δαψιλῶν ὑπάρχειν.
> If the height of the city-walls is given by some collaborator, the correct measure (summetria) of the ladders will become evident. For, if the wall's height happens to be, for example, ten of some (measure), it will be necessary that the ladders have a good twelve of such (measures).

Being a pragmatic soldier, Polybius does not assume that there is agreement between the besieger and the besieged concerning measuring units, because sieges happen all around the Mediterranean, and collaborationist citizens use local measuring systems. As is typical for Greek practical mathematics, he packages the recommendation to his reader of how to compute the ladders' length not into an abstract formula, but presents it as an example which then leads to a relation: The length of the ladders needs to be, in relation to the wall's height, as 'a good twelve' to ten, i. e. a little more than 6/5. In addition, Polybius provides a second instruction that, geometrically speaking (that is, by adopting a perspective unknown to or avoided by Polybius), allows also for the computation of the second leg of the right-angled triangle that results from ground, wall, and ladder. He says that the apobasis, that is, the distance between wall and ladder on the ground, should be about half as long as the ladder's length, in order to keep it from either collapsing under the weight of the soldiers running up (if it were too long and thus its inclination too little) or being overthrown by the besieged (if its inclination were too steep):

> τὴν δ' ἀπόβασιν τῆς κλίμακος πρὸς τὴν τῶν ἀναβαινόντων συμμετρίαν ἡμίσειαν εἶναι, ἵνα μήτε πλεῖον ἀφιστάμεναι διὰ τὸ πλῆθος τῶν ἐπιβαινόντων εὐσύντριπτοι γίνωνται μήτε πάλιν ὀρθότεραι προσερειδόμεναι λίαν ἀκροσφαλεῖς ὦσι τοῖς προσβαίνουσιν.

Where is the error, however? Unfortunately, the two relations given do not conform: Both >12:10 (ladder to wall) and 2:1 (ladder to apobasis) cannot be realized in the same set of conditions. The numbers that Polybius gives would lead to a distance of ladder-on-the-ground (apobasis) to wall of at least 6.63 of 'some measure', and possibly more, that is, at least 10% too much. Put differently, according to the tactician's instructions, the ladder should actually be 11.66 measures long, that is, not a 'good twelve', but rather 'a good eleven and a half').[44]

The situation as sketched by Polybius and the perspective and terminology adopted are, however, clearly close to the practitioner's realm (for example, Polybius refuses to phrase

[44] One cannot be absolutely certain that Polybius with ἡ τῶν ἀναβαινόντων συμμετρία meant the length of the ladder (although this is most probable) and not the wall's height, and thus one should calculate the latter case, too. Then, the ladder would be an ideal 11.18 measures long, that is, even further removed from 12.

the problem as one of computing sides of triangles). After all, the ladders would probably fulfill their function, which makes it impossible, I think, to judge Polybius' instruction as simply wrong. Thus, 'false' error again.

Moreover, the scaling ladders would be a little bit too long and would, therefore, avoid the main problem a strategos might have with such ladders. By way of example, Polybius tells of the ill-fated surprise siege of Meliteia, a polis near to Pharsalus, in Phthiotis: at daybreak, Philip's army attacked and would have certainly taken the city by surprise—if the scaling ladders that they had prepared in advance, had not proved too short (V 97.5 f.). Thanks to negligence or computational error on the side of the Macedonian strategos, Meliteia's inhabitants escaped conquest, and one can perhaps imagine some laughter pouring down from the walls onto the frustrated aggressors climbing down from their ladders. The story was probably well known as a cautionary tale; and thus, this is apparently the main problem with ladders that Polybius wants to prepare his readers against.

Despite his presentation of the problem, however, Polybius presents himself as a master of theoretical knowledge. For readers who approach mathematical problems from the theoretical perspective, it is thus difficult to avoid gloating,[45] because Polybius here proves less than competent in precisely the area that he recommends so vividly to the would-be strategos, namely geometry (IX 14.5):

> τὰ δ' ἐκ τῆς ἐμπειρίας προσδεῖται μαθήσεως καὶ θεωρημάτων, καὶ μάλιστα τῶν ἐξ ἀστρολογίας καὶ γεωμετρίας ...
> Knowledge from experience needs, in addition, learning and theory, and especially from astronomy and geometry ...

Albeit reluctantly, I think that we have to concede that in this respect Polybius' mathematical instruction is wrong, i. e., that this is a case of 'true' error. Polybius claims that he can provide his addressees with a mathematically exact and geometrically sound method, which he cannot. The problem emerges in the transition from practical to theoretical knowledge: Polybius has a grasp on the practical aspects, but aspires to the cultural capital of the theoretical presentation.

[45] Netz 2002, 213.

4. Conclusion: Community and Work-Flow

I have occasionally mentioned a 'historical' or 'historian's' perspective. It will become clearer what I mean by that when I set up the Platonist stance in mathematics as its counter-part. All computations discussed above were erroneous, as judged by such a view which claims, in our case, that a given and well-defined surface has a certain area, no matter when it is computed and by whom.[46] Except for clear judgments, however, such a stance provides not much insight into the cases it discusses.

How, then, can one understand 'error' and at the same time avoid the Platonist's perspective, which is still the one we, thanks to our systems of education, almost automatically adopt? From a sociological perspective, error is perhaps nothing but a dysfunctional course of action, that is, a sequence of actions prescribed or expected that then does not take place because one step did not work as it was expected to. More precisely, one can understand error as "an interruption" of "the ordinary flow of work".[47]

According to the work-flow criterion, the computations of Hiram, Ps.-Hero, Ps.-Demosthenes und Polybius are not errors. Quite the contrary, they even enhance work flow: as one can see in the speech against Phaenippus on the antidosis, 'work flow', because of its connection with the intended goal of an action, is itself a notion the meaning of which depends on interpretation. It seems to me that 'true error', for the work-flow theorist, would be, e. g., a procedure which makes the besieger always or in most cases show up at a siege with ladders that are too short (in this context, 'work flow' strikes me as inappropriate to a tragicomical extent).

Ps.-Hero's computations of areas are an interesting case. The text never mentions that the context of the collected procedures is a practical one. Historical reconstruction, however, that relies on comparisons with other mathematical traditions prove that this is, indeed, the case, mainly by looking at literary-formal criteria.[48] The reader who reads the text in the way it apparently wants to be read, that is, as a philosophical-theoretical one,[49] will have to judge

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[46] Such a perspective, from an Egyptologist's view, is discussed and rejected in general terms by Imhausen 2010, esp. 335 f., who also touches upon the Unguru controversy.

[47] For Star & Gerson 1987 errors ("mistakes") are simply an "event" of a group they call "anomaly" and which they define as "interruption to routine". The expressions quoted above, however, occur directly connected to "mistakes or accidents". Cf. Allchin 2001, 39.

[48] Compare, e. g., Høyrup 1997; Asper 2007, 377-380.

[49] The first two prefaces describe the benefits of geometrical knowledge in purely theoretical terms, namely geometry as the 'eye of astronomy', or as propaedeutic to Platonic philosophy, respectively (vol. 4, p. 172-175). What the text then calls "Hero's Beginning of geometrical Knowledge" proceeds, admittedly, from surveying in Egypt, but then states that its breadth is due to philomathia (p. 176).

its errors accordingly, that is, as 'true errors'. As I mentioned above, the same holds true for the theorist in Polybius, that is, that aspect of his authorial persona that intends to impress the reader by his theoretical approach to art-of-war problems rather than simply provide the reader with a well-working procedure for computing scaling-ladder lengths. Thus, this is a case of 'true' error, too.

A hindsight diagnosis of error becomes historically meaningful only when the past had a choice between competing perspectives. In the cases discussed above, these competing perspectives are the ones of practitioners and theorists, respectively. For the theorist, 3 as the co-efficient π is as insufficient as 22/7 (at least as long as there is no awareness of the epistemic status of these co-efficients). Not for the practitioner, however. Depending on what his intention is, perhaps to rhetorically impress the reader, as in the case of I Kings, or to compute areas, both solutions may even be acceptable at the same time. In discourses that are open only to one class of experts, that is either theorists or practitioners, errors are clearly identifiable; in 'mixed' discourses, however, the case is often unclear.

From our perspective, all cases discussed are 'true' errors. It remains doubtful, however, what kind of insight our perspective can actually provide. If the philosophers of science who advocate "communitarian epistemology", in recent times especially Martin Kusch, are right, knowledge, including modern scientific knowledge, is nothing but a community's consensus.[50] According to Kusch, there is neither truth nor even objectivity beyond the confines of a consensus reached by the community.[51]

This concept makes it, I think, impossible to speak of positions that conform to established problem-solutions of their respective epistemic communities, as error. This is why I have called this category 'false' error. Only if there existed, in that historical context, an external perspective similar to ours on that communitarian knowledge, that is, only in the case of an epistemic alternative, will it make some sense to speak of real, 'true', error. Thus, the focus has shifted from mathematical truth to extension and something of an epistemic community.

[50] At this point, more important questions arise, e. g., what is consensus? Its meaning cannot be that all assent, because that is almost never the case. How exactly can one describe the boundaries of a given 'community'? See Ziman 2005, 293.
[51] See Kusch 2002, 220-222 who criticizes Peirce's idea that truth be "idealized consensus"; 249-267 with criticism of positions that search for any objectivity that goes beyond consensus.

The diagnosis of error turns out to be a statement about the social distribution of knowledge.[52] If inter-subjectivity is the gauge of scientific knowledge,[53] those of the ancient authors quoted above, in whose discourse there was no subject that would have denied them consensus, could not possibly have committed error. David Bloor sketches out exactly this case for his constructed 'alternative mathematics', which he discusses by stressing the contrast of 'alternative mathematics' in Greek and our mathematics. That contrast he explains by "social causes".[54] Bloor tries to make an argument for how problematic the category of error actually is, even in the field of mathematics. If there exist several 'alternative mathematics' at the same time in the same place (as was the case in ancient Greece), from the perspective of the one the other must be full of errors of all kinds (which does not mean that the two would disagree on all 'truths' and, thus, one avoids a completely relativist stance towards truth or error, respectively).

In the end, one can doubt whether 'error' is a useful category at all for historians of science (some have denied that).[55] Perhaps one should refrain from adopting an absolute perspective, and rather experiment with more contextually-focused notions such as synchronic 'success' or 'failure', which would be closer to the notion of 'work-flow'.[56]

[52] Allchin 2001, 48: "Error arising from the organization of knowledge among professional communities might well cap the global end of a spectrum of error types."
[53] Ziman 2005, 294.
[54] Bloor 1976/1991, 108-134, quote 129.
[55] Star & Gerson 1987, 148: "This work [i. e., sociological research on mistakes and accidents] demonstrates that a mistake or any anomaly in scientific work never exists in some absolute sense; rather, it always is defined relative to a local or institutional context. Nothing except the negotiated context of work organization itself compels any scientist to correct or even take into account an anomalous event of any magnitude."
[56] I conclude my notes by mentioning Popper who argued for replacing the notion of 'truth' with the one of 'success' ("bewährt", 1934/1994, 220) when it comes to evaluating theory.

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Chapter 5

EMBEDDED STRUCTURES: TWO MESOPOTAMIAN EXAMPLES*

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Auch unter Schlangen gibt's Idioten –
Man erkennt sie an den Knoten.

Jiri Kandeler

This contribution is not on error, but very much about a scientific concept: study and systematisation of (geometric) complexity. Embedded structures, i.e. spaces arranged inside others in a particular way,[1] are an important field of interest in modern mathematics, e.g. knot theory. There is evidence that this concept has also been a subject of study in Ancient Mesopotamia. Two examples are considered in detail. The exposition is merely descriptive, not of any "theoretical" nature.

5.1 Embedding Lines in 3-Space: Knots

The reverse of the clay tablet VAT 9130 from Early Dynastic Šuruppak (modern Fara) contains an assembly of five drawings of knotted snakes (see fig. 1).[2] Even though the tablet has been known and dealt with for some time now, so far little attention has been paid to these drawings.[3] Friberg (2007, 418) seems to have been the first to state that "These drawings can be understood as another early

*This paper originated from part of my work within the Excellence Cluster 264 TOPOI, 2008-2010. Thanks to Helga Vogel for making Jiri Kandeler's lines known to me.

[1]There is no need to give the precise mathematical definition of a (topological) embedding here. The essential point in this context is that there do not occur any self-intersections of the components arranged in the surrounding space.

[2]Photographs of VAT 9130 can be found in Nissen, Damerow and Englund (1993, 113) and as CDLI-Nr. P010670.

[3]For example, in the original publication of the tablet, Deimel (1923, 71 (text 75)) just states "RS unbeschrieben."

example of a mathematical theme text," and they will be our first example for the systematic study of embedded structures.

Figure 1: Reverse of VAT 9130. Photograph: CDLI P010670.

In the following, the drawings of VAT 9130 rev will be adressed by numbers assigned to them according to their position on the tablet:

	1	
2		3
4		5

While the drawings 1, 2, 4, and 5 show one knotted snake each, entangled in itself (i.e., a *one-component knot*), there are two snakes entangled with each other (i.e., a *two-component knot*) in drawing 3. Even though it seems quite natural, it is worth mentioning that the knots are represented by means of 2-dimensional projections in very much the same way as it is done in modern knot theory[4] where they are called *knot diagrams*, see the examples below. A snake's body is just discontinous when undercrossing another part. We start with a structural analysis of the single knots using knot diagrams.

With exception of no. 2 all of the one-component knots are "true" knots, meaning that you cannot force the snake into a straight line by pulling its head and tail in opposite directions. Snake no. 2, however, as it is drawn on the tablet, *can* be pulled into a straight line and thus represents what is called an *un-knot* in knot theory. In view of the subsequent analysis it seems probable, however, that there is one erroneous crossing in the drawing and that in fact a "trefoil knot" was intended (fig. 2).[5]

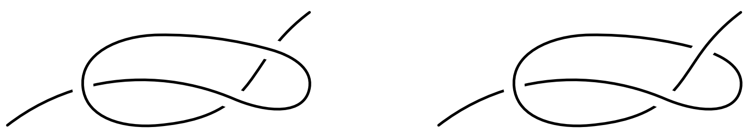
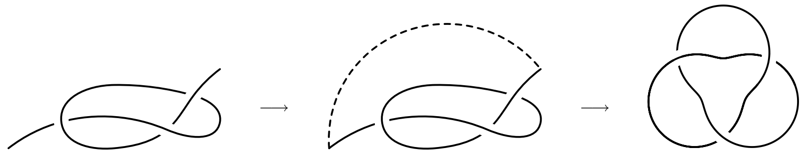


Figure 2: Left: the unknot drawn as no. 2 on the tablet. Right: the trefoil knot probably intended.

The knots nos. 1 and 5 come in the shape of two braids as depicted in fig. 3. Interestingly, also the trefoil knot allegedly intended in no. 2 can be considered the most elementary braid of the same principal structure as nos. 1 and 5. The corresponding deformation is illustrated in fig. 4. So the three knots nos. 1, 2 (corrected), and 5 turn out to be a three-element selection from an (infinite)

[4]For a nice introduction to this field see, e.g., Adams (1994).

[5]The name "trefoil knot" comes from the fact that by connecting the two ends (head and tail of the snake), you obtain a knot that can be deformed into a form looking like a trefoil:



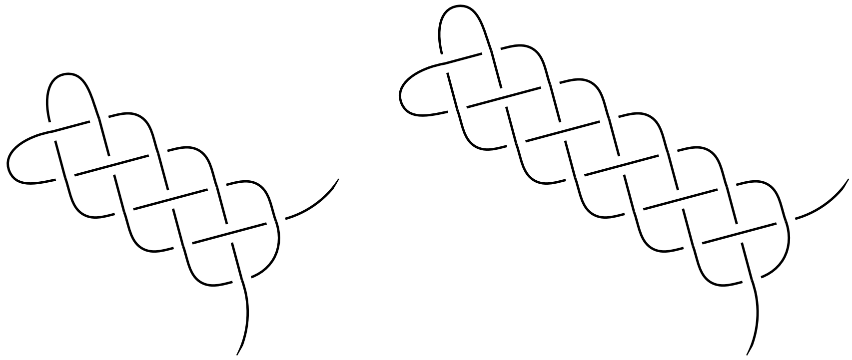


Figure 3: The braid-shaped knots in drawings 1 and 5.

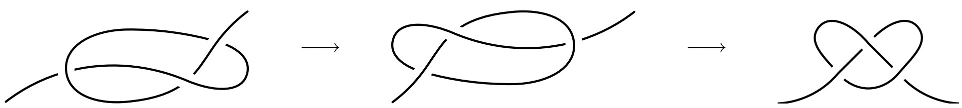


Figure 4: Transforming the alleged trefoil of no. 2 into the shape of an elementary braid by first rotating it by 180° and then slightly deforming it.

sequence of structurally similar knots of increasing complexity (figs. 6 and 5).

The knots 3 and 4 do not, however, fit into this pattern. Whereas no. 3 is a two component-knot (as mentioned above), no. 4 – while being a one-component knot like nos. 1, 2, and 5 – has a singular structure, even though a braid-like pattern in its center part is clearly discernable. To start with the simpler of the two drawings, let us first consider the knot diagram for the two component knot (or *link*, as knots with more than one component are also called) no. 3, the two components being depicted in different colours, fig. 7.

Note that none of the two components is knotted "in itself", both are representatives of the unknot (simple loops in fact). It is only through the interlinking

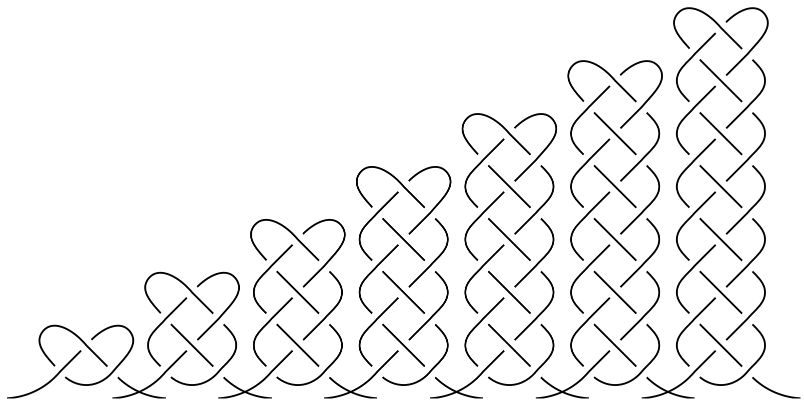


Figure 5: The first seven elements of a sequence of braids ...

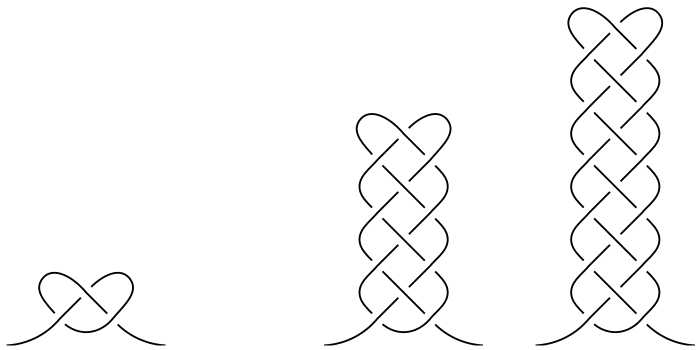


Figure 6: ... and the three elements found on VAT 9130 (nos. 2, 1, and 5, in this order).

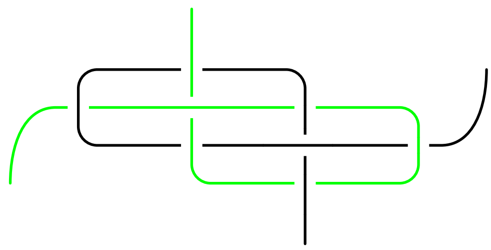


Figure 7: Knot diagram of the two-component link no. 3.

between the two components that the resulting two-component knot is non-trivial,[6] and due to the special interlinking type present here it is rather complex. It is tempting to try whether by concatenating the two components (i.e. by connecting the one snake's head to the other one's tail, cf. fig. 8), one could obtain one of the braids from the series in fig. 5.

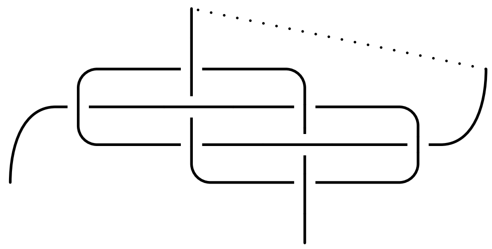


Figure 8: Making a braid out of no. 3 by concatenating its two components.

But this is not so, because in all the examples the number of crossings in the diagram representing the knots is the minimal number needed. And this is an integer multiple of 3 for the braids above, whereas it is 8 for the result of concatenating the components of no. 3. So this is an example of a truely different braiding type.

To finally investigate knot no. 4, looking at the drawing on the tablet, one sees that the scribe seemes to have had a hard time gettig the central part in order. It is not in all cases discernable whether we are dealing with overcrossings or undercrossings (or with a crossing at all, as in the upper right corner of the central part, see below). The situation is depicted in fig. 9.

In order to reconstruct this central part we make use of the most probably intended symmetry of the structure.[7] There seem to be present two symmetries, one with respect to each of the two diagonal axes. The first is obvious from the

[6]This is similar to a gallow's sling which in itself is an unknot. The situation becomes nontrivial only through the presence of a second component, a neck for example.
[7]Of its graphical representation, to be more precise.

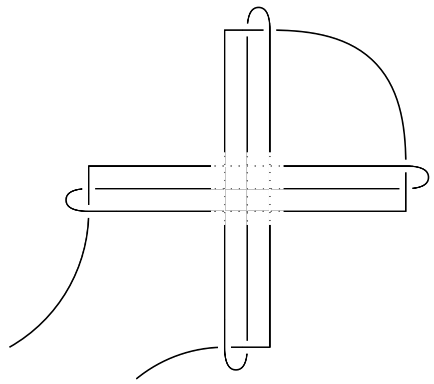


Figure 9: Knot diagram of no. 4 with less than clear central part.

drawing, but the other one is obscured because of the two loose ends (head and tail) of the snake. It can be made visible, however, by *closing* the knot, as is shown in fig. 10.

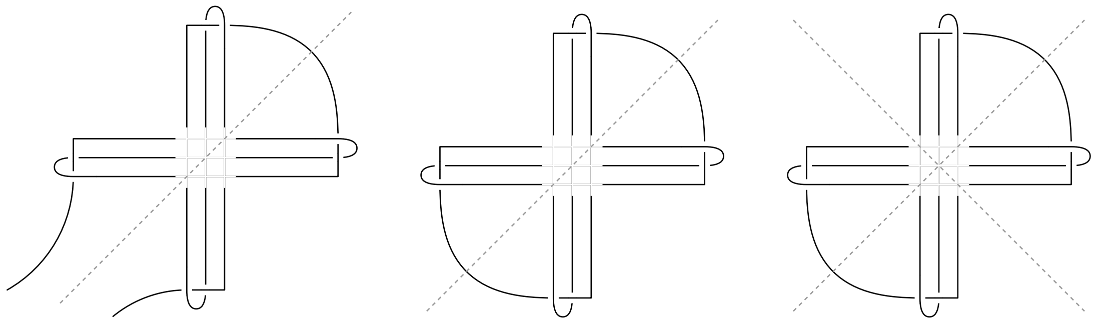


Figure 10: The symmetries of no. 4, one of which is established by closing the knot.

Making one of the two possible choices for the horizontal and the vertical central skeins, namely that the former overcrosses the latter, we end up with the situation shown in fig. 11. Now, the drawing on the tablet seems to indicate that the rightmost descending vertical skein, instead of crossing the horizontal skeins, just turns around and moves to the right.[8] Even though it seems most likely that

[8]This should be checked with the original, of course. Unfortunately, the tablet is not available

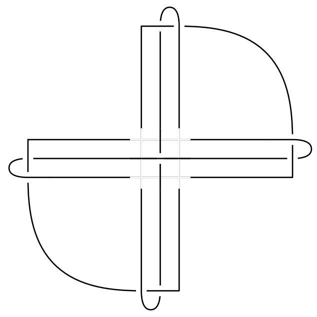


Figure 11: Making a choice for the horizontal and the vertical central skein.

each of the three vertical skeins was meant to cross each of the three horizontal ones, it is challenging to actually “prove” it. If the assumption of a turn instead of a crossing was indeed correct, the alleged symmetry would produce a similar situation in the lower left part of the central area (where the drawing is unclear) and we would (no matter what our choice for the behaviour of the central skeins was) be dealing with a situation as shown in fig. 12. But this would lead to a

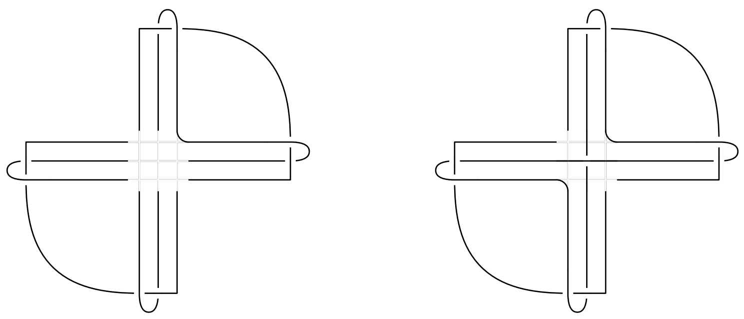


Figure 12: Supposing there is a turn in the upper right part of the central area ...

decomposition of the knot into two components one of which is an ordinary snake (after re-opening the knot), but the other one is a closed loop with neither head nor tail (fig. 13). Therefore, the above assumption is probably wrong, and we end up with a number of possibilities for reconstructing the central part of the knot

for collation at the moment.

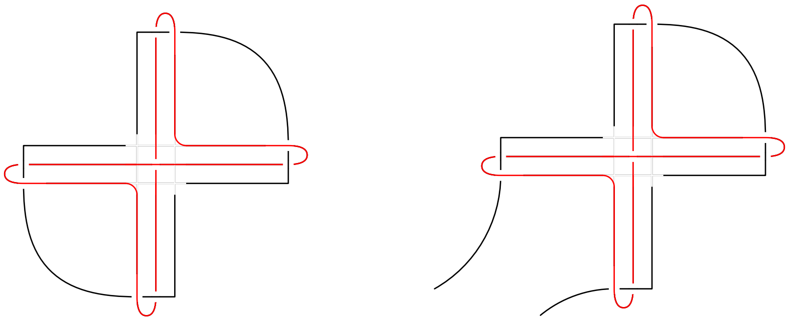


Figure 13: ... the snake decomposes into two components.

some of which are drawn in fig. 14.

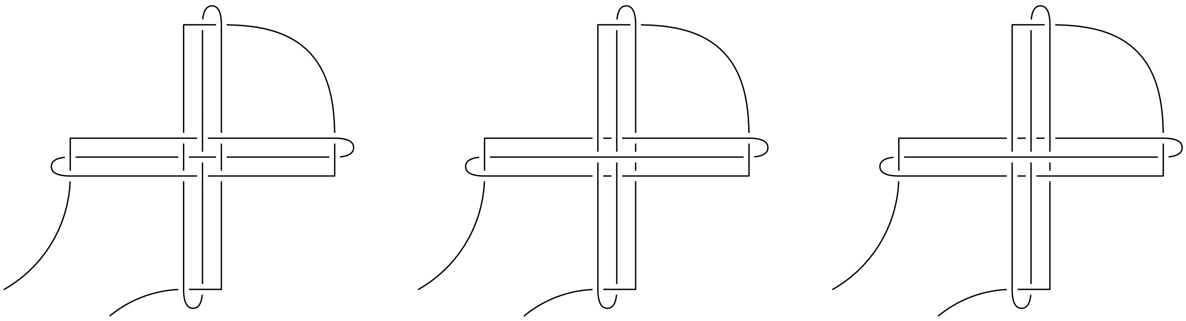


Figure 14: Some possible reconstructions of no. 4.

After the structural analysis of the single drawings of VAT 9130, some remarks concerning the composition as a whole are in order. First, it strikes us that all the drawings occupy about the same amount of space on the tablet surface. The more complex the knots they represent, the more minutely the drawings have to be executed. In this regard it is especially remarkable that it is just the simplest of the five knots, namely the alleged trefoil (no. 2), which is (allegedly) misdrawn. Note also that this drawing appears to be the most "naturalistic" and is executed in a less scematic and formalized way than the others. Possibly, it is the first one that has been drawn on the tablet.

It seems plausible to think of the reverse of VAT 9130 as of a "list",[9] very much like the one on the obverse which is a copy of the well-known Sumerian titles and professions list,[10] the main differences being the following. First, the subject of consideration is not some semantic or lexical field but geometric complexity, in this special case the complexity of embedded lines in 3-dimensional space. And second, unlike the lexical list on the obverse (and all other lexical lists, come to that) this list is not written in lines and columns. This is perhaps mainly due to the fact that this kind of list is not yet standardized and especially not yet canonized (and probably has never been; remember that VAT 9130 is the only example known so far). But then the normal list format is not even to be expected here because there is no such thing as a linear order on the complexity of knots. And maybe it was not even needed since, whatever the exact ordering criterion might have been, the ordering is encoded intrinsically in the graphic representations of geometric complexity itself — obvious at least for the case of the series of braids.

Note in particular that it takes some training to carry out such drawings in a more or less correct and precise manner as they are found — apart from the center part of no. 4 and some other minor glitches — on the tablet. One may assume that the scribe used a template for the knots as well as for the text on the tablet's obverse side. This as well as the rather schematic design of the drawings (except for no. 2) indicates that these structures and their systematisation have been part of the (scribal) education and thus of scientific consideration. Yet, after all, we might think of VAT 9130 as of something like an early version of modern tables of knots, for an example of which see, e.g., Adams (1994, 280-290) (reproduced from

[9]In view of Assyriologists' use of the word "list" for a very specific text format in ancient Mesopotamia, Friberg's expression "theme text" (see above) is more adequate. However, Eva Cancik-Kirschbaum is at present working on a much more general approach to the concept of lists.

[10]For this list see Nissen et al. (1993, 110-115) and Nissen, Damerow and Englund (1991, 153-156), and for the autograph Deimel (1923, 71 (text 75)). For general information on lexical lists see Cavigneaux (1980–83).

Rolfsen (1976)).

5.2 Embedding a Rectangle in 2-Space: Surface-filling Bands

Possibly also the tablets MS 4515 and MS 3194 (Friberg (2007, 219-21 and 224-27, respectively), see fig. 15) can be considered as part of a list (better: series) dealing with the collection and study of complex embedded structures, in this case surface-filling bands.

Figure 15: Obverse of MS 4515 (left; photograph: CDLI P253616) and of MS3194 (right; photograph: CDLI P274587). Reverse sides and edges blank.

They have been studied extensively by Friberg (*op. cit.*) who interpreted them as labyrinths having one “good” and one “bad” path each, meaning a path reaching the center or not, respectively (each starting at one side of the array). However, the photographs seem to indicate that he miscopied the central part of the array in both cases and that there is in fact *only one* path each, entering the array on one side, spiraling towards the center, turning around, spiraling out again and leaving the array on the other side.

Here we only analyze MS 4515 as it seems to follow from the photographs (Friberg (2007, 489 top); CDLI Nr. P253616). Fig. 16 shows a sketch of the drawing (not one hundred per cent to scale and rotated 90 degrees compared to Friberg's drawing). It consists of two different connection components each of which is a polygon with rectangular turns only (drawn in black and red colour, respectively), with appendices (green) protruding from some of the nodes. These lines make the borders of a path which fills the whole surface (except, of course, the bordering lines themselves), cf. fig. 17.

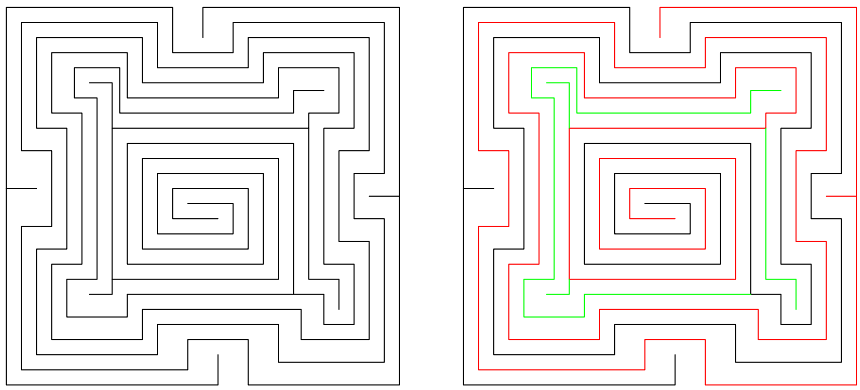


Figure 16: Drawings of MS 4515 (not exactly to scale); in the right drawing the different connection components and appendices are represented in different colours.

The same seems to be true for the much more complex pattern of MS 3194. However, in this case the CDLI photograph (P274587) is too low a resolution and the photograph of the then uncleaned tablet in Friberg (2007, 490 top) is too unclear to be absolutely sure. If so, then in both cases, but surely in MS 4515 the situation is the same as in the drawing on the bottom of the clay cone MS 3195 (Friberg, 2007, 223, fig. 8.3.8.) which then is, as Friberg suggests, indeed a possible precursor of the structures considered above.

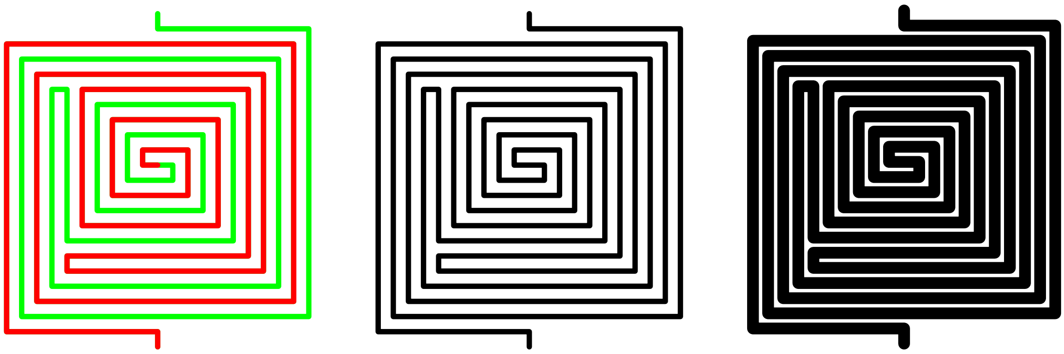


Figure 17: Schematic representation of the “path” of MS 4515, neglecting bulges; in the leftmost drawing its ingoing and outgoing parts are differently coloured, the rightmost drawing tries to visualize the path as a surface-filling band.

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CHAPTER 6

FALLACIES IN CICERO'S THOUGHTS ABOUT DIVINATION.

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Cicero's remarkable essay on divination (De Divinatione) has begun to attract serious attention from scholars working on Babylonian omens (e.g. Rochberg 2004: 45–48), although even more careful scrutiny could be devoted to this work from the viewpoint of Mesopotamian scholarship. What seems abundantly clear is that Cicero was hardly basing his knowledge of omens on Etruscan hepatoscopy, to which he hardly refers (and probably had little respect for), but he acknowledges his sources as being Stoic thinkers, among whom were Diogenes of Babylon (see also Rochberg 2010: 411). Cicero was quite critical of the logic of omens, the validity of which he could not accept; for him the entire system was fallacious. Nevertheless, before assessing Cicero's judgments on divination, it is worth noting how well-informed he seemed to be on highly technical and complex systems of divination, such as liver omens, which are best known from Mesopotamia. How would Cicero have managed to acquire such knowledge, if indeed he did?

Before even beginning to answer this question, the question of cultural contacts must first be addressed. Although one would ideally like to find similar approaches to Wissenschaften among Greeks and Babylonians in the Persian and Hellenistic periods – when approaches to mathematics and astronomy and even medicine were developing along parallel lines – this quest may be too ambitious. Greece and Persia were long-standing enemies and rivals, and firm contacts had not yet been established between these rather inimical societies. Hellenisation equally took a long time to make fundamental changes in the extensive Seleucid Empire. Similarly, only after Roman domination of the Levant and Egypt do we begin to see substantive variations in legal norms and everyday practices in those regions, as witnessed by numerous papyri from Egypt (Yiftach-Firanko 2009: 542, 555). By Cicero's era, Rome had become a virtual melting pot of cultures from Roman colonies where a great deal of new ideas were seeping in from the East, including new religious cults and practices which challenged Roman traditional beliefs and practices. The question is whether Cicero may have been influenced by these new ideas being introduced into Roman intellectual circles.

We begin by examining Cicero's statements which describe a divination system easily recognisable from Babylonian sources. Cicero first appears to defend the basis for divination, namely that signs and omens are provided by gods to allow mankind to foretell the future, by setting up a fallacious bogus syllogism to prove his point:

1) gods exist and have power to know the future

2) gods possess kindness, and therefore give us signs for the future

3) gods (being kind) would not give signs without means of interpreting them

4) ergo because signs are not useless, divination is valid

(Cicero DD. I 9f. = Loeb 232f.)

For Cicero to refute such logic was hardly difficult, but more interesting than Cicero's counterarguments is the question of where the argument originates and how Cicero came upon it. The idea of gods communicating their thoughts or plans through signs of omens is a dominant motif of Mesopotamian divination, which was based upon a dual approach to interpreting divine semiotics; signs could either be 'provoked' (e.g. through regular oracle inquiries or examining animal livers for signs), or 'unprovoked' omens, e.g. prodigies which occur spontaneously or unexpectedly, such as the appearance of a fox in the street or a snake falling onto the bed. Since the idea of provoked and unprovoked omens is discussed in De Divinatione, along with a fairly good grasp of various categories of omens known best from cuneiform sources, it is worth asking how Cicero knew so much about divination in general, and from where he derived his information.

Cicero gives various examples of provoked omens, which he refers to in one place as 'forced auspices' (auspiciis coactis), which include the examining of entrails (extis) as well as the flight of birds (I. 28, Loeb p. 256–259). He elaborates further by describing two types of divination, unum quod particeps esset artis, alterum quod arte careret, 'one which is allied with art, the other, which is devoid of art' (I. 34, Loeb p. 262f.). He explains this by describing the diviners who employ art as observing what is already known to deduce that which is unknown, as opposed to diviners who divine through unrestrained emotional excitement, such as frenzied prophecies or even oracles (ibid.). Elsewhere Cicero describes this same procedure of provoked omens somewhat differently, as 'artificial means of divination', which include examining of entrails and astrology, as opposed to 'natural divination' (divinatio naturalis), based on ad hoc inspiration (DD I 109f. = Loeb p. 340–343). This is a distinction known from Mesopotamian sources as well, in which the 'art' of divination is a highly-developed technique based upon inferences derived from numerous observations of the same phenomena, often recorded, as opposed to the mantic statements of an ecstatic (mahhu) or prophet, whose insights cannot be controled or measured by any collection of data. Also remarkable is the fact that Cicero considers oracles to represent 'natural' rather than 'artificial' divination (DD 37 = Loeb 266f.), since in Mesopotamian divination oracles also fall outside the normal *technē*. Instead of following the usual casuistic format of omen literature, 'if x ... then y', Mesopotamian oracle literature only records the questions asked rather than the answers received, and the descriptions of oracles read like case studies of individual situations, which is most uncharacteristic of Akkadian omen texts (cf. Lambert 2010 and Starr 1990).[1]

As to whether Cicero had any real knowledge of Mesopotamian divination has been partially addressed by John Jacobs, who has found convincing examples of Akkadian *šumma* izbu-type omens (from anomalous births or vaginal discharges) in De Divinatione (Jacobs 2010). Jacobs speculates as to the route which Akkadian omens may have taken to arrive at Cicero's door but without adducing much convincing evidence for direct borrowing, except to suggest that this genre of omens was also attested in Ugaritic, HitHi

Hittite, and Hurrian, all of which were long obsolete by Cicero's time (ibid. 333). Other similarities to Akkadian omen literature can be found throughout Cicero's treatise, such as snake portents which resemble Akkadian *šumma ālu* terrestrial omens.[2]

We turn now to astrology, another of Cicero's interests, for which he specifically holds Babylonians responsible for promulgating this sort of divination. The crucial passage (DD I 36) must be cited in full: contemnamus (var. condemnemus) etiam Babylonem et eos qui e Caucaso caeli signa servantes numeris [et motibus] stellarum cursus persequuntur, [3] loosely translated in Loeb as: ‘Let us scorn the Babylonians[4], too, and those astrologers who, from the top ofMount Caucasus, observe the celestial signs and with the aid of mathematics follow the

[1] Although oracle questions are technically anonymous, the questions are addressed on behalf of 'So-and-so, son of so-and-so', for very specific circumstances, such as whether the questioner might be appointed as royal eunuch (Lambert 2010: 106-109). The formulation allowing for a name to be inserted into the text follows the patterns of model contracts, which were used for training scribes, but this does not alter the fact that such texts describe specific situations, in contrast to the epigrammatically listed 'if x ... then y' format of technical omen texts.

[2] DD I 36 = Loeb 264-7, in which Cicero cites the case of a well-known Roman official who found a male and female snake in his house and was advised by diviners that releasing the female snake would result in his own death while releasing the male snake would result in his wife's death. Although Akkadian omens offer no exact parallel to this report, the opposition between male and female characteristics and the resulting opposition of the death of either the owner of the house or his wife is very typical of the genre of Mesopotamian snake omens. Cicero refers to the same narrative again in DD ii 62 (Loeb 440f.), where he also refers to a report of an omen based upon a snake coiled around a beam, which is a rather common motif in Akkadian *šumma ālu* omens; see Freedman II 57: 53', in which 'nesting' in the beams is a homonym for coiling (iqnun and iknun).

[3] ed. Teubner (1975) p. 24.

[4] The text actually reads 'Babylon'.

courses of the stars' (Loeb 267). It is quite plausible that the Babylonians referred to here were indeed astrologers who made regular mathematical calculations of celestial movements. The Latin text, however, does not refer to any mountains but only to a mysterious place, Caucasus, which has so far defied reasonable explanation.[5] We propose an alternative explanation, since Babylonia (ie. Iraq) is completely devoid of mountains, except for the large artificial mounds and ziggurat towers characteristic of ancient Mesopotamia.[6]

In fact, we know that the primary place for celestial observations was the city of Babylon itself, the archives of which have produced numerous astronomical diaries and almanacs, spread over some 700 years, beginning in the mid-7th century BCE (Sachs and Hunger 1988, 1989, 1996).[7] These observations were likely to have come from the same place, namely the Esagil-temple in Babylon and perhaps from its ziggurat, the Etemenanki, at least until being razed by Alexander for restoration.[8] Could 'Caucasus' refer to this temple in Babylon? One possibility is that 'caucasus' is a corruption of an Akkadian epithet markasu referring to this particular temple and either misunderstood by Cicero himself or by later generations of copyists.[9] This typical epithet for Babylonian temples, markasu, was used in temple names as a cosmic cognomen describing the 'bond of heaven and earth' (dur.an.ki = *markas šamê u* erṣeti), referring at first to Nippur and then later to Babylon as centres of the universe (George 1992: 261–262).[10] The essential idea is that the Babylon and its temple, as 'bond of heaven and earth', was the appropriate place to make terrestrial observations of the heavens, and the term markasu, referring to the Esagil temple or Babylon, was somehow communicated to Cicero; the Akkadian word could have been later corrupted into 'Caucasus' because the allusion was no longer understood.

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5 cf. W. Wardle's commentary, p. 201, suggesting Afghanistan (reference courtesy Klaus Geus).

6 Babylonian temples were often characterized as 'mountains' in their Sumerian names and epithets, such as the important Ekur-temple at Nippur, the name of which literally means 'temple mount'.

7 See now the preliminary editions of R. van der Spek and I. L. Finkel, www.livius.org.

8 See R. van der Spek's comments on http://www.livius.org/cg-cm/chronicles/bchp-ruin_esagila/ruin_esagila_01.html.

9 A Late Babylonian commentary employs the term markasu to describe the Esagil temple in Babylon, see CAD M/1 283 lex. The idea of a temple being a metaphor for a 'mountain' is central to Babylonian temple terminology, as well as being 'bond of heaven and earth'. The literal meaning of Esagil in Sumerian is 'temple, (its) head is elevated', ie. reaching up to heaven. This is exactly the kind of topographical space which astronomers used for their calculations, in Babylon. Although one would have preferred to find a corruption of Esagil or Etemenanki (lit. 'Temple, temenos of heaven and earth') in Cicero's text, markasu was used as an epithet and then perhaps even as a proper noun for this temple in some learned contexts, to explain the use of this temple for celestial observations. What is certain is that the term Caucasus in Cicero is wrong and must be a corruption in the transmission of this text; no original manuscripts from Cicero's hand survive.

10 Coincidentally, one text from the Babylonian school curriculum, listing the topography of Babylon, was transliterated into Greek, probably contemporary with Cicero, and the orthography markas appears in the Greek transliteration (see George 1992: 38, pl. 6).

Although Cicero makes frequent reference to the examining of entrails or heaptoscopy,[11] a typically Mesopotamian divinatory practice (Koch-Westenholz 2000), it is tempting to assign his knowledge of this art to Etruscans and their famous liver model, demonstrating local interest in this form of soothsaying. It is highly unlikely, however, that Etruscans were the source of Cicero's information on hepatoscopy, despite the discovery of the liver model in Piacenza. For one thing, the Etruscan liver model is nothing like its Mesopotamian counterparts, since the labels on the Italian liver model refer to celestial phenomena, indicating that the liver model serves the needs of astrology rather than Mesopotamian-style liver divination (Scharf 1988:14–15). Moreover, Cicero is rather condescending towards Etruscans as representing folk traditions which do not have the status of philosophy or intellectual discourse,[12] which are Cicero's preferred sources, and especially Stoic philosophers whom he mentions consistently. It is worth looking more closely at Cicero's informants.

Cicero's narrative shows a clear preference for Stoic philosophers who took a logical stance (his opinion fallacious) on the subject of divination and prediction. Among these philosophers were Zeno and Chrysippus, who came from Cyprus and Tarsus in Cilicia respectively,[13] which had previously been part of the Assyrian Empire, as well as Chrysippus' disciple Diogenes of Babylon (DD I 6 = Loeb 228f.);[14] other Stoics came from Tarsus (Cilicia) and Apamea (Syria). It is not known how much Oriental wisdom may have influenced Stoic philosophy, since most Stoic writings do not actually survive and the remaining fragmentary sources never suggest Oriental influence, but we intend to raise the question whether a scholar from Babylon, like Diogenes, could have brought 'alien wisdom' to Rome and eventually to Cicero himself.

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[11] DD I 93 = Loeb 324f., associating extispicy specifically with Etruscans, DD I 28 = Loeb 256f., in which Cicero remarks that examining entrails has become less common as a form of divination, which was true in Mesopotamia as well, after the advent of astrology, cf. also DD I 109 = Loeb 340f.

[12] See DD I 35, 'I will not allow myself to be persuaded that the whole Etruscan nation has gone stark mad on the subject of entrails' (translation Loeb 265), and elsewhere (i 93 = Loeb 324f.) Cicero compares Etruscans with nomadic Arabs, Phrygians, and Cilicians, or rural peoples who indulged in soothsaying as folk magic rather than scholarship. Cicero, however, is not any more convinced about the science of Assyrians or Babylonians (i.e. Chaldeans, and it is significant that he distinguishes between the two), since he credited the Assyrians with astronomy primarily because of the geographical advantages of living on a flat plain with a good view of heavens, while the Chaldaeans are thought to have perfected a science (scientiam putantur effecisse) which is capable of making accurate predictions regarding a person's future and his fate at birth; see DD I 2 = Loeb 222-225. Arguments for Etruscans borrowing Assyrian liver models (Burkert 1997: 46-53) are unconvincing.

[13] Zeno is also claimed to have been of Jewish descent (De Crescenzo 1988: 406). Tarsus was an important academic centre and would have belonged to the Seleucid Empire during the time when important Stoic philosophers came from there. Although no cuneiform library has as yet been found in Tarsus, Akkadian was alive and well during this period and Babylonian science still flourished.

[14] See ibid., 415, although no significance is attributed to his country of origin.

Diogenes himself is known from a few fragmentary sources outside of Cicero's work. Galen, for instance, cites Diogenes of Babylon (in connection with Chrysippus) on the question whether voice has physical properties as it passes through the windpipe, and whether it reflects articulated speech coming directly from the mind (ie. either brain or heart). Galen argues against the point of view, held by Diogenes, that speech emanates from the heart (instead of the brain), and Galen ridicules the idea of the heart as organ of cognition by referring to heartburn as a condition of the stomach; Galen argues that the expression 'mouth of the stomach' actually refers to the heart, thereby ruling out the heart for cognition. Galen reinforces his argument by referring to Diogenes's statements about the heart being the organ which receives nutriment and pneuma, although for Galen the latter term also refers to the soul; Galen then ridicules Diogenes' statement that the 'soul' represents 'vaporization' (cf. On the Doctrines of Hippocrates and Plato ii 8.36–51). However, if Diogenes's arguments indeed reflected Babylonian science, the following points would apply. 1) In Babylonian anatomy, the term libbu referred generally to the stomach and only specifically to the 'heart' when designating the organ of cognition or emotion, or the psyche. 2) The 'mouth of the stomach' (Akkadian pî karši), which is the identical expression used by Galen, can be associated with nutriment in Babylonian contexts (see Geller 2010: 4–8)[15], but the term *karšu* 'stomach' can also refer to the 'mind' as a synonym for libbu 'heart'.[16] This is precisely the double entendre which Galen is ridiculing in Diogenes. 3) Galen's reference to Diogenes' theory of the soul as 'vaporization' and the connection with pneuma goes back to Akkadian terminology to *napištu*, 'breath', which was later equated with 'soul' in Semitic philology (Hebrew *npš*). The contextualising of Diogenes' statements within Babylonian science somewhat clarifies the nature of Galen's objections, although without assuming any special knowledge on Galen's part of Babylonian thought.

A similar kind of analysis could be applied to Diogenes's statements regarding the physical properties of the voice in relation to language, as seen from a Babylonian perspective. Diogenes Laertius claims that Diogenes of Babylon wrote a treatise On Voice or on language (Diog. Laert. VII 55–57), apparently treating the physical nature of voice and its connection with phonetics in language.

[15] The unique Hellenistic text edited in this passage is a list of diseases associated with regions of the body, perhaps affected by Zodiacal influences, and the first two regions listed are libbu 'heart' and *pî karši*, 'mouth of the stomach'. The diseases associated with libbu, such as epilepsy, seizure, and depression, were thought to have psychological dimensions (caused by demons), while those associated with the 'mouth of the stomach' include dental problems, drospy, and 'bile', associated with digestion.

[16] Chicago Assyrian Dictionary K 224f.

The Akkadian word for 'voice', rigmu, refers to the human and animal voice as well as to sound in general, or to the roar of thunder, and also has cosmic significance; in the myth Atrahasis, the god Enlil brings on the Flood because mankind makes too much 'noise' (rigmu) and disturbs his sleep. Human speech, therefore, is a form or articulated noise, which appears to be Diogenes' argument as well, as noted by Galen above. Furthermore, in scholastic contexts rigmu can refer to vowels,[17] and Akkadian lexical lists can not only provide the orthography of Sumerian terms but also their phonetic values (MSL XV = Civil 2004), and the concept of 'voice' as phonetics also appears in Babylonian grammatical texts (MSL IV = Landsberger 1956: 130).[18] In fact, one of the odd coincidences is that Diogenes Laertius mentions a puzzling passage in which quoted Diogenes saying, 'Reduced to writing, what was voice becomes a verbal expression, as "day"' (Diog. Laert. VII 56). The connection is quite abstruse but might have something to do with the fact that one Babylonian lexical equation reads ù UD = ri-ig-mu or alternatively u-[ud] UD = ri-ig-mu,[19] equating Akkadian rigmu with Sumerian u_4 or ud, which normally means 'day'.[20]

This kind of circumstantial evidence for Babylonian concepts within Stoic dialectics is hardly overwhelming, but additional data can be adduced from Diogenes' other writings, which may help somewhat. Pseudo-Plutarch also refers to Diogenes of Babylon in relation to a treatise on embryology (as presented Tieleman 1991). The actual fragmentary passages are translated by Tieleman as follows: 'Diogenes [believes] that the embryos (children) are born when the innate heat is heated up (?): that as soon as the child is poured forth the cold is poured into the lungs' (ibid. 108). An alternative version reads that 'embryos are generated without soul but in heat', and that heat and not cold is drawn into the lungs at birth. In both versions of Diogenes's statement, the child is described as being 'poured forth' from the womb, which is a typically Babylonian image of childbirth; the foetus is perceived as a boat floating in the womb on a lake of amniotic fluid (see Stol 2000: 10, 64–65, 71, 125), and the

[17] E.g., in the Sumerian school composition Examenstext, see for convenience Chicago Assyrian Dictionary R, 331.

[18] The so-called Neo-Babylonian grammatical texts begins with a series of vowels (ù, a, i, e) isolated from Sumerian prefixes, infixes and suffixes indicating 1st, 2nd, and 3rd person pronouns, showing how vowels have phonemic values in Sumerian. Diogenes Laertius applies Diogenes' comments to Greek, but we cannot be sure that Diogenes' methodology was not acquired from his Babylonian schooling.

[19] Chicago Assyrian Dictionary R 328 (citing the lexical list A III/3 14 and 34).

[20] The remainder of the passage in Diogenes Laertius is remarkable for citing Diogenes' work on dialectics and his discussion of the conjunction 'if': 'Now this conjunction promises that the second of two things follows consequentially upon the first, as, for instance, "if it is day, it is light" (Diog. Laert. VII 71 = Loeb 179). Not only does the Babylonian term umu mean both 'day' and (less commonly) 'daylight', but the casuistic form of the statement is typically Babylonian (if ... then ...), while the statement in Greek actually makes little sense and appears to be a tautology.

idea of a baby being poured out fits this image rather well. The idea of being born 'without soul' is puzzling in a Classical context, but if we return to the idea of Akkadian *napištu* being 'breath' or breathing, the passage seems to discuss the first signs of breathing at birth. However, an analysis of Diogenes' comments based upon Stoic conceptions of the soul may in fact miss the mark, since it is equally possible that the comment attributed to Diogenes out of context may simply refer to paediatric medical symptoms.[21]

Although none of this fragmentary evidence amounts to very much, it is assembled only to suggest the possibility that Diogenes' cited theories do not appear to contradict Babylonian science and that he may have been a credible informant for Cicero. If his presumed book on divination had not been lost, we would have known conclusively whether Diogenes was familiar with Babylon divination or not and whether he conveyed this information to a Rome public.

One further line of inquiry remains to be investigated. Diogenes of Babylon was not the only Babylon scholar with a Greek name whose writings were known to the Roman world; another prominent example was Seleucus of Babylon, a Babylonian astronomer who was famous for promoting a theory of a heliocentric universe, presumably following upon the work of his teacher Aristarchus; Seleucus was probably a contemporary of Diogenes (Russo 2004: 311ff.). Although Seleucus is not mentioned by Cicero, his work is of interest for another reason, as another possible example of Babylonian science being communicated to the West. In this particular case, Seleucus' primary contribution to arguments for a heliocentric universe was based on his studies of tidal movements in the Persian Gulf relating to full moon at the solstice and equinox (ibid. 313f.). One reference to Seleucus, from Aetius, tells us rather cryptically, 'Seleucus the mathematician (also one of those who think the earth moves) says that the moon's revolution counteracts the whirlpool motion of the earth' (ibid. 315). Although the statement is not very elucidating, it seems to suggest the connection between tides and the moon, which is a significant discovery. What is of interest to us, however, is that the association between the moon and tides was probably already recognised by earlier Babylonian astronomers, as recorded in Babylonian astronomical diaries.

[21] There is no evidence that Babylonians recognised any pulmonary functions of lungs, since breathing was accomodated in Babylonian medical texts by the nostrils and the throat (also *napištu*), despite the fact that lungs were associated with coughing. Nevertheless, the brief passage attributed to Diogenes could refer to symptoms occuring at birth, since the opposing signs of hot and cold are common in Babylonian diagnoses, e.g. 'if a baby's right nostril is cold and his left one is hot, (he suffers from) the Hand of Lamashtu(-demon) (Labat 1951: 224, 54).

According to a recent article on Babylonian astronomical diaries, Babylonian scholars regularly charted high tides in the Euphrates over a long period, from the mid-7th century onwards, and high tides were associated in the astronomical diaries with appearances of the new moon and full moon, as well as lunar and solar eclipses (de Meis 2011: 132 and 146f.). The implications of this data were not lost on ancient astrologers, who would have seen the effect of the moon on tides as a basic confirmation of celestial influence on terrestrial phenomena. Ptolemy's Tetrabiblos specifically mentions that the 'ebb and flow of the tide respond to the phases of the moon' (Tetrabiblos II 12), while the Roman astrologer Manilius sees the point quite clearly, giving the following proof for celestial influences on human life: 'the sky affects the fields, thus gives and takes away the various crops, puts the sea to movement, casting it on land and fetching it therefrom, and thus this restlessness possesses ocean, now caused by the shining of the moon, now provoked by her retreat to the other side of the sky ...' (Astronomica 2: 85–90). There is no hint, however, of either scholar having made the required calculations or gathered the empirical data, and it is likely that associating high tides with lunar phases originated in Babylonian scholarship. It is also possible that Seleucus' interests in tides stemmed from his Babylonian background or upbringing.

This leads to yet another matter of speculation, which may in fact exceed all other examples of highly circumstantial evidence proposed in the present paper. One wonders if Babylonian scholars known in the West by their Greek names (such as Diogenes or Seleucus of Babylon) might be known by another – Babylonian – name in late cuneiform sources. It is known, for instance, that some prominent citizens of Seleucid Babylonia had two names, one Babylonian and one Greek, which were not semantically related in any way. Two Babylonian officials with (coincidentally) the same name Anu-uballit also bore Greek names Kephalon and Nikarchos respectively (Oelsner 1986: 164), much in the same way as Alexandrian Jews had both Hebrew and Greek personal names (Tcherikover and Fuks 1957). There are several examples of Babylonian astronomers being so famous as to be cited in the West by their Babylonian names: Kidinnu (Kidinas), Nabû-rimanni (Nabourianos) and Šuma-iddina (Soudinos) are all known from colophons of Babylonian astronomical texts (Neugebauer 1956: I 16) and from Strabo (XVI i 6). We have no way of knowing how many other Babylonian astronomers may have been known in the West. However, other important scholars from Babylonia may have found recognition beyond Babylonia, such as the late 4th century BCE savant Tanittu-Bel from Babylon (Finkel 1991: 91)[22] or his contemporary from Uruk, Iqiša,[23] or a 3rd century BCE astronomer-priest Anu-Belšunu from Uruk (Steele 2011: 339). All of these Babylonian scholars are well-known from colophons of scientific tablets and their works could possibly have travelled to the West in translated versions under the guise of Greek names, perhaps through disciples;[24] unfortunately, even if such were the case, we would be unlikely to identify the Babylonian behind the Greek nomenclature.

Conclusive evidence to support the speculative propositions in the current paper is unlikely to appear any time soon. There is no smoking gun nor would any of this evidence stand up on court. Nevertheless, there is some room for wondering about closer links between Babylonian and Western science during the remarkable ecumene of the Roman world, which united so many different cultures under a single political regime. The fallacies which Cicero found in ancient systems of divination were probably not meant to refer only to local practices, either in Rome itself or in Etruria, but to pertain to divination in general, known in Rome under cosmopolitan conditions which were unprecedented in the ancient world. The writings and teachings of Diogenes of Babylon may have played a decisive role in introducing this brand of 'alien wisdom' to Rome and eventually to Cicero himself.

[22] According to colophons from some of the 260 fragments found by Finkel in the Babylon collection of the British Museum, Tanittu-Bel was a contemporary of Alexander the Great; the bulk of his surviving scholarly oeuvre consisted of incantations.
[23] He made copies of some 250 literary and scientific texts and authored many commentaries.
[24] Diogenes of Babylon had a disciple also from Babylonia, Apollodorus (officially from Seleucia-on-Tigris), but nothing is known about him.

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Modelling sundials: ancient and modern errors

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ABSTRACT. Three systems of celestial coordinates, the ecliptical, the equatorial and the horizontal, as well as their projection onto the shadow-receiving plane, define the geometry of sundials' construction. A new method is proposed to model planar sundials with arbitrarily oriented planes and shadow-casting parts based on a simple vector equation in combination with the application of a sequence of rotational matrices. This method allows one to draw the shadow maps for sundials with errors due to wrong determination of geographical latitudes or erroneous construction. Applications for modelling some ancient sundials are considered and their shadow maps are discussed.

Contents

1 Introduction

According to J. Needham,[1] three systems of celestial coordinates - the equatorial, the ecliptical and the horizontal - were used preferentially for surveying purposes by the Chinese, Greek and Arabic cultures respectively. This point of view now appears to be oversimplified (Hipparchus' system was, e.g., undoubtedly equatorial); what one can, however, say with certainty is that the spatial thinking of these civilizations was impacted by their respective preferred coordinate systems. In particular, however, the manufacturing of sundials, which has been observed in all three cultures, requires working knowledge and understanding of the interplay between all three systems:

- the equatorial system (because the visible daily motion of the Sun lies on a circle parallel to the equator),
- the ecliptical system (because the annual motion of the Sun lies on the ecliptic),
- the local horizontal system (because the horizon constrains the visible path of the Sun).

The orientation of the equatorial plane relative to the local horizontal plane is given by the angle $90° - \varphi$, where φ is the geographical latitude of the location. Thus, the correct determination of the latitude of the place where a sundial was to be installed was of crucial importance. Whereas the geographical latitudes

[1] "*Astronomy in ancient and medieval China*", Phil. Trans. R. Soc. Lond. A. **276**, 67–82, 1974.

of famous localities were known,[2] the locations of smaller cities had to be guessed or extrapolated on the basis of distances to the known cities. This was, in fact, the primary purpose of geographical mapping in antiquity.

The inclination of the ecliptic relative to the plane of the celestial equator, approximately 23.5°, was well known in antiquity. In the analemma construction of Vitruvius,[3] it was taken to be 24° for purely geometrical reasons – the central angle over the side of a regular 15-gon was easy to construct by compass and straightedge and therefore seen as a convenient and elegant approximation. The correct orientation of a sundial relative to the north-south direction as well as the calibration of the daily and hourly curves, however, demanded the usage of astronomical methods.

The local horizontal plane could be determined to a very high degree of precision with various technical tools available in antiquity and used primarily in architecture.[4] The direction perpendicular to the local horizon (that is, the local zenith direction) is even simpler to determine as the upward direction of the plumb line.

Finally, the shadow map of a sundial depends on the position of the shadow-receiving plane which should be specified relative to the coordinate systems involved. The mathematical tools would therefore be applied to the engineering solution based on the information provided by geographical and astronomical data. Because the visible daily motion of the Sun also determines the position of the hour lines on the shadow receiving plane, we have chosen the equatorial plane as the primary reference plane for constructing a mathematical model for the different kinds of sundials. The position of the Sun in the modern equatorial coordinate system is given by the declination δ relative to the equatorial plane and the hour angle h measured towards the west from the meridian transition (that is, culmination) of the Sun (see Fig. 1).

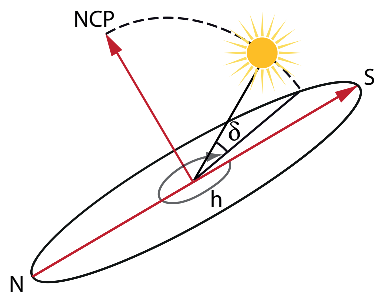

Figure 1: Coordinates in equatorial system: declination δ and hour angle h.

The method will be applied to two different sundials: the equatorial sundial of Amphiareion and the old Egyptian shadow-casting instrument.

2 Mathematical solution

Let us consider a standard equatorial sundial - that is, one with the shadow-receiving plane lying parallel to the celestial equator and the shadow-casting part (gnomon) parallel to the rotational axis of the Earth. The coordinate system used in the mathematical solution which will be proposed in the text is illustrated in Fig. 2.

[2]The geographical latitudes were determined (and unambiguously defined in this way) as the ratio of the length of a gnomon to that of its shadow at equinox, or as the ratio between the length of the longest day of the year and the shortest day. To find out the geographical longitudes, on the other hand, one requires either simultaneous astronomical observations of eclipses at different locations or knowledge of the circumference of the Earth together with the directions and distances between the localities.

[3]*On Architecture*, IX, 7.

[4]See, e.g., Lewis M. J. T., *Surveying Instruments of Greece and Rome*, Cambridge University Press, 2001.

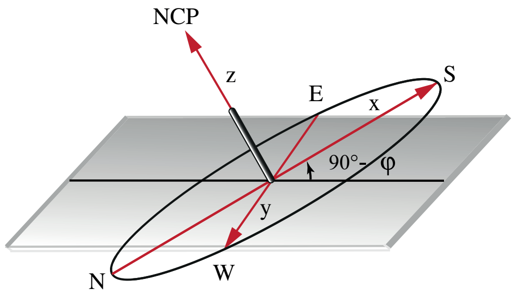

Figure 2: Orientation of an equatorial sundial relative to the horizontal plane. The gnomon is directed along the rotational axis of the Earth towards the north celestial pole (NCP). The geographical latitude of the location is φ.

We will now introduce a *left-handed* Cartesian coordinate system with the x-axis directed towards the south, the y-axis towards the west and the z-axis directed towards the northern celestial pole. Let us furthermore introduce a unit vector in the gnomon's direction,

$$\mathbf{e}_g = \begin{pmatrix} 0 \\ 0 \\ 1 \end{pmatrix},$$

and a unit vector in the Sun's direction defined in the standard way in terms of the Sun's equatorial coordinates as

$$\mathbf{e}_s = \begin{pmatrix} \cos\delta\cos h \\ \cos\delta\sin h \\ \sin\delta \end{pmatrix}.$$

While the orientation of the gnomon vector remains constant, the vector in the Sun's direction will, naturally, change its orientation with time. The equation for a sunbeam that, coming from the Sun, goes through the gnomon's tip can be written in vector form as

$$\mathbf{x}(\lambda; \delta, h) = \lambda\mathbf{e}_s + \mathbf{e}_g,$$

where $\lambda \in \mathbb{R}$ is a numerical parameter. The geometry of the problem is shown in Fig. 3. A sunbeam hits

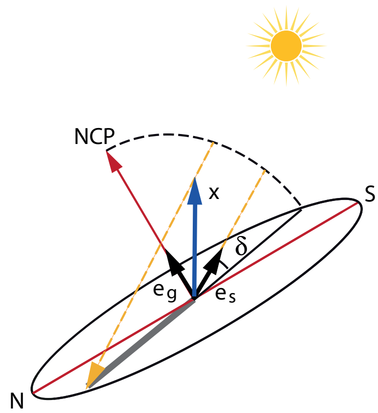

Figure 3: Coordinate system used to obtain a vector solution for a shadow model.

the equator plane $z = 0$ when the z-component of the vector $\mathbf{x}$ attains zero:

$$\lambda z_s + z_g = 0.$$

With $z_s = \sin\delta$ and $z_g = 1$, one obtains the corresponding value for λ :

$$\lambda^* = -\frac{1}{\sin\delta}.$$

This value for λ allows us to calculate the x- and y-components of the end of the shadow in the equatorial plane as

$$x(\lambda^*) = \lambda^* \cos\delta\cos h = -\cot\delta\cos h,$$
$$y(\lambda^*) = \lambda^* \cos\delta\sin h = -\cot\delta\sin h.$$

Combination of these two formulae gives the shadow's equation

$$x^2 + y^2 = \cot^2\delta,$$

which describes a circle with radius $R = \cot\delta$ around the point $x = 0$, $y = 0$. The visualization[5] of this well-known result for an equatorial sundial placed at latitude 40° is shown in Fig. 4. At our latitudes, the

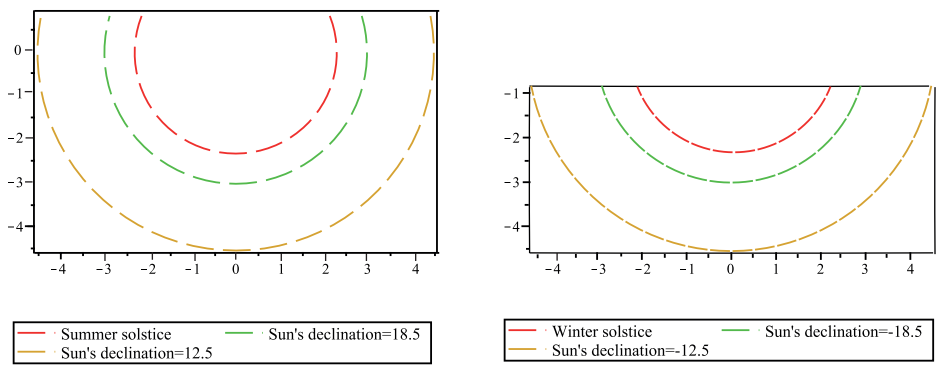

Figure 4: Numerical results for an equatorial sundial mounted at latitude 40°. Left: summer side. Right: winter side.

part of an equatorial sundial oriented towards north (the summer side) exhibits circular shadow paths with lengths larger than corresponding semicircles, whereas the winter side (lying opposite) exhibits shadow paths with lengths shorter than corresponding semicircles. This observation can be easily understood with the help of Fig. 5. The method described above can be easily adopted for planar sundials inclined arbitrarily relative to the equatorial plane. A horizontal sundial, for instance, can be obtained with a simple rotation of an equatorial sundial around the y-axis by an angle $\theta = -(90° - \varphi)$ (Fig. 6). This rotation can be realized with the help of a rotational matrix $\mathbf{R}_y(\theta)$, given, in our case, by

$$\mathbf{R}_y(\theta) = \begin{pmatrix} \cos\theta & 0 & \sin\theta \\ 0 & 1 & 0 \\ -\sin\theta & 0 & \cos\theta \end{pmatrix}.$$

Assuming that the gnomon is kept at a right angle to the shadow-receiving plane (normally, that will be the case because right angles are the most simple to realize), the unit vector in the gnomon's direction in the new coordinate system is given again by

$$\mathbf{e}_g^1 = \begin{pmatrix} 0 \\ 0 \\ 1 \end{pmatrix},$$

[5] All the shadow maps in the text are normalized relative to the height of a gnomon adopted as unity. The 0-point lies at the basis of the gnomon. The computations are made with the help of the computer algebra system MAPLE 12; the programs can be made available on request.

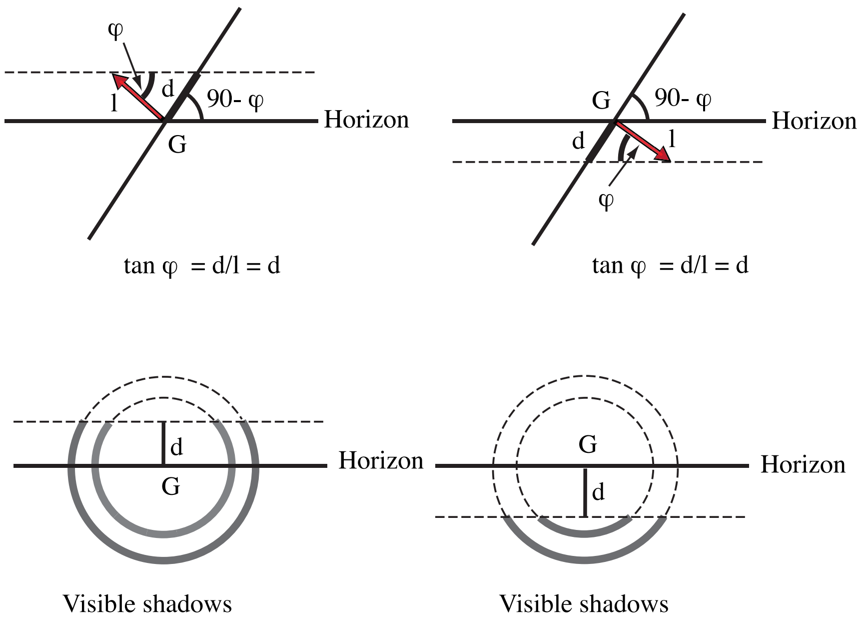

Figure 5: Visible shadow paths for an equatorial sundial mounted at latitude φ on the northern hemisphere. Left: summer side. Right: winter side. The distance $d = \tan\varphi$ gives the displacement between the line which bounds the visible shadow paths and the point G which lies at the basis of a gnomon with length $l = 1$. The daily shadow curves are segments of circles with radius $R = \cot\delta$ around the basis of the gnomon. The Sun's declination δ is assumed to remain constant over the course of a day.

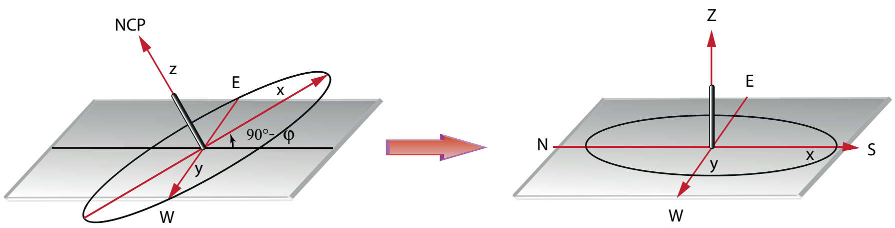

Figure 6: Transformation from an equatorial to a horizontal coordinate system by a rotation about the y-axis by an angle of $-(90° - \varphi)$.

and the unit vector in the Sun's direction will be transformed as

$$\mathbf{e}_s^1 = \mathbf{R}_y \mathbf{e}_s.$$

To draw a shadow map in the horizontal plane, one can now apply the procedure discussed above for the vector equation

$$\mathbf{x}^1(\lambda; \delta, h) = \lambda \mathbf{e}_s^1 + \mathbf{e}_g^1.$$

Vertical sundials can be modeled with a rotation of a horizontal sundial

a) about the x-axis by an angle of 90° for west-side receiving surfaces,

b) about the x-axis by an angle of −90° for east-side receiving surfaces,

c) about the y-axis by an angle of 90° for north-side receiving surfaces,

d) about the y-axis by an angle of −90° for south-side receiving surfaces,

where the rotation about the x-axis by an angle of θ, for instance, is given in a standard way with the help of a rotational matrix $\mathbf{R}_x(\theta)$:

$$\mathbf{R}_x(\theta) = \begin{pmatrix} 1 & 0 & 0 \\ 0 & \cos\theta & -\sin\theta \\ 0 & \sin\theta & \cos\theta \end{pmatrix}.$$

With an appropriate sequence of standard rotational matrices, one can model every orientation of sundials' shadow-receiving planes as well as every orientation of gnomons. As an example, a shadow map

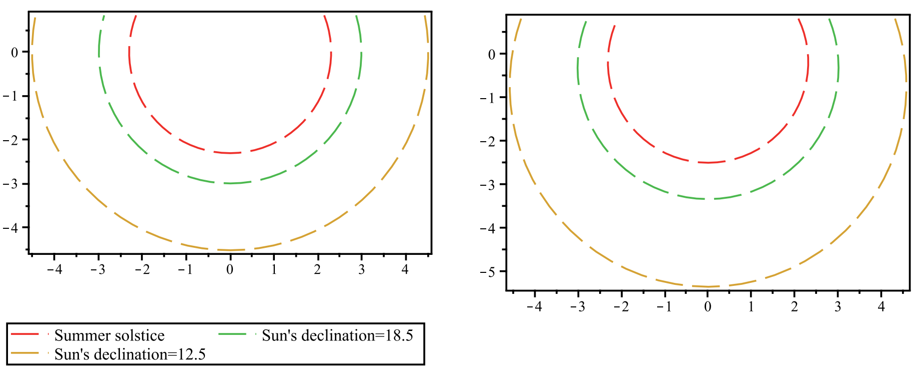

Figure 7: Numerical results for an equatorial sundial at latitude 40° (summer side). Left: equatorial sundial mounted correctly. Right: sundial mounted with an error of 2°.

for an equatorial sundial mounted with an orientation error of 2° and a gnomon perpendicular to the shadow-receiving plane has been calculated with the algorithm discussed above. The results are given in Fig. 7. One can easily see that even such a small orientational error, caused by a wrong determination of geographical latitude or by a maladjustment during construction, would produce an obvious visible consequence: the shadow map would be stretched along the north-south axis. In fact, this allows one to easily determine the geographical position of a surveyor: one should arrange a simple equatorial sundial in such a way that the path of the gnomon shadow traces a circle arc around its basis over the course of a day. The inclination of the shadow-receiving plane must then necessarily be $90° - \varphi$. Exploiting this circumstance, one can easily approximate φ to a very high precision over the course of a few days at an arbitrary time of the year – avoiding a notable restriction on the methods employed widely in antiquity, based on a measurement of the length of the gnomon's shadow at equinox or the duration of daylight at summer and winter solstice.

So far, we have only discussed the daily curves of sundials. To draw the equinoctial hour lines, one should first calculate the x and y coordinates of the shadow of the gnomon's tip at 1-hour increments of the Sun's hour angle h and then proceed to connect these points in the shadow receiving plane. Because the Sun can, at this level of precision, be assumed to complete one full rotation (360 degrees) around the northern celestial pole in 24 hours and its daily path can be approximated as a circle parallel to the equatorial plane, the same result may be obtained by dividing the full circle around the basis of a gnomon into sections equal to 15° (360 /24). The result of such a calculation is shown in Fig. 8.

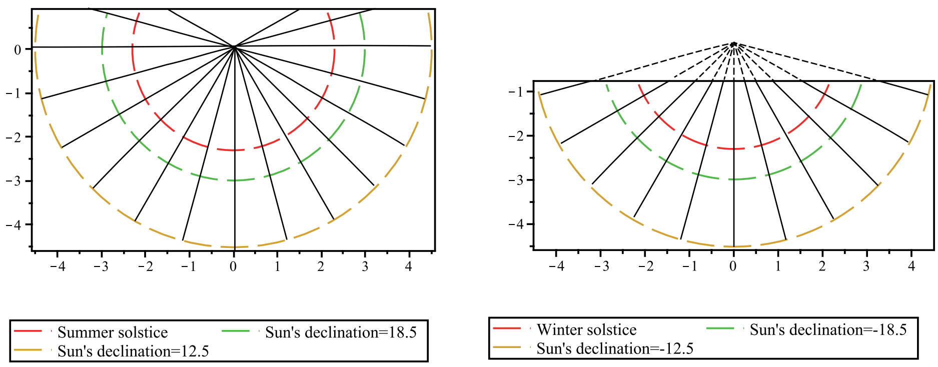

Figure 8: Shadow maps for an equatorial sundial mounted at latitude 40° supplemented with equinoctial hour lines. Left: summer side. Right: winter side.

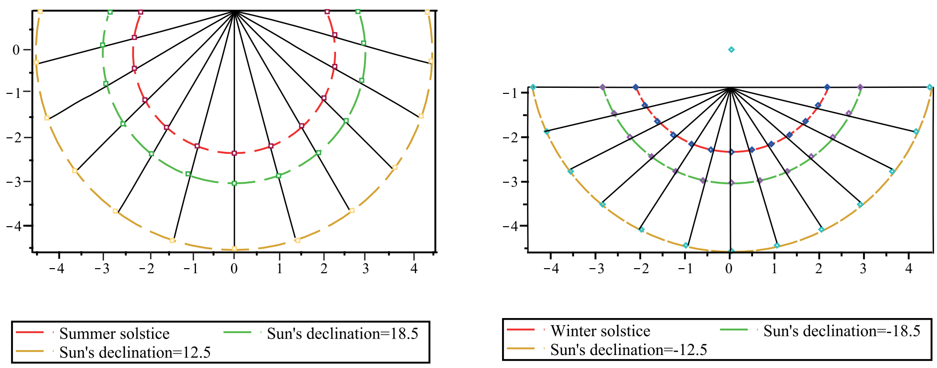

Figure 9: Shadow maps for an equatorial sundial mounted at latitude 40° supplemented with lines approximating the seasonal hour curves. Left: summer side. Right: winter side.

The seasonal hour lines can be calculated by dividing the daylight duration into twelve parts (for equinox, this will yield the equinoctial hour lines again), calculating of the x and y coordinates of the shadow at appropriate time increments and finally interpolating the thus obtained points with appropriate curves. The results are shown in Fig. 9. One can see that the seasonal hour curves connecting the points on the daily curves calculated for the same seasonal hours do, practically, not deviate from the straight lines[6] which converge towards a point lying on the line which bounds the shadow paths.

In conclusion, the construction of equatorial sundials is based on a mathematical model which is simple from an astronomical perspective and their usage provides many advantages in comparison with the more complicated types, that is, spherical, conical, horizontal and vertical sundials. Equatorial sundials are not only simple to construct, they can also easily be marked with daily curves representing the seasonal as well as the equinoctial hours. Last but not least, errors in the orientation can be easily detected by observing deviations from the expected circular shape of the daily shadow traces. This also allows one to use an equatorial sundial to find out the latitude of the location of observation by measuring the angle $(90° - \varphi)$ between the shadow receiving part and the horizontal plane.

The sole disadvantage that equatorial sundials suffer from is their unsuitability for calendar-keeping purposes: it is not possible to detect the date of equinox. Additional instruments could have been applied together with equatorial dials for this purpose.

3 The sundial of Amphiareion

It is a strange historical fact that equatorial sundials were underrepresented in ancient Greece whereas this type was prevalent in old China. Equatorial sundials are not even mentioned in the Vitruvius' famous list of dials and their inventors[7] unless we identify the arachne (whose discovery was attributed to Eudoxus) as a kind of equatorial sundial on purely optical grounds. The only preserved specimen is also one of the oldest known Greek sundials. It is thoroughly discussed by K. Schaldach,[8] and has been dated to 350–320 B.C. The sundial has graticules on both sides of the plate consisting of semicircles divided into equal sections with a network of hour lines (Fig. 10). On one side, the hour lines run up

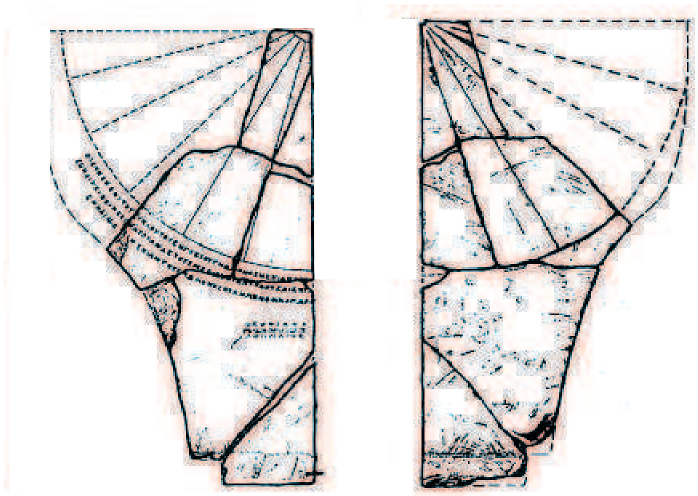

Figure 10: The sundial of Amphiareion. Material: marble. Dating: 350–320 B.C. Left: winter side; right: summer side (courtesy of K. Schaldach).

to the gnomon groove, while on the winter side, they end at a small concentric circle below it. Being mostly in accordance with the reconstruction of Schaldach, we will show how to apply our method to calculate the shadow maps for this exceptional sundial with a nonstandard positioning of the gnomon and a known orientational error. In fact, at least on the side identified as a winter side, the gnomon should

[6] At least, for the latitude of Greece.

[7] *Architecture* IX, viii, 8.

[8] "*The Arachne of the Amphiareion and the Origin of Gnomonics in Greece*", JHA, XXXV, 2004.

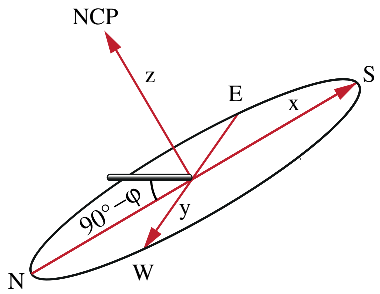

Figure 11: Orientation of the horizontal gnomon relative to the equatorial plane for an Amphiareion-type sundial.

not lie perpendicular to the shadow-receiving surface but in the horizontal plane. A schematic outline of an equatorial sundial with a horizontal gnomon is given in Fig. 11. The unit vector in the gnomon's direction for such a dial is given by

$$\mathbf{e}_g = \begin{pmatrix} -\sin\varphi \\ 0 \\ \cos\varphi \end{pmatrix}.$$

The unit vector in the Sun's direction is of the same form as for a standard equatorial dial:

$$\mathbf{e}_s = \begin{pmatrix} \cos\delta\cos h \\ \cos\delta\sin h \\ \sin\delta \end{pmatrix}.$$

The equation for a sunbeam going through the tip of the gnomon again has the form

$$\mathbf{x}(\lambda;\delta,h) = \lambda\mathbf{e}_s + \mathbf{e}_g, \qquad \lambda \in \mathbb{R}.$$

A sunbeam meets the equatorial plane ($z = 0$) when

$$\lambda^* z_s + z_g = 0$$

which can be solved for the parameter λ^* to yield

$$\lambda^* = -\frac{\cos\varphi}{\sin\delta}.$$

With this value for λ, the x- and y-components of the end of the shadow in the equatorial plane are determined by

$$x(\lambda^*) = \lambda^* \cos\delta\cos h + \sin\varphi = -\cot\delta\cos\varphi\cos h - \sin\varphi,$$

$$y(\lambda^*) = \lambda^* \cos\delta\sin h = -\cot\delta\cos\varphi\sin h.$$

Combination of these formulae gives the shadow equation

$$(x + \sin\varphi)^2 + y^2 = \cot^2\delta\cos^2\varphi$$

which describes a circle with radius $R = \cot\delta\cos\varphi$ around the point $x = -\sin\varphi$, $y = 0$ in the equatorial plane. The same method applied to the winter side of the sundial shows that the shadow circles will be centered around the point $x = \sin\varphi$, $y = 0$. An illustration of this result is given in Fig. 12.

The daily curves for an Amphiareion-type sundial calculated with our algorithm for the latitude of the specimen are given in Fig. 13. One can see that the centers of the concentric circles representing the daily shadow paths do not coincide with the position of the groove of the gnomon adopted as the zero-point of the diagram, but are displaced along the north-south line. This displacement is $-\sin\varphi$ for

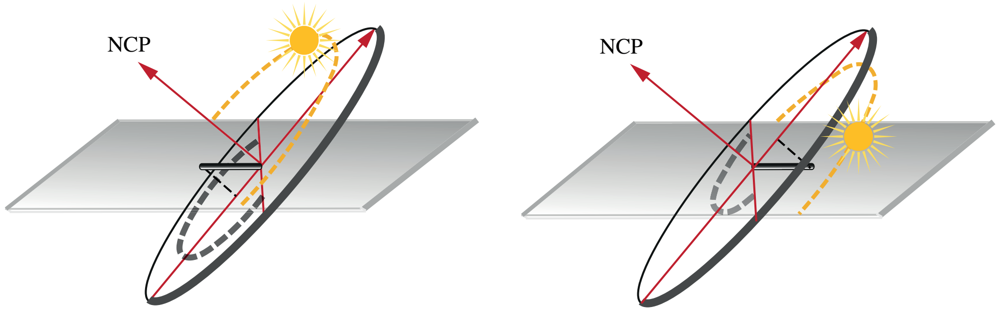

Figure 12: Equatorial sundial with a horizontal gnomon. Due to the Sun spending a part of the day below the horizon, one can see only a part of the shadow circle daily. Left: summer side. Right: winter side.

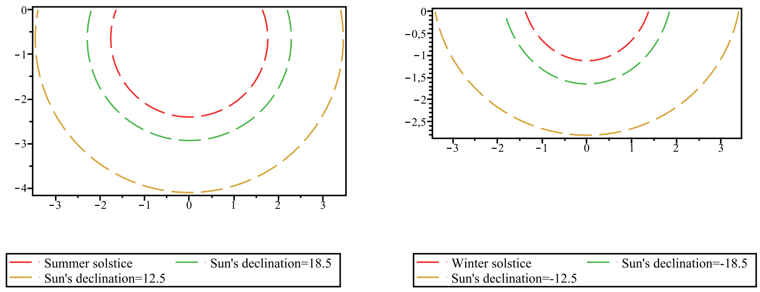

Figure 13: Numerical results for an Amphiareion-type sundial for selected days of the year.

the summer side and $+\sin\varphi$ for the winter side of the shadow-receiving surface. The geometry of this setup is illustrated in Fig. 14. The comparison of Figs. 5 and 14 reveals a possibility to distinguish the sundials with a polar from those with a horizontal gnomon orientation. First, the displacement of the line which bounds the daily shadow circles relative to the center of these circles is different ($\tan\varphi$ vs. $\sin\varphi$). Second, the radii of the daily curves are different for the same dates ($\cot\delta$ vs. $\cot\delta\cos\varphi$). In fact, the Amphiareion sundial does not have any marked daily curves on the summer side. The inscription on the winter side identifies the small circle by the gnomon's groove as the circle for winter solstice. It also claims that the biggest semicircle marks the equinoctial line. Let us recall that in standard equatorial sundials, the equinoctial lines cannot be implemented on the shadow-receiving surface at all. The Amphiareion sundial is constructed in such a way that the thickness of the plate varies from 24 mm at the upper edge to 53 mm at the lower edge. Provided that such thickening proceeds with a linear progression, we can model this thickening with an appropriate inclination of the receiving plane. Our calculation has shown that the equinoctial circle would, in fact, lie far beyond the edge of the sundial, so that one would need a perpendicular border on the sundial's edge to capture the equinoctial shadow.

Both sides of the Amphiareion sundial are supplemented with hour lines. The shadow maps calculated according to our scheme show the position of equinoctial hour lines vs. the lines approximating the seasonal hour curves in Fig. 15 for the summer side and in Fig. 16 for the winter side. It is this kind of shadow hour lines (see Fig. 10) which forced K. Schaldach to conclude that the summer side of the Amphiareion sundial is engraved with seasonal hours (the hour lines converge towards the groove of the gnomon) and the winter side – with equinoctial hours (the convergence point lies outside the groove). Another explanation is also possible. Observe that not even the shadow line as important as the one for summer solstice is marked on the summer side. One can imagine that the sundial was initially designed

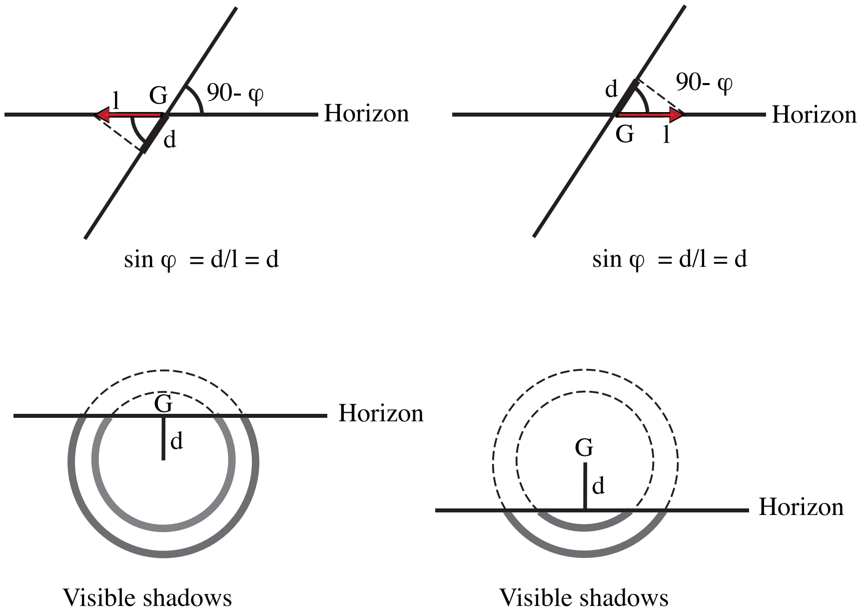

Figure 14: Visible shadow lines for an Amphiareion-type sundial mounted at latitude φ. Left: summer side. Right: winter side. The distance $d = \sin\varphi$ gives the displacement between the line which bounds visible shadow paths and the point G which lies at the basis of a gnomon with length $l = 1$ mounted horizontally. The daily shadow curves are segments of circles with radius $R = \cot\delta\cos\varphi$ around the projection of the tip of the gnomon onto the equatorial plane. The Sun's declination δ is assumed to remain constant over the course of a day.

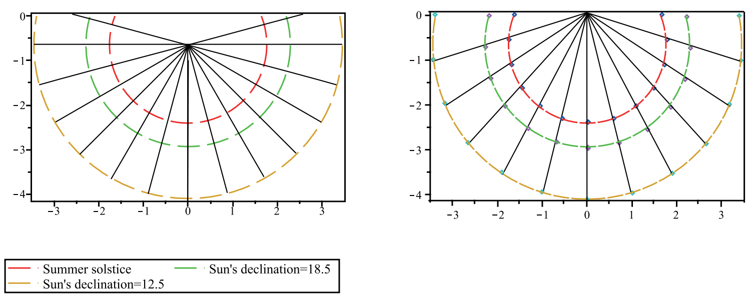

Figure 15: Shadow maps for an Amphiareion-type sundial (summer side) supplemented with equinoctial hour lines (left) and lines which approximate the seasonal hour curves (right). The points on the right picture represent the positions of the end of the gnomon's shadow with an interval of one seasonal hour.

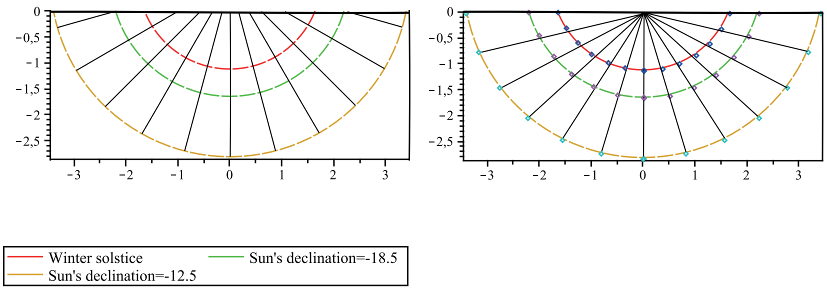

Figure 16: Shadow maps for an Amphiareion-type sundial (winter side) supplemented with equinoctial hour lines (left) and lines approximating the seasonal hour curves (right).

for usage with a gnomon directed towards the north celestial pole, as is the rule; this would require a receiving plane wider than a semicircle on the summer side. The Amphiareion sundial is made from a very thin marble plate; at a thickness of only 5 cm, it would have been very difficult to produce with methods available at that time. It may be the case that a part of the plate broke off during the work process and the artisan decided to at least salvage the winter side. With a horizontally positioned gnomon, a semicircle would suffice. Having thus been rendered unusable, the summer side would have been left unfinished.

To sum up, provided that the gnomon was aligned horizontally on both sides of Amphiareion's sundial, the shadow maps show that the winter side was marked in equinoctial, but the summer side was marked in seasonal hours. The big semicircle claimed on the winter side to mark the equinoctial line could not, in fact, represent the daily curve of that day. Let us also observe, that provided that the latitude of the place where the find was made coincides with the latitude where the sundial was mounted, the measured error of about 2° in the position of the equatorial plane of the Amphiareion sundial would produce an obvious deviation of the shadow maps from those of a correctly mounted sundial (see Figs. 17–18).

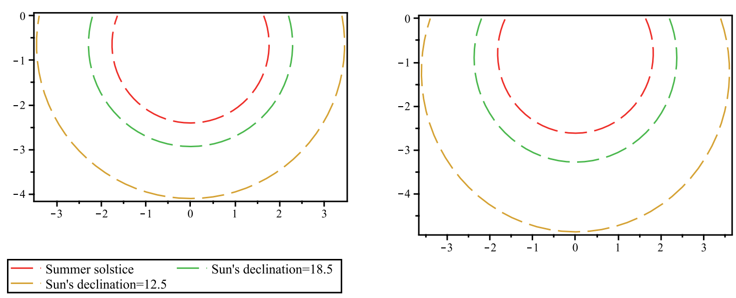

Figure 17: Shadow maps for an Amphiareion-type sundial constructed for the correct latitude (left) and for a latitude determined with an error of 2° error (right): summer side.

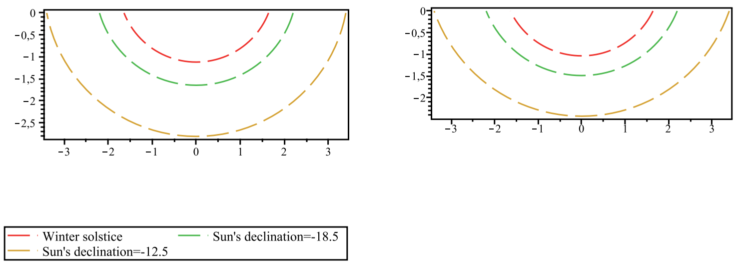

Figure 18: Shadow maps for an Amphiareion-type sundial constructed for the correct latitude (left) and for a latitude determined with an error of 2° error (right): winter side.

4 The Egyptian shadow-clock

A very different type of sundial – the ancient Egyptian shadow-clock – was based on the lengths rather than on the directions of the shadows (see Fig. 19). Such devices were first discussed by Borchardt[9] and are still preserved in the Egyptian Museum of Berlin (Inv. No. 19743, Inv. No. 19744). Along

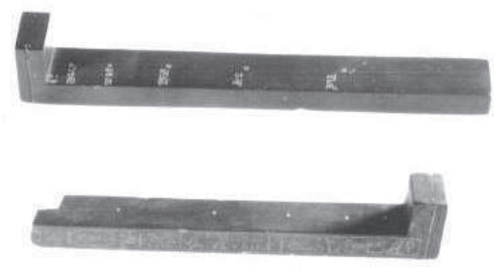

Figure 19: Old egyptian shadow-clocks. Material: Green slate. Bought by L. Borchardt from the merchant M. Nahman in Cairo. Top: Berl. Mus. Inv. No. 19743, 1000 - 600 B.C . Possible place of discovery : Fayum. Down: Inv. No 19744, 1501-1447 B.C. (Tuthmosis III).

the shadow-receiving ruler some markers are engraved which indicate, according to the inscriptions, the first, second, fourth and fifth hours after sunrise. The shadow-casting piece of the instrument's body, a vertical head affixed at one end, itself serves as an indicator for the sixth hour (noon). The hour lines are obviously marked according to a simple arithmetic scheme. Let us denote the distance between the rim of the shadow-producing part and the marker of the fifth hour with a (according to Borchardt, $a = 2/3$ Egyptian finger). Then the distance between the fifth hour-marker and the fourth marker is $2a$, between the fourth marker and the third marker $3a$, between the third marker and the second one $4a$, between

[9] Borchardt, L. (1911). *Altägypt. Sonnenuhren*, ZÄSA, **48**; Borchardt, L. (1920). *Die Altägypt. Zeitmessung*, Bd. I, Die Geschichte der Zeitmessung und der Uhren, ed. E. v. Bassermann-Jordan, Berlin&Leipzig.

the second marker and the first one $5a$ (see Fig. 20). The height of the end piece relative to the ruler is quoted in the literature as about $2a$. To estimate the precision which can be achieved with such a simple

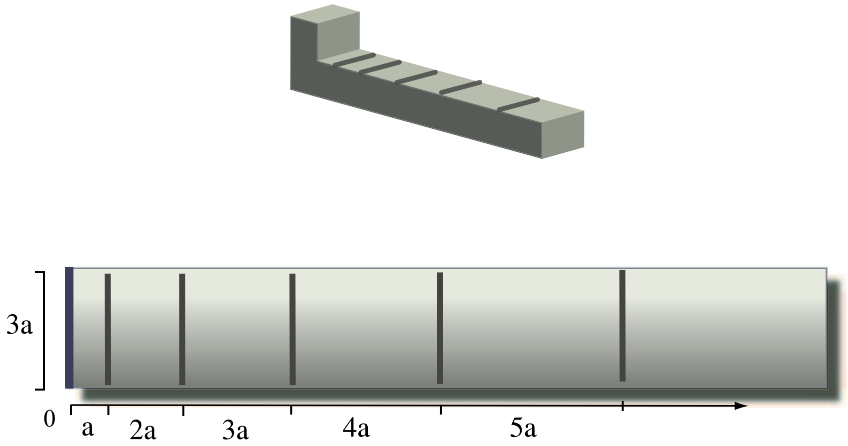

Figure 20: Schematic outline of the shadow-casting device No. 19743 with a top view marked with hour lines ($a = 2/3$ Egyptian finger).

scheme,[10] we should first discuss how the device might have been aligned. According to the interpretation of Borchardt, the instrument should be oriented at sunrise towards the east with the shadow-receiving plane along the east-west line. The hours could then be read off with no adjustments to the orientation until noon; afterwards, the device would be turned in the opposite direction to read the hours from noon until sunset.[11] However, no discussion of such a mode of operation is found in the preserved inscriptions.

The method for calculating the shadow maps discussed in the previous sections was designed for a single gnomon. To apply this method for the Egyptian sundial, we proceed as follows. Assume the sundial has been aligned in a determined direction at a latitude of 29° (the latitude of Fayum, also adopted in Borchardt's calculations). Plot the trajectory the shadows of each corner of the end piece traverse from sunrise until noon and connect the two points corresponding to each hour (seasonal or equinoctial) with a line (Fig. 21). We have first calculated the shadow lines for the case of the ruler having been aligned along the east-west direction – this is the favored theory regarding the instrument's orientation. The results are given in Figs. 22–24 for summer solstice[12] and equinox respectively. Throughout the graphs, the calculated shadow lines are drawn with dashed lines and marked with Arabic numerals; the marking lines preserved on the sundial are drawn with straight lines and marked with Roman numerals. We neglect the corrections for atmospherical refraction and the sun's visible diameter, as Borchardt also did, in order to compare our results with his graphs.[13] One can see that the shadow lines at solstices diverge away from the ruler and cannot match all the engraved hour markers. At equinox, all the shadow lines stay on the ruler but lie closer to the shadow-producing part than the markers on the instrument. These two observations were, in fact, the reason for Borchardt to suggest that a crossbar was mounted above the shadow-producing part in order to ensure that the shadows fall on the ruler not only at equinox but also at other dates and to move the shadow map closer towards the markers. Borchardt writes:[14]

> Durch diese Überlegungen kommt man dazu, anzunehmen, daß oben auf dem Aufsatzzapfen

[10] The positions of the markers match these simple arithmetical scheme with a precision of a couple of millimeters. The height of the shadow producing part relative to the shadow-receiving plane is, in fact, about $2.5a$ (see Rau, H., *Berliner Instrumente der altägyptischen Tageszeitbestimmung*, http://www.regiomontanus.at/berlin-egypt.htm). Because this parameter is very important for construction of shadow maps, this value was also used in our calculations.

[11] The orientation towards the sunrise point of date would also be possible.

[12] The obliquity of the ecliptic for the epoch of question was about 23.87°.

[13] It seems, however, to be more natural to count hours from the moment of the first appearance of the sun's rays: that would change the zenith distance of the sun to 90.85° instead of the adopted value of 90°.

[14] Borchardt, L. (1911). *Altägypt. Sonnenuhren*, ZÄSA, **48**.

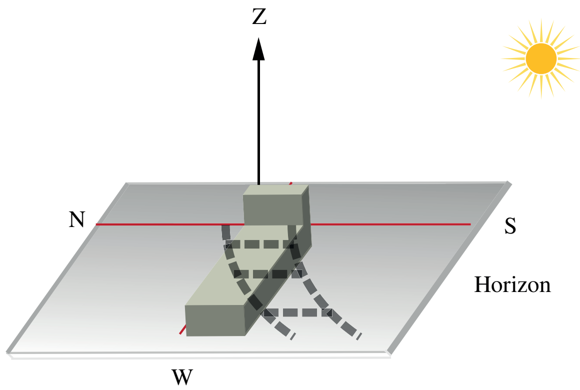

Figure 21: The outline of the method applied for constructing shadow maps for old Egyptian shadow-clocks.

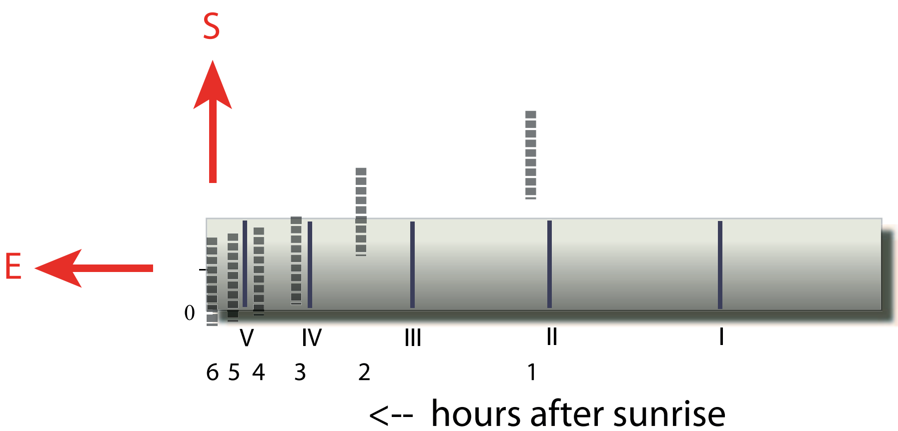

Figure 22: Shadow map at summer solstice for latitude 29°. The shadows are calculated at intervals of one seasonal hour.

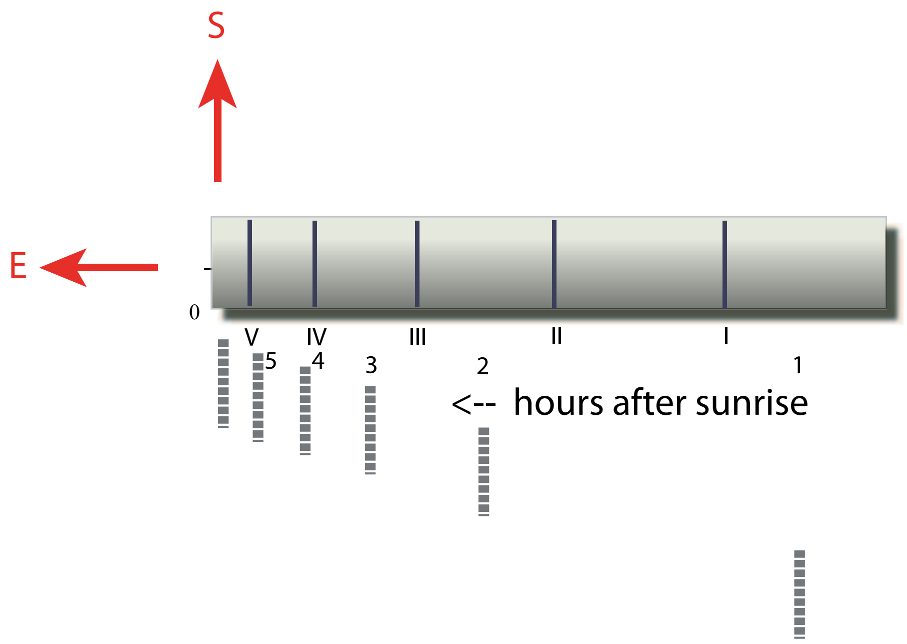

Figure 23: Shadow map at winter solstice for latitude 29°. The shadows are calculated at intervals of one seasonal hour.

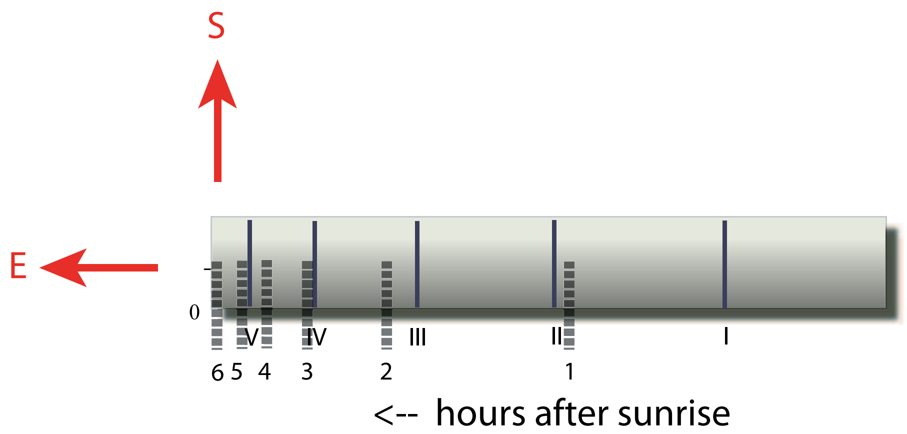

Figure 24: Shadow map at equinox for latitude 29°. The shadows are calculated at intervals of one equinoctial hour.

unseres Instruments ein langes Lineal horizontal in der Meridianebene gelegen haben muß... Sucht man nun unter den altägyptischen Gebrauchsgegenständen nach einem, der dieser so gefundenen Vorstellung entspricht, so drängen sich uns ohne weiters die bekannten Votivellen auf, die meist auch aus metamorphischem Schiefer bestehen, wie unsere Instrumente.

As suggested in Borchardt's proposal, a crossbar of roughly the same height as the end piece of the device was adopted as a standard interpretation (Fig. refQuerbalke). It solved the problem of the diverging shadows at solstices but could not improve the precision of the instrument.[15] The shadow map for

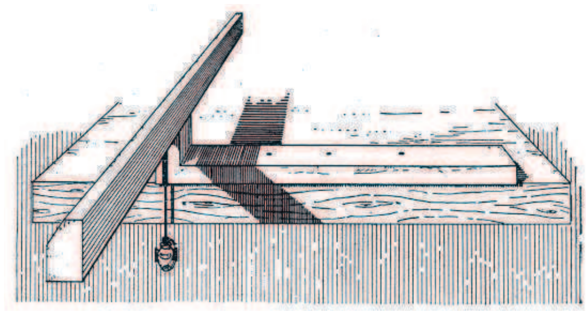

Figure 25: The usage of a crossbar on the old Egyptian shadow-casting instrument according to Borchardt. From Borchardt, L. (1911), *Altägypt. Sonnenuhren*, ZÄSA, **48**.

equinox calculated by our method with a crossbar which is 35mm thick, as proposed by Borchardt, is given in Fig. 26. Such a crossbar matches the hour markers from the second hour after sunrise until noon with a moderate error but absolutely fails to match the first hour marker after sunrise. One can only

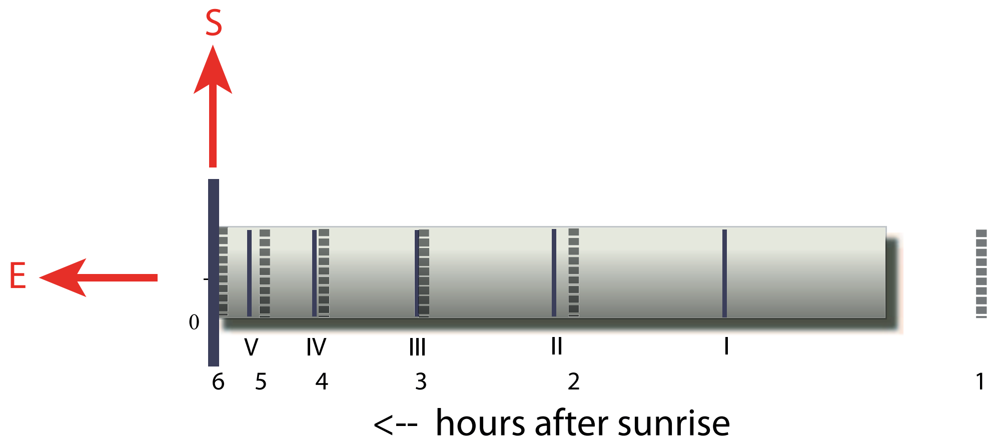

Figure 26: Equinoctial shadow map for the Egyptian shadow-clock with a 35mm thick crossbar.

wonder why the people of Old Egypt had not noticed such a serious flaw in the marking. The problem might lie, in fact, in the adopted height of the crossbar. According to our calculations, one can obtain much better corresponding with a thinner crossbar. The shadow map for equinox with a 12mm thin crossbar (which is exactly equal to a) is given in Fig. 27. One can see that with this choice, the first hour after sunrise and the last hour before noon are matched up perfectly. The simplistic arithmetic scheme inside this interval renders the time-telling inaccurate. The length of a crossbar could easily be chosen by trial and error in such a way that the shadows fall onto the ruler not only at equinox but also at solstices.

[15]Due to Bruins (Bruins, E. M., "*The Egyptian shadow clock*", Janus, 1965, **52**, pp. 123-137) a set of three crossbars of different heights would serve as a good approxumation to mark seasonal hours.

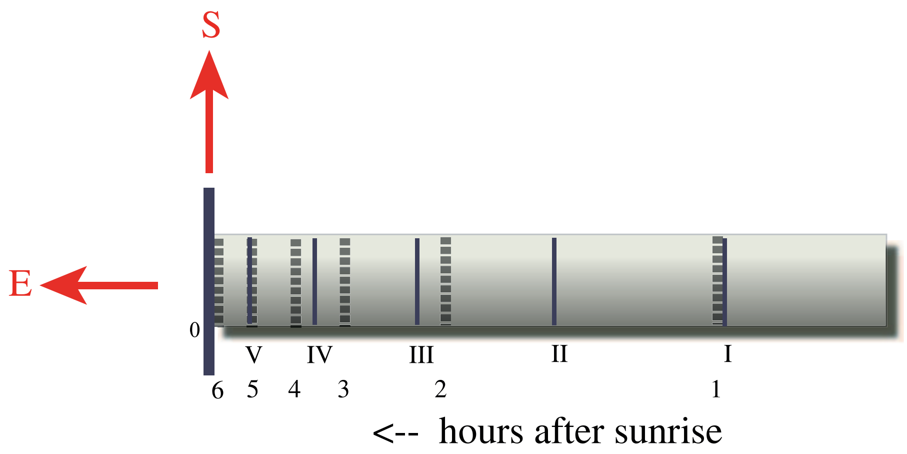

Figure 27: Equinoctial shadow map for the Egyptian shadow-clock with a 12mm thick crossbar.

One alternative orientation of the device[16] is also widely discussed in the literature: one can permanently aim the instrument towards the Sun and read the hour markers (Fig. 28). In this case one does not need any crossbar: the shadows will always fall onto the shadow-receiving ruler. The position of a

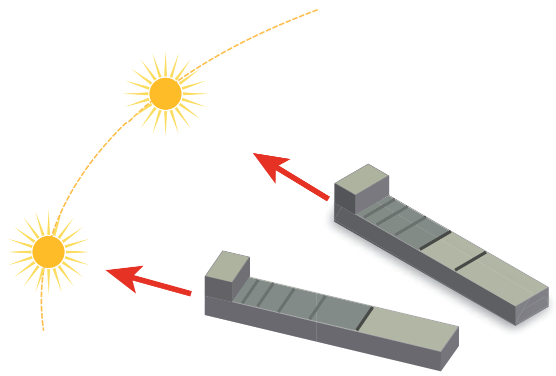

Figure 28: Egyptian shadow-clock permanently aimed at the Sun. The shadows always fall onto the shadow-receiving plane.

shadow line in this case is simple to calculate through the relation

$$\tan e = c/b\,,$$

where c is the height of the shadow-producing part, b is the length of the shadow and e is the height of the Sun above the horizon (elevation). Given the Sun's declination δ, latitude of the surveyor φ and the hour angle h of the sun, the height above the horizon e can be found from the so-called nautical triangle through the relation

$$\cos(\pi/2 - e) = \sin\delta\cos\varphi + \cos\delta\sin\varphi\cos(h).$$

The geometry of the problem is illustrated in Figs. 29–30. The shadow maps for summer solstice and at equinox calculated according to this scheme are given in Figs. 31–32. One can see that, although such an orientational scheme does not require any additional crossbar and the shadows remain on the

[16]See, for instance, Symons, S., "*Ancient Egyptian Astronomy: Timekeeping and Cosmography in the New Kingdom*", 1999, Ph. D. Thesis , Dept. of Math. and Comp. Science, University of Leicester. The same idea is also supported by Mills, A., "*Ancient Egyptian Sundials*", Bull. of the British Sundial Society 23 (iii), 2011, pp. 16–19.

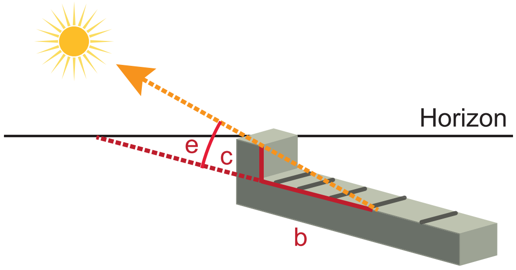

Figure 29: The instrument is permanently aimed at the sun. The height of the shadow-producing part is c, the length of the shadow is b; the height of the Sun above the horizon of an observer is e.

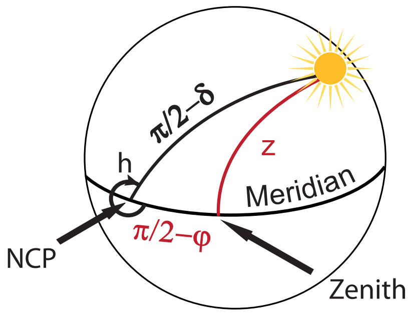

Figure 30: Nautical triangle. NCP stands for the North Celestial Pole, h is the hour angle of the Sun, φ is the latitude of an observer, δ is the Sun's inclination. The zenith distance z is related to the elevation of the Sun above the horizon by $e = \pi/2 - z$.

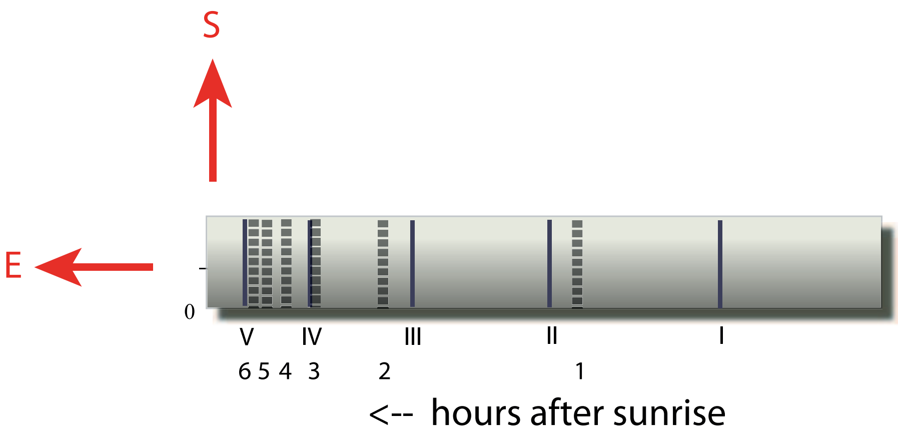

Figure 31: Shadow map at equinox for latitude 29°. The instrument is permanently aimed at the Sun. The shadows are calculated at intervals of one equinoctial hour.

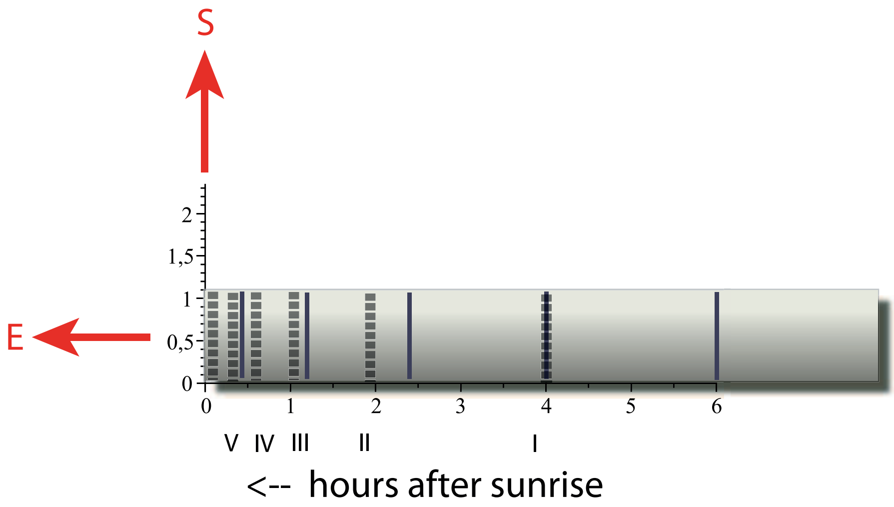

Figure 32: Shadow map at summer solstice for latitude 29°. The instrument is permanently aimed at the Sun. The shadows are calculated at intervals of one seasonal hour.

ruler, positioning of the hour markers precludes the instrument's usage as an accurate time-telling device. Moreover, in this case, the noon shadow does not lie along the edge of the end piece and, in fact, requires a special new marking line for the sixth hour after sunrise which is absent on the preserved specimens.

A very different mode of application would be to use the shadow-casting instruments as **time-marking** devices. For the arrangements of social life, it was not, in fact, necessary to know what time it really is (in the astronomical sense of the word). Rather, it was sufficient to simply agree upon a set of reproducible markers to time common events and interactions. Simply speaking, is does not matter whether the clocks run correctly, it is enough if they all run identically wrong. The necessary requirement for such simple constructions as discussed above would, in this case, be a prescribed ratio between the height of the end piece and the positions of the markers on the rule.

For this mode of usage, the uniformity of the shadow-clock would be of interest. Was it steady-going? How big was the difference between the positions of shadow lines at equinox and solstice respectively, representing two extremes which had to be matched? To give an idea of how it would be possible to use the Egyptian shadow-casting instrument as a time-marking device throughout a year, we place the hour lines for equinox and summer solstices on the same ruler, omitting the hour markers.

The comparison made for the Egyptian shadow-clock instrument aligned along the east-west direction with a crossbar which is 12mm thick demonstrates a comfortable degree of agreement (Fig. 33). At the latitude of Egypt, the difference in position of the shadow lines throughout a year is very small past the first hour after sunrise, and the instrument could serve very well for timing common events.

In the calculations presented in the text, we have restricted ourselves to marking the shadow lines between sunrise and noon. Due to the inherent symmetry of the problem, the same lines would serve as markers in the opposite direction (first hour after noon, second hour hour after noon, etc.) after the Sun's transit through the meridian with the instrument turned towards the west.

A more interesting question is, in fact, not the precision of the old Egyptian sundials but in which way they were actually marked. How could one know that one or two hours after sunrise had passed? How could the operator identify a time interval as one hour? To measure means, in the most general sense, is to compare the measured object against some invariable reference unit. In what way could this unit be realized? The first preserved water-clock dated back to the time of Amenophis III is younger than the

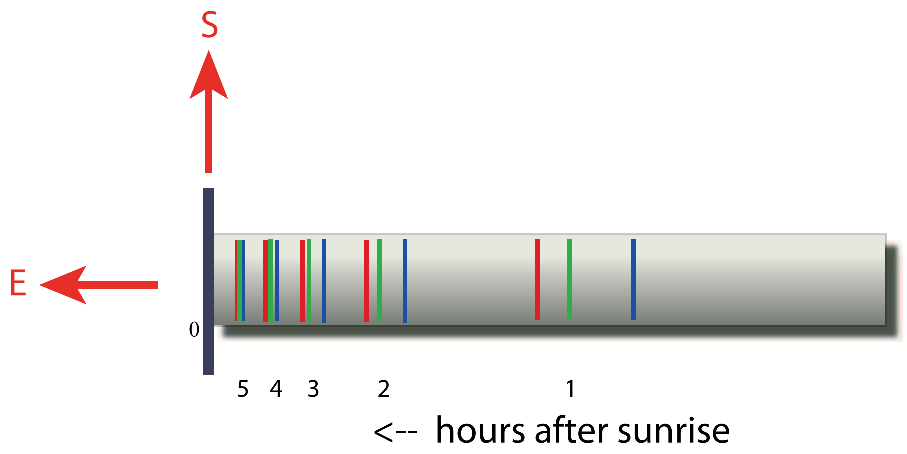

Figure 33: The instrument is placed along the east-west direction and has a crossbar which is 12mm thick. The shadows are calculated with an interval of one seasonal hour for summer solstice (red) and winter solstice (blue) and with an interval of one equinoctial hour for equinox (green).

shadow-casting instruments discussed above and is marked (like later Egyptian water-clocks) according to a different mathematical scheme. It determines even the moment of noon with an error of up to 3/4 hours, which is detectable trivially by daytime observations.[17] According to Borchardt, the water-clocks were used mainly for telling the time during the nights. The stars' positions could, of course, serve as a uniformly going clock, but this would only be realized after making the observation that they do, in fact, rotate with constant speed. How could one take note of this constant and uniform rotation with water-clocks which did not go in a uniform way themselves, conveying a notion of an hour which is, at the beginning of the scale, about 1/4 hour too long and at the end of the scale is too short by about another quarter? Conversely, if the Egyptians did postulate the uniformity of heaven's rotation, why did they not attempt to correlate the celestial time with the time readable from the water-clocks?

If the water-clocks did not serve as a calibrating device for the shadow-casting sundials, which other device could tell the sundial-makers when exactly one hour had passed? These questions are not yet explained and deserve further investigations.

5 Conclusion

Notwithstanding the variety of ancient planar sundials which exhibit, depending on the position of the shadow-receiving plane and the orientation of the shadow-casting part, quite different daily curves and hour lines, a very simple mathematical model is sufficient to construct the shadow maps for all the possible cases. We have presented here a model based on a simple vector equation in combination with the application of sequences of standard rotational matrices. As an example sundial, an equatorial sundial has been chosen; for such a dial, the construction of shadow lines for two possible orientations of the gnomon, in the horizontal position and towards the northern celestial pole, as well as the possibility to distinguish between these two cases on the basis of preserved shadow lines was discussed. Quite unexpectedly, our calculations have shown that the shadow maps of equatorial sundials are very sensitive to wrong orientation of the shadow-receiving surface, which should ideally lie parallel to the celestial equator. Even an error of about 2° in positioning would be immediately noticeable. Because the inclination of the celestial equator to the local horizontal plane is equal to $90° - \varphi$, where φ is the local latitude, an equatorial sundial can therefore be used to determine the latitude of the place where a sundial was to be installed.

[17]Borchardt, L. (1911). *Altägypt. Sonnenuhren*, ZÄSA, **48**, B11-B15.

As an example of an equatorial sundial with a non-standard horizontal orientation of the gnomon, the sundial of Amphiareion was considered and its shadow maps were calculated and discussed.

The mathematical models for all other known types of planar sundials - horizontal, vertical and inclined - can be obtained with our method with the help of a sequence of simple rotations of a standard equatorial sundial.

As an application, the old Egyptian shadow-receiving device, described first by L. Borchardt, has been studied for two different modes of application: first, directed east and second, directed permanently towards the Sun. The shadow lines which mark the position of equinoctial and seasonal hours have been drawn and compared with the markers preserved on the instrument. Our calculations speak in favor of the first mode of application with a 2/3 Egyptian finger thick crossbar mounted on the shadow casting part. Such a crossbar would match the first hour after sunrise and the last hour before noon perfectly well, but would diverge in between due to the arithmetical scheme adopted by sundial's makers for marking the hour lines being too simple.

The possibility to use the Egyptian shadow-receiving device as a time-marking instrument to schedule common events and interactions, rather than an objective time-telling instrument, was discussed. At the latitude of Egypt, the difference in position of the shadow lines throughout the year is almost negligible after the first hour after sunrise, and the instrument could serve very well for determining an agreed upon point in time reproducibly.

Acknowledgement

The authors thank M. Soloviev for critical reading of the text and K. Geus for fruitful discussion.

CHAPTER 8

SCHWEINE, FISCHE, INSEKTEN UND STERNE: ÜBER DAS BEMERKENSWERTE LEBEN DER DEKANE NACH DEM GRUNDRISS DES LAUFES DER STERNE[*]

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Bei den sogenannten Dekanen handelt es sich um eine Gruppe von 36 Sternbildern in der südlichen Hemisphäre, die in der altägyptischen Astronomie von herausragender Bedeutung waren[1]. Leider sind sie überwiegend nicht eindeutig identifizierbar mit modernen Sternen und Sternbildern. Die genaue Identifizierung wird noch dadurch erschwert, daß es im Laufe der ägyptischen Geschichte verschiedene Listen der Dekane gab, die teils voneinander abweichen. In der Forschung werden sie in „Familien" eingeteilt, die jeweils nach ihrer ältesten erhaltenen Bezeugung benannt sind[2]. Einige dieser Listen geben auch Anzahlen für die Sterne an, aus denen das jeweilige Sternbild besteht. Der wichtigste Dekan Sothis etwa besteht passend zu seinem[3] Namen *spṭ.t* „die Spitze" aus drei Sternen[4], nämlich α, δ und ε Canis maioris – tatsächlich der einzig sicher identifizierbare Dekan!

Die ältesten erhaltenen Quellen zu den Dekanen sind die sogenannten Diagonalsternuhren auf den Innenseiten von Sargdeckeln des Mittleren Reiches, ca. 1900–1800 v. Chr.[5] Tatsächlich aber gibt es gute Gründe, anzunehmen, das Konzept habe es bereits im Alten Reich gegeben, denn einerseits folgen die Namensbildungen einiger Dekane klar einem

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[*] Dieser Beitrag wurde mit Unterstützung der Deutschen Forschungsgemeinschaft während eines Fellowships am Lichtenberg-Kolleg der Georg-August-Universität, Göttingen verfaßt. Für diese Möglichkeit möchte ich allen Verantwortlichen herzlich danken.

[1] O. Neugebauer/R.A. Parker, Egyptian Astronomical Texts I, The Early Decans, London 1960, diesn., Egyptian Astronomical Texts III, Decans, Planets, Constellations and Zodiacs, London 1969. Vorschläge zur Identifizierung macht etwa Ch. Leitz, Altägyptische Sternuhren, OLA 62, Leuven 1995, 58–116. Derzeitige Standardstudie zur antiken und nachantiken Dekankonzeption ist W. Gundel, Dekane und Dekansternbilder, 1936 (Darmstadt 1969[2]). Eine vollständige Behandlung der Geschichte der Dekane wird von J. F. Quack, Beiträge zu den ägyptischen Dekanen und ihrer Rezeption in der griechisch-römischen Welt (Habilitationsschrift Berlin 2002, in Druckvorbereitung für OLA) vorgelegt werden.

[2] Neugebauer/Parker, Egyptian Astronomical Texts III, 105–167: abgesehen von vier unklassifizierbaren Einzelbelegen sind dies die Senmut-, Sethos I A-, Sethos I B-, Sethos I C- und die Tanis-Familie.

[3] Korrekt „ihrem", denn Sothis ist im Ägyptischen Femininum.

[4] Erst später findet sich bei antiken Autoren verkürzend die Gleichsetzung von Sothis und Sirius (d.h. α Canis maioris) als Einzelstern.

[5] Neugebauer/Parker, Egyptian Astronomical Texts I, 1–32, Pl. 1–23, 26–29, K. Locher, Middle Kingdom Astronomical Coffin Lids: Extension of the Corpus from 12 to 17 Specimens since Neugebauer and Parker, Proceedings of the Seventh International Congress of Egyptologists, OLA 82, Leuven 1998, S. 697–702, J. Kahl, Textkritische Bemerkungen zu den Diagonalsternuhren des Mittleren Reiches, SAK 20, 1993, S. 95–107.

sprachlich altägyptischen Muster, andererseits spielen Sothis und Orion bereits in den Pyramidentexten des Alten Reiches als göttliche Korrelate von Isis und Osiris eine dominante Rolle[6]. In dieser Eigenschaft werden sie auch im Mittelfeld der Diagonalsternuhren bildlich dargestellt.

Die Diagonalsternuhren sind natürlich keine wirklichen Uhren, sondern lediglich tabellarische Hilfsmittel, um zu wissen, welcher Dekan zu welcher Zeit über den Himmel wandert. Das nämlich ist die Hauptaufgabe der Dekane – sie wurden zur zeitlichen Strukturierung nicht nur der einzelnen Nacht, sondern auch des ganzen Jahreslaufes benutzt. Daher gibt es 36 von ihnen, entsprechend der 36 Zehntagewochen eines Jahres, wobei nach jeweils zehn Tagen ein neuer auf- und ein anderer untergeht. Dieses System ist eigentlich sehr klar strukturiert, hat aber den Nachteil, daß es die fünf Tage „auf dem Jahr“, d.h. die Epagomenen, nicht berücksichtigt, von dem in Ägypten vor dem Dekret von Kanopus[7] 238 v. Chr. fehlenden und danach auch praktisch kaum angenommenen Schalttag alle vier Jahre einmal ganz abgesehen. In der Praxis war das System also weniger brauchbar, als man es sich hätte wünschen mögen. In den Diagonalsternuhren finden sich daher als Lückenbüßer noch weitere Sternbilder, denen die Forschung den naheliegenden Namen „Dreiecksdekane“ gegeben hat[8]. Diese Probleme, verbunden obendrein mit den Folgen der Präzession, mögen der Grund für das Aufkommen neuer Dekanfamilien in der Praxis gewesen sein. Dies freilich hinderte die Ägypter nicht daran, auch die anderen Familien aus religiösen Gründen weiterzutradieren. Das Onomastikon Tebtynis I[9] beispielsweise enthält neun verschiedene Dekanlisten mit Angabe der Sternzahlen, darunter identifizierbar drei Versionen der Tanis-Familie, teils mit zugeordneten Tierkreiszeichen und Göttern, sowie vermutlich einmal die Senmut- oder Sethos I C-Familie und einmal mutmaßlich die Sethos I B-Familie in rückläufiger Reihenfolge. Diese Listen waren also essentieller Bestandteil des priesterlichen Wissenskanons. Die religiöse Bedeutung ist auch der Grund für das Auftauchen solcher astronomischer Texte und Bilder auf Sargdeckeln oder Tempel- und Grabdecken.

Man muß sich ganz klar machen, daß diese uns heute oft als einzige Quellen vorliegenden Monumente nicht zum praktischen Gebrauch gedacht waren und etwaige Fehler und

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6 R. Krauss, Astronomische Konzepte und Jenseitsvorstellungen in den Pyramidentexten, ÄA 59, Wiesbaden 1997, S. 146–206.

7 S. Pfeiffer, Das Dekret von Kanopos (238 v. Chr.), AfP Beiheft 18, München/Leipzig 2004, 249–257.

8 Neugebauer/Parker, Egyptian Astronomical Texts I, 107–113, Neugebauer/Parker, Egyptian Astronomical Texts III, 3, 164–166.

9 J. Osing, Hieratische Papyri aus Tebtunis I, The Carlsberg Papyri 2, CNIP 17, Kopenhagen 1998, 187–197, Taf. 16-17A.

veraltete Daten daher nicht als Argument für den jämmerlichen astronomischen Kenntnisstand der Alten Ägypter herangezogen werden dürfen, wie dies namentlich Otto Neugebauer getan hat[10]. Die Praxis spiegelt sich, wenn überhaupt, in Papyri aus Bibliothekskontexten, die leider nur selten auf uns gekommen sind.

Was die Ikonographie der Dekane betrifft, so tauchen sie in bildlichen Darstellungen astronomischen Inhaltes ab dem Neuen Reich immer wieder auf, wobei sie als Götter personifiziert werden. Seit der 21. Dynastie., also ca. um 1000 v. Chr., wird eine Darstellungsform als Schlangen mit Armen und Beinen sehr beliebt. Diese Form ist vor allem in Form von Fayenceamuletten zu Hunderten belegt, die v.a. von Frauen und Kindern getragen wurden, um Seuchen abzuwehren. Den Dekanen wurde nämlich ein potentiell gefährlich-dämonischer Charakter zugeschrieben, der sich insbesondere in der Verursachung von Seuchen äußerte[11].

Diese Aspekte, die nur sehr kursorisch gestreift werden können, spielten alle eine wichtige Rolle für die weitere Karriere der Dekane in der Spätzeit.

Vermutlich schon in der Spätzeit, spätestens aber unter den Ptolemäern wurde in Ägypten der aus Mesopotamien stammende Zodiakos übernommen. In der Folge wurde jedes der 12 Tierkreiszeichen in drei Abschnitte zu je 10° eingeteilt, die jeweils nach Jahreszeit einem der Dekane unterstellt wurden. Auf diese Weise konnte man schematisch die 360° des Tierkreises unter den 36 Dekanen aufteilen. Aus dieser Phase stammt denn auch die griechische Bezeichnung Dekanos als Anführer einer Zehnergruppe von Graden. Allerdings führte dies natürlich zu einer dramatischen Veränderung der Dekankonzeption. Der ursprüngliche Bezug zu den realen Sternbildern zur Zeitmessung ging verloren und wurde ersetzt durch eine rein theoretische Korrespondenz mit Zeiteinheiten des Jahres. Gleichzeitig wurde jedoch der magische Bezug zu Krankheiten bestimmter Körperteile gestärkt und systematisiert (sog. Dekanmelothesie).

In dieser Form hat das ägyptische Dekankonzept seinen Weg über griechisch-römische Vermittlung in die geistige Oikumene der Antike genommen und in Ost und West einen Siegeszug angetreten. Dieser Konzeption entsprach wiederum eine neue ikonographische Konzeption, die Verf. in Abgrenzung zu der anthropomorphen und ophiomorphen Form als Dritte

[10] Vgl. dazu den Entwurf einer Religiösen Astronomie in A. von Lieven, Der Himmel über Esna. Eine Fallstudie zur Religiösen Astronomie in Ägypten am Beispiel der kosmologischen Decken- und Architravinschriften im Tempel von Esna, ÄA 64, Wiesbaden 2000, S. 186–190.

[11] Einen klaren Textbeleg für entsprechende Vorstellungen liefert namentlich der Naos mit den Dekaden, Leitz, Altägyptische Sternuhren, S. 3–57, weitere Fragmente dieses bedeutenden Monumentes konnten durch neueste Unterwassergrabungen geborgen werden, s. A.-S. von Bomhard, The Naos of the Decades. From the Observation of the Sky to Mythology and Astrology, OCMA Monograph 3, Oxford 2008.

Reihe bezeichnet hat[12]. Sie findet sich außerhalb von Textbeschreibungen vollständig nur auf den Tafeln von Grand[13] und stilistisch stärker romanisiert auf der Tabula Bianchini aus Rom[14]. Daneben war jedoch gerade diese Form auch auf Amuletten sehr beliebt, wobei auf Gemmen fast immer nur der Dekan Chnumis (äg. *knm.t*) vorkommt, der zweite Dekan des Krebses, dem der Magen zugeordnet war. Vermutlich sollten die betreffenden Gemmen weniger gegen die Folgen übermäßiger Gefräßigkeit, als gegen Vergiftungen schützen.

Diese Gemmen der Römerzeit datieren etwa in dieselbe Zeit, aus der die Papyrushandschriften des Grundrisses des Laufes der Sterne stammen, der wichtigsten Quelle über das Dekankonzept überhaupt[15]. Freilich ist dieser Text, auch als sogenanntes Nutbuch in der Forschung bekannt, keineswegs ein Produkt der Römerzeit, sondern er blickt auf eine buchstablich jahrtausendelange Überlieferung zurück.

Aus sprachhistorischen Gründen dürfte der Basistext oder zumindest der überwiegende Teil davon, im Alten Reich entstanden sein, denn auch hier finden sich einige Eigenheiten, die den Text klar als Altägyptisch, nicht Mittelägyptisch, bestimmen. Zu diesem Befund paßt, daß sich im ersten Kapitel des Textes, das in den Monumentalfassungen illustriert ist, zwei verschiedene Dekanfamilien inkorporiert finden. Einerseits gibt es am Leib der Himmelsgöttin Nut eine Liste mit Namen und Sternzahlen, aber mit nur einem kanonischen Datum, nämlich dem heliakischen Frühaufgang der Sothis am idealen Neujahr. Diese Liste entspricht dem ältesten Dekantyp, der ebenfalls, wie erwähnt, ins Alte Reich zu setzen sein dürfte. Es ist die sogenannte Sethos I A-Familie. Unter dem Leib der Göttin hingegen finden sich Spalten mit Daten, die drei Phasen des Zyklus pro Dekan auflisten, wobei die wenigen angegebenen Namen zeigen, daß es sich um die Sethos I B-Familie handelt. Diese Daten liefern aber einen Sothisaufgang ohne spezifische kanonische Relevanz, ganz offensichtlich ein reales Datum, das dem Moment entsprach, als diese Familie in die Vorlage integriert

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[12] A. von Lieven, Die dritte Reihe der Dekane oder Tradition und Innovation in der spätägyptischen Religion, ARG 2, 2000, S. 21–36.

[13] J.-H. Abry (Hg.), Les tablettes astrologiques de Grand (Vosges), Lyon 1993.

[14] F. Boll, Sphaera. Neue griechische Texte und Untersuchungen zur Geschichte der Sternbilder, Leipzig 1903, Taf. V.

[15] Die folgenden Ausführungen bauen auf der vollständigen Edition mit ausführlichem Kommentar in A. von Lieven, Grundriß des Laufes der Sterne. Das sogenannte Nutbuch, The Carlsberg Papyri 8, Carsten Niebuhr Institute Publications 31, Kopenhagen 2007 auf. Der folgende abschnittsweise Kommentar ergänzt den dort vorgelegten insofern, als sich seit dessen Publikation Verf. einige Sachverhalte noch klarer erschlossen haben. Die Angabe der Paragraphen der Edition hier erleichtert das Auffinden der jeweiligen dortigen Behandlung. Zu den mathematisch-astronomischen Implikationen der Datenliste des Nutbildes ist darüber hinaus nach wie vor grundlegend O. Neugebauer/R.A. Parker, Egyptian Astronomical Texts I, The Early Decans, London 1960, 95–115. Dort finden sich pl. 30-33 auch Photos der Osireion-Fassung. Für Farbabbildungen der Version Ramses' IV. s. Z. Hawass, Bilder der Unsterblichkeit. Die Totenbücher aus den Königsgräbern in Theben, Mainz 2006, 290–293.

wurde, offenbar ohne zu merken oder sich daran zu stören, daß die Namenliste eine andere Familie repräsentierte. Dank dieses konkreten Datums läßt sich diese Redaktionsstufe um 1850 v. Chr. datieren, also ins Mittlere Reich! Die erste erhaltene Abschrift des Textes hingegen stammt erst aus dem Neuen Reich, um etwa 1290 v. Chr., aus dem Osireion in Abydos, auch bekannt als sog. Kenotaph Sethos' I.[16] Weitere Monumentalversionen, die sich jeweils aus Platzgründen auf das erste Kapitel beschränken, liefern die Gräber Ramses' IV. um 1150 v. Chr. und der Hofdame Mutirdis um 630 v. Chr. Etwa zur Zeit der Mutirdis zitiert ein Mythologisches Handbuch des Deltas[17] wörtlich aus dem Mondkapitel des Textes.

Die üppigste und interessante Phase der Überlieferung liefert schließlich das Städtchen Tebtynis im Fayum, wo im 2. Jh. n. Chr. in der Tempelbibliothek sechs verschiedene Handschriften des Werkes verfügbar waren[18], davon zwei mit einer jeweils satzweisen Übersetzung und Kommentierung in Mittel-Demotisch, einer jüngeren Sprachstufe des Ägyptischen[19]. Drei weitere enthalten nur den Basistext in hieratischer Schrift[20], eine war in Hieroglyphen geschrieben und als einzige auch illustriert[21]. Interessanterweise lassen die hieratischen und bilinguen Fassungen die Datenliste mit den veralteten Daten weg bzw. reduzieren sie auf ein Rechenbeispiel, das im demotischen Kommentar ausführlichst kommentiert wird.

In seiner am vollständigsten erhaltenen Form enthält der Grundriß des Laufes der Sterne mindestens vier durch Inhalt und Layout recht klar abgrenzbare Kapitel. Das erste Kapitel, das Verf. als Nutbild bezeichnen möchte, da es in den Monumentalversionen mit einer Darstellung der Himmelsgöttin Nut illustriert ist, befaßt sich mit dem Lauf der Sonne während eines Tages, sowie dem Verhalten der Dekane relativ zur Sonne. In diesem Bereich, der auch die Datenliste enthält, dominiert die nüchterne Beobachtung und Beschreibung astronomischer Phänomene sehr stark. Außerdem enthält das erste Kapitel einen Exkurs über Herkunft und Verhalten der Zugvögel. Natürlich lassen sich auch in diesem Kapitel „Irrtümer" dingfest machen, so etwa die sehr biologistische Beschreibung des Sonnenaufganges als

[16] Dies ist der Grund der Bezeichnung der im Text vorkommenden Dekangruppen als Sethos I A- und Sethos I B-Familie.
[17] D. Meeks, Mythes et légendes du Delta d'après le papyrus Brooklyn 47.218.84, MIFAO 125, Kairo 2006, 14, 78–80, 216–218, 486f., von Lieven, Grundriß des Laufes der Sterne, 455–463.
[18] Fünf der Papyri sind in der Edition und auch hier der Übersichtlichkeit halber mit ihren Carlsbergnummern benannt, es sei jedoch darauf hingewiesen, daß mit Ausnahme von Papyrus Carlsberg 1 alle so bezeichneten Handschriften physisch de facto über die Sammlung der Carlsberg-Papyri im Carsten Niebuhr Institut, Kopenhagen, das Istituto Papirologico „G. Vitelli", Florenz, und die Papyrussammlung des Ägyptischen Museums Berlin verteilt sind, ein Fragment befindet sich überdies im British Museum, London. Den betreffenden Sammlungen und ihren Verantwortlichen sei auch an dieser Stelle noch einmal für die Publikationserlaubnis und die guten Arbeitsbedingungen vor Ort herzlich gedankt.
[19] Papyrus Carlsberg 1, Papyrus Carlsberg 1a.
[20] Papyrus Carlsberg 228, Papyrus Carlsberg 496, Papyrus Carlsberg 497.
[21] Papyrus Oxford 79/105 (jetzt Centre for the Tebtunis Papyri, Berkeley).

Geburtsvorgang, bei dem die Morgenröte als das dabei abgesonderte Blut interpretiert wird. Dennoch beschränkt sich das Mythologisieren sehr auf die simple Erklärung der beobachtbaren Fakten.

Das ändert sich bereits merklich mit dem zweiten Kapitel, das gänzlich den Dekanen gewidmet ist und das im Folgenden genauer betrachtet werden soll[22]. Es folgt das Mondkapitel, das die Mondphasen vom Altlicht bis zum Vollmond behandelt, wobei die astronomischen Fakten unter der mythologischen Verklausulierung im Hinblick auf den Streit zwischen Horus und Seth kaum zu erkennen sind. Noch schwerer verständlich ist das vierte Kapitel, das Verf. Planetenkapitel genannt hat, da mindestens drei Namen von Planeten vorkommen. Andererseits spielt auch dort der Mond noch eine gewichtige Rolle. C. Leitz[23] hat deshalb das Vorkommen der Planeten bezweifelt und vorgeschlagen, in den betreffenden Namen ebenfalls Verweise auf den Mond bzw. andere göttliche Wesenheiten zu sehen. Dies ist natürlich möglich, dennoch scheint es Verf. nach wie vor naheliegender, in einem dezidiert astronomischen Text drei als Planetenbezeichnungen bekannte Namen auch als solche ernst zu nehmen, als anzunehmen, der Verfasser des Textes habe ganz zufällig gerade diese Namen in den Text eingeführt, ohne sich über die Assoziation der Planeten im Klaren zu sein. Dies gilt insbesondere für „Horus, Stier des Himmels“ (Saturn) und den Phönix *bnw* (Venus).

Daß nur drei verdächtige Kandidaten vorkommen, darf angesichts des traurigen Erhaltungszustandes gerade dieses Bereichs kaum als Gegenargument gelten. Daß der Mond in diesem Kapitel dennoch immer noch eine wichtige Rolle spielt, steht davon abgesehen außer Frage und war ja auch bereits in der Edition vermerkt worden. Unbedingt zuzustimmen ist Leitz aber darin, daß der betreffende Abschnitt so obskur, verderbt und obendrein fragmentarisch erhalten ist, daß eine sinnvolle Gesamtdeutung letztlich unmöglich ist. Von daher erübrigt sich eine Glaubensdiskussion um die Details auf dem derzeitigen Quellenstand eigentlich. Im Sinne von Leitz könnte man immerhin anführen, daß im Mondkapitel der Mond als Stier durch die Erwähnung des Festes *snsn kꜣ.wỉ* „Vereinigung der beiden Stiere“ für die Opposition von Sonne und Vollmond impliziert wird. Andererseits legt aber der deutliche Trennstrich zwischen Mondkapitel und folgendem Kapitel im Osireion doch nahe, daß

[22] Zwischen Nutbild und Dekankapitel schaltet das Osireion die Beschreibung einer Schattenuhr mit Illustrationen ein, die in allen anderen Textzeugen fehlt und deshalb in der Neuedition durch Verf. nicht als zugehörig angesehen und bearbeitet wurde (gültige Bearbeitung ist noch immer die durch A. de Buck in H. Frankfort, The Cenotaph of Seti I at Abydos, EES Memoir 39, London 1933, 76-80, Pl. LXXXII–LXXXIII, für ein Photo des Textes s. Neugebauer/Parker, Egyptian Astronomical Texts I, Pl. 32).

[23] C. Leitz, Zu einigen astronomischen Aspekten im sogenannten Nutbuch oder Grundriß des Laufes der Sterne, Enchoria 31, 2008/2009, S. 17–19.

hier eine radikal neue Thematik ins Visier genommen wird. Dies ist aber bei den nachweislich sachlich völlig unterschiedlichen Kapiteln zu den Dekanen und zum Mond nicht weiter markiert, tatsächlich gehen beide innerhalb einer Kolumne einfach ineinander über. Das läßt sich sinnvoll eigentlich nur damit erklären, daß der Übergang zwischen Dekan- und Mondkapitel eben sachlich evident genug war, wohingegen der zwischen Mond- und Planetenkapitel gerade wegen der noch immer wichtigen Rolle des Mondes extra verdeutlicht werden sollte.

Verf. wird daher bis zum definitiven Beweis des Gegenteils durch neue Textfunde das vierte Kapitel weiterhin als Planetenkapitel ansprechen, gibt aber gerne zu, daß auf dieser Deutung keine weiterreichenden Hypothesen aufgebaut werden sollten, insbesondere auch deshalb, da die Planeten in älterer Zeit außerhalb des Klassischen Himmelsbildes[24] in ägyptischen religiösen Texten und Bildwerken extrem selten vorkommen. Wirklich prominent werden sie erst in späten astrologischen Texten und entsprechenden Zodiakaldarstellungen.

Ob der Text des Grundrisses des Laufes der Sterne passend zu seinem ägyptischen Originaltitel noch weitere astronomische Phänomene behandelt hat, läßt sich nicht sagen, da der erhaltene Teil auch der umfangreichsten Handschrift mitten im Planetenkapitel abbricht.

Doch nun endlich zum Text selbst. Dabei entspricht der Text in Fraktur jeweils dem Basistext jedes Lemmas, der eingerückte Text in Antiqua hingegen dem demotischen Kommentar des Papyrus Carlsberg 1. Das Layout und die Schriftwahl versucht dabei, etwas den Charakter des Originaltextes in Papyrus Carlsberg 1 zu vermitteln. Allerdings beginnt dort der Kommentar bereits nach einem kurzen Spatium in derselben Zeile wie das Lemma. Außerdem müßte man, um den Originalcharakter wirklich adäquat wiederzugeben, den Basistext nicht nur in Fraktur formatieren, sondern auch sprachlich auf Althochdeutsch!

Zu den Dekanen äußert sich der Grundriß erstmals im Nutbild im Zusammenhang der Datenliste. Diese liefert für jeden Dekan die Daten für drei wichtige Punkte seines Lebenszyklus: *tp.t* „Erste (Stunde)“, *šnı̱ ṭwꜣ.t* „Die Duat umkreisen“ und schließlich *ms.t* „Geburt“. In moderner astronomischer Terminologie handelt es sich dabei um die akronyche Kulmination, den akronychen Untergang und den heliakischen Frühaufgang. Tatsächlich ist es fraglich, ob die Forschung das zugrundeliegende Konzept so ohne Weiteres hätte erschließen können,

[24] D.h. Darstellungen vom Typ Neugebauer/Parker, Egyptian Astronomical Texts III, Pl. 3 (Sethos I C).

wenn nicht der antike Kommentar die doch sehr spartanischen Angaben der Liste ausführlicher an einem Rechenbeispiel erläutert hätte[25].

Zum Begriff der Duat sei angemerkt, daß diese zwar üblicherweise als „Unterwelt" übersetzt wird, die tatsächliche Lokalisierung in den ägyptischen Texten allerdings zwischen dem Himmel und einer Region unter der Erde bzw. unter dem Horizont schwankt, wobei die älteren Texte eher ersterer, die jüngeren letzterer Deutung folgen. Ursprünglich dürfte die Duat daher schlicht den Ort bezeichnen, wo die Gestirne sind, wenn man sie nicht sieht. Dazu paßt die Schreibung mit der Hieroglyphe ⊛. Wie der vorliegende Text zeigt, dachte man sich diesen Ort als im Leib der Himmelsgöttin Nut, also durchaus am Himmel, da die Göttin die Sonne und die Dekane verschluckt. Die unterirdische Lokalisierung hingegen dürfte sich der Beobachtung verdanken, daß eben diese Himmelskörper ja scheinbar in der Erde verschwinden, wenn sie unter dem Horizont untergehen.

(§ 44–44a)

> Der Anfang des Aufrufens der Sterne. Sieh die Art des Aufrufens von einem jeden von ihnen. Das Ende der Schrift ist, was entsprechend der „Auflösung" ist. Sie werden aufgerufen vom 3. Monat *ꜣḫ.t*, Tag 26 <bis zum> 1. Monat *pr.t*, Tag 6. Diese fünf Sterne sind gegenüber von *knm.t* bis *tmꜣ.t*.

pḥ.wỉ-ç̌ꜣ.t. **Erste: 1. Monat** *pr.t* **6.**

> *pḥ.wỉ-ç̌ꜣ.t*: beim ersten Monat *pr.t*, Tag 6, das heißt, daß er aufhört, Arbeit zu leisten am 1. Monat *pr.t*, Tag 6. Es geschieht, daß er anfängt, Arbeit zu leisten am 4. Monat *ꜣḫ.t*, Tag 26, das heißt die zehn Tage, die er, das ist das Buch „Auflösung", genannt hat, nämlich „Ein Stern stirbt, ein Stern lebt alle zehn Tage".   das im Jahr umläuft.

Zunächst wird im Prinzip nur die Datenliste verbalisiert. Wenn von „Sternen" die Rede ist, so ist zu beachten, daß das Ägyptische keinen terminologischen Unterschied zwischen einem Einzelstern und einem Sternbild macht. Der Zyklus beginnt mit der als „Arbeit" bezeichneten Kulmination. Daher ist eine ägyptische Bezeichnung spezifisch für Dekane „Arbeitende" (*bꜣk.tỉw*). Das Paradigma wird nun am Beispiel des Dekans *pḥ.wỉ-ç̌ꜣ.t* durchexerziert. Eine auch als Spatium markierte Stelle, wo Text im Kommentar fehlt, ist neben dem mitteldemotischen Sprachzustand ein klares Indiz dafür, daß der Kommentar nicht von dem Schreiber des Papyrus selbst stammen kann, wie die ältere Forschung angenommen hatte, sondern daß auch

[25] In letzter Zeit wurde namentlich durch S. Symons, The 'Transit Star Clock' from the Book of Nut, Under One Sky. Astronomy and Mathematics in the Ancient Near East (Hg. J.M. Steele/A. Imhausen), AOAT 297, Münster 2002, S. 429–446 die seit Neugebauer und Parker gängige Interpretation des Textes in Abrede gestellt. Symons verwirft den antiken Kommentar als „spät" und damit ihrer Meinung nach a priori unbrauchbar. Ihre eigenen Deutungsversuche allein anhand der Datenliste ohne Kommentar können aber nicht überzeugen.

der Kommentar einem Tradierungsprozeß unterlegen haben muß. Aus sprachlichen Gründen dürfte er vermutlich in der 30. Dyn. oder der frühen Ptolemäerzeit entstanden sein.

Bereits hier ist deutlich zu sehen, daß der Kommentar gerne aus anderen Referenzwerken, die explizit mit Titel benannt werden, zitiert[26]. Am häufigsten wird dabei das Buch *bl* „Auflösung“ (im Sinne von „Erklärung“) genannt. Dieses Werk wird allein in Papyrus Carlsberg 1 mindestens neun Mal angeführt, ebenso in Papyrus Carlsberg 1a (mindestens fünfmal). Auch im Kommentar zum Buch vom Fayum[27], in einem weiteren, noch unpublizierten Kommentarwerk[28] und mutmaßlich im Onomastikon Tebtynis I[29] wird die „Auflösung“ immer wieder als Autorität angeführt, so daß man wohl davon ausgehen darf, daß es sich um eine Art ägyptischen Brockhaus gehandelt haben dürfte. Leider ist davon nichts erhalten außer den Zitaten. Andere Werke quasienzyklopädischen Zuschnittes zeigen aber, was man sich davon vermutlich erwarten dürfte.

(§ 44b–c)

Die Duat umkreisen: 4. Monat *pr.t* 6.

> Das heißt, er kennt die Unterwelt, um dorthin zu gehen am 4. Monat *pr.t*, Tag 6. Zähle vom 1. Monat *pr.t*, Tag 6, das ist der Tag, an dem er aufhören wird, Arbeit zu leisten, <bis zum> [4.] Monat *pr.t*, [Tag] 6. Es ist im Westen, daß er das tut. Das sind die drei Monate, die das Buch „Der Einfluß der Sothis“ genannt hat. Die Sterne verbringen sie im Westen, nachdem sie ihre Arbeit abgeleistet haben.

Geburt: am 2. Monat *šmw* 16.

> Das heißt, daß er am Himmel aufgeht aus der Duat am 2. Monat *šmw*, Tag 16. Zähle vom 4. Monat *pr.t*, Tag 6, welches der Tag ist, an dem er unterging, bis zum 2. Monat *šmw*, Tag 16, welches der Tag seines Aufgangs ist. Das macht 70 Tage. Das sind diejenigen, die er in der Duat verbringt. Er geht auf am 2. Monat *šmw*, Tag 16. Er verbringt 80 Tage im [Osten], bevor er Arbeit leistet. Er verbringt 120 (Tage), indem er in der Mitte des Himmels Arbeit [leistet]. Er verbringt die 10 Tage, die oben (genannt) sind, in ihnen. Er [verbringt drei] Monate [im] Westen. Die 12 Monate sind eine Art, die 36 Sterne zu nennen. Sieh die Art des Aufrufens seitens des 1. Monats *pr.t* 6 bis zum 1. Monat(?) [.......... Er sagte] danach [.......] Tag 16. Er sagte danach 1. Monat *pr.t*, Tag 26, er sagte danach 2. Monat *pr.t* 6. Wenn er sagt 1. Monat *pr.t*, so deshalb, weil der 1. Monat(?) *ꜣḫ.t* [........................].

Nachdem die Basisdaten für den Paradigmadekan abgehandelt sind, geht der Text nun dazu über, allgemeine Zeitangaben für die jeweilige Phase im Zyklus zu liefern, die überdies mit

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[26] S. dazu ausführlich von Lieven, Grundriß des Laufes der Sterne, 284–290. Insgesamt wird über den ganzen Text verteilt aus mindestens neun verschiedenen Büchern zitiert.
[27] Edition einiger unkommentierter Handschriften: H. Beinlich, Das Buch vom Fayum, ÄA 51, Wiesbaden 1991, der antike Kommentar ist noch unpubliziert.
[28] Nach Autopsie der Fragmente.
[29] Osing, Hieratische Papyri aus Tebtunis I, 188, Taf. 16, 16A, s. dazu von Lieven, Grundriß des Laufes der Sterne, 286.

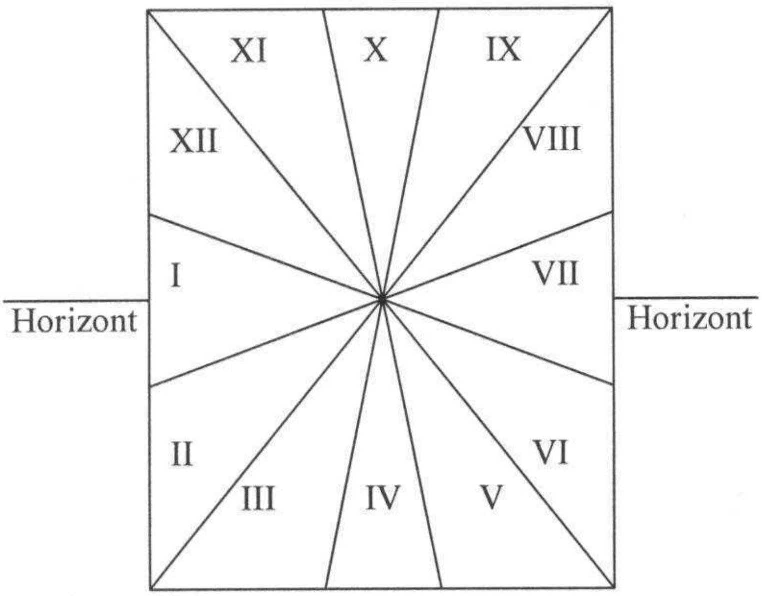
einer Positionsangabe am Himmel verknüpft werden. Es folgen danach allgemeine Angaben, wieviele Sterne das jeweils zu einem Zeitpunkt betrifft:

(§ 47)

Was anbetrifft, was zwischen dem Stern ist, der die erste Stunde macht bis zu dem Stern, der die Duat umkreist, ohne daß er in der Duat ist, [das sind 9] Sterne.

[Was anbetrifft], was zwischen dem Stern ist, der die erste Stunde macht, <das heißt> dem Stern des Abends, <bis zu> dem Stern, [der] sich der Duat nähert, b[evor er zur D]uat geht, das heißt, um zu ihr zu gehen, [das ist] der Stern, der am Eingang der Duat ist, das heißt, was anbetrifft, was zwischen dem Stern ist, [der den See berührt] bis zu dem Stern, der [in] der Dysis ist im Westen, das sind die 9 Sterne. Er spricht hier nicht über sie. Das Buch „Auflösung“ ließ sie die 9 [Sterne] im Westen des Himmels sein, indem es sagt: „Sie haben ihre Arbeit vollbracht“. Variante: ohne in der Duat zu sein. Das Äußere der Du[at. Der Stern, der] aufgehen wird, [das sind] 7 Sterne. Betreffs der 7 Sterne, „Sie gehen auf“ sagt das Buch „Auflösung“.

Interessant sind dabei vor allem die Positionsangaben, die auch einen See erwähnen. Der Stern, der die erste Stunde macht, d.h. der akronych kulminiert, wird mit dem, „der den See (*šỉ*) berührt“; der, der sich am Eingang der Duat befindet, d.h. akronych untergeht, mit dem in der Dysis (*r-ᶜ-ḥtp* „in Bezug auf den Untergang“) identifiziert. Die beiden Begriffe *šỉ* (vollständig *pꜣ šỉ p.t* „der See des Himmels“) und *r-ᶜ-ḥtp* sind aus der demotischen Astrologie als Häuser X (μεσουράνημα/medium caelum) und VII (δύσις/occasus, der Deszendent) der Dodekatropos bekannt[30]:



[30] Zur demotischen Terminologie A. von Lieven, Divination in Ägypten, AoF 26, 1999, S. 100f., 123f. Für die Detailargumentation des Folgenden s. die ausführlichere Fassung in von Lieven, Grundriß des Laufes der Sterne, S. 146f.

Auch wenn die astrologische Verwendung natürlich erst spätzeitlich ist, so läßt sich die Terminologie „See des Himmels“ und „See der Unterwelt“ für die kardinalen Punkte IV und X der Dodekatropos so bereits in den Pyramiden- und Sargtexten nachweisen. Damit wird deutlich, daß die zugrundeliegenden astronomischen Konzepte in Ägypten eine weit längere Tradition haben. Was in älterer Zeit und auch im vorliegenden Text fehlt, sind lediglich die mit der Dodekatropos verknüpften Wertungen der einzelnen Himmelssegmente, die in der Astrologie für Prognosen benutzt werden.

Dennoch muß dieser Befund die Frage aufwerfen, wo und wie die Dodekatropos eigentlich entstanden ist. Üblicherweise werden astrologische Konzepte aus Mesopotamien oder Griechenland abgeleitet, Ägypten wird in den gängigen Darstellungen marginalisiert. Tatsächlich dürfte es aber so sein, daß nicht die Ägypter von den Griechen die mesopotamische Astrologie übernahmen, sondern daß sie es waren, die das mesopotamische Material vermischt mit eigenen Konzepten den hellenistischen Griechen (etwa in Alexandria) übermittelten. Man darf also fragen, ob hier ein bestehendes eigenes System lediglich erweitert wurde oder ob ein fremdes System mit eigenen traditionellen Begriffen benannt wurde. Angesichts der Tatsache, daß auch die antiken Quellen selbst die Dodekatropos konsequent auf ägyptische Autoren (Asklepios = Imhotep, Nechepso und Petosiris) zurückführen und die Salmeschiniaka[31], das berühmteste Grundlagenwerk ägyptischer Astrologie nach Hephaistion, Apotelesmatika II 18,74–76 gerade auch eine Behandlung der vier κέντρα I, IV, VII und X enthalten haben soll, also gerade jener Punkte, die sich in ägyptischen Texten seit dem Alten Reich[32] nachweisen lassen, scheint die Frage eindeutig zugunsten Ägyptens als Ursprung geklärt.

Im vorliegenden Fall wäre vor allem interessant, ob der späte Kommentator des Grundrisses des Laufes der Sterne die astrologische Konnotation mit im Auge hatte oder lediglich im Rahmen einer althergebrachten Tradition dachte.

Doch zurück zum Text. Zunächst werden die dem Untergang sich nähernden und die bereits in der Unterwelt befindlichen Sterne genannt, danach die insgesamt sichtbaren, wobei die schon erwähnten untergehenden noch einmal mit eingerechnet sind:

[31] Zu diesem Werk s. H.J. Thissen in Leitz, Altägyptische Sternuhren, 49–55 und demnächst Quack, Beiträge zu den ägyptischen Dekanen (iVb.).

[32] In älterer Zeit sind für die Häuser I und VII statt dem demotischen *r-ᶜ-ḫ̮ꜣw* „in Bezug auf den Aufgang“ und *r-ᶜ-ḥtp* „in Bezug auf den Untergang“ natürlich die beiden Horizonte im Osten und Westen anzusetzen, die als wichtige Stationen des Laufes der Sonne und anderer Gestirne stets Interesse erregten.

(§§ 48–50)

Was nun das angeht, was zwischen dem Stern der Geburt ist, bis zu dem Stern, der die erste (Stunde) macht, das sind 20 Sterne, macht (zusammen) 29,

Nun, betreffs [desjenigen, was zwischen dem Stern ist, der] aufgeht und dem Stern, der die erste Stunde macht, das heißt dem Stern des Abends, das sind 20 Sterne. Das heißt, bezüglich desjenigen, [was zwischen dem Stern der Geburt] im Osten ist und demjenigen Stern, der den See berührt, das sind 20 Sterne. Dann spricht er über die [.............. Ost]en: 8, Mitte des Himmels: [12]. 8 Sterne sind im Osten des Himmels, 12 [Stern]e sind bei der Arbeit in der [Mitte des Himmels ....], sie sind es, die die 36 Sterne voll machen. Der Rest, 29, das heißt, der Rest der Sterne, die [täglich] am Himmel zu sehen [sind], das sind [29 Sterne]. Nur zur Information: Westen 9, Mitte des Himmels 12, Osten 8 von [ihnen]. das heißt [............................], indem sie in der Duat sind.

die dementsprechend am Himmel leben und arbeiten.

Weil diejenigen, die aufgehen, am Himmel Arbeit leisten, das heißt, die [Sterne], von denen ich gesprochen habe, sind 12. Das sind die 12 Sterne, die in der Mitte des Himmels arbeiten unter den 29 Sternen.

Einer stirbt und ein anderer lebt am Beginn einer Dekade.

Einer geht auf und ein anderer geht unter in den 10 Tagen.

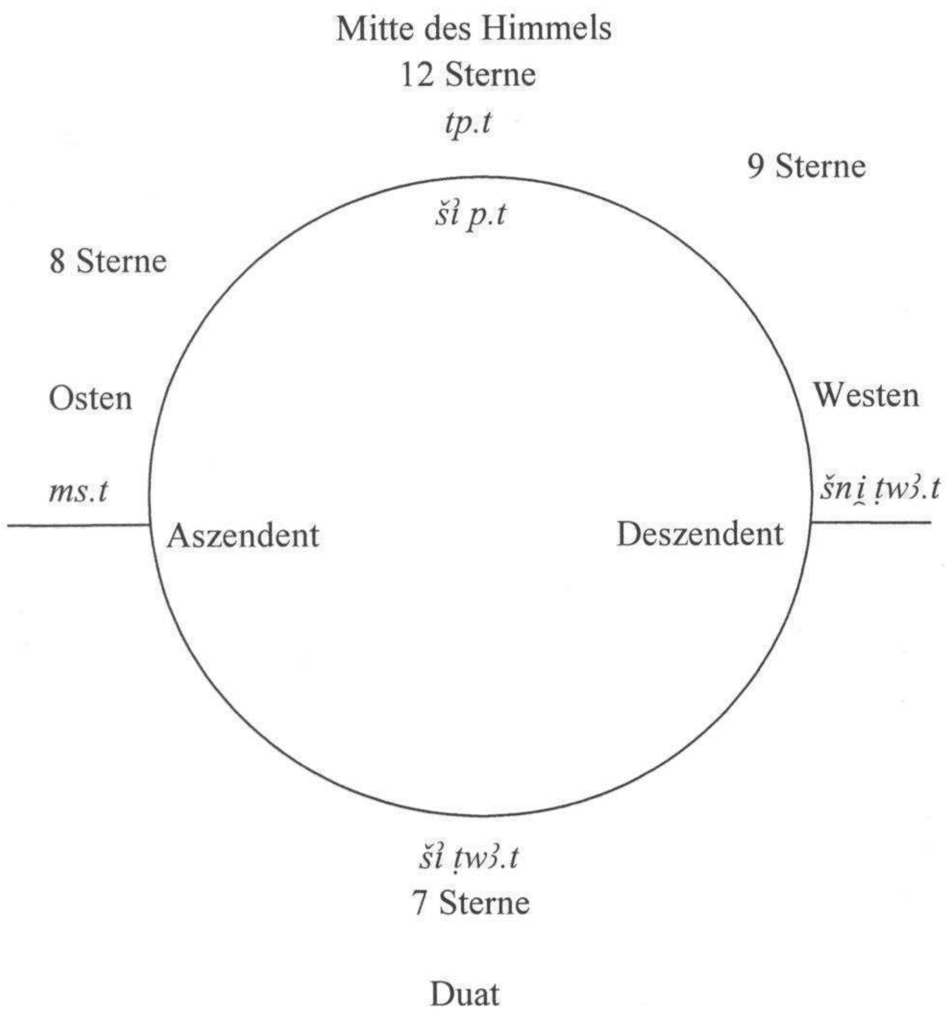
Hübsch ist vor allem das Resümee der jeweiligen Zahl im Hinblick auf ihre Position. Zwölf Dekane zeigen jeweils die Stunden an, dies ist im Prinzip auch das den Diagonalsternuhren zugrundeliegende Schema. Dabei wechselt die genaue Position je alle zehn Tage.

(§ 51)

Was nun das angeht, was zwischen dem Stern der Geburt ist bis zu dem Stern, der die Duat umkreist, das sind 29 auf der Breite des Himmels als Sterne.

Nun, betreffs desjenigen, was zwischen dem Stern der Geburt ist [bis zu dem] Stern, der sich der Duat nähert, das sind 29 Sterne. Am Himmel macht er alle diese Sterne. Betreffs des Sternes, der am Eingang der Unterwelt im Osten ist und dessen, der am Eingang [der] Unterwelt im Westen ist und dessen, der im „See“ ist, um drei Sterne vollzumachen, so sind sie außerhalb [derer], die er vorher genannt hat. Betreffs der 29 Sterne, so sind es die Sterne des Himmels. Die anderen sieben Sterne, die er genannt hat, sind in der Unterwelt. Die 36 Sterne.

Auf diese Art sind also immer 29 Dekane sichtbar, von denen aber nur 12 kulminieren und damit „arbeiten“. Der Rest ist entweder noch nicht soweit, oder bereits über dieses Stadium hinaus. 7 sind jeweils ganz unsichtbar. Gleichzeitig entspricht jeder Dekan verteilt aufs Jahr einer Zehntagephase. Das Schema faßt den etwas verwirrenden Befund des Textes noch einmal übersichtlich zusammen:



Der soeben vorgestellte Bereich des Textes ist der auch nach modernen Begriffen Wissenschaftlichste überhaupt. Unter der Überschrift des „Irrtums" ist er eigentlich deplaziert. Doch gerade deshalb sollte er hier vorangeschickt werden, um zu zeigen, daß die Alten Ägypter auch zu solchem Denken fähig waren. Dies scheint um so wichtiger, als im folgenden Dekankapitel Aussagen über die Dekane getroffen werden, die aus der Sicht einer modernen Astronomie eher erstaunen und die Präsentation des Textes unter der Kategorie „Irrtum" nur zu berechtigt erscheinen lassen.

Das Dekankapitel ist, wie erwähnt, vollständig den Dekanen gewidmet. Dabei lassen sich zwei Unterabschnitte ausmachen[33]. Zunächst einer, der offenbar die tägliche Unsichtbarkeit der Dekane am Tag erklärt (§§ 85–103), anschließend ein etwas längerer, der sich mit der

[33] Diese klare inhaltliche Abgrenzung, die der textkritischen exakt korrespondiert, wurde in der Edition noch nicht recht erkannt, ist beim zweiten Hinsehen jedoch evident.

periodischen Unsichtbarkeit der Dekane während einer jeweils 70-tägigen Phase befaßt (§§ 104–143). Dabei sind die 70 Tage eine kanonische Zeitspanne, die nicht als astronomische Realität verstanden werden darf (für Sirius selbst z.B. beträgt die Unsichtbarkeitsphase bei einem Beobachtungsstandort in Memphis tatsächlich nur 69 Tage).

Die beiden Unterabschnitte sind auch dadurch voneinander gut abgrenzbar, insofern als der Text des ersten der beiden in allen Papyri – mit vielleicht einer Ausnahme[34] – durcheinander gewürfelt ist und ausschließlich im Osireion in seiner korrekten Abfolge belegt ist. Die Ursache dafür ist wahrscheinlich ein Wechsel der Schriftrichtung in der Vorlage[35]. Vermutlich kamen die beiden Teile ursprünglich aus zwei verschiedenen Quellen zusammen, so wie dies ja auch für die verschiedenen anderen Kapitel wahrscheinlich ist.

Aus naheliegenden Gründen soll hier nur die korrekte Abfolge, wie sie das Osireion liefert, zur Debatte stehen, unter Auslassung des demotischen Kommentars, der sich in der Reihenfolge natürlich auf die späte Fassung bezieht. Im Osireion lautet der betreffende Textabschnitt:

> (§§ 85–103)
>
> „Diese Sterne fahren bis zu den Grenzen des Himmels an ihrer Außenseite in der Nacht. Wenn sie erscheinen, werden sie gesehen. Wenn sie tagsüber in ihrem Inneren fahren, erscheinen sie nicht und werden nicht gesehen. Sie treten ein hinter diesem Gott und sie gehen hinter ihm hervor. So fahren sie hinter ihm auf den Hochhebungen des Schu. (Sie) ruhen an (ihren) Plätzen, nachdem seine Majestät im Westhorizont zur Ruhe gegangen ist. Sie treten ein in ihren Mund am Ort ihres Kopfes im Westen. Sie frißt sie. So stritt Geb mit Nut, weil er zornig war wegen des sie Fressens. Ihr Name wird genannt als „Sau, die ihre Ferkel frißt“, weil sie sie frißt. So erhob sie ihr Vater Schu, indem er sie auf seinen Kopf hob, wobei er sagte: „Geb hüte sich. Er soll nicht mit ihr streiten, weil sie (ihre) Kinder frißt. Sie gebiert sie, sie leben, sie gehen täglich hervor an dem Ort unter ihrem Hinterteil im Osten, so, wie sie (auch) Re täglich gebiert“. Man sagt nicht ihren Namen als „Gottesmutter“, bis sie sie (erneut) gebiert. Nicht einer kommt in ihr zu Fall.“

Mit „ihrer“ Außen- und Innenseite ist die Göttin Nut gemeint. Der Gott, hinter dem die Sterne, d.h. die Dekane herziehen, ist der Sonnengott. Es wird also festgestellt, daß die Dekane und die Sonne dieselbe Bahn haben. Die „Hochhebungen des Schu“ ist eine andere Bezeichnung für den Himmel. Als Himmelsträger ist er ja auch im Nutbild dargestellt. Schu ist die göttliche Personifikation von Luft und Licht, zugleich mythologisch der Vater von Geb und

[34] Papyrus Carlsberg 497, dessen Text sonst sehr eng der Osireionfassung entspricht, ist in diesem Bereich leider vollständig verloren.
[35] Der verwürfelte erste Abschnitt müßte also ursprünglich retrograd geschrieben gewesen sein, der zweite nicht.

Nut. Wie es dazu kam, daß er Nut als Himmel erhob und so von ihrem Gemahl, dem Erdgott, trennte, wird im Text gleich anschließend erklärt. Die Einrichtung der Welt, wie wir sie kennen, erfolgt also letztlich als Reaktion auf den Streit der beiden Götter untereinander. Geb durchschaut nicht die Tatsache, daß die Dekane durch das Gefressenwerden eigentlich nicht zu Schaden kommen, sondern lediglich einmal durch die Nut hindurchwandern, um wieder aufs Neue geboren zu werden. Dasselbe geschieht ja auch mit der Sonne, worauf Schu explizit hinweist.

Die Bezeichnung der Nut als „Sau, die ihre Ferkel frißt" entspricht einerseits einem bei Schweinen tatsächlich in Streßsituationen beobachtbaren Verhalten, andererseits schließt sich daran eine Ikonographie an, die in Form von Fayenceschweinchen als Amulett fruchtbar gemacht werden konnte. Derlei Schweineamulette sind seit der Frühzeit zahlreich nachweisbar und manchmal explizit als Nut bezeichnet. Ihre genaue Funktion muß allerdings leider unklar bleiben. Denkbar wäre, gerade in funerärem Zusammenhang, daß sich die Besitzer selbst in den ewigen Lebenszyklus der Dekane einschreiben wollten, was textlich vor allem für die Spätzeit durchaus zu belegen ist. Alternativ wäre aber auch denkbar, daß diese Amulette dem Schutz vor den Dekanen dienen sollten, wie dies für die schlangengestaltigen Dekanamulette sicher anzunehmen ist.

Dieser erste Abschnitt erklärt also die Unsichtbarkeit der Dekane bei Tag. Einerseits wird ganz sachlich und korrekt festgestellt, daß sie auch tags vorhanden sind, aber nicht gesehen werden können, andererseits besteht der Irrtum in der konkreten Erklärung der Unsichtbarkeit dadurch, daß angenommen wird, sie verschwänden in einem dreidimensionalen Raum, nämlich dem Leib der Nut.

Im folgenden zweiten Teil wird nun auch noch die kanonische 70-tägige Unsichtbarkeit erklärt. Für diese scheint zunächst einmal nicht Nut verantwortlich zu sein, sondern nun erscheinen die Dekane selbst als handelnde Subjekte, die sich nach einer ihnen eigenen Gesetzmäßigkeit verhalten. Hier kann und soll auch wieder auf den antiken Kommentar zurückgegriffen werden, auch deshalb, weil er tatsächlich interessante Aspekte zur Deutung einführt.

(§ 104)

Derjenige, der sich zur Erde begibt, stirbt und tritt in die Duat ein.

> Derjenige, der [zur] Erde [geht], um in der Duat zu ruhen, das heißt [..................] das ist derjenige, der darin umherzieht. Er dreht sich. Es geschieht, [........] daß er wieder aufgeht.

Die 70-tägige Unsichtbarkeit wird also tatsächlich als ein zumindest temporärer Tod verstanden, ganz im Gegensatz zu dem Durchmarsch durch den Körper der Nut, von dem ja explizit klargestellt worden war, daß er völlig unschädlich für die betroffenen Sterne sei. Nun hingegen steigt der Dekan tatsächlich in die Unterwelt hinab, die auch als Haus des Geb verstanden wird, da sie unter der Erde lokalisiert werden kann.

(§ 105–106)

So bleibt er stehen im Haus des Geb für 7 Dekaden.

Sein Stehenbleiben in der Duat für 7 Dekaden.

So löst er sein Übel zur Erde während 70 Tagen.

Er löst die [Übe]l ab während 70 Tagen, das heißt, nach 70 Tagen. Man sagt: „De[shalb] legt man [sie] für 70 Tage [in] die Balsamierungswerkstatt“.

Anscheinend muß sich der Dekan von einer als „Übel“ bezeichneten Verunreinigung „lösen“. Die 70 Tage der Unsichtbarkeit werden dabei mit der ebenfalls 70-tägigen Dauer der Einbalsamierung korreliert. Historisch dürfte es wohl so sein, daß diese kanonische Zeitspanne aus naheliegenden symbolischen Gründen an die Unsichtbarkeitsdauer der Dekane angeglichen wurde. Jedenfalls wäre aus rein technischen Gründen keine Dauer gerade von 70 Tagen zwingend notwendig.

(§ 107)

S: **Ihr Name „Lösen“ wird nicht genannt bis zum siebten** (korrekt wohl: 70.) **Tag**/PC1: **Dieses Sagen des Namens „Lösen“ zur 7.**

Das heißt, man sagt „Lö[sen“ (*sfḫ*), Variante:] „Lösen“ (*sẖf*) als Name [der] 7 (*sfḫ*) bis zum heutigen Tag.

In Ägypten wurde stets gerne tieferer Sinn aus dem lautlichen Gleichklang von Wörtern oder gar ganzen Phrasen gesogen. Das ägyptische Wort für sieben klang sehr ähnlich dem Wort für „lösen“, so daß für den Autor zumindest der Version des Papyrus Carlsberg 1 der Bezug beider Wörter aufeinander ausgemachte Sache war.

(§ 108)

Der Name „Leben“ wird (S: **nicht**) **zum Lösen gesagt.**

Es ist so, daß man den Namen „Leben“ zu dem Lösen sagen wird.

Bereits bei der Feststellung, daß man Nut erst dann als Mutter der Götter bezeichne, wenn sie die Sterne wiedergeboren hat, war deutlich zu sehen, daß es in Ägypten gewisse Sprachtabus gab. Diese kommen auch hier zur Anwendung. Erst wenn die 70-tägige Regeneration abgeschlossen ist, darf vom Lösen bzw. Leben des betreffenden Dekans gesprochen werden. Auffällig ist, daß die Fassung im Osireion und in den späten Papyri gerade im für den Sinn entscheidenden Detail voneinander abweicht. Die praktische Einhaltung gerade dieser Tabus darf zumindest in Zweifel gezogen werden. Vielleicht hat man sich das Ganze so vorzustellen, daß nach erfolgreichem Wiederaufgang dieses Faktum rituell durch einen Sprechakt zu konstatieren war, was natürlich erst erfolgen durfte, wenn der Dekan auch wirklich wieder seinen heliakischen Frühaufgang erlebte. Daß tatsächlich das Wiedererscheinen Anlaß für gewisse Feierlichkeiten gewesen sein dürfte, wird weiter unten noch explizit thematisiert.

(§§ 109–110)

Sein Übel gelangt auf den Boden.

Das Abwerfen seiner Übel, das heißt, danach geht er auf.

So reinigt er sich, so entsteht er im Horizont wie die Sothis.

Er geht auf und entsteht im Horizont nach Art der Sothis, das heißt, ein jeder von ihnen. Wenn er Sothis genannt hat, so deshalb, weil sie 70 Tage in der Duat verbringt und dann wieder erscheint.

Hier wird noch deutlicher von einer Reinigung gesprochen. Leider wird mit keinem Wort erwähnt, wodurch und womit sich die Dekane eigentlich verunreinigt haben sollen. Ob sie sich wohl bei der „Arbeit“ in der Himmelsmitte, also beim Kulminieren, schmutzig gemacht haben?

(§ 111–112)

Sie sind rein, sie leben, sie zeigen ihre Köpfe im Osten.

Sie werden aufgehen, sie werden leuchten und sie werden im Osten erscheinen.

Es geschieht, daß einer stirbt und ein anderer lebt am Beginn einer Dekade.

Der Untergang von einem findet statt, und ein anderer geht auf von ihnen alle 10 Tage.

Hier liegt die prägnantest mögliche Definition dessen vor, was einen Dekan nach klassischer ägyptischer Sicht, d.h. vor der Einführung des Zodiakos, eigentlich ausmacht. Interessant ist, daß dieses überlieferte Konzept auch noch in der Römerzeit Interesse fand, sonst gäbe es die Tebtynisabschriften des Werkes ja nicht.

(§ 113)

Das sind die Köpfe der Götter.

Das sind diese Aufgänge der Götter. Variante: Das sind diese, nämlich Orion und Sothis, die die Ersten unter den Göttern sind, das heißt, sie verbringen üblicherweise 70 Tage in der Duat <und

gehen dann> wieder <auf>.

Sothis und Orion werden hier als Paradigma für das Verhalten der Dekane angeführt und nun wird verständlich, warum gerade diese beiden auch im Bildfeld der Diagonalsternuhren so dominant dargestellt worden waren. Aufschlußreich ist, daß hier explizit von den Köpfen der Götter die Rede ist. Der Grund dafür dürfte sein, daß die Dekane Sternbilder und keine Einzelsterne sind. Will man aber definieren, wann ein Sternbild seinen heliakischen Frühaufgang hat, dann liegt es nahe, das Erscheinen des zuerst wieder sichtbaren Sterns des betreffenden Sternbildes zum relevanten Fixpunkt zu nehmen. Dieses ist dann jeweils der Kopf, den es zeigen kann. Im Falle von Sothis ist dies ganz klar Sirius α. Im Falle des Orion ist die Sache etwas komplexer, da nicht wirklich sicher ist, was die Ägypter eigentlich zu ihrem Sternbild *sꜣḥ* zählten, das die Ägyptologie konventionell als „Orion" übersetzt. Außerdem ist *sꜣḥ* nicht selbst als ganzer ein Dekan, sondern er besteht aus Dekanen. So tauchen als einzelne Dekannamen etwa in der Liste auf dem Leib der Nut der Arm bzw. sowohl der Ober- und der Unterarm, das obere und untere Bein, der Pfeiler(?) (*ỉwn*) und sogar das Ohr des Orion auf. Natürlich kann Orion als ganzer dann unmöglich 70 Tage unsichtbar sein. Hier übersimplifiziert der Kommentar also um der „theologischen Korrektheit" willen, denn schließlich sind Sothis und Orion ja Isis und Osiris. Isis und der Unterarm des Osiris wären da etwas unschön gewesen.

(§ 114)

Das ist nun ihr Anfangsfest im Osten.

Das heißt das Feiern ihres ersten Festes, das man machen wird. Bei ihrem Aufgang im Osten feiert man üblicherweise ein <Fest> für sie. Wenn er das gesagt hat, so deshalb, weil sie sich üblicherweise zurückbegeben(?), und man bringt ihnen ein Opfer dar bei ihrem Aufgang im Osten.

Hier wird nun klar gesagt, daß für jeden neu wiedererscheinenden Dekan ein Fest gefeiert wird. Dies sind quasi die Sonntage der Zehntagewoche. Daß sie tatsächlich gefeiert wurden, läßt sich auch anderwärtig belegen. Die späten Kultvereine feierten beispielsweise alle zehn Tage ihre „Tage des Trinkens" und die Assoziation der ophiomorphen Form der Dekane mit ihren Weinkrügen ist da nur naheliegend. Interessant ist, daß bereits der Basistext dieses Lemmas sprachlich neuägyptisch ist und sich damit deutlich vom Rest des tendenziell eher alt- als mittelägyptischen Basistextes abhebt. Es handelt sich damit eindeutig um eine im Laufe der Überlieferung in den Text gewanderte Glosse, also quasi eine erste Kommentarschicht, die im demotischen Kommentar ihrerseits nun kommentiert wird.

(§§ 115–116)

Der Kopf von einem wird einem (anderen) unter ihnen gezeigt.

> Der Aufgang von einem anstelle eines anderen unter ihnen, das heißt, der Aufgang von einem Stern unter ihnen, wenn ein anderer untergeht.

S: So fallen ihre Knochen zur Erde, wenn die Bas, die zur [Erde gefallen waren], hervorgehen./PC1: So <fallen> ihre „Übel“ zur Erde, und die Bas, die zur Erde gefallen waren, gehen hervor.

> Das Fortfallen ihrer „Übel“ ist der Aufgang der Sterne, und sie ruhen (dann) in der Duat.

Nachdem das generelle Prinzip vorgestellt wurde, geht es nun in die Details. Wie genau vollzieht sich der Lebenszyklus eines Dekans? Sobald er reif ist zum Untergang, fällt offenbar sein als „Knochen“ bezeichneter Leichnam zur Erde und seine Seele, äg. Ba, entfleucht. Der Kommentar sagt leider nicht, was in der Duat ruht, der Leib oder die Seele, aber eigentlich kann nur ersterer gemeint sein. Der Ba hingegen setzt den Lebenszyklus in anderer Gestalt fort. Die „Übel“ (*ḳsn*) in Papyrus Carlsberg 1 könnten ein Textfehler für „Knochen“ (*ḳs.w*) sein, ausgelöst durch die Erwähnung von „Übeln“ weiter vorn im Text. Nach dem textkritischen Prinzip recentiores non deteriores könnte es aber auch umgekehrt sein, wobei noch hinzukommt, daß die Version im Osireion teils mit kryptographischen Sonderschreibungen durchsetzt ist, die Lesart „Knochen“ also vielleicht nur vordergründig suggeriert wird. In der Edition hatte sich Verf. für erstere Lösung entschieden, nach nochmaligem Überdenken der Frage scheint jetzt jedoch die zweite Lösung zumindest gedanklich stringenter. Allerdings mag dies gerade der Grund gewesen sein, warum der späte Schreiber zu „Übeln“ emendierte. Letztlich ist eine Entscheidung also in beide Richtungen nicht abzusichern. Den Grund für die mögliche Präferenz für die späte Lesart liefert der nächste Paragraph, wo es heißt:

(§ 117)

So fallen ihre Tränen herab und verwandeln sich in Fische.

> Ihre Träne fällt herab und wird zu einem Fisch, das heißt, ihre Träne fällt in den Fluß, das heißt die Sterne, das sind ihre Ausflüsse. Das heißt, ein Fisch ist ihre Wasserform.

Nachdem ja eigentlich bereits das Herabfallen der Knochen gereicht hätte, das Verschwinden der Dekane zu erklären, werden etwas unvermittelt nun Tränen erwähnt. Die späte Fassung mit ihren „Übeln“ scheint da weniger problematisch, weil die Übel ja nicht substanzmäßig definiert sind, also theoretisch eine Umschreibung für die Tränen sein könnten. Die Tränen (*rmy.wt*) aber werden nachgerade notwendig wiederum für ein Wortspiel gebraucht, um zu erklären, wieso sich die Sterne plötzlich in Fische (*rm.w*) verwandeln können.

Handelt es sich bei derartigen Ätiologien um ein ganz traditionelles Verfahren ägyptischen Denkens, so erinnern die „Ausflüsse“ (*rç̌.w*) der Sterne im Kommentar eher an die „Abflüsse“ (ἀπόρροια) griechischsprachiger astrologischer und gnostischer Texte, einen geheimnisvollen Materiestrom, der von Gestirnen auf andere Gestirne, aber auch auf die Erde und die Lebewesen darauf einwirken kann. Insbesondere ähnelt das vorliegende Konzept dem hermetischen Text über die Dekane Stobaiosexzerpt VI,11[36], wo von den Dekanen gesagt wird, sie säten (bzw. ejakulierten?) als τάναι[37] bezeichneten Samen auf die Erde, wobei diese sowohl heilsame als auch verderbliche Wirkung haben könnten. Da die „Ausflüsse“ hier nicht näher spezifiziert werden, muß leider unklar bleiben, ob der späte Kommentator dieses wichtige Element astrologischer Theorie hier im Auge hatte oder nicht.

Für den Basistext ist das natürlich ausgeschlossen und der spricht ja auch nur von Tränen, nicht von Ausflüssen. Doch was sind für ihn diese Tränen gewesen? Möglicherweise handelt es sich um eine Bezeichnung eines Meteorstroms. Man vergleiche in unserer eigenen Kultur die Bezeichnung der Perseiden im Volksmund als „Laurentiustränen“. Natürlich kommen aufgrund der Verbindung zu den Dekanen nur bestimmte Meteorströme in der südlichen Hemisphäre in Frage. Besonders die Orioniden im Oktober, d.h. im ägyptischen Monat Choiak, schienen aufgrund ihrer scheinbaren Herkunft und der Bedeutung des Orion für die Ägypter von größter Wahrscheinlichkeit als Ursprung der Idee von den Dekantränen.

(§§ 118–119)

Das Leben eines Sterns entsteht im See.

> Das Leben eines Sterns entsteht im See. Die Wasserformen haben die Gestalt von Fischen, wie das Buch *siꜣ.t*(?) (sie ihnen) zuschreibt. Variante: Es ist der Aufgang, den ein Stern machen wird, und er wird den See berühren.

Er entsteht als Fisch und kommt aus dem Wasser hervor.

> Er wird zum Fisch bei seinem Entstehen im Wasser, das heißt, er entsteht, indem er die Gestalt eines Fisches annimmt bei dem Entstehen, das er im Wasser machen wird, <nämlich> derjenige, der den See berührt.

Die Dekane haben sich nun also in Wasserlebewesen verwandelt, um einen neuen Lebenszyklus zu starten. Das Buch *siꜣ.t*(?) könnte ein astronomisches Werk gewesen sein, denn es wird

[36] A.-J. Festugière, Corpus Hermeticum III, Paris 1954, S. LVI–LVIII, 36.

[37] Da dieses Wort im Griechischen keinen rechten Sinn ergibt, dürfte es sich höchst wahrscheinlich um die griechische Wiedergabe eines ägyptischen Wortes handeln. Leider ist bislang kein passender Kandidat auszumachen. Dies könnte daran liegen, daß es sich um einen Spezialterminus handelt, entsprechende Texte aber bislang nur in geringem Umfang publiziert und erst recht nicht systematisch lexikographisch ausgewertet wurden.

auch noch in einem weiteren, bislang unpublizierten Kommentartext aus Tebtynis im Zusammenhang mit dem Sonnengott zitiert.

Aber wie kommt der Dekan nun wieder an den Himmel zurück?

(§§ 120–124)

S: **Er flattert nach oben**/PC 1: **Er tritt in den oberen Himmel ein**

Er zieht dahin am Himmel, nämlich in seiner Himmelsform.

aus dem Meer als Imago.

Im Wasser in der anderen Gestalt, das heißt seine Wasserform zieht ihm entsprechend dahin in der Gestalt, nämlich in seiner Himmelsform, in der Form, die das Buch *sꜣ.t* genannt hat.

Das ist das Leben der Sterne.

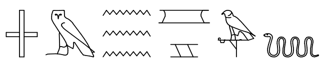
Der Aufgang der Sterne, das heißt, der Aufgang, den die Sterne machen. Er wird darüber sprechen.

Sie gehen aus der Duat hervor

Das ist ihr Hervorgehen aus der Duat.

und entfernen sich zum Himmel.

Und sie entfernen sich zum Himmel, das heißt, sie gehen am Himmel auf, indem sie fern von der Erde sind.

Offensichtlich macht der Dekan eine Metamorphose wie ein Insekt durch, das im Larvenstadium wie ein Fisch im Wasser lebt. Daß eine solche Larve als Fisch bezeichnet worden wäre, darf nicht zu sehr stören, denn z.B. im Brooklyner Schlangenpapyrus[38] werden 38 Schlangen aufgelistet, von denen die letzte auch Beine hat – der Beschreibung nach dürfte es sich nämlich um ein Chamäleon handeln. Tatsächlich ist das Wort  *ỉmỉ-mw* „Wasserform“ nicht mit einem Fisch, sondern einem schlangenartigen Würmchen (freilich ohne Beine) determiniert – allerdings hat die „Himmelsform“ *ỉmỉ-p.t* zumindest einmal dasselbe Determinativ.

In jedem Falle festzuhalten ist, daß es sich hier wohl um die einzige, aus Ägypten bekannte Beschreibung der Metamorphose von Insekten handelt, die grosso modo korrekt ist – aber in ihrer Gesamtinterpretation als astronomisch relevantes Phänomen natürlich irrig.

[38] S. Sauneron, Un traité égyptien d’ophiologie, Bibliothèque generale 11, Kairo 1989, 35. C. Leitz, Die Schlangennamen in den ägyptischen und griechischen Giftbüchern, Abhandlungen der Geistes- und sozialwissenschatlichen Klasse 1997, 6, Akademie der Wissenschaften und der Literatur, Mainz, Stuttgart 1997, 142–145 möchte darin eher eine Agame sehen, im vorliegenden Kontext allein relevant ist die Tatsache, daß es sich in jedem Falle um ein vierfüßiges Reptil, nicht um eine Schlange, handelt.

Besonders heiße Kandidaten als biologisches Vorbild des Dekanzyklus sind wohl Libellen. Einerseits gibt es aus dem Mittleren Reich tatsächlich eine Reihe von Libellenamuletten aus Gräbern der Residenznekropole in Lisht[39] – also gerade aus der Zeit, als die Datenliste in das Nutbild eingefügt worden sein muß und als anderenorts[40] Särge mit Diagonalsternuhren im Deckel dekoriert wurden. Die Libellenamulette könnten also wiederum für den Wunsch der Einbindung des Verstorbenen in den Zyklus der Dekane und damit auch sein Wiederaufleben stehen.

Andererseits sind Libellen aufgrund der Größe und Wendigkeit der Larven und der Auffälligkeit des Schlupfprozesses wie des fertigen Insekts ideale Kandidaten für die Beobachtung des natürlichen Vorgangs durch einen antiken Priestergelehrten. Der Schlupf einer Libelle dauert vom Aufplatzen der Puppe bis zum flugfähigen Imago ca. zweieinhalb Stunden, ist also ohne weiteres beobachtbar, wenn man das Glück hat, zufällig zur rechten Zeit am rechten Ort zu sein.

Der Text nimmt nun eine etwas abrupte Wende, denn er führt plötzlich wieder den Streit von Geb und Nut ein:

(§§ 125–128)

So wurde Geb zum Fürsten der Götter.

Das Werden des Geb zum Fürs[ten] der Götter. Er wird darüber sprechen.

So: Geb, Nut: Streiten. Geb und Nut.

Derjenige, der mit Nut ein anderes Mal gestritten hat. Das Streiten, er wird darüber sprechen.

Er befahl, daß sie ihre Köpfe im Osten zeigen sollen.

Er wird ihnen befehlen, um zu veranlassen, daß sie im Osten erscheinen, das heißt, Geb wurde zum Fürsten der Götter wegen dessen, was er Nut befahl, um sie im Osten erscheinen zu lassen, das heißt, die Sterne.

2. Mal. So sprach Geb zu den Göttern. Variante: Nut und Geb, er sprach zu Nut:

Das 2. Mal. Denn er redete mit Nut selbst. Eine Erklärung über das abgeben, wodurch Geb zum Fürsten der Götter wurde, ist, was er tut.

Scheinbar etwas unmotiviert wird unterstellt, auch im Hinblick auf die periodische Unsichtbarkeit der Dekane hätten Geb und Nut gestritten, obwohl Nut hieran gar nicht weiter schul-

[39] D. Arnold, Falken, Katzen, Krokodile. Tiere im Alten Ägypten. Aus den Sammlungen des Metropolitan Museum of Art, New York, und des Ägyptischen Museums, Kairo, Zürich 2010, 48–49 (Kat. Nrn. 36-38). Den Hinweis auf diese Stücke verdanke ich S. Lippert.

[40] Die Diagonalsternuhren auf Sargdeckeln sind für Assiut typisch.

dig zu sein scheint. Zumindest das Erscheinen des Geb ist insofern sinnvoll, als ja am Anfang des zweiten Unterabschnitts gesagt worden war, die Dekane verbrächten die 70 Tage im Haus des Geb, also in der Erde respektive der darin liegenden Unterwelt.

Interessant ist, daß der mit der periodischen Unsichtbarkeit der Dekane befaßte Abschnitt davon ausgeht, daß sie diese Unsichtbarkeitsphase in der Erde verbringen, der Abschnitt über die tägliche Unsichtbarkeit am Tage diese jedoch mit dem Aufenthalt am Himmel im Inneren der Nut erklärt. Der Text konnte also zwei verschiedene Modi von Unsichtbarkeit auch mit zwei verschiedenen Aufenthaltsorten der Dekane verknüpfen.

(§ 129)

S: **Fischt eure Köpfe!**/PC1: **Rettet eure Köpfe!**

Eilt, zieht eure Aufgänge zurück! - das heißt, das, was sie seit diesem Tag getan hat.

„Sie“, d.h. der Kommentar sieht hier offenbar Nut am Werk, während der Basistext nur eine Anrede an die Dekane enthält. Die zumindest sachlich akzeptable Textvariante *nḥm* „retten“ im Lemma und die wenig passende Übersetzung als *ḥm* „zurückziehen“ gehen auf ein Mißverständnis der originalen Schreibung für  *ḥꜣm* „fischen“ zurück, die im Kontext auch offenkundig die passendste Lesart ist.

(§§ 130–130a)

So befahl Thot, daß sie ihre Köpfe fischen sollten.

Thot befahl ihnen betreffs ihrer Aufgänge.

So entstand Geb.

Das heißt, derjenige, der entstand bei dem Entstehen, das Geb machte, es zu sagen (?), das heißt, sagt er das üblicherweise?

Das plötzliche Erscheinen des Thot statt Geb ist völlig rätselhaft und hat offenbar den antiken Kommentator ebenfalls verwirrt. Vermutlich liegt ein Überlieferungsfehler sehr früh in der Textgeschichte vor, das Lemma § 130a fehlt auch im Osireion. Als Ursache wäre eine Verwechslung der Vogelzeichen, mit denen Thot  und Geb  geschrieben werden denkbar, hinzu kommt, daß auch *ḥꜣm* „fischen“ mit einem ähnlichen Vogel  geschrieben ist. Tatsächlich ist im Osireion in § 130 hier sogar (ernst gemeint oder kryptographisch?)  geschrieben, als ob *gmi̯* „finden“ zu lesen wäre. Die Schreibung mit dem Ibis mag erst recht zu der Assoziation mit Thot geführt haben.

(§ 131)

Sie leben und ihre Köpfe entstehen.

Sie lebten und ihre Aufgänge entstanden, das heißt, Geb veranlaßte, daß sie leben. Er ließ für sie ein Erscheinen entstehen, als er sie rettete. Das ist es, wodurch er zum Fürsten der Götter wurde. Es ist so, daß er sie untergehen läßt, wenn er wünscht, sie untergehen zu lassen und daß er sie aufgehen läßt, wenn er wünscht, sie aufgehen zu lassen.

Während im ersten Abschnitt deutlich gesagt wurde, daß Nut für die tägliche Unsichtbarkeit am Tage verantwortlich ist, scheint die periodische Unsichtbarkeit auch während der Nacht auf das Konto des Geb zu gehen. Die Tatsache, daß die Dekane diese Phase der Regeneration ebenfalls benötigen und Geb sie immer wieder aufleben läßt, ist offenbar der Grund für seinen Standardtitel „Fürst der Götter". Dabei ist zu berücksichtigen, daß in Ägypten die Auffassung der Sterne als Götter so etabliert war, daß die Hieroglyphe des Sterns sogar in Kryptographie bzw. in spätägyptischer Monumentalorthographie (vulgo „Ptolemäisch") ganz regelhaft zur Schreibung des Wortes Gott verwendet werden kann.

(§ 132)

Die „Übel" (S: **Knochen**) **werden zu Menschen.**

Die „Übel" werden zu Menschen, das heißt, das ist das Gleiche an Tagen des „Übels", was für Menschen an diesem Tag entsteht, das heißt, die 70 Tage, die sie in der Balsamierungswerkstatt verbringen. Beginn des Rezitierens von ihrer Seite. Ihre Art des Ruhens ist es, die er in dem Buch „Das Obere" beschrieb, wobei er wiederum an das Sprechen der 42 Worte bei ihm dachte.

Offenbar wird der ganze Zyklus noch einmal resümiert, wobei erneut der Konnex zwischen Balsamierungsdauer von Menschen und Unsichtbarkeitsphase der Dekane hergestellt wird. Aber damit nicht genug, die Dekane müssen offenbar auch ein Totengericht über sich ergehen lassen. Das „Sprechen der 42 Worte" ist nämlich nichts anderes, als das bekannte Negative Sündenbekenntnis des Totenbuches, wobei man gegenüber jedem der 42 Beisitzer eine Sünde als nicht begangen deklarieren muß. Das Buch „Das Obere" mag auch astronomischen Inhaltes gewesen sein, ist aber sonst nicht bekannt.

(§§ 133–134)

Es entsteht aber seine Lebenszeit in der Duat.

Das ist das Entstehen seiner Lebenszeit in der Duat, nämlich von einem jeden der Sterne, nämlich die 70 Tage.

S: **(???)**/PC1: **Daß der, der stirbt, handelt, ist in diesem(?).**/PC228: **Sterben als Imago.**

Derjenige, der untergeht, ist derjenige, der dies tut, das heißt, der Stern unter ihnen, der zur Duat geht.

Daß gerade die 70 Tage der Unsichtbarkeit scheinbar als Lebenszeit bezeichnet werden, ist eine etwas unglückliche Verkürzung. Gemeint ist wohl, daß die eigentliche Lebenszeit bzw. das Leben überhaupt während der 70-tägigen Regenerationsphase wieder neu entsteht. Was folgt ist offenbar erneut eine stark verderbte Passage. Es scheint, als ob die späten Papyri nach Kräften versucht haben, den unverständlichen Text der Vorlage wieder zu etwas halbwegs Sinnvollem zu emendieren.

(§§ 135–138)

Bei Nacht gehen die Bas beim Dahinfahren im Himmel hervor.

Der Aufgang der Sterne, wenn sie dahinziehen am Himmel in der Nacht, das heißt, der Aufgang, den die Sterne machen, und sie machen ihre Wanderung, wenn sie bei Nacht am Himmel aufgehen.

S: **Es geschieht, daß sie bei Tag bis zu den Grenzen des Himmels wandern**/PC1: **Es geschieht, daß sie bei Tag wandern und dem Himmel fern sind**

[........] Es geschieht, daß sie dahinziehen, indem sie sich tagsüber zum Himmel entfernen, das heißt, es kommt so, daß sie tagsüber am Himmel gehen, indem sie fern von den Menschen sind. Er wird es dich erkennen lassen.

und nicht sichtbar aufgehen.

Es gibt keinen Aufgang für das Gesicht, das heißt, und es geschieht, daß ihre Aufgänge unsichtbar sind für das Gesicht der Menschen.

Das nun ist das Gesehenwerden durch die Lebenden.

Sein Anblick seitens der Menschen findet am Tag nicht statt.

Zum Abschluß wird noch einmal resümiert, daß die Bas, also die Dekane, wenn sie wieder voll einsatzfähig sind, nachts sichtbar sind, tags aber nicht. Dabei ist interessant, daß astronomisch völlig korrekt festgestellt wird, daß sie auch tags aufgehen, aber diese Aufgänge für die Menschen nicht sichtbar sind.

(§ 139–143)

Erschienen am Himmel innerhalb der Stunden der Nacht führt er seinen Lauf durch.

Das Durchführen seiner Wanderung[en], wenn er in den Nachtstunden am Himmel aufgeht, das heißt, das Durchführen seiner Wanderungen, die ein Stern macht, wenn er [in] den [Nacht]stunden am Himmel aufgeht. [Die] Angelegenheit ist es, was er sagen wird.

Den Himmel befahren bis zum Aufhören.

Und er zieht dahin am Himmel [......].

Das bedeutet, sein Leben dort zu sehen.

Dein Sehen seiner Aufgäng[e] am Himmel.

Der Gang der Sterne, er begibt sich in ihr Inneres

Der Lauf eines Sterns [...] in ihr, das heißt, der Lauf, den ein Stern im Inneren [des Himmels macht, wenn er] aufhört in ihr, nämlich der Duat.

wie sie es (üblicherweise) tun.

[.......] in der Art der Umläufe, das heißt, die „Auflösung" sagte: „[Ihr] Aufgang zusammen mit den Umläufen, die sie machen".

Der Schlußpassus führt das Resümee fort, wobei die Formulierungen etwas schwammig sind. Der Kommentar zitiert schließlich noch einmal aus seinem Lieblingsreferenzwerk, der „Auflösung".

Damit schließt das Dekankapitel und der erhaltene antike Kommentar, der zuvor[41] noch vermerkt, das folgende Mondkapitel sei „der Beginn von anderen Dingen" und er habe es nicht verstanden.

Vorliegender Beitrag soll einen kleinen Einblick in das bedeutendste erhaltene Werk der ägyptischen Astronomie vermitteln, dessen Hochschätzung durch die Ägypter selbst sich in seiner Jahrtausende überspannenden Tradierung und dem wissenschaftlichen Umgang mit ihm durch Übersetzung, Kommentierung und Versionsvergleich zeigt[42]. Der Text zeigt exemplarisch die Stärken und Schwächen der ägyptischen Astronomie auf. Man darf dabei aber auch nicht vergessen, wie alt der Basistext ist – mindestens frühes 2., wahrscheinlich gar 3. Jahrtausend v. Chr. Die römerzeitlichen Papyrushandschriften hingegen spiegeln zwar die Wertschätzung des Altüberlieferten, aber gleichzeitig lagerten in derselben Bibliothek quasi im nächsten Regal demotische astronomische und v.a. astrologische Texte mit erheblich moderneren Inhalten[43]. Diese spiegeln die Gedankenwelt der späten Ägypter natürlich weit besser, aber ihre wissenschaftliche Erschließung steht erst am Anfang.

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[41] § 144, der Kommentar bezieht sich faktisch bereits auf den ersten Satz des Mondkapitels als Lemma.

[42] A. von Lieven, Religiöse Texte aus der Tempelbibliothek von Tebtynis — Gattungen und Funktionen, Tebtynis und Soknopaiu Nesos — Leben im römerzeitlichen Fajum. Akten des Internationalen Symposions vom 11. bis 13. Dezember 2003 in Sommerhausen bei Würzburg (Hg. S. Lippert/M. Schentuleit), Wiesbaden 2005, S. 64f., dies., Grundriß des Laufes der Sterne, 258–273, 284–292, 296–299, dies., Translating the Fundamentals of the Course of the Stars, Writings of Early Scholars in the Ancient Near East, Egypt, Rome, and Greece (Hg. A. Imhausen/T. Pommerening), Beiträge zur Altertumskunde 286, Berlin 2010, S. 139–150.

[43] K. Ryholt, On the Contents and Nature of the Tebtunis Temple Library. A Status Report, Tebtynis und Soknopaiu Nesos — Leben im römerzeitlichen Fajum. Akten des Internationalen Symposions vom 11. bis 13. Dezember 2003 in Sommerhausen bei Würzburg (Hg. S. Lippert/M. Schentuleit), Wiesbaden 2005, 141–170. Speziell zum astrologischen Material s. J. F. Quack, Egypt as an Astronomical-Astrological Centre between Mesopotamia, Greece, and India, in: D. Brown, H. Falk (Hrsg.), The Interactions of Ancient Astral Science, iDr.

Man sollte jedoch die aus heutiger Sicht eklatanten „Irrtümer“ des vorliegenden Textes nicht zu sehr belächeln, denn es ist doch zumindest deutlich, daß die antiken Gelehrten eine stringente Theorie der beobachtbaren Phänomene liefern wollten und dabei zunächst einmal auf ihren sonstigen Erfahrungshorizont, insbesondere die Beobachtung tierischen Verhaltens, zurückgriffen. Dies ist immerhin eine Methodologie, wenn auch natürlich eine nach heutiger Sicht irrige.

Haben also die Ägypter wirklich geglaubt, die Dekansternbilder seien Ferkel oder Insekten? Vielleicht. Aber vielleicht liegt der größte Irrtum hier auch beim modernen Leser, wenn er dies annimmt. Kann es nicht auch sein, daß diese Art der Erklärung nur eine Modellbildung ist, die in symbolischer Sprache versucht, kosmische Phänomene anschaulich zu machen?

Immerhin spricht ja auch die moderne theoretische Physik von Wurmlöchern im Universum, ohne daß jemand ernsthaft glauben würde, daß hier wirkliche Würmer am Werk wären. Zugegebenermaßen eine gewagte Hypothese, aber vielleicht dennoch des Bedenkens wert.

CHAPTER 9

ACCIDENT AND DESIGN, FAILURE AND CONSEQUENCE: IDENTIFYING AND EXPLOITING ERROR IN THE ARCHAEOLOGY OF MUSIC

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Abstract: In attempting to unravel some of the complexities of ancient musical knowledge and behaviour, previous interpretive models have often tended tacitly to endow historical documents with authenticity and authority whilst at the same time assuming purposefulness and success in actions that are evidenced in excavated musical finds. Such optimistic assumption has run several risks. This paper takes a fresh look at issues of authenticity and intention that pertain to the nature and consequence of error and accident in ancient music's fossil record, particularly during Late Antiquity and the early Middle Ages. Recognizing that identification and characterisation are the essential first steps towards that understanding, and to turning it to our advantage, some case studies are discussed in which accident and error appear to shed light upon connexions of an inter-cultural and traditional kind, in different aspects of ancient musical knowledge. Whilst such earthy relics are only just beginning to afford insights into the musical backgrounds of writers such as Isidore, Boethius and the earlier Roman and Greek theorists, the ubiquity and quantity of the archaeological evidence, and the quality of the information it holds, promise us access to developments in musical knowledge both within periods of European history and far beyond.

Contents

1. Introduction

1.1 Accident and error: ancient musical perspectives

The perspective which I hope to bring to the subject of this volume is that of music, or more properly, that of an archaeologist, medievalist and prehistorian who has made a particular study of the origins and evolution of musical and other acoustical behaviours. Today such concerns lie at the heart of the growing interdisciplinary field known as 'Music Archaeology and Archaeoacoustics'.

In observing that my declared interests seem not to include Classical Studies, the reader may be excused for wondering whether I am intending to sidestep the rich field of Greek and Roman musical thought, and in particular the evidence which Greek, Roman and early medieval writers have left us for musical theory and musical practice in Classical Antiquity. Equally, by representing music as an aspect of behaviour, rather than as a form of art, I might be suspected of eschewing classical methods of musical criticism in favour of more broadly anthropological, sociological and even ethological approaches. In fact, both of these suppositions would be correct, to an extent, at least insofar as this article is concerned. Certainly I shall not be scouring the documents of Greek and Roman philosophy for conceptions and misconceptions of the phenomena of sound and vibration, attractive though that thought might be. Rather I shall take as my principal theme music's fossil record which, whether by accident or design, has come down to us from the remote past through its deposition in the ground.

I do not wish to apologise at any length for the apparent obliqueness of this approach. Although they may seem (and indeed in most academic traditions they still remain) unconnected disciplines, the experiences of prehistoric archaeology and of medieval archaeology seem to me not only to enrich one another but also to bear profitably upon hermeneutical aspects of Classical Studies and of musicology, both individually and in tandem: informing especially the interpretation of such textual evidence as we have for musical habits and ideas and knowledge in the ancient world, and how they mesh. Some of that textual evidence, it must be acknowledged, still presents quite considerable interpretive challenges, especially in matters of terminology, and remains poorly understood.

The use of archaeology to supplement and even contest textual evidence has itself a long pedigree. In Britain we trace it at least to the time of William Stukeley, the prophet of modern British field archaeology. In his monumental Itinerarium Curiosum of 1726 Stukeley envisages and advocates future archaeological endeavour as promising some compensation for the lacunae that then impoverished (and indeed would otherwise still impoverish today) studies of Late Roman Britain. In truth it was mostly lacuna. 'If in any people action has outdone the capacity of rhetoric,' he writes,'or [if] in any place they have left historians far behind in their valour and military performances, it was [the Romans] in our own country; and we are as much surprised in finding such infinite reliques of theirs here as [we are] that we have no history of them that speaks with any particularity of the last three hundred years that [they] dwelt in Britain and rendered it [so] perfectly provincial. [Their] learned memoirs are very short; and it is well [for us that] they were guided with such a spirit as left monuments sufficient to supply that defect, when handled as they deserve.'[1] He was to be amply vindicated. Later too, he would involve himself in music's origins, in those days surely a less hopeful cause even than Late Roman antiquity, when in February 1755 he procured and personally presented the first paper to be read on any musical subject to the Society of Antiquaries of London: Samuel Gale's 'An Historical Dissertation upon the antient Danish Horn, kept in the Cathedral Church of York', later published in the first volume of the Archaeologia (I, 1760: 168–182).

In the two hundred and fifty years since then archaeology has come to challenge history's role as the primary discipline, even in some regions of historical time. Since the 1970s the methods of the prehistorian have increasingly been adopted by medieval, Classical and Near Eastern scholars in investigating economic, demographic and even linguistical aspects of ancient life. The question of 'error', of course, concerns archaeologists profoundly, just as it does the engineer and the neuroscientist. Significant parts of the archaeological record seem to constitute or include outcomes of error, or of accident, or at least of unintended acts and unforeseen natural events of one kind and another. The unconsidered detritus of ancient industries reveals information about those industries, particularly statistical information, that finished product alone cannot reveal. In the study of lithic industries, examination of débitage, waste flakes and discarded blades, adds meaning to the character, and thereby the intended function, of the finished tools. Evidence of failure and disposal that is implicit in débitage complements evidence of exploitation that is often preserved in surface polish, damage and repairs on finished products, because so often distributed differently in the wider landscape. Changes of plan are also of great interest, both amongst finds and in the

[1] I have adjusted Stukeley's text slightly, in square brackets, for the sake of readers unfamiliar with eighteenth-century English scholastic phrasing: the original passage can be found in Glyn Daniel's The Origins and Growth of Archaeology, 1967: 46-7.

architectural environment, to the especial benefit, for example, of frontier and communications studies. An instance of this can be seen in the adjustments of course, and changing standards of construction, that are evidenced in the building of the Antonine frontier in northern Britain (Gillam 1975).

If such things may afford the archaeologist useful insights, I see no reason why they should not also help the musicologist and the organologist. Indeed I would argue that an appreciation of the relationship between intention and error, of the ubiquity of error in the real world and of the part error may play in enabling processes of change, is of central importance to all studies of the ways in which musics change.

From an interpretive viewpoint, error is just as prevalent amongst the documents which the historical archaeologist must deal with as it is amongst the realities which such documents purport to represent: as true, for example, of an inaccuracy in an author's description or an artist's depiction of a thing (or of an act) as it is of some blemish in the manufacture or performance of the original. Often it can be shown to develop something of the complexity of the 'whispering game'. It must surely be of importance to us to identify and understand such processes, rather than merely to dismiss their product as incompetent work; for even ineptitude, like waste, may have its story to tell. The converse is also true, of course. Without an appreciation of the nature of error, accident, deviation and anomaly, and without drawing it into our interpretive process, we risk modelling an unreal past in which things were always as they now seem. Wherever they can be identified therefore, error, failure and examination of ancient responses to, corrections to and compensations for such error or failure, may afford us a powerful interpretive tool: as it were a mirror through which to redefine and re-examine a thing, an act or an idea in terms of what it was not, as well as what it was or may have been. Whether in a tool, a procedure or an institution, success is after all defined by criteria, or norms, or rules, within whose limits lies 'normality' and beyond which lies failure. I will offer a brief selection of examples which from my own experience seem to illustrate this point, and consider their wider implications for the future study of ancient musical and acoustical knowledge.

1.2 Musical hardware: form, failure and repair

Amongst the principal goals of my early work in the archaeology of music was a more thorough understanding of developments in lyre-making and lyre-playing between around AD

300 and 900, during the transition from Late Antiquity to the early Middle Ages. Such periods of intense cultural change are of great intrinsic interest, from a musical viewpoint; but before I could come at the processes themselves I needed to be sure to maximise and exploit all possible sources of evidence. With this in mind I developed a twofold plan combining the customary archaeological pursuit of new finds (including both unidentified finds and finds incorrectly assigned to other categories) with a programme of practical experiment. Since my particular aim was to discern relationships between preserved forms and traditions of manufacture and structure, I was naturally drawn into the fields of taxonomy and engineering design.

My interest in previous misidentification was first stimulated and encouraged by the work of Rupert and Myrtle Bruce-Mitford, who had recently published the upper part of a late sixth- or early seventh-century lyre from the aristocratic grave-mound at Taplow, Buckinghamshire, hitherto labelled as the handle of a cup (Bruce-Mitford/Bruce-Mitford 1970). It proved to be a rewarding line of enquiry. Musical finds are often highly fragmented and, to the uninitiated, the musical character of individual pieces may well not be evident. As a consequence, musical identification is often applied in a spirit of cheerful optimism to things which are not in fact musical at all, whilst truly musical pieces can and do go unrecognized and neglected. The realisation that such error was both commonplace and widespread in our national and regional collections had itself important implications, and, to be sure, my work came to profit from a better understanding of how such error can occur and what remedies there might be. I will return to some of the outcomes directly. But in the meanwhile my engagement in structural engineering introduced me to something of still more immediate benefit: the nature of engineering structure and of structural failure.

Structural failure is usually the consequence either of some unintentional event or of a mismatch between the design or manufacture of a thing and the function that it is meant to serve. In short, unless it is brought about by some deliberate act of damage it tends to be a result either of accident or of error. The dramatic structural failure of the Bruce-Mitfords' first reconstruction of the Sutton Hoo lyre, at an important public event, is now a matter of legend amongst Anglo-Saxonists of a certain age; but it also held a valuable lesson in design. It seems not to have been accident, in the sense of a sudden, extraneous event, such as a physical blow, but rather an intrinsic weakness in the mortised joints, under tension from the strings. Was this due to a fault in the reconstruction? Was it due to over-tensioning? Or might it have been caused by some defect in the original design? The design of any stringed instrument is a compromise between demands of acoustical and musical performance and the limits of physical structure. Whilst scholars have naturally tended to focus first upon their musical and acoustical character, for makers and players of early lyres structure is the sine qua non. Without structural integrity—that happy combination of rigidity and elasticity, in the appropriate parts—no instrument can begin to function successfully. And even in the 1970s some indications of failure were already being observed in the ancient finds themselves.

At first glance decorative, the superstructural metalwork found on Anglo-Saxon lyres, and sometimes hinted at in their early medieval iconography, might also have served, or perhaps attempted to serve, some remedial purpose (Lawson 1978). It certainly seems analogous with the metal braces added to some surviving medieval and later harps, and indeed these correspond closely to the points at which that first Sutton Hoo reconstruction (and several others since then) failed. At the same time, inspection of the one surviving image of the lost Oberflacht, Württemberg, lyre revealed another of the instrument's weak spots: along the lower edge of the sound-board.

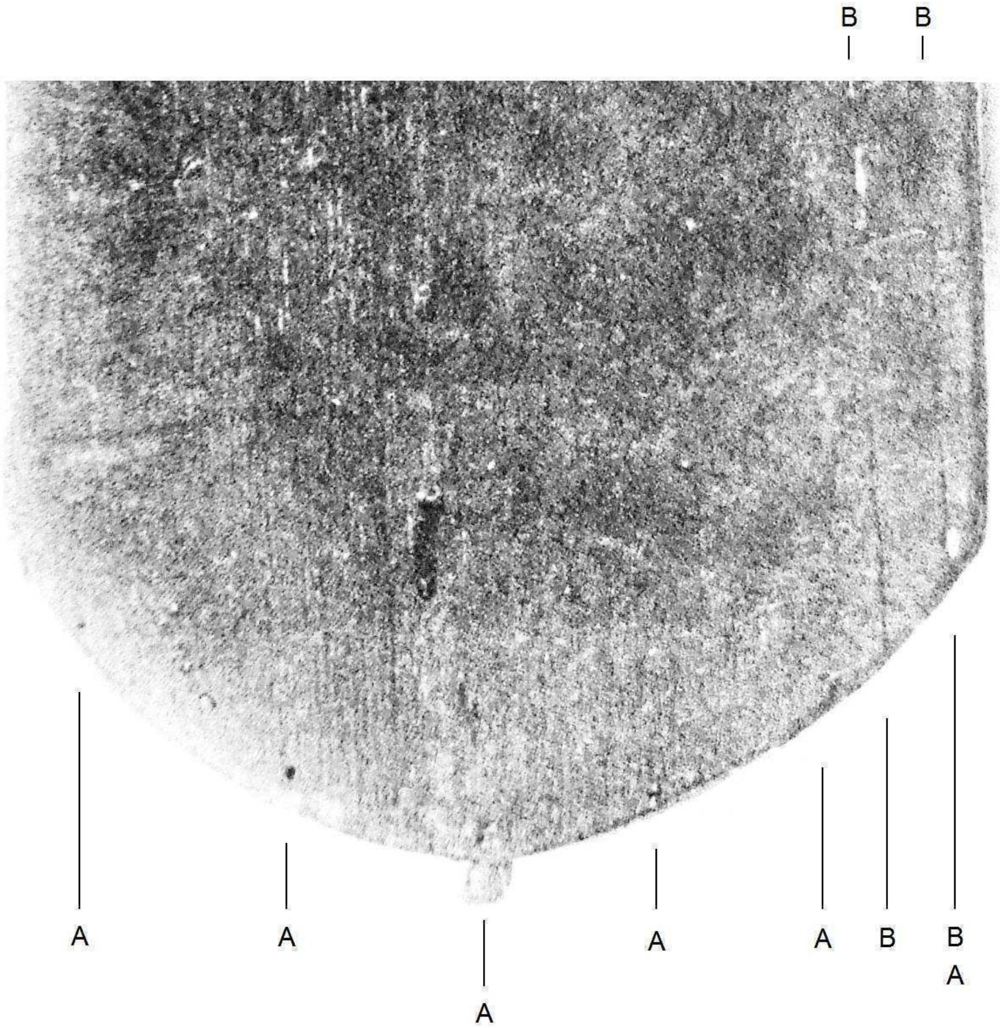

Figure 1: The lower end of the lost Oberflacht (Württemberg) lyre, showing (A) fixing pins and (B) cracking. Image enhancement and analysis: G. Lawson, based on the unique surviving image in Veeck 1931, Tafel 4 B (9), after Lawson 1980: 207 & Plate 26.

After the strings themselves, the sound-board is the most important acoustical element of any stringed instrument. Its effectiveness as a diaphragm (it functions rather like a drum head) is dependent on its tensile strength yet also upon its thinness. Typically no more than 4 mm thick, pressure from the strings, acting through the bridge at its mid-point, puts it at risk of fracture along any margin where the end of the wood grain is exposed to tension, as in this case. Some cracking is probably inevitable, with use and age, and may well have mattered little, up to a point. Barbara Theune-Großkopf reports comparable damage in the lyre from the musician's grave, Grave 58, at Trossingen in the Swabian Jura (also waterlogged), where the very same edge of the sound board has become detached from the ribs. There the fault has been remedied by the insertion of small metal pins: evidently successfully, although the process has still given rise to small cracks (Theune-Großkopf 2004: 9, Fig. c; 2006: 279, col. 1; 2010: 55 and 70–1, Fig.) This combination—of defect and repair—relates it closely to evidence of failure that is now emerging from finds in England, failure of an altogether more catastrophic kind.

Close examination of the complete but badly decomposed lyre discovered recently at Prittlewell, Essex, shows an instrument that has not merely been affected by minor cracking but seems at some point in its working life to have disintegrated completely, split into at least two, perhaps more, detached pieces (Lawson/Barham in preparation). Because the instrument was found in dry earth, quite unlike the waterlogged surroundings of the Trossingen and Oberflacht instruments, its wooden structure owes its survival almost entirely to chemical preservation. In most places reduced to little more than a dark organic stain on the pale sand of the chamber floor, small patches of solid wood nevertheless persist wherever copper-alloy reinforcements have been affixed: it seems that toxins from the copper have enhanced its resistence to bacterial and fungal attack. At such points the split itself can still be seen. It runs the whole length of the instrument.

The implications of such massive structural failure are not limited to its cause. Like the Trossingen lyre, and perhaps the Oberflacht instrument too, it has been repaired, and thus leads us on from Irrtum into the still more intriguing territory of Folgen. What we are seeing here may be no mere patch, or temporary fix, but a very ingenious, labour-intensive and expensive attempt to rebuild, as it were by stitching it back together. The stitches themselves are of silver and of gilt bronze, with decorative fluting, while a finely wrought frame of thin iron sheet reunites the shattered superstructure. Already we may infer from this that it may be neither an instrument that has been deliberately 'killed' for burial nor one that has been repaired merely for effect, to serve as a mute token amongst purely symbolic grave-goods. These seem to be functional repairs. But did it function, and how effectively would it have functioned as a musical instrument, in such a repaired state? Why have they not just discarded the pieces and built a new one? Were there other, cultural, values which demanded its repair, regardless of cost and utility?

Such questions are made only more pressing by a review of two examples of similar repairs from past excavations in Kent, within the early Anglo-Saxon cemeteries at Mill Hill, Deal, and Buckland, Dover (Parfitt/Brugmann 1997; Evison 1987; Lawson forthcoming).

They also relate to wider evidence of instrument repair, and in other periods besides. One such example is to be seen in a bundle of copper-alloy trumpet sections found in the River Thames at Billingsgate in 1982/3 (Lawson/Egan 1988; Lawson 1991). Thought to date from around AD 1400, only the conical 'bell' and one of the three cylindrical tubes seem wholly original, and even these have been much damaged and restored before deposition. Radiography shows that a small, plain, shield-shaped crest soldered onto the bell itself, at first glance purely decorative or symbolic, is in fact a patch that conceals a repaired puncture. The all-important mouthpiece is so crudely fashioned, and in a weaker alloy, that it is clearly a replacement. Indeed the very context of the find begs questions, lying as it does not in the hold of some sunken ship but merely jettisoned in the mud of the tidal foreshore. How could an apparently complete instrument, in working condition, have come to be there, and how should that affect our interpretation of the object's function immediately prior to its deposition?

It is all too easy for the organologist to overlook such questions when attempting to build a reconstructed instrument and to discern the intentions and capabilities of the original maker. But Billingsgate shows what we have always known: that manufacture is merely the beginning of any artefact's temporal trajectory. Evidence of accident and repair tells us in a very immediate—if sometimes still enigmatic—way how much may have been expected of an instrument during its long lifetime. This has some important consequences.

2. Error and the transmission of musical knowledge

2.1 Anomalies and error in representation: images of lyres

A slow but steady growth in archaeological evidence since the 1970s now permits us to trace the structural development of lyres in considerable detail through at least a thousand years of European history, from the end of the Iron Age to the high Middle Ages. One of benefits to accrue from such progress has been the opportunity to review and test (rather than merely verify or corroborate) the ancient records which formed the foundations of previous models of theory and performance. The results are by no means comforting. They reveal numerous inconsistencies, discrepancies and contradictions which do little to increase our confidence in the written and pictorial sources as documents of living practice. Rather the contrary, in fact. That neither texts nor pictures represent the technological background in any useful detail is perhaps only to be expected. The meanings of key words, such as Old English hearpe ('harp', usually glossed or translated as Latin cithara) and hearpe naegl (most often glossed or translated plectrum) are naturally problematical also: no early medieval excavation has yet produced the slightest shred of evidence for either a harp, in the modern triangular sense, or any form of plectrum associated with a lyre. Indeed, on the contrary, the excavated instruments seem better adapted to strumming with fingers and thumb, while in modern folk traditions the name 'harp' or harpa can be seen attached to instruments of several other kinds, including lyres. If such ambiguities and inconsistencies call into question the value of texts as sources of organological detail, in pictures we find something at least as worrying: indications of misrepresentation. These things can be difficult to reconcile within a single, internally consistent, theoretical framework.

One such inconsistency is in the diversity of shapes and string numbers portrayed in early medieval art. In the earliest clear images, of the eighth century, lyres are consistently shown broader than their archaeological counterparts; and whereas the finds themselves repeatedly bear provision for six strings, the illustrations show anything from five to seven (Lawson 1980: 54; 2004: 65–7). The reasons for these discrepancies are still unclear. Are they errors? Perhaps. Over the next three hundred years some become so poorly drawn as to suggest unfamiliarity, while forms and numbers diverge still further.

Figure 2: King David's harp and a lyre from the frontispiece of an 11th-century Anglo-Saxon manuscript drawing. The harp is competently executed but the detail of the lyre is confused, perhaps suggesting unfamiliarity or at least unimportance. Cambridge University Library MS Ff. I. 23, folio 4 verso. Image: Cambridge University Library.

Some superstructures appear so enlarged and delicate that one doubts whether they could have functioned, either structurally or acoustically. String numbers come to vary from three to ten or even more. And yet as late as the eleventh century the archaeological evidence still indicates a long, narrow, more-or-less straight-sided instrument, always with six strings (Lawson 2005: 104–5).

One could perhaps plead that the instruments in the pictures and the instruments buried in the ground are not the same instruments, or from precisely the same tradition: or, to put it another way, that the pictures show instruments for which there is as yet no archaeological evidence and the remains are of instruments which no artist chose to portray. This cannot be dismissed lightly. Images of triangular harps which survive in the early medieval iconography of King David as Psalmist lack archaeological support until later in the Middle Ages. (For some of these images see especially Steger 1961; Bruce-Mitford/Bruce-Mitford 1970.) Indeed some scholars, such as the American philologist Robert Boenig, have continued to insist that the 'hearpe' of OE Beowulf must signify some instrument other than lyres of Sutton Hoo or similar kind (Boenig 1996, especially 305 ff, 319–20.) But any notion of two or more contrasting lyre traditions, which is to say the presence among the English, Franks, Danes, Swedes and Goths of two parallel yet distinct traditions of lyre manufacture (and use), one of them represented only in the pictures, the other only in the archaeology, becomes increasingly difficult to justify with each new archaeological example that emerges. As geographical distributions expand and cultural associations become more diverse, the form of the found instruments remains broadly the same. Equally, their uniformity seems to transcend context: the physical traces are not restricted to grave finds of the pagan or transitional period (such as at Trossingen and Prittlewell) but are to be found jettisoned amongst the cultural detritus of our earliest towns.[2] Yet still there is no support for the different imaged forms and numbers. There was at one time some discussion as to whether the lyre bridge of antler from ninth-century Birka, Björkö, Sweden, which alone of all the early European lyre bridges possessed seven and not six notches, might be the first archaeological evidence of a separate seven-string tradition (e.g. Crane 1972: No. 313.14; Lawson 1980: 256, note 132; 2004: 67; Lund 1974a: 39, no. 64; 1974b: 25, no. 64). However, this was profoundly undermined by the discovery of the Trossingen lyre. The Trossingen bridge too possessed seven notches, but the lyre itself had provision for only six strings (Theune-Großkopf 2004: 9, caption; 2010: 52–3) . Clearly, some other explanation was now required.

[2] Part of a lyre from eleventh-century Haithabu, Schleswig-Holstein, is described in Lawson 1984; a bridge from Birka features in Lund 1974a: 39, no. 64; 1974b: 25, no. 64; a wooden bridge from York is in Hall 1984: 115; Morris 2000. For an account of a piece from Bremen, comparable with the Haithabu object but almost thousand years earlier, see Bischop 2002.

Of course, the likelihood of error in representation, amplified with repetition, is rarely remote. It may be significant that pictorial variation tends to multiply, and become ever more extravagant, during the lyre's years of real decline. No doubt there really would have been local variation and even hybridisation, here and there, of the sort that might account for the development of our two lingering survivals, the modern Welsh crwth and the bowed lyres of Scandinavia and the eastern Baltic. It is also true that such error, when disseminated often enough, might easily develop the character of tradition in its own right, and yet remain a 'virtual', iconographical tradition only, possessing little or no basis in contemporary reality.[3] On the other hand, even this might have the power to tell us something about the cultures to which it belonged, and about their knowledge of the world that they inhabited. The sketchy execution and subsidiary placement of the lyre in Figure 2, while the harp is carefully drawn in the hands of the principal figure, surely alerts us to the possibility that there has been some change to the lyre's former pre-eminence in England, at least within the illustrator's experience.

2.2 Margins of error in material acculturation: imitation and replication in ninth-century Francia

Amongst my earliest concerns was the need to consider what sorts of music might have suited such lyres, and to consider the tuning systems within which that music might have been framed. It is from these categories of knowledge, performative and structural, that I will take my next examples.

Ancient histories and other narrative records contain no shortage of tales in which erroneous knowledge turned out badly, at least for those who would base their actions in such knowledge. For the most part we might expect such acts to concern daily matters: religious, political, economic and military, as well as matters of architectural structure, of navigation and of the manufacture and operation of things such as ships and weapons; and indeed they do. We might also suppose that it would be unusual to find musical examples, and on the whole this also is true; yet there are some noteworthy and, it turns out, useful exceptions. Two have come down to us from the end of the ninth century, in casual remarks made by the anonymous 'Monk of St Gall', often identified with the writer Notker Balbulus, Notker the

[3] Another series of instrument forms which evolve elaborately in ancient art without archaeological corroboration may be found in Roman and Late Antique images of lyres. For a discussion of these pictorial traditions and a consideration of the 'representativeness' of documentary evidence, including the capacity of material culture to imitate art, see Lawson 2008. See also, more generally, Winternitz 1961; 1967.

Stammerer, who died at St Gall (St Gallen), Switzerland, in AD 912. These remarks touch matters which bear in turn upon important questions of musical origins, tradition and innovation: specifically upon the acquisition of extra-cultural knowledge, or of the formation of knowledge of extra-cultural matters. Whilst the one concerns knowledge of foreign material culture—specifically of musical technologies—the other concerns imports of a more ephemeral nature: acquisition of musical repertoire itself. The occasion is the arrival at the court of Charlemagne of an embassy from the Byzantine empire, which has been variously dated to 802 or 812.

'The aforesaid Greek envoys', the Monk writes,[4] 'brought not only all kinds of organ, but also various other things with them.'[5] Such information is naturally of great interest to the organologist, who can readily imagine the impact that the introduction of such new and unfamiliar designs might have had on the native instrumentarium. Indeed this seems confirmed by the Monk's next statement. 'All of these things the wise Charlemagne's craftsmen copied with the utmost accuracy...'

It is true that the Monk is not a wholly reliable witness in terms of the historicity of events which had ostensibly taken place at least a lifetime before he wrote.[6] He is as much concerned to tell a good story as he is to marshal his sources according to strict chronology. Indeed amongst his known anachronisms is one later in this same chapter, which describes in detail the gift to Charlemagne of a large and complicated organum: probably a true pipe-organ. This most likely refers to an event which actually occurred a generation earlier, involving a gift to his father, Pepin the Short, from Constantine V in 757[7].[8] However, what particularly interests us here is not whether the event may have occurred in 812 or 757 or indeed at any other date, but rather the fact that it is reported at all, and the way in which the writer reports the processes: he throws his remark away quite casually, as though what he is saying seems to him, at the end of the ninth century, in no way unusual, or unlikely, or in need of further explanation. Most importantly, he describes the act of copying not as 'exact' or 'perfect', as we might have expected from a biographer who so obviously venerates his subject, but using the expression 'with the utmost accuracy' (accuratissime). Here he invokes

[4] Monachus Sangallensis De Carolo Magno II.7.

[5] Adduxerunt etiam idem praefati Grecorum missi omne genus organorum, sed et variarum rerum secum. Quae cuncta ab opificibus sagacissimi Karoli, quae dissimulanter aspecta, accuratissime sunt in opus conversa... Text after G. Meyer von Knonau 1920: 38.

[6] The Monk was writing at St Gall between AD 884 and 887.

[7] The Revised Annales regni francorum, anno 757: Misit Constantinus imperator regi Pippino cum aliis donis organum, qui in Franciam usque venit.

[8] The Lorsch Annals, anno 757: Venit organus in Francia.

the concept not just of accuracy but of measures of accuracy—a mode of discrimination which surely carries with it alternative notions of inaccuracy and even of error. Even if we cannot measure how far this accuratissime compares with lesser degrees accuracy, the mere implication is of value to us. We have long recognized that the ninth and tenth centuries mark a period of transformation in the forms of Western musical instruments, both in the iconographical records and in the archaeology, and that within such transformation are some step-changes in design. Amongst the most dramatic are to be seen in the changing form of the Western harp, clearly derived from Eastern prototypes but now incorporating some very new structural adaptations (Lawson 1980: 171–7; Rimmer 1969: 13–27; for other innovations see also Bachmann 1964; 1969). Could error, and in particular inaccuracies of improvisation, necessitated by unfamiliarity, have played some enabling role in such changes?

2.3 Whispering games: on acculturation in Frankish and other musical repertoires

Another of the Monk's asides appears to offer us a rare glimpse of mechanisms of acculturation in an ancient musical repertoire. The passage is worth quoting in full. 'Eight days after Epiphany' he writes, 'when the morning's Lauds had been celebrated in the Emperor's presence, these [same] Greek envoys privately sang to God, in their own tongue, certain antiphons using the same chants and content as [are now used in] Veterem Hominem and those [of our chants] that follow it. The Emperor ordered one of his chaplains, who knew some Greek, to render their substance into Latin, keeping the same tunes, taking especial care to ensure that, as far as their nature allowed (quantum natura sineret), the movements of individual syllables in each of the melodies should not be dissimilar.' He adds 'this explains how all are of the same tone [or key, Lat. tonus], and how in one of them "conteruit" was found in the place of "contrivit"'.[9]

The apologetic tone of quantum natura sineret seems significant, making explicit the notion of margin-of-error that was merely implicit in his earlier use of accuratissime. He even expands the point with an example of the kind of error that can happen in such processes of transmission. What might those processes have been, exactly? Did they reflect some practice

[9] Monachus Sangallensis De Carolo Magno, II.7. Cum igitur Greci post matutinas laudes imperatori celebratas in octava die theophaniae secreto in sua lingua Deo psallerent antiphonas eius melodiae et materiae, cuius sunt: Veterem Hominem cum sequentibus, precepit imperator capellano cuidam suo Grecismi perito: ut ipsam materiam in eadem modulatione Latinis redderet, et singulis eius modulaminis motibus singulas syllabas dare sollicite curaret, ne, quantum natura sineret, in illo dissimiles forent. Inde est, quod omnes eiusdem sunt toni,et quod in una ipsarum pro contrivit conteruit positum invenitur. Text after G. Meyer von Knonau 1920: 38; translation: GL

familiar to him, or were they something he found extraordinary? Were the chants committed only to memory, in the first instance, or were they somehow taken down in writing? Did the memorising or transcription benefit from repetition, or had they to be received at a single hearing? Does secreto give us any hint as to the circumstances? Some translators have thought so; yet it might equally signify no more than a withdrawal to some other, less public place. Sadly, we are given nothing further to go on, beyond the implication that the event occurred, allegedly, on a single day in the liturgical calendar. However, the same set of questions may also be asked of some supporting evidence that has survived independently.

The very chants that the Monk identifies are to be found preserved in the later Roman and Greek liturgies and indeed can be traced back almost to the Monk's own lifetime through the early Byzantine and Frankish manuscript traditions. One such example from his own monastery of St Gall, can be seen annotated with neumes in Figure 3. By comparing each chant closely with its Greek counterpart we can begin to discern patterns of similarity and of difference (one might even say varying degrees of authenticity) which resemble patterns of similarity and difference in other forms of tradition, not least those in the philological domain. Together with other preserved instances of musical transmission, both between cultures and down the generations, such comparisons may reveal something of the nature of ancient musical exchange, in a variety of musical milieux.

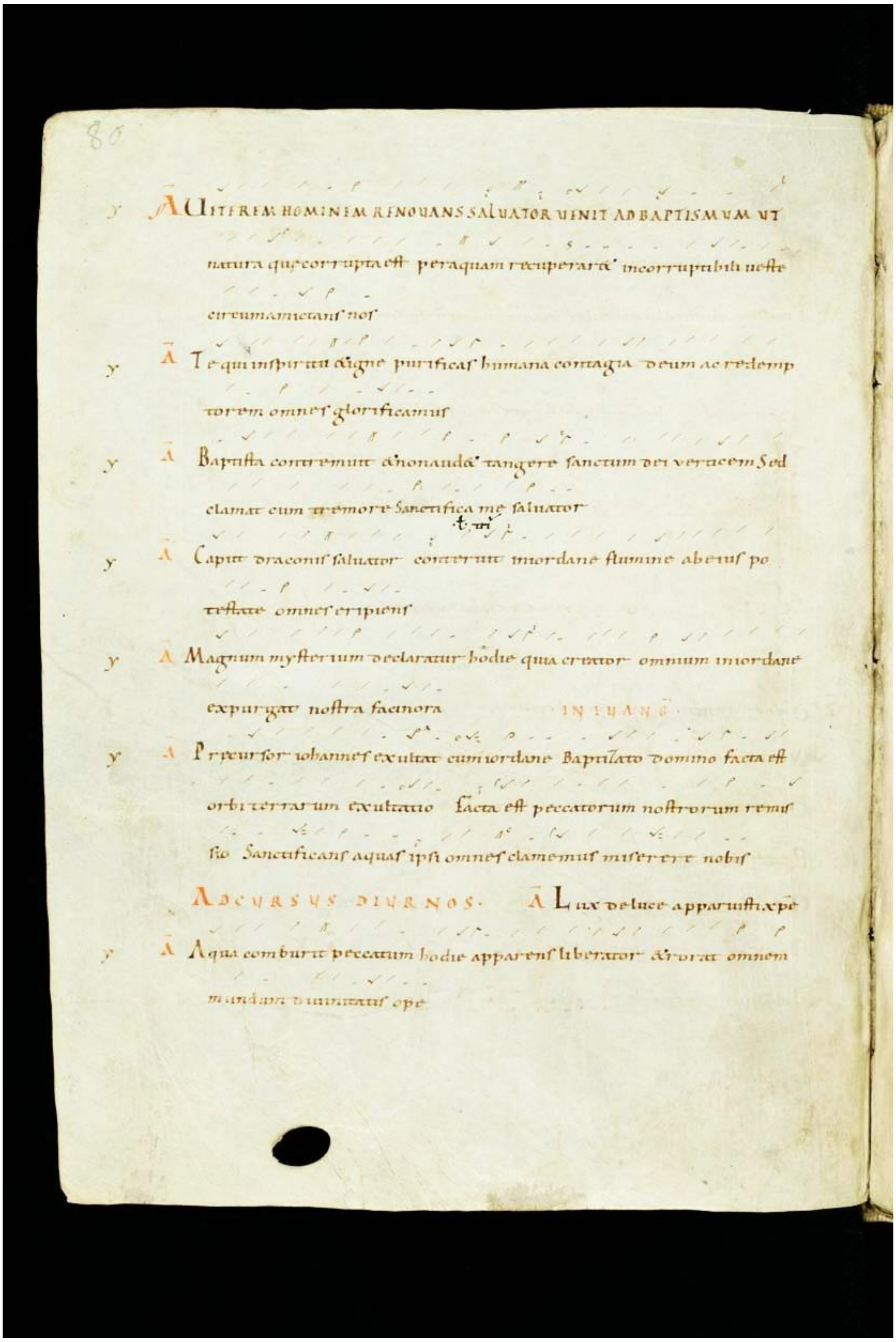

Figure 3: Antiphons for the Octave of Epiphany: *Veterem Hominem cum sequentibus* ('Veterem Hominem and what follows it'). Late 10th-century copy. Text with musical annotation in heighted neumes. St Gall, Switzerland, Stiftsbibliothek, MS Lat. 390, folio 80 *recto*. Image: © [1] Stiftsbibliothek, Sankt Gallen; © [2] Universität Freiburg, Switzerland <http://www.e-codices.unifr.ch/manuscripts/film/csg-0390/csg-0390_080.jpg>; [online source] Cantus: A Database for Latin Ecclesiastical Chant <http://cantusdatabase.org/node/350021> Cantus ID: 005373.

In the late 1950s two papers by the American musicologist and Byzantinist Oliver Strunk explored the matter of *'Veterem Hominem* and those [Roman chants] that follow it' in detail, taking his cue from earlier papers by Jacques Handschin (1947) and Egon Wellesz (1954). What emerges is quite remarkable. In 'The Latin antiphons for the Octave of the Epiphany' (1964; 1977: 208–219) he shows how closely *Veterem Hominem* and its companion *Caput Draconis* translate Greek texts by eighth-century Andrew of Crete (d. *circa* 740); to be precise, the *heirmoi*[1] for the fifth and sixth odes of his canon Εἰς τὰ ἅγια Θεοφάεια.

Even without the Monk's narrative, such close musical connexions between Francia and the Byzantine world at this time ought not to surprise us. There is ample evidence of Greek influence of various kinds in other areas of early medieval Western chant. Contact at the level of music theory (as distinct from the philosophy of *ars musica* and the practicalities of musical composition) is attested, for example, in discussions of Byzantine modes and terminology by Aurelian of Réomé, in his *Musica disciplina* of around 843 (Wilson 1990: 41; Strunk 1977: 153). A Greek friend has advised him, Aurelian tells us, on some of the detail. Indeed we know from one manuscript, the so-called Tonary or *Tonarius* of Saint-Riquier,[2] that the Byzantine eight-mode system or *oktoechos* was already known in the West by 800, maybe even earlier (Strunk 1977: 301). In 831/2 moreover, Amalarus, bishop of Metz, was able to recall a personal visit that he had made to Constantinople 813/4, in particular to Constantine the Great's basilica of Hagia Sofia; and he could report, amongst other things, having heard specific psalms sung there 'in principio missae' (*De ordine Antiphonarii* XXI; Strunk 1977: 319; Cattin 1984: 54; Wilson 1990: 12).

Now when the Monk's 'cum sequentibus' links his named example *Veterem Hominem* to a sequence of other unnamed Latin antiphons, it is evident that he anticipates that his reader will immediately understand which antiphons he means. Indeed we do. They are the series known as the *Antiphonae in Octavas Theophaniae ad Matutinas*, the Proper antiphons for the Octave (eighth day) of Epiphany, which is to say around the Feast of the Baptism of Jesus.[3] They are extensively documented in manuscript.[4] The oldest Western source for the Latin

[1] *Heirmoi* and antiphons are introductory or linking hymns in the Byzantine and Roman rites.
[2] Paris, Bibliothèque Nationale, fonds lat. MS 13159.
[3] Epiphany falls annually on January 6th. The Baptism of Jesus has been variously celebrated, at different historical times and according to different Eastern and Western traditions, but is usually associated with either the Octave itself, January 13th, or the first Sunday after January 6th.
[4] The grouping was changed during the reforms of the sixteenth century, but still retains its original form in the current breviaries of the Cistercian and Dominican orders. In the Dominican Breviary they are specified for Lauds (early morning) on the Octave Day, as the antiphons for the Psalms, occurring in the following order: 1.

texts is Frankish: the ninth-century Compiègne Antiphoner (Paris, Bibliothèque Nationale, MS lat. 17436). From it Strunk takes the texts of each of three titles. The first, *Veterem hominem* itself, he shows to match Andrew's fifth *heirmos,* Ὠδὴ ε, Τὸν παλαιὸν ἄνθρωπον ('Ton palaion anthropon'), while the second, *Caput draconis*, matches Andrew's sixth *heirmos,* Ὠδὴ ϛ, Τὴν κεφαλὴν τοῦ δράκοντος ('Ten kefalen tou drakontos'; 1977: 209–210). The third, *Te qui in spiritu*, he compares with a Byzantine processional *sticheron*, headed Ἰδιόμελον (Idiomelon), also for Epiphany: Σὲ τὸν ἐν πνεύματι (*Se ton en pneumati*) by the eighth-century hymn writer Cosmas of Jerusalem (Strunk 1977: 211).[5]

What can these equivalences tell us about the processes that connected the Latin and Greek versions, and thereby about the nature of cultural connexion between the Latin and Greek musical traditions of which each formed a part? Are they indeed Copy and Source, as the Monk suggests, transmitted at the very moment which he describes? Or might they be simply variants, developed from some older original? In other words: might they be more like his *organum*? Might he simply be inventing an association with Charlemagne, anchoring a known East-West relationship within historical time in order to add verisimilitude and zest to his narrative? Let us look, with Strunk, at some of the detail of *Te qui in Spiritu*.

We can see at the outset that the Latin lines *Te qui in spiritu / et igne purificas* ... agree closely with both the meaning and the structure of Cosmas' text, Σὲ τὸν ἐν πνεύματι / καὶ πυρὶ καθαίροντα ... *(Se ton en pneumati / kai puri kathaironta).* If we then look at the surviving melodies, further similarities emerge. Immediately we can see that both are in the same mode, the ἦχος τέταρτος (*echos tetartos*), and that seems significant, for the Monk claims that all the chants in the group performed in the Franks' hearing were *eiusdem toni* (of the same *tonus*).

More remarkable still, inspection of the melodic lines reveals that the very tune of Cosmas' *sticheron*—not just the words and mode but the actual rise and fall of the notes—is echoed in the Latin tradition. In his earlier paper 'The influence of the liturgical chant of the East on that of the Western Church' (1957; 1977: 151–6), Strunk had already supplied transcriptions in modern staff notation which, if we convert them into a simpler, alphabetical code, give an easier measure of this relationship:

Veterem hominem; 2. *Te, qui in Spiritu;* 3. *Baptista contremuit;* 4. *Caput draconis;* 5. *Magnum mysterium.* They also survive in the Armenian rite (Thorpe 1969: 194, note 77).

[5] Although the Monk declines to name any of these other antiphons, at least one of them, *Caput Draconis*, is indicated by his reference to the discrepancy between *contrivit* and *conteruit*. The text reads *Caput draconis Salvator contrivit/conteruit in Jordanis flumine: et ab ejus potestate omnes eripuit*).

Example A

Roman melody

Te qui in Spiritu: GDDDDCCB DDE CDCD G...

Se ton en Pneumati: GDDDDCC CD EE D DBAG...

Greek melody

The result is at first a little disappointing. It is not so much the similarities that stand out as it is the differences. It is true that they open in a manner that is recognizably the same, and that they then proceed along broadly similar trajectories; but if this really is transcription it is hardly what one would care to call *accuratissime*. Why such discrepancy? Might we be looking at error? If so, of what kind? Could this be what the Monk meant by *quantum natura sineret*?

Strunk offers us another East-West example which may serve as a form of control, having no obvious connexion with either the Monk or the *Veterem Hominem* series: it is the antiphon *O quando in cruce*, which he compares with the Greek ode ὅτιν τὸν σταυρόν (*Otin ton Stauron*). Once again, the texts are similar, though at first glance the the melodic relationship is obscured by the different modes in which they are set:[6]

Example B i

Roman melody

O quando in Cruce: EDCDEEE EFG FEDE DC DCDEE DEE

Otin ton Stauron: BAGABB DDCBABCBAG GAB GACABB

Greek melody

Nevertheless, transposing the Roman chant downwards by four points makes it abundantly clear:

[6] These code characters A to G represent, roughly speaking, the white notes of those names as they occur on the modern pianoforte or electronic keyboard, rising in tones and semitones from A (low) through G (high) and repeating upwards, octave on octave. In this simple notation the familiar nursery tune ‘Twinkle twinkle little star, how I wonder what you are’ becomes CCGGAAG FFEEDDC, assuming that it starts on the note C; or GGDDEEDCCBBAAG if it starts on G.

Example B ii

Roman melody

O quando in Cruce: BAGABBB BCD CBAB AG AGABB ABB

Otin to Stauron: BAGABB DDCBABCBAG GAB GACABB

Greek melody

The full extent of the match is now self-evident, and proves a considerably better fit than the we saw in the previous example. Only three progressions of the Byzantine melody are absent from the Western, while only four of the Western progressions are absent from the Byzantine. How can we explain this? Is it perhaps the result of a different processes of memorizing and/or transcription, or is it due to some difference in the medium of tradition? Or, once again, could they instead be variants of some earlier, but perhaps less remote, prototype?

Two final comparisons from my own experience provide an even closer fit, and bring with them the additional benefit of a rather different cultural perspective. The first involves the traditional Welsh folk song *Ar hŷd y nôs* and an early medieval *alba* or dawn-song, *Phoebi claro nondum orto iubare*, which survives in manuscript from the early medieval period. Although in this case committed to writing by a monkish hand, probably during the 11th century, the *alba* is an ancient secular genre which usually takes as its topic, or reference, the figure of the night watchman and the reluctant parting of lovers at the approach of day. The Welsh folk-song first appears in Edward Jones' *Musical and Poetical Relicks of the Welsh Bards* (1784, new ed. 1794: 73).[7] The similarities are undeniable. In order to compare like with like I set them out here in the same manner as before:

Example C

Welsh folk song

Ar hŷd y Nôs: CBACDCBGAB C / repeat once / ...

Phoebi Claro: CBACDCBCABA / repeat twice / ...

Provençal *alba*

[7] This resemblance was first drawn to my intention by Wendy Lawson in 1982 during a collaborative study of ancient melody; an instrumental variation on the *alba* melody was included our recording *Sounds of the Viking Age* in 1985. For another possibly very ancient survival amongst the *Cantigas de Santa Maria* of Alfonso X el Sabio, of Castile, see Lawson, W., 1987.

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FEFGA GFEFEDCED CB              / reprise line 1
   GAA        BCBCDDDCCBDCAGABCA / repeat
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Except for their final note, the opening eleven-note lines show no discrepancy at all. Could this similarity be mere coincidence, unrelated except by chance? Other aspects suggest not. Both songs are set (or at least begin) in the same melodic mode. Both melodies are organized strophically and both have internal repetition. Moreover, the folk-song, like the *alba*, deals with a nocturnal theme: its refrain (also its title) is well known in Anglo-Welsh tradition as 'All through the night'. Jones himself calls it 'The Live-long Night'.[8] Finally, there is nothing in any of the changes that resembles error, such as we saw among the *Veterem Hominem* chants. And yet, such survival would surely be remarkable in two melodies separated by so many centuries. I have so far been unable to find any intermediate examples. The implication is of connexion through some so far unrecorded oral tradition. It was, of course, from just such a tradition that Jones or his informant seems to have sourced much of his material. But how could a purely oral record have maintained such lengthy and robust continuity?

The idea that any modern folk song could have been sustained in popular imagination since the early Middle Ages (or indeed before) risks stretching the credulity of any archaeologist accustomed to the more material aspects of ancient material culture, to say nothing of the cultural historian accustomed to webs of written connexion. Yet that credulity may be stretched still further by my next and final example. To students of ancient European music performance and re-enactment, perhaps the best known of all ancient musical survivals is an epitaph of the Hellenistic era from Aydin in Turkey, ancient Tralles in Asia Minor. The inscription, on a column of marble, commemorates the life of one Euterpe, deceased, by or for her survivor Seikilos, and includes the text of a *skolion* (or banqueting song) in capitals, thus: ΟΣΟΝ ΖΗΣ ΦΑΙΝΟΥ / ΜΕΔΕΝ ΟΛΩΣ ΣΥΛΥΠΟΥ / ΠΡΟΣ ΟΛΙΓΟΝ ΕΣΤΙ ΤΟ ΖΗΝ / ΤΟ ΤΕΛΟΣ Ο ΧΡΟΝΟΣ ΑΠΑΙΤΕΙ (*Hoson zes phainou, miden holos silipou / pros oligon esti to zen / to telos o chronos apaitei*). Unusually the engraver has inserted between the lines a sequence of Greek alphabetical characters which preserves both the pitch and the rhythmic structure of the entire melody. The form of this notation is now well known, and well

[8] In his New Edition of 1794 (author's own impression, p. 73, col. 1, under 'Welsh Sonnets' or *pennillion*) Jones also indicates that the text of the sonnet 'Nid ai i garu vŷth ond bynny' is to be sung 'to the tune of *Ar hŷd y Nôs*'. He gives the music, together with a set of instrumental variations, on pp. 151-2, with the words *Er bod rhai yn taeru'n galed, ar hŷd y nôs ...*

understood, as is the melody itself. Indeed the tune is beginning to suffer a little from repeated use; for as it becomes ever more familiar it can sometimes seem difficult to discern much artistic merit in it. But is it fair of us to treat it in this way, as representative of Hellenistic music? Might it not simply be a personal favourite, perhaps even a composition of Seikilos himself? It seems, somewhat to our surprise, that the answer is no: it was not only well known in its time but must also have been widely known; and it survived. Performers of the Greek text today seem times be unaware that it exists in another quite separate version, but it does. It is to be found in a very different tradition, in the Western Christian liturgy, as the Palm Sunday antiphon *Hosanna filio David* (Hosanna to the Son of David):

Example D

Western antiphon

Hosanna filio David: GGDDDCBCDEDCBCDEDCDCBCAGGAF ...

Oson zes phainou: GgDDd BCD dCBCD C cB AGgAF ...

Hellenistic skolion

```
...ACCABABGGG GBDCBCCBGAFGBACGDDECABAGGGED
...           GBDCBCcBGAFGBAC Dd   B GGGED
```
[9]

Are these the same? Surely they are. Every note of the *skolion* melody occurs in the antiphon, in its correct order (Reese 1941: 115). Moreover an even fuller match than this can be realised by holding over those notes of the *skolion*, such as the initial G, where the corresponding note in the antiphon is double-struck (these extensions are indicated here in lower case). Of the antiphon's remaining accretions, seven are one-, two- or three-note adaptations to changes of syllable. The only major innovation is the ten-note phrase ACCABABGGG which we may see as an interpolation of fresh material to accommodate the different wording (it coincides with the phrase *in nomine domini*). Afterwards it reverts instantly to the melody of the *skolion*.

Such behaviour seems to me to bear all the hallmarks of genuine adaptation, indeed of *contrafactum*, the application of melody to different texts. One can even hear in the text of the

[9] Characters underscored are to be imagined sounding one octave lower than their unscored equivalents. The full text of the antiphon is *Hosanna filio David/benedictus/qui venit/in nomine domini/rex israel/Hosanna/in excelsis.*

antiphon, despite its different language and meaning, some of the sound of the *skolion*, as the opening line *Oson zes phainou* seems to foreshadow *Hosanna filio*. None of this can be coincidental. Yet if the date of the Hellenistic original is correct, and if the connexion is indeed oral (for here too I have been unable to locate any intermediate version)[10] how can we explain the fewness of errors? Was authenticity maintained by some now-lost written connexion? Or may that authenticity be telling us something about the nature and robustness of oral transmission, under the right social conditions?[11]

3. Error and the tuning of melody

Whether they are in alphabetical code, non-alphabetical symbols, tablatures or undulating lines of dots, any practical exploration of these ancient notated melodies quickly raises questions as to the exact pitches that they represent, and the precise relationships or intervals between them. In general, most traditional European melodic systems today employ units approximating the tone and the semitone, five tones and two semitones making up each *diatonic* octave. The result is the *heptatonic* (seven-interval) mode or scale. But how long have humans been organizing melody in this way? Here archaeology has an important contribution to make.

In any heptatonic mode or scale, spanning the octave from, say, middle C to the C above it there will be eight such notes and seven intervals,[12] and in our common C major series based on the notes *do*- (or *ut*-) *re*- *mi*- *fa*- *so*- *la*- *ti*-, the seven intervals are tone, tone, semitone, tone, tone, tone, semitone. (In a minor mode in which the third and seventh notes are lowered (or 'flattened' by one semitone) it will be tone, semitone, tone, tone, tone, semitone, tone.) Now, on the pianoforte keyboard today all those tones measure 200 cents, all the semitones 100, resulting in a twelve-semitone octave of exactly 1200 cents; and to anyone accustomed to such intervals a melody that uses them will seem both in-tune and natural. However, in reality they are neither of these things but rather the results of a sequence of

[10] Popular tradition in Turkey does suggest a much later survival of the *skolion* into the modern Anatolian folk repertoire, in the love song *Uzun yillar bekledim*. I analyse this connection elsewhere (Lawson, *forthcoming*). It may be related to the *skolion*, the opening *Uzun* recalling both *Oson* and *Hosanna*; but then again it may not. In any case it shows no sign of any direct relationship to the antiphon.

[11] For discussion of some of the many attendant issues, which are far beyond the scope and purpose of this chapter, see especially Huglo 1967; 1973; Stevens 1986; Treitler 1974, 1981. For a recent discussion of contrafactum in relation to the ancient world, see Mirelman 2008.

[12] In some cases one or more of these notes may be omitted, for example in the so-called pentatonic scale which generally omits the fourth and seventh notes, or in more elaborated systems where additional notes (such as a lowered third or a raised fourth or seventh) may be inserted according to need.

compromises worked out during the 17th and 18th centuries to enable keyboard instruments to sound as far as possible equally concordant in all modes. However, folk musical practices from around the world, including Western Europe, warn us that such values are rarely ever applied in the popular domain. Folk-musical octaves may well measure 1200 cents, since the octave is the most universal concord of all; but within that total there may be substantial variation from interval to interval. This variation can therefore produce, to our equal-tempered ears, some curious results. It is nevertheless borne out also by the archaeological evidence, as I found in the 1980s when I began to explore tonalities of bone pipes and wooden panpipes. Naturally, such ancient variation poses some fascinating challenges. Does it occur, as it were, accidentally: the unintended outcomes of manufacturing processes? Or does it develop consistent and identifiable patterns? How significant were such variations to the people who experienced them? How far was their adoption (or rejection) a matter of cultural, or even individual, preference? Error and accident, it turns out, were recurrent features of tuning in the crafts and industries that served at least some ancient musical traditions.

3.1 Bone pipes: Jiahu

Since the mid-1960s, excavations of a prehistoric settlement and cemetery complex at Jiahu in Henan province, China, have revealed numerous graves of Peiligang culture that contained musical pipes, buried with the dead (Huang 1989; Wu 1994; Zhang/Harbottle/Lee/Kong 1999; Zhang/Xiao/Lee 2004). The whole ritual complex spanned the Farming Neolithic and early Bronze Ages, which is to say between about 9000 and 6000 years ago in that region. All the pipes had been made from the same kind of bone, the ulna,[13] from the same kind of bird: the crane (*Grus* sp.). Each bore a series of between five and eight finger holes.

It should be stressed that no written music, or even description of music-making, survives from this early time, and indeed for some thousands of years still to come, so we are entirely dependent upon the archaeological material when we seek to understand and model the musical capacities, experiences and knowledge of the people who used them. However, this does not present as bleak a prospect as it might appear. It is true that we lack the tunes that ancient people played on such pipes, but the finds possess several qualities that more than compensate. These afford us access to details of musical structure and to nuances of expres-

[13] The ulna is the thicker of the pair of bones that in mammals connect the elbow to the wrist. Naturally hollow, and typically around 30 cm long, the ulnas of cranes, swans and the larger birds of prey are repeatedly selected for musical exploitation across the whole of Eurasia, from the Upper Palaeolithic to the post-medieval period.

sion which no ancient text could ever provide, and they benefit additionally from unambiguous spatial, chronological and cultural attribution. Indeed they take us close to the very heart of ancient musical experience, even in periods whose written documents and images are plentiful.

In describing their pipes the Jiahu authors report that most are sufficiently complete as to enable reconstruction of their original tunings, and that these tunings have clearly been engineered, and planned. Their evidence for this takes two main forms: firstly, they say that the positioning of the finger-holes in each instrument follows a similar pattern; secondly, feint marks around the holes suggest that those patterns were laid out before the holes were bored. It is further supported, they argue, by another find which they interpret as a 'pitch pipe', either for pitching the human voice, in song, or for adjusting the tuning of other pipes in manufacture. In drawing their conclusions, they see the most important outcome to be the musical scales that result from these patterns, which, from data obtained by playing the original instruments,[14] they show to be made up of series of intervals with semitone values close to that of the modern hundred-cent semitone. They further argue, from the same data, that the musicians of Neolithic and Bronze Age China not only employed a measure of chromatic tuning but even, through time, achieved fully equal temperament of the kind which now characterises Western keyboard music.[15] I must stress that this extreme level of ingenuity does not entirely accord with my own reading of the published data. Yet it also seems to me that the authors underestimate the broader implications of their finds. What is surely remarkable is not so much the apparent modernity of any tunings that the makers are supposed to have achieved: it is rather their having set out to achieve particular tunings of any sort. It seems that from as long ago as the Neolithic, Chinese pipe tunings exhibit Intention. Their makers began with schemes in mind, and had some means of projecting them onto the geometry of the unworked bone. How widespread might such discrimination and prediction have been in the ancient world, and how far back in time might we trace them through the archaeological record?

[14] For reasons which I will explain, the playing and handling of ancient instruments in this way is damaging unless steps are taken to protect the surfaces from friction (see Lawson 1986, 1999, 2008). The authors do not mention such precautions. Since by varying the flow of air the experimenter can also, within certain limits, vary the pitch that an individual finger-hole will produce, it is also an exercise fraught with subjectivity. Again, the authors do not attempt to express or acknowledge such deviation in presenting and interpreting their data.

[15] Chromatic tuning uses the twelve-semitone scale to enable a mode or melody to begin on any note and yet sound broadly the same, merely higher or lower. Equal temperament is a standardisation of chromatic tuning which enables a mode or melody to sound precisely the same, merely higher or lower. The identical values of equally tempered intervals opens up the possibility of more advanced harmonic development, which would previously have sounded out-of-tune.

3.2 Bone pipes: medieval finds from Ipswich and the East of England

Within European archaeology are several comparable samples of pipes with finger holes, spread out across almost the entire timespan of Europe's occupation by anatomically modern humans. The earliest pipes of bird bone and ivory belong to cultures which we associate with the Aurignacian industry of the Upper Palaeolithic, some 36,000 radiocarbon years ago, most of them from a small number of cave sites (for the recent literatures see Lawson/d'Errico 2002; Lawson 2004; Conard/Malina 2008). From the Iron Age, the cultures of ancient Greece and the Roman Empire have left us many finely made pipes of wood, bone, ivory and metal: the various *auloi* and *tibiae* of Classical literature (for which likewise see Byrne 2000; 2002; Psaroudakes 2002; 2008; Hagel 2004; 2010). But perhaps the largest and fastest-growing sample of all has been the bone pipes, complete and fragmented, that have emerged from excavations of medieval towns and cities. In spite of their obvious temporal separation and some major differences in their designs and manufacture, all three groups present morphological similarities that face us with the same interpretive challenges and seem in turn to respond to the same analytical approaches. Some challenges they share with stringed instruments, such as the Prittlewell lyre: these include especially questions of intention, of accident and of error. I will conclude my series of case studies by extracting from the medieval sample one of the British finds which I have made the subjects of detailed microscopic study.

In 1987 two bone pipes of an unusual type were recovered from eighth-century Ipswich, Suffolk, at the start of what is known to English archaeologists as the Middle Saxon period. They had been deposited, with other occupation debris, in two adjacent pits, perhaps during an episode of clearance of the site. At any rate, there was no suggestion of any ritual in the filling, neither was it a site of manufacture: the pipes seem simply to reflect an aspect of everyday life.

It is a remarkable find for several reasons. Firstly, the two pieces undoubtedly form a pair, made from a matching pair of bones (deer metatarsus, left and right), shaped using the same tools, and bound tightly together before completion. Secondly, under magnification the surfaces between the finger holes can be seen to have been marked at some time with a series of fine scratches that correspond closely to some of the markings we would later see at Jiahu. Since these marks are so delicate as to be nearly invisible to the naked eye, we may conclude that they neither served a decorative purpose nor had any bearing on fingering in performance. However, during the process of manufacture they would certainly have been useful in positioning the holes. Thirdly, close inspection of the holes themselves reveals two important anomalies. One hole is quite markedly displaced, which would have affected its pitch significantly.

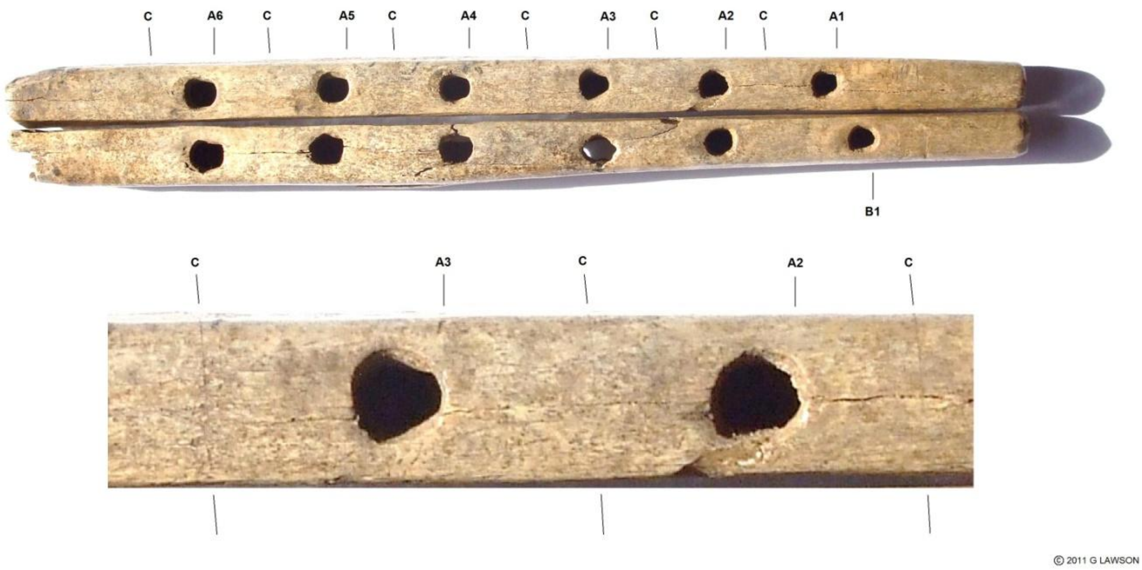

Figure 4: Reed-voiced double pipes from Anglo-Saxon Ipswich, Suffolk, showing errant placement of one of the first and fifth finger holes and fine adjustment of others. Ipswich, St Stephen's Lane, 8th century AD. Deer metatarsus. Length: 17.1 cm. Unpublished. IAS 3104/183 & 272. Photo.: G. Lawson, courtesy of Suffolk County Council Archaeological Service.

Indeed the mismatch between it and its pair would have yielded a raucous tone when played together; and yet it seems to have been deliberate. More generally, the bores of several of the holes have been modified, enlarged by undercutting, all of them on the same (proximal) edge. In later instruments this is characteristic of fine-tuning. Fourthly and finally, large areas of the bone are highly polished, evidently through repeated handling and playing.

Figure 5: Ipswich double-pipes, detail showing surface modifications due to handling and playing. Photo.: G. Lawson, courtesy of Suffolk County Council Archaeological Service.

It is not possible to calculate with certainty the pitches that such adjustment would have generated, as we would in a flute, because this instrument is not a flute. It lacks the sound-producing apparatus of a flute: either the ducted mouthpiece of a block- (or fipple-) flute or the sharp lip of edge-blown flutes and Panpipes. It is a reed-pipe, a double reed-pipe, perhaps even the chanter (melody pipe) of a bagpipe; and the reed is missing. Whereas in flutes the survival of both the air column and the sound source enable retrieval of absolute tuning data, reed-pipe tunings are dependent upon the nature of the reed which, being usually absent, cannot be replicated reliably. But this has less bearing on matters of *Irrtümer und Folgen*. The vital element for us is rather the identification of adjustment, for adjustment suggests remedy, and remedy identifies Error. Having been placed according to a pre-existing plan, of the kind reported at Jiahu, the holes have been carefully adjusted for error. That such correction has been necessary indicates discrimination. That it is so slight suggests either that the tolerances have been very fine or that the initial placements were already close to the permissible range. The sceptic might doubt the efficacy of such adjustment, were it not that the surfaces of the instrument bear enough wear and polish to provide ample demonstration, and indeed a measure, of its successful deployment.

In the hand-crafting of pipes from naturally hollow bones, whether the bones are from wild birds or domestic mammals, most of the work of preparation can be seen to have been achieved with a sharp cutting edge, either a metal knife or, in earlier cases, a stone blade. To the naked eye this is evidenced by patterns of near-parallel facets covering the exposed cortical bone, running parallel with the bone’s long axis and circling around the various cut features: the ends, sound holes, finger holes and finger-hole platforms. Under the magnifying glass these facets are characterised by parallel striae, caused by tiny imperfections in the blade. Often the striae remain crisp, but in other cases they are less so. Under the low-power microscope some prove to have become degraded through polishing, resembling a much-worn inscription. In some rare cases it can seem as though the whole instrument has been deliberately burnished; but in most the smoothing is confined to the ridges between the facets, to other exposed features and to particular areas of the instrument. Some localised polishes seem to relate both to general handling, in antiquity, and to fingering during ancient performance. Under the higher resolutions permitted by scanning electron microscopy, areas of high gloss resolve themselves into matrices of criss-crossing scratches, each relating to a single event or movement. Sometimes these are complicated by subsequent degradation from natural processes in the earth (known to archaeologists as taphonomic processes), and all too often they are also compromised by unwary cleaning, and even attempts at playing, in the conservation laboratory. Nevertheless, by selecting and comparing pristine fragments, and by means of experimental recreation, it becomes possible to characterise and distinguish between performance wear and these later sources of contamination (Figures 6, 7).

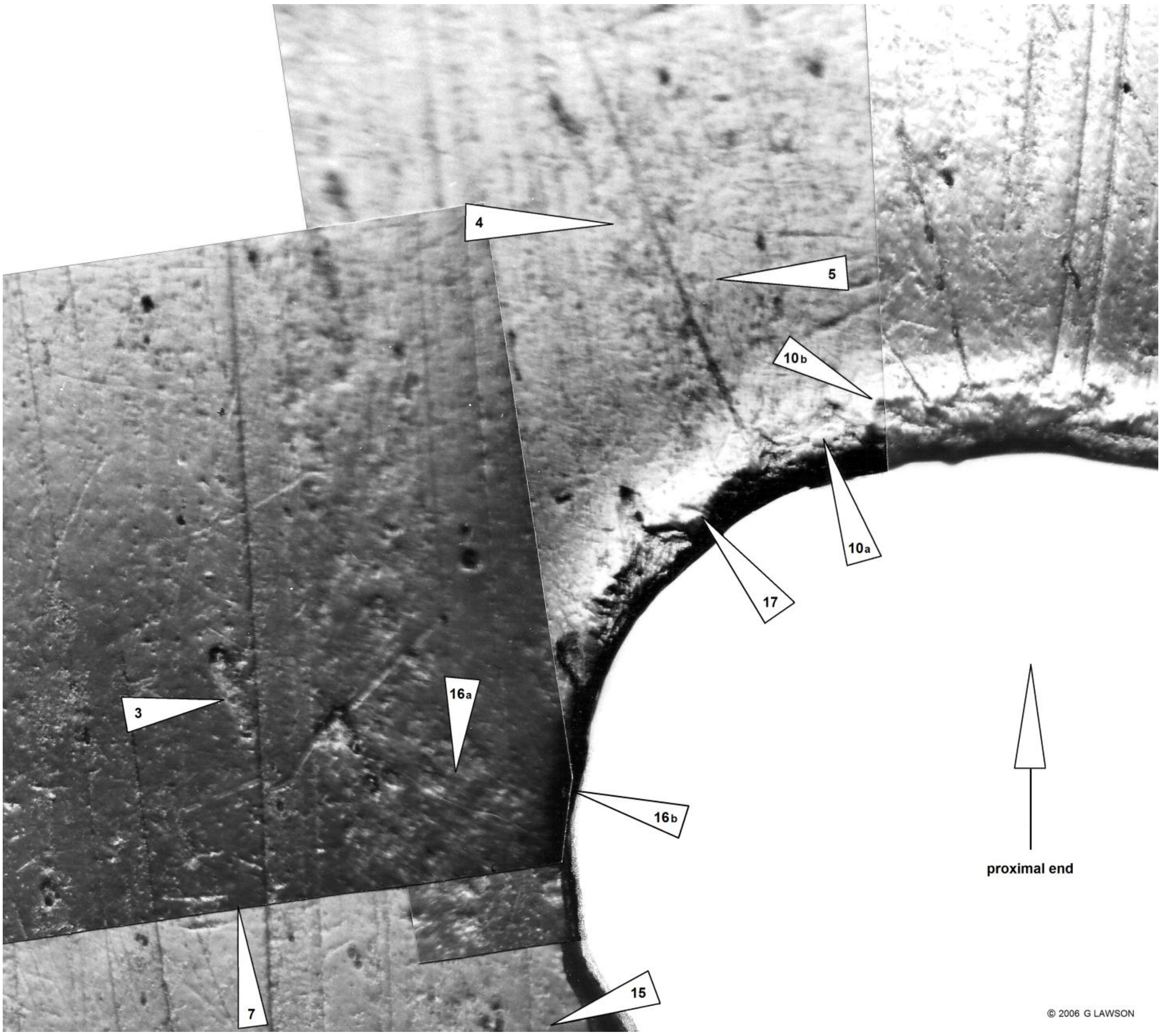

Figure 6: Optical micrographic mosaic, mapping use-wear around one of the finger holes of a medieval bone pipe. Such maps are essential aids to navigation when surveying at the higher magnifications afforded by scanning electron microscopy. Images and montage: G. Lawson, using acetate peel impressions viewed in transmitted light. Dimensions of image: approximately 3 x 3.4 mm.

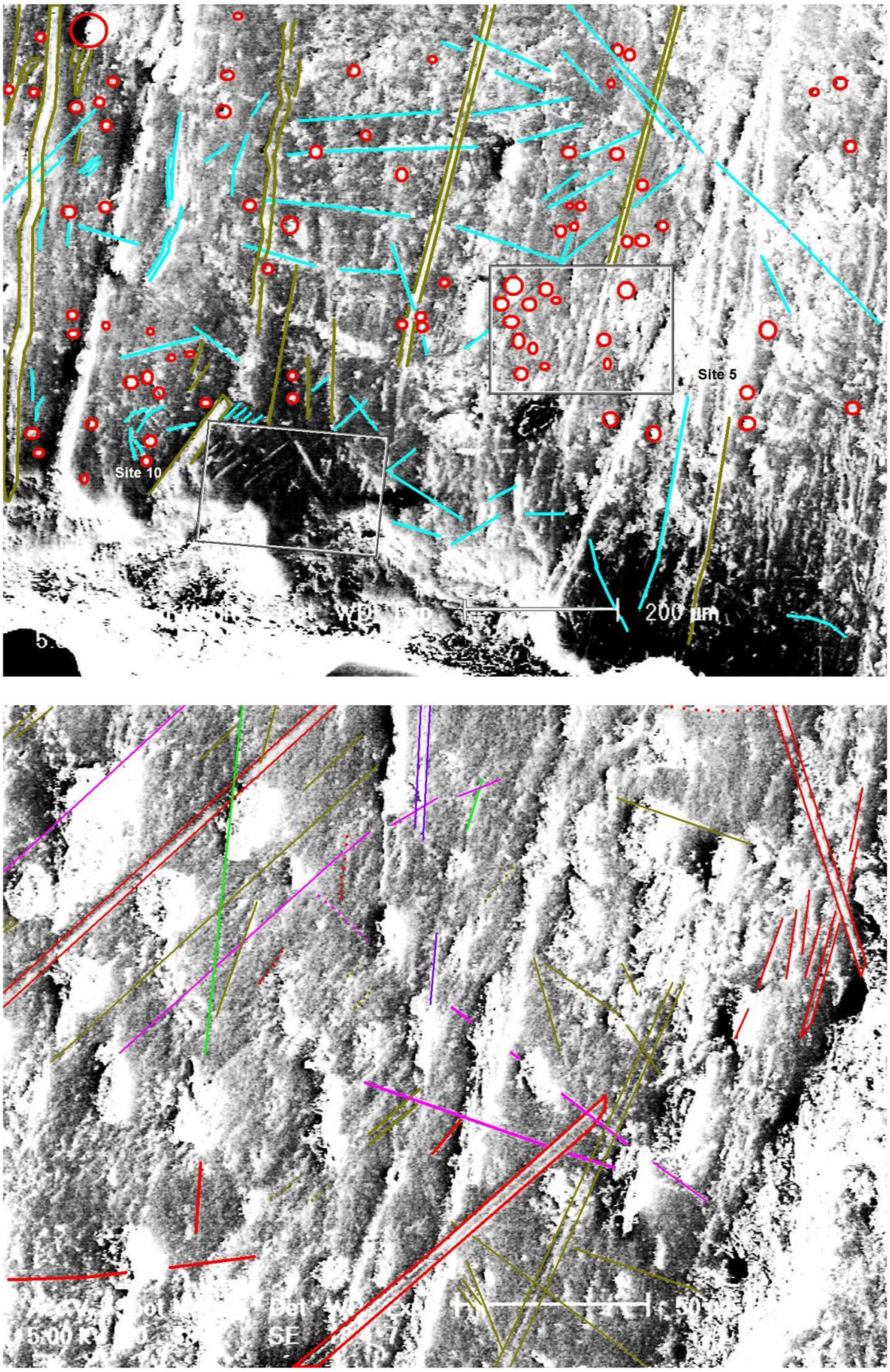

Figure 7: Scanning electron micrograph of an acetate peel impression from the finger-hole margin shown in Figure 006, showing micro-scratches in polish close to the edge of the finger hole. Images: G. Lawson, at the Cambridge University Multi-Imaging Centre. Scales: upper, 200 µm; lower, 50 µm.

The more we look at bone pipes of all kinds the more important does discrimination between success and failure become, and the more important is use-wear to that process. Setting aside the obvious benefits that such studies promise to bring to the choreography of fingering in ancient performance, they are also enabling us to identify and separate out an important category of instruments: those that have somehow failed to enter musical service. Why might they have failed?

For me the question first arose in the late 1970s, whilst studying medieval finds from the county of Norfolk. A flute of sheep (or goat) tibia from the castle at Castle Acre and a smaller flute of goose ulna from the town of Thetford both exhibited small knife-marks next to some of their holes, evidently put there before the holes were made. They looked like location marks.[16] Were this to be the case, it would mean that the instruments had been tuned according to some kind of rule, or at least that it had involved a measure of planning (Lawson 1982; 1993: 160, 163 Fig. 161/17). Yet, for all that, experiments with replicas showed that their tunings did not correspond closely to any known scheme. They seemed to be *irrational*. Other investigators had already begun to resign themselves to the same observation. In 1961, Vincent Megaw seemed to find a broadly diatonic tuning in pipes from White Castle, Monmouthshire, and elsewhere (e.g. Megaw 1961: 179–80); but within only a few years he found irregularity the norm. In 1968, reviewing both the published literature and the results of his own detailed survey, including especially his studies of tunings of complete pipes preserved in the Netherlands, he found the outcome 'entirely inconclusive' (Megaw 1968a: 337–9; 1968b: 125). Returning to the issue in 1990 he reports that 'experimentation... has shown an apparently total lack of consistent tonal patterning which would allow useful comparison with what is known of contemporary tonal systems in Europe... All that can be said about the tonal range [of Winchester pipe 2259] is that it is non-modal' (1990: 437). What could this mean? Might the rules of finger-hole placement be merely geometrical, without thought for their consequences? Or might there be some other cultural process at work?

An attendant consideration seemed to me to be the way in which complete instruments such as these were so often to be found in pits and middens, jettisoned amongst general waste. Broken pieces presented no such interpretive difficulty in such a context, but functional completeness required some explanation. A site at Schleswig in northern Germany offered the first breakthrough.

[16] That such marks had not previously been noted, and indeed are only rarely preserved, does not detract from their identification as location marks, since in most cases such marks would be obliterated by the very process that followed them.

In 1979 Christian Reimers produced a first archaeological report on the Schleswig finds, following it a few years later with a fuller treatment in collaboration with Volker Vogel (Reimers/Vogel 1989; also Lawson 2004: 73–5). Pipes had also been found at nearby Haithabu, the *Hedeby* of the Danes, and in two other sites within medieval Schleswig itself; but excavations along Plessenstraße had struck musical gold. Amongst the extensive bone-working residues found there were more than sixty separate finds of whole or spoiled or unfinished flutes. Mostly of sheep or goat tibia, they ranged in date from the eleventh century to the thirteenth. The unfinished pieces seem caught at the moment of rejection, part-way through the process of manufacture. What test did they fail? We have no certain access to any of the instruments that passed such tests: presumably these simply moved out into the wider urban landscape. But we can see those points in the process where rejection occurred. Are they, in any manner of speaking, the same moment that Strunk earlier identified in the Monk’s account of *Veterem Hominem*?

A review of all the Schleswig data left the finger of suspicion pointing firmly at the voicing and tuning processes. In most of the unfinished instruments, apart from those that are merely roughed-out, work has either ended at the point when the voicing apparatus has just been completed and before the finger holes have been bored, or it has stopped part-way through the drilling of the holes. Of the latter process various stages are represented. Most importantly, they show the remaining hole positions already marked up for boring.

Figure 8: Unfinished bone flute from medieval Schleswig, Germany (Reimers/Vogel 1989: No. SR11): detail showing marks predicting the positions of the finger holes. The two surviving marks are three-quarters of an inch (19 mm) apart: the distance from the centre of the finger hole to the nearer mark is five-eighths of an inch (16 mm). Sheep or goat tibia, Plessenstraße, twelfth century AD. Image: G. Lawson, courtesy of Schleswig-Holsteinisches Landesmuseum.

That mistuning is a likely enough source of failure is consistent with experience gained during experimental replication in the laboratory. In pipes of naturally hollow bone, especially those made from the tibiae of domesticated breeds, there is considerable internal variation, according to the animal's breed, age, health and growth. This makes it more difficult to predict the outcome of a given finger-hole position than would be the case using a bone from a wild animal. The sheep or goat tibia carries the further geometrical complication of a bi-conical shape, narrow at the mid point and expanding towards each end. Such characteristics might seem to make it an unsuitable material for so sensitive a musical application. Nevertheless analysis of butchery waste shows that in most medieval settlements domestic animal bones had the great advantage of abundance. It is reasonable therefore to suppose that makers could afford the occasional failure, and even experiment. The worry, of course, is that it is perfectly possible that many pipes that passed the maker's testing may still have gone on to fail the requirements of the performer in practice. If so, it is sobering to think that in the wider archaeological record such failure might be represented as well as, or even better than, success. It is also a sobering thought that there may have been varying degrees of success and failure. Clearly we need some method to help us sort the chaff from the wheat. Here analysis of surface wear promises a powerful method of both discrimination and measurement.

The capacity of something as simple as polish and friction-wear to distinguish success from failure when applied to tunings that bone flutes (and indeed other instruments besides) preserve, has important consequences and benefits. Once we can begin to understand what they mean, the ubiquity and abundance of medieval flutes permits statistical analysis of both tunings and distributions. Their evidence is anchored firmly and, for the most part, unambiguously in geographical space and historical time. Moreover, an initial urge to exclude from our calculations any pipes that prove to have been unused soon gives way to an appreciation of what they might be able to tell us from as it were the other side of the coin, about the limits of tolerance in these matters. Whilst this kind of work is still at an early stage, already we can see that it is going to have an impact far beyond the Middle Ages. It has especial implications for the interpretation of those most ancient of all indicators of our human use of sound, the pipes of the Upper Palaeolithic.[17]

[17] For a discussion of the Isturitz series and a review of the literature, see Lawson/d’Errico 2002 and most recently d’Errico/Vanhaeren/Henshilwood/Lawson et al. 2009.

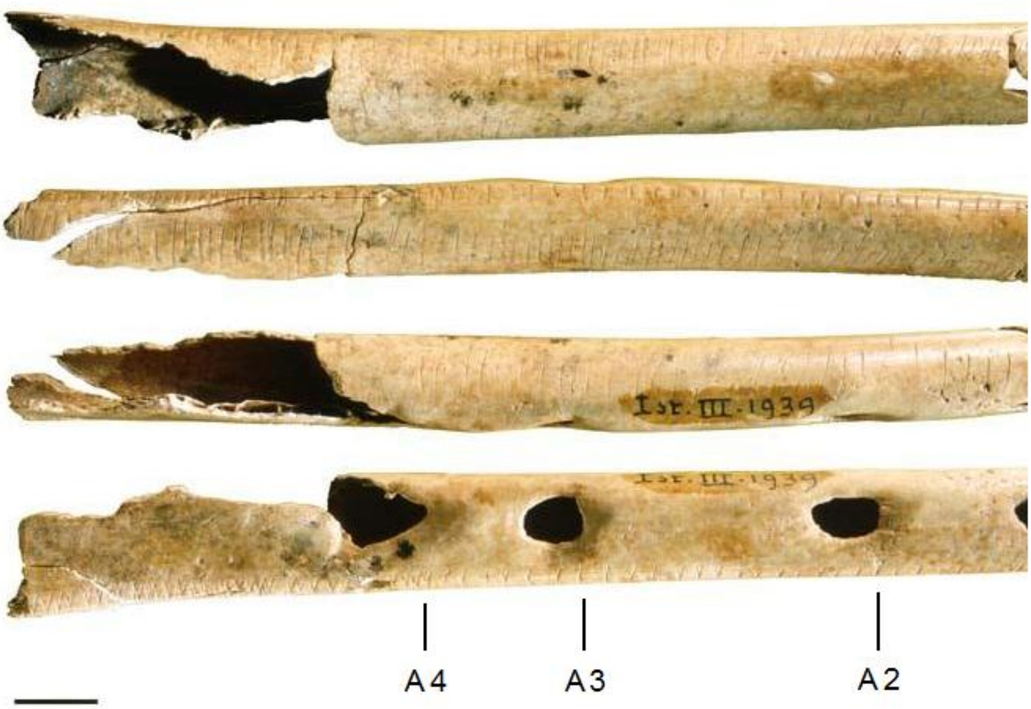

Figure 9: Detail of a bird-bone pipe from the Grotte d'Isturitz Pyrenées Atlantiques, showing possible adjustment of the proximal edge of one of the sound-holes. Paris, St-Germain-en-Laye, Musée Nationale des Antiquités. Contemporary with the Gravettian industry, around 26,000 radiocarbon years before present. Image: F d'Errico, courtesy of Musée Nationale des Antiquités.

Conclusions

Of the errors and accidents that I have described, all have had the appearance of errors and accidents of the moment: evidenced in disparities between intention and outcome, impacting upon individual actions, perceptions and processes. As such they may seem to have had little bearing upon the wider issues of understanding and construction that informed (and no doubt misinformed) ancient philosophical discourse on music. Nevertheless I believe that they offer a beginning, and may already have signalled some applicability both to our own theories of the past and to our investigative methods.[18] In each of my examples it transpires that our

[18] Within the field of music archaeology and acoustics Classical philologists are already embracing archaeological materials and data in their approaches to Greek and other theories of music (see for example Hagel 2004;

ability to identify consequences of, and responses to, accident and error gains us a measure of access to the character of those underlying errors and accidents, their contexts, and the cultural normalities within which they occurred.

Amongst the hundreds of surviving medieval bone pipes, evidence of correction, adjustment and rejection is alerting us to the subtlety and variability of practical norms, allowing us to infer, and then begin to evaluate, margins of error, or tolerances, in otherwise unrecorded technical details—such as especially systems of tuning and nuances of tonality. This is not to say that we are any closer here to finding correlations between medieval popular practice and the rationalised systems inherited from Greek and Roman musics. Far from it. But it is at least possible to propose that their hitherto baffling diversity may now be due not to a lack of system, or purpose, but rather to variability, an almost infinite variability, of systems and purposes, within a real world of varying cultural and even individual preferences. When extended to other archaeological finds this offers increasing opportunities to trace such behaviours back to hitherto unsuspected—and otherwise unknowable—musical traditions of our remotest prehistory.

Amongst stringed instruments, error in the execution of images of Roman and medieval lyres, in both cases increasing through time, hints at a growing distance between artist and subject that may well correspond with periods in the instruments' decline. In identifying and exploring ancient damage and repair in their preserved remains we move closer to an appreciation of the tolerances within which they were expected to function as acoustic systems. At Prittlewell, measurement of wear on the repairs themselves, now in train, will soon enable us to gauge the temporal separation between repair and burial.

These same early medieval lyres, both images and finds, link us directly into Latin and vernacular oral traditions of liturgical chant and heroic and lyric verse, whose mnemonic aspects are hinted at, sometimes captured, in surviving documents. From Late Antiquity to the nineteenth century, such documents show the transmission of melody expressing considerable variation in degree of error, from the corruption of *Te Qui in Spiritu* to the almost faultless *Hosanna filio David*. Such variation may indicate that patterns of acculturation varied according to the frequency and circumstances of transmission. It is tempting, of course, to explain the remarkable authenticity of *Hosanna filio David* as implying the existence of some lost written record linking it with its ancient source: but, quite apart from the absence of notation

2006; 2008; 2010, especially chapters 9-10, 327-441). Hagel's contribution seems to me to be the closest we have yet come to a fully integrated, finds-based, as it were archaeological, critique of any ancient music theory.

in common use before the tenth century, this would surely be to underestimate the power of oral memory, either in the practised learning of specialists or in the shared and pooled, as it were choral, memory of communities. Choral memory, of the sort that characterises performance of football chant or the long, chanted texts of modern Faeroese ballad dance, is a very powerful thing. Any individual lapse of memory or misjudged entry passes quite unnoticed in the common tide of words and melody.

Whilst all the things that I have mentioned are naturally of intrinsic interest, each also relates, in a modest way, to some of the big philosophical questions of music's antiquity: to questions of our ultimate origins as a species and the part that musical capacities may have played in our becoming anatomically and cognitively modern; and to questions of cultural modernity also. How ancient, we might wonder, are modern musical behaviours? How old is structured musical creativity? And how ancient is the relationship between musical tradition, knowledge and text? Of course error and accident open windows too on the intuitive musical thinking and habits of ordinary people, which is to say on the diverse, sometimes chaotic realities within which music theory sprang and of which ancient philosophers and theorists, from Plato to Isidore, sought to make rational sense. As growth in the archaeological evidence enables more subtle, nuanced interpretation, the more restricted and abstracted ancient musical writings sometime seem in terms of what may or may not have been happening in the world beyond (and beneath) the writers' immediate fields of view. So it may prove well for us, to paraphrase Stukeley, that ancient peoples were indeed 'guided with such a spirit as left [us remains] sufficient to supply that defect, when handled as they deserve', and that with them we are able to contemplate such questions from a new and independent perspective. My own feeling is that, in the fullness of time, we will indeed be able to handle such finds as they so clearly deserve, through scientific study; and that in the end a deeper understanding of the complexities that they reveal will only increase our admiration for those who struggled to rationalise that complexity and reduce it to manageable proportions.

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CHAPTER 10

ERROR AS A MEANS OF DECEPTION: ARISTOTLE'S THEORY OF SOPHISTICAL PREMISSES

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Abstract: Aristotle determines eristic argument as argument which either operates upon the basis of acceptable premisses (ἔνδοξα) and merely gives the impression of being deductive, or argument which truly is deductive but operates upon the basis of premisses which seem to be acceptable, but are not so (or, again, argument which uses both of these mechanisms). I attempt to understand what Aristotle has in mind when he says that someone is deceived into accepting premisses which seem to be acceptable but which are really not, and how this disqualifies such arguments from being dialectical. In the first section of the paper I interpret Aristotle's notion of ἔνδοξα in terms of a relational concept of acceptability. Real ἔνδοξα are propositions which are accepted by a qualified group or individual. False ἔνδοξα may also be accepted by someone or some group, and may even be true, but they are used to serve the purposes of eristical argumentation, which departs from certain standards of dialectical argumentation articulated in the notion of ἔνδοξα as a norm for premiss-acceptance. In particular, eristic arguments may even be valid in the sense of a συλλογισμός while still failing to be proper dialectical arguments. In the second part of the paper I consider how this can be, in examining certain types of fallacies in the *Sophistical Refutations* and the relationship between fallacious argumentation and false ἔνδοξα.

1. Introduction

In the first chapter of the *Topics* Aristotle introduces the notion of "eristic argument":

> Eristic is deductive argument upon the basis of premisses which seem to be reputable, but are not, as well as argument which seems deductive, be it from reputable or reputable-seeming premisses (ἐριστικὸς δ' ἐστὶ συλλογισμὸς ὁ ἐκ φαινομένων ἐνδόξων μὴ ὄντων δέ, καὶ ὁ ἐξ ἐνδόξων ἢ φαινομένων ἐνδόξων φαινόμενος). (Top. A 1, 100b24–25; cf. SE 2, 165b7–8)

This passage identifies two mechanisms by which the eristic arguer deceives his interlocutor. The first mechanism makes an argument seem to fulfill the conditions of a συλλογισμός, a deductive argument, when it really does not fulfill these conditions. Eristic arguments by this mechanism are not deductive arguments at all; their mechanism of deception is to seem deductive. The second mechanism of eristic argument may also be employed in deductive argument. It is therefore not, strictly speaking, a means of *logical* deception. It operates on the level of individual propositions used as premisses and aims to effect the acceptance of certain propositions as reputable premisses when they are not. But what does Aristotle mean when he says that we may be fooled with regard to the reputability of premisses which are offered as reputable and acceptable (ἔνδοξα), and how may we characterize this error with a view to the properly logical error of deeming conclusive a fallacious, i.e. not really deductive, argument? My purpose here is to outline the right answer to these questions.

The argumentative errors corresponding to the two mechanisms of eristic argument above are of course not mistakes on the part of the practitioner of eristic, who knows very well what he is doing and intends to do what he does. They correspond rather to sources of error in the recipient of the argument, who is deceived into believing that he has been legitimately refuted by an eristical arguer. The criterion or norm for identifiying the first source of error is clear, and holds for all types of argument in Aristotle: it is the criterion of deductiveness in the sense of a συλλογισμός. (As will be noted below, this concept is wider than that of deductive validity. However, the fact that Aristotle recognizes that certain types of argument, such as inference from signs, can be acceptable and rational while nevertheless failing to meet the criterion of a συλλογισμός, does not relativize his criterion of deductiveness for arguments.) The criterion or criteria for identifying the second type of error is less clear. Are φαινόμενα ἔνδοξα trivially and recognizably false premisses? This would certainly make them forfeit their status as ἔνδοξα, but then they would hardly qualify as φαινόμενα. Or are they *prima facie* acceptable premisses which turn out to be not so acceptable as they seem at first sight, regardless of their truth? As I shall argue, this is more likely. A distinction between degrees of acceptability is well-suited to Aristotle’s theory of dialecttical argumentation, which is not primarily concerned with the truth of the premisses of dialectical argument. But there is a problem here. The claim that certain things which seem acceptable are not really so involves more than just a notion of relative acceptability; it seems to imply a sharp distinction between real and false, or genuine and merely apparent, acceptability. These are highly normative epistemological notions. In what follows, I attempt to understand how they are brought to bear within the context of Aristotle’s theory of dialectical and eristic argumentation.

2. False ἔνδοξα as superficially acceptable premisses (Topics A 1)

One place to seek some elucidation of the notion of error through acceptance of merely apparently reputable premisses is in the passage immediately following the distinction adumbrated above (Top. A 1, 100b26–101a4). Here, Aristotle reiterates that not everything which seems to be acceptable is acceptable, and he justifies this statement in the following way:

> An eristic argument is a deduction from premisses which seem to be ἔνδοξα, but are not really, as well as merely apparent deduction from real and apparent ἔνδοξα: for not everything which seems to be endoxical really is so. None of the aforementioned ἔνδοξα have their φαντασία completely on their surface, as the starting-points of eristic arguments happen to do. For the nature of the false in them usually becomes quickly apparent to those who are able to detect nuances as well.[1] The first of the aforementioned types of eristic argument may be called a deduction, but the other eristic argument is not deductive, since it merely seems to deduce, but does not (Top. A 1, 100 b23–101a4).[2]

In interpreting this passage we must give an account of how we are fooled with regard to the acceptability of dialectical premisses, and of how we can know that something presented as a real ἔνδοξον is in fact a false or counter-feit ἔνδοξον. And we must account for what it is for propositions to “have their φαντασία completely on their surface” (100b27–28), as this is offered as a criterion for the distinction between false and real endoxical premisses.

The interpretation of this passage in Alexander of Aphrodisias (In Aristot. Top. 19.28–23.19) is extensive. He understands the qualification “superficial” (cf. “on the surface”, 100b27) with regard to premisses to mean that such premisses are refuted upon the basis of brief observation; and he classifies eristic premisses as being of this type (In Aristot. Top. 20.9). As examples of φαινόμενα ἔνδοξα Alexander cites such trick propositions as “what you

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[1] I adopt here a non-orthodox reading of the phrase τοῖς καὶ μικρὰ συνορᾶν δυναμένοις suggested by Brunschwig (1967, 114–15, ad loc). This phrase is usually rendered in one of two similar ways, either by taking the καί to modify μικρά (so e.g. Smith (1997, 1), “those capable of even modest discernment”) or with τοῖς δυναμένοις e.g. Pickard-Cambridge in Barnes 1984, 167, “even to persons with little power of comprehension”). The former construal seems preferable; adverbial καί modifying a preceding word (or “post-placed” καί) is a possible construction, but not one peculiar to Aristotle. Both of these readings take μικρά as adverbial to συνορᾶν δυναμένοις, in assuming that the epistemic capacity for recognizing pseudo-ἔνδοξα will be minimal. I shall argue against that assumption in what follows; for the moment it must suffice to point out that this reading runs against the often positive connotation of συνορᾶν in Aristotle, not as a minimally basic faculty of reasoning but as a developed faculty of observation and judgement. See e.g. De gen. et corr. 316a5, Met. A 3, 984b2, and the conspicuous frequence of the phrase συνορᾶν δύνασθαι in the *Topics*: A 17, 108a14; A 18, 108b20; Γ 2, 117a6; Θ 2, 158a4–5; and especially Θ 14, 163b9-11: “for knowledge and philosophical wisdom, being able to see and to have detected (τὸ δύνασθαι συνορᾶν καὶ συνεωρακέναι) what follows from either of two theses is no small tool.”

[2] Top. A 1, 100b23-101a4: ἐριστικὸς δ' ἐστὶ συλλογισμὸς ὁ ἐκ φαινομένων ἐνδόξων μὴ ὄντων δέ, καὶ ὁ ἐξ ἐνδόξων ἢ φαινομένων ἐνδόξων φαινόμενος· οὐ γὰρ πᾶν τὸ φαινόμενον ἔνδοξον καὶ ἔστιν ἔνδοξον. οὐθὲν γὰρ τῶν λεγομένων ἐνδόξων ἐπιπόλαιον ἔχει παντελῶς τὴν φαντασίαν, καθάπερ περὶ τὰς τῶν ἐριστικῶν λόγων ἀρχὰς συμβέβηκεν ἔχειν· παραχρῆμα γὰρ καὶ ὡς ἐπὶ τὸ πολὺ τοῖς καὶ μικρὰ συνορᾶν δυναμένοις κατάδηλος ἐν αὐτοῖς ἡ τοῦ ψεύδους ἐστὶ φύσις. ὁ μὲν οὖν πρότερος τῶν ῥηθέντων ἐριστικῶν συλλογισμῶν καὶ συλλογισμὸς λεγέσθω, ὁ δὲ λοιπὸς ἐριστικὸς μὲν συλλογισμός, συλλογισμὸς δ' οὔ, ἐπειδὴ φαίνεται μὲν συλλογίζεσθαι, συλλογίζεται δ' οὔ.

have not lost, you have" and "whoever has vision, sees". The paradoxical conclusions which may be inferred from such propositions ("one-eye has eyes", "the sleeper (who has a dream, an ὄψις) sees") show, he argues, that these inferences have "superficial plausibility" (their πίθανον is ἐπιπόλαιον), since their falsehood quickly becomes clear to those with the faculty of discerning the μικρά (In Aristot. Top. 20.10–19).[3] Real ἔνδοξα are distinguished by the fact "that it is not easy for falsehood to be detected in them" (In Aristot. Top. 20.19–20).

Alexander's comments focus on an important aspect of this passage which most modern English translations fail to make clear: at issue is the plausibility and acceptability of endoxical premisses, not their "appearance".[4] Ascribing φαντασία to premisses is a substantival way of expressing their φαίνεσθαι, their "true-seeming".[5] The problem with "phony" ἔνδοξα arises because there are different ways of seeming true, and not every thing which seems true is acceptable in the sense of an ἔνδοξον. But in making a distinction between real and phony ἔνδοξα a problem arises which we have already advertised above: the concept of acceptability which Aristotle employs is a relational one – what seems acceptable to some might be transparently eristic and unacceptable to others – and so it does not seem able to support a *definite* distinction between real and false or phony ἔνδοξα in terms of their content (see Smith 1997, 48–9). We must therefore explain how Aristotle can employ the distinction between real and apparent ἔνδοξα while assuming a relative concept of acceptability as part of his notion of ἔνδοξα.

Aristotle claims that "none of the aforementioned ἔνδοξα" (οὐθὲν γὰρ τῶν λεγομένων ἐνδόξων) have their seeming-true "completely" or "exclusively" "on the surface". I take the "aforementioned ἔνδοξα" to refer to the passage immediately preceding in which Aristotle determines ἔνδοξα as "things which seem true to all, to the many, and to the wise, either to all the wise, or many of them, or to those who are most recognized and reputable" (100b21–3).[6] The common feature of ἔνδοξα on this description is their *seeming true to someone* (δοκεῖν

[3] Alexander is not explicit about his understanding of this phrase, but it is clear that he does not support the now orthodox adverbial reading of μικρά, which he takes as direct object in his paraphrase.
[4] In standard English translations of the *Topics*, φαντασία is rendered as "appearance"; see Barnes 1984, 167 and Smith 1997, 1.
[5] See Bonitz 1870, 811a31ff. The substantive φαντασία has a verbal root, the verb φαίνεσθαι; and like this verb it can connote either a tendencially correct or incorrect seeming, be that seeming perceptual or cognitive. In our passage, the participle φαινόμενον serves to describe the cognitive pull of certain ἔνδοξα and indicate their tendencially incorrect or "mere" seeming. It is a cognitive pull in the wrong direction. The substantive φαντασία, in contrast, I take to be neutral in the sense that it affirms of a seeming that it has a certain cognitive pull, regardless of whether that pull is in the right direction. We should therefore understand the word in the sense of "plausibility" here, although it can also mean "reputation" (Lidell / Scott 1996, s.v. φαντασία), which is an important factor in Aristotle's concept of acceptability (Barnes 1980, 493).
[6] Top. A 1, 100b21–3: ἔνδοξα δὲ τὰ δοκοῦντα πᾶσιν ἢ τοῖς πλείστοις ἢ τοῖς σοφοῖς, καὶ τούτοις ἢ πᾶσιν ἢ τοῖς πλείστοις ἢ τοῖς μάλιστα γνωρίμοις καὶ ἐνδόξοις.

τινι). This, I take it, is the relational feature of acceptability at issue in Aristotle's notion of ἔνδοξα. For the relation of δοκεῖν τινι implies, at least in dialectical context, not just a seeming true to someone, but the acceptance of a proposition.[7] But *seeming true to someone* is not the same as *seeming true to just anyone*; in qualifiying the relation of δοκεῖν τινι, Aristotle limits ἔνδοξα to that accepted by groups ("all", "the many", and "the wise", either all or many of them) as well as by persons who are "most recognized and reputable" (μάλιστα γνώριμοι καὶ ἔνδοξοι, 100b23). The notion of ἔνδοξα can also accommodate propositions which are logically equivalent to explicit ἔνδοξα, such as "things which are similar to ἔνδοξα" and the negation of things which contradict ἔνδοξα (Top. A 10, 104a12–14). And it includes not just the opinions of those reputed to have the authority of "wisdom" in the widest sense (poets, seers, and the like), but embraces also expert authority in a more proper sense, "the opinions of the sciences which have been discovered" (Top. A 10, 104a14–15).

The identity of the group or individual which or who is said to accept a premiss will have a bearing on the degree to which that premiss seems acceptable. In this way, the acceptedness in the sense given in the determination of ἔνδοξα is – in Aristotle' theory of dialectical argumentation and dialectical theory of rhetoric, at least – an indicator that the statement is true, and acceptable as a premiss. It has been noted by others that in this Aristotle seems to operate upon a somewhat surprising underlying assumption concerning acceptedness and acceptability: that things which are accepted by certain groups and individuals have, by that reason, some claim to the truth, and thus for (truly) seeming acceptable.[8] A modern theory of acceptable premisses is, by contrast, less prone to invoke acceptedness as a criterion for acceptability. This assumption is in fact there and it does have non-trivial consequences; but it should noted that Aristotle makes it in the context of a theory of dialectical argument, and moreover that, in light of his description of ἔνδοξα, only things accepted *by certain groups and kinds of people* qualify as being acceptable on the grounds that they are accepted.

With this brief overview of "real" ἔνδοξα in view, let us return to the false ἔνδοξα in our passage and the question of how something can seem to be, but not be, an ἔνδοξον. In speaking of φαινόμενα or "merely apparent" ἔνδοξα, Aristotle refers to the ἔνδοξα of his own

[7] For a general theory of acceptability, it is important to distinguish between belief and acceptance, as Laurence Jonathan Cohen (1992) did in *An Essay on Belief and Acceptance*. For the present purposes, it will suffice to note that modern epistemic concepts of "belief" come with certain assumptions which might lead to difficulties in understanding Aristotle's notion of ἔνδοξα, or even the Greek philosophical notion of δόξα in general.

[8] See e.g. Burnyeat 1986, 12: "The fact that a proposition is believed by the majority or by experts is not for Aristotle just a sign that, if we asked them, they could cite evidence for the proposition. Their belief, as he treats it, is already some evidence in favour of what they believe; even if the opinion is not correct, it is likely to contain an element of truth which the dialectic can sift out and formulate clearly".

description, and thus is saying that there are propositions which *seem* to seem true to someone and to be accepted by them, but which really do not seem true to them and in fact are not accepted by them. How are we to detect such a thing? This is a question on which most modern interpreters of our passage seem to agree. Most translators, at least, think it must be easy to detect false ἔνδοξα, for – as noted above (fn. 2) – they render the sentence in more or less the following way:

> For none of the opinions which we call reputable show their character entirely on the surface, as happens in the case of the principles of contentious arguments; for the nature of the falsity in these is obvious immediately, and for the most part even to persons with little power of comprehension (Top. A 1, 100b26–101a1).[9]

The evaluation of this rendering, and quite generally the interpretation of the entire passage, must be guided by Aristotle's positive determination of the notion of ἔνδοξα. In introducing the notion of ἔνδοξα to describe dialectical premiss-taking, Aristotle makes it flexible but by no means arbitrary, naming specific sources of endoxical authority (all, the many, the wise, technical experts). The word ἔνδοξα itself is a term of art, coined to describe the best and most successful kind of dialectical practice for which Aristotle's dialectical manual, the *Topics*, is supposed to prepare its reader. It is not clear why "persons with little power of comprehension" – a general description which will fit many people who don't even engage in dialectic – should be a concern for the author of a manual on the particular practice of dialectic as Aristotle conceives it.[10] Furthermore, even if we suppose that dialectical procedures such as Aristotle describes them were practiced with people outside the Academy or some other school-like setting, it does not seem likely that persons having only little power of comprehension would see through the principles of eristic argument. How could the art of eristic argumentation ever become the threat it was perceived to be if its principles are so obviously and transparently false? At the beginning of the *Topics* it would seem more likely for Aristotle to warn the would-be eristician that there will be skilled dialecticians ("those who can detect nuances, as well") who could, for the most part even immediately, see through their premisses. But as we shall see in the second part of this article, explaining why some-

[9] Translation by Pickard-Cambridge, based upon the text of Brunschwig, and printed in Barnes 1984, 177. The Greek text is cited above in fn. 3.

[10] The fact that Aristotle mentions "encounters" (ἐντεύξεις) with the many among the three uses of his dialectical πραγματεία suggests that the use of dialectic for engaging "the many" was not at all self-evident (Top. A 2, 101a25–101b4). Moreover, the particular use of his study which Aristotle cites in that passage – "having enumerated the opinions of the many, we shall engage them not upon the basis of foreign opinions, but upon the basis of their own opinions, in changing whatever they seem to us not to say well" (101a31–34) – is indicative of the rhetorical application of Aristotle's theory of dialectic, and not of its employment in any sort of public debate. On this passage and the dialectical component of Aristotle's rhetorical theory see Rapp 2002, IV.1, 236 ff., and on this passage 252.

thing is an eristic and not a properly dialectical premiss – this is a central interest in *Sophistici Elenchi* – is another matter yet again, and not a trivial one.

There remains the question why Aristotle characterizes the premisses of *eristic* argument as φαινόμενα ἔνδοξα. There might be other kinds of argument, even non-dialectical arguments, which rely upon false ἔνδοξα. The deceptive use of "common notions" in a pseudo-scientific proof for the squaring of the circle, for example, might seem to be a case in which someone makes arguments using false ἔνδοξα outside of a dialectical context. This, one could suppose, is not eristic, i.e. pseudo-dialectical argument, but παραλογισμός, pseudo-scientific argument. Yet Aristotle explicitly determines παραλογισμός in such a way that ἔνδοξα are explicitly ruled out as premisses. The "drawer of false figures" (ὁ ψευδογραφῶν) who makes pseudo-scientific arguments does so neither upon the basis of true or primary premisses, nor upon the basis of ἔνδοξα in Aristotle's sense of the term (things which seem true to all, the many, or the wise, etc.), but from "premisses particular to a science, but not true" (Top. A 1, 101a9–15). This limitation of pseudo-scientific reasoning to faulty reasoning upon the basis of premisses peculiar to a certain science has some interesting consequences. To name just one of them: it brings Aristotle to see the use of ἔνδοξα in the context of scientific questions as eristic argument, as in the case of Bryson's squaring of the circle through a logical argument based upon relative magnitude (SE 11, 171b16ff.).[11] This indicates that false ἔνδοξα can be manifest in the whole variety of problems and questions which may be discussed dialectically, and thus also eristically.

If real ἔνδοξα are the premisses of real or legitimate dialectic, and phony ἔνδοξα are the premisses of eristic, then what makes ἔνδοξα real or phony should also help us understand Aristotle's distinction between genuine dialectic and its corrupted counterpart, eristic. There seems to be an ethical dimension to the opposition between real and merely apparent ἔνδοξα, one which concerns the "spirit" of the argument and standards of dialectical fair play. In employing "argument from ἔνδοξα" as a positive description of dialectic at the outset of the *Topics*, Aristotle is describing a certain established practice of argumentation by picking out what is for him the feature which distinguishes it from other types of argument. The feature of using ἔνδοξα as premisses may well express a rule governing premiss-acceptance in (standard) dialectic for the sake of "training", as Oliver Primavesi has suggested.[12] The notion of

[11] For a reconstruction of Bryson's argument along these lines, see Dorion 1995, ad loc.
[12] See Primavesi 1996, 23–24: "Die für Aristoteles nach *Top*. A 1 essentielle Bindung der dialektischen Argumentation an *Endoxa*, d.h. an weithin anerkannte Prämissen, beruht also auf einer bestimmten 'Spielregel' des in *Top*. A 2 an erster Stelle genannten Übungsgesprächs".

ἔνδοξα and its use in Aristotle's determination of dialectic is, in any case, regulative: dialectic in a general and as yet undifferentiated, but nevertheless proper, sense admits only premisses which are ἔνδοξα (or at least premisses not more ἄδοξα than the conclusion defended by the answerer), whereas argumentation from merely apparent, or false, acceptable premisses (as well as apparently deductive argumentation) just does not deserve to be called dialectic.[13]

In interpreting the notion of false or phony ἔνδοξα, it is helpful to recall this normative dimension of Aristotle's concept of ἔνδοξα and the dignity it bestows upon dialectic as a co-operative activity, which it is made out to be in the *Topics*. In a chapter on the critical evaluation of arguments (ἐπιτίμησις, Top. Θ 11), Aristotle speaks of the "bad companion" in terms of argument:

> A bad companion is one who hinders the joint enterprise, and it is clear that this holds also in the case of argument. For there is a joint enterprise set out also in these things, except in the case of competitive arguers. It is not possible for both of them to obtain their goal, since it is impossible for more than one to win. And it makes no difference if they do this through answering or questioning, for the contentious questioner will argue poorly, and in answering he will not grant what seems true (τὸ φαινόμενον) and will reject whatever the questioner wishes to inquire (Top. Θ 11, 161a37–b5).[14]

The moral of the passage, as Aristotle goes on to say, is that one must not judge an argument and the questioner who sets it up by the same standards, as the questioner may have argued as well as possible, and nevertheless accomplished a poor argument (161b5–8): we get a taste of such dialectical meltdown in Plato's *Gorgias*. On the other hand, it is the mark of a good (i.e. co-operative) interlocutor to grant things which may undermine his thesis – so long as they seem acceptable, or more acceptable than the conclusion sought by the interlocutor.

With this moral of argumentation in view, we can see how the notion of false or phony ἔνδοξα can refer to more than just a certain kind of deceptive acceptability. Even the most endoxical propositions can be employed deceptively by contentious arguers, and with absurd results: "plugging" propositions in to test their acceptability will, in any case, not be an acceptable test of acceptability in the sense of Aristotle's notion of ἔνδοξα (*pace* Brunschwig 1967, 115). It is also difficult to see how the notion of apparent ἔνδοξα could denote some formal

[13] The so-called Principle of Overarching from Topics Θ 5 states that when defending an endoxical thesis, the answerer may be compelled to co-operate by admitting not just things which seem acceptable, but also those which, though not acceptable, are at least more acceptable than the non-endoxical conclusion for which his opponent will be arguing (Top. Θ 5, 159b16–20). The principle is thus not innocent. It is discussed in Wlodarczyk 2000.

[14] Top. Θ 11, 161a37–b5: ἐπεὶ δὲ φαῦλος κοινωνὸς ὁ ἐμποδίζων τὸ κοινὸν ἔργον, δῆλον ὅτι καὶ ἐν λόγῳ. κοινὸν γάρ τι καὶ ἐν τούτοις προκείμενόν ἐστι, πλὴν τῶν ἀγωνιζομένων. τούτοις δ' οὐκ ἔστιν ἀμφοτέροις τυχεῖν τοῦ αὐτοῦ τέλους· πλείους γὰρ ἑνὸς ἀδύνατον νικᾶν. διαφέρει δ' οὐδέν, ἄν τε διὰ τοῦ ἀποκρίνεσθαι ἄν τε διὰ τοῦ ἐρωτᾶν ποιῇ τοῦτο· ὅ τε γὰρ ἐριστικῶς ἐρωτῶν φαύλως διαλέγεται, ὅ τ' ἐν τῷ ἀποκρίνεσθαι μὴ διδοὺς τὸ φαινόμενον μηδ' ἐκδεχόμενος ὅ τί ποτε βούλεται ὁ ἐρωτῶν πυθέσθαι.

flaw of argument, since Aristotle already has a very adequate description for formally fallacious eristic arguments: those which seem to be deductive, but which are not.[15] The notion of false or phony ἔνδοξα is informed by a morale of argumentation, and indicates a kind of argument which makes pretensions to be dialectical, but which does not adhere to a key code of conduct in dialectical argumentation as Aristotle understands it.[16] False ἔνδοξα are not only, not even necessarily, false in the sense that they are not true. They are false in the sense that they are deceptive "in regard to the matter", i.e. they effect a deception concerning some matter in someone, often intentionally (though not necessarily) (see SE 11, 171b18–22). The description of eristic as argument from merely apparent ἔνδοξα is a way of describing a *lack* of morale which Aristotle ascribes to the taking and accepting of premisses in genuine dialectic, and hence it can also serve as a description of at least one important aspect of eristic: a lack of fairness.[17]

Just as premiss-taking in standard dialectical argument is described in the *Topics* in terms of ἔνδοξα, and various dialectical procedures are specified through expanding or further qualifying this notion (as in Top. A 10 and Θ 5), so too we may Aristotle expect to give some account of premiss-acceptance using the notion of false ἔνδοξα in his treatment of eristic argument. In the second half of this article, I explore this further account of eristic and particularly eristical premisses.

3. False ἔνδοξα and fallacious argumentation in De Sophisticis Elenchis

Realizing that one has been deceived in such as way as to have accepted the unacceptable is one thing. Accounting for the mechanisms of deception with regard to acceptance is another matter. This is the proper province of Aristotle's *De Sophisticis Elenchis*, which provides an account of the mechanisms of eristic argument, and to which we must look for an account of the mechanisms involved in merely apparently acceptable premisses, or false ἔνδοξα.

It is general consensus that Aristotle's *Sophistical Refutations* marks the beginning of a tradition in the study of fallacies, but Aristotle did not possess a term directly equivalent to our word "fallacy". This word is used in a range of meanings more or less closely related to

[15] *Pace* Smith 1997, 48. See Top. A 1, 100b25; Top. Θ 12, 162b3–5; SE 2, 165b7–8; cf. SE 11, 171b18–25, and Alexander, In Aristot. Top., who in fact distinguishes between materially and formally eristical arguments.
[16] Eristic is described as concerned with the same things as dialectic in SE 11, 171b3–12 and 171b34–7.
[17] See SE 11, 171b22–5: "Just as there is a certain form of dirty fighting which consists in foul play, the dirty fighting in the art of combative argument is eristic, for the ones who strive to conquer utterly and by any means are eristic arguers".

the concept of a valid argument: an "historical fallacy", for example, is simply an anachronistic claim, and does not refer to a particular formal defect of an argument. The original sense of the word in the English language is "deception, guile, trickery", and hence it can refer also to the state of being in error and the making of false statement, as well as to fallacious argument.[18]

What, for Aristotle, is fallacious argument? Aristotle developed, at some point, a concept of συλλογισμός as an argument which is deductive, the conclusion of which presents something other than that in the premisses, and which is caused through the premisses.[19] It is common to equate this with the concept of "valid" argument, but also somewhat misleading. Two main stipulations for συλλογισμός – that the conclusion 1. present something "new" which 2. is caused by the premisses – clearly do not hold for all valid arguments. Thus arguments may be fallacious by the standards of συλλογισμός without being invalid. Moreover, with regard to refutations, a certain type of συλλογισμός, Aristotle states that not only is that refutation "sophistical" which seems to be a συλλογισμός when it is not: also an argument which is a συλλογισμός, but which is not "appropriate to the matter" (οἰκεῖον τοῦ πράγματος), counts as a sophistical refutation (SE 8, 169b20–3), or fallacious argument. One may also add those arguments in which there is an only apparent contradiction between the deduction's conclusion and the original thesis, a notorious effect of equivocation.[20]

It is not quite accurate to say that "a fallacious argument, as almost every account from Aristotle onwards tells you, is one that *seems to be valid* but *is not* so" (Hamblin 1970, 12, his own italics). For at least Aristotle's notion of sophistical and eristical refutation, which will cover certain types of "fallacious argumentation", includes, as we have seen, not just arguments which seem to be "valid" in the sense of συλλογισμός but are not. It also includes those arguments which are valid in the sense of a συλλογισμός but have phony endoxical premisses, as well as deductive arguments inappropriate to the matter. It is also somewhat misleading to characterize the *Sophistical Refutations* as a study in fallacious argumentation *tout court*, for Aristotle examines in this work a particular kind of fallacious argument, namely pseudo-dialectical argumentation, which he calls "eristic", but also "sophistic": though Aristotle can dif-

[18] See *The Oxford English Dictionary*, s.v. "fallacy" (Simpson / Weiner 1989, 693-694).
[19] An. Pr. A 1, 24b18–22; Top. A 1, 100a25–7; SE 1, 164b27–165a3 (ἔλεγχος is a συλλογισμός with the contradiction of the conclusion); Rhet. A 2, 1356b16–18.
[20] This kind of false refutation motivates the first part of the exhaustive determination of ἔλεγχος in SE 5, 167a23–7: "A refutation is a contradiction of one and the same thing, not in respect to the name but in respect to the matter, by use of a word which is not equivocal but the same, and which follows of necessity from the things granted".

ferentiate these two in terms of motive (trying to win vs. trying to seem wise), both are conceived as fallacious argumentation upon the basis of dialectical premisses.[21] Fallacious argumentation upon the basis of principles proper to a discipline constitutes an object of study and refutation for the practitioner of that discipline, whereas eristic and sophistical fallacies can concern anything, just as dialectical argumentation can.[22] Nevertheless, the study of eristic and sophistical refutation and fallacies evidently belongs, for Aristotle, to a theory of dialectical argumentation.

In accord with the treatment of eristic as argumentation which is defective in a specifically dialectical way, Aristotle identifies and classifies particular mechanisms of eristic argumentation. He divides eristic arguments into those "by means of linguistic expression" (παρὰ τὴν λέξιν, *in dictione*) and those "without linguistic expression" (ἔξω τῆς λέξεως, *extra dictionem*) as "two types of refuting" (SE 4, 165b23–24). In his identification of six "things inducing true-seeming through linguistic expression" (τὰ μὲν παρὰ τὴν λέξιν ἐμποιοῦντα τὴν φαντασίαν, b24–5), we find phrasing which recalls the passage on φαινόμενα ἔνδοξα from the beginning of the *Topics*. The six mechanisms of linguistic deception do not refer directly to inferential procedures: "homonymy" and "amphiboly" are sources of error with respect to the signification of words, and the mechanisms of "composition" and "division" are performed on single propositions in order to manipulate their sense.[23] The same also holds for "prosody" or "accentuation", a means of changing the meaning of a statement by changing the accent on a particular word in that statement, and for "figure of speech", malapropopism which enters by way of etymologically established patterns of expression.[24]

The linguistic fallacies are of interest to Aristotle as means of eristically fallacious argumentation, but are not, in themselves, arguments which seem to be deductive but are not so.

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[21] "Eristic" is Aristotle's term of choice for designating degenerate dialectical argumentation; see especially SE 11, 171b34–172a2: "The practitioner of eristic bears a relationship to the dialectician which is similar to that of the drawer of false figures is to a genuine geometrician. For he argue upon the same basis as the dialectician, and the drawer of false figures argues upon the same basis as the geometrician. But the drawer of false figures is not an eristic arguer, since he draws false figures upon the basis of the principles and conclusions which fall under his discipline, whereas the one who argues eristically upon the basis of dialectical principles will clearly practice eristic also in regard to other things". The term "eristic" is conspicuously frequent in Aristotle's *Rhetoric* (e.g. A 11, 1371a7; B 24, 1402a3, 14, 27; Γ 14, 1414b28). "Sophistic" is a word with a wider scope and more baggage; in Aristotle, it can refer to the use of defective dialectical argumentation in philosophical contexts. Cf. e.g. Plato, Soph. 223b5 (sophistic as the pursuit of the new, rich, and ἔνδοξα/ἔνδοξοι) and Plato, Prot. 316d (the sophistic art is an old and venerable one) with SE 1, 165a21–3 (sophistic seems to be wisdom but is not, and the sophist is a peddler of true-seeming but false wisdom) and Met. Γ 2, 1004b17ff. (dialecticians speak about all things to which being is attributed, which is also the realm of the philosopher; dialectic tests the things which philosophy knows and sophistic seems to know, but does not).

[22] See Top. A 1, 101a8–10.

[23] Homonymy and amphiboly: SE 4, 165b30–166a21, composition and division: SE 4, 166a23–38.

[24] Prosody or accentuation: SE 4, 166b1–9, figure of speech: SE 4, 166b10–19.

They are rather means of manipulating language at the level of “names and phrases” (ὀνόμασι καὶ λόγοις, 165b29). It is easy to see how such mechanisms can be grasped in terms of the notion of false ἔνδοξα. In order to have any sheen of plausibility, false ἔνδοξα must be presented as things which everyone does in fact accept. Linguistic conventions are just such things. Consider this argument from an equivocation: “it is the knowers who learn (μανθάνουσιν); for those who can learn the alphabet know (μανθάνουσιν, i.e. understand) what is dictated” (SE 4, 165b31–2).[25] The premiss – that those who are capable of knowing letters (οἱ γραμματικοί) understand what is dictated – is, by itself, acceptable and plausible on an easy understanding of the words it contains. Only when presented as evidence for a statement featuring a different meaning of the homonymous μανθάνειν does the premiss seem suspect.

This argument, like most from the stock of sophistic, trades on semantic ambiguity; but not all fallacies παρὰ λέξιν are semantic, even though they are sometimes generated by instances of syntactical ambiguity.[26] However, Aristotle’s analysis often seems to straddle the distinction between the syntactical and the semantic. For example the eristical question: “What someone knows, does he know this/does this know?” trades on the opacity of the reference of the third person singular active in this Greek expression (the neuter pronoun τοῦτο could grammatically be either object or subject of the verb). Aristotle diagnoses the problem this way: “the knower and the known can be expressed as if they were both knowers by this phrase” (166a7–9).[27]

Aristotle’s purpose here, as in his discussion of the other fallacies *in dictione* – combination, division, accent, and form of expression – is to account for how grammatically well-formed and conventionally acceptable expressions may be misappropriated or misapprehended in such as a way that they become false, or abet false reasoning. These types of fallacy have in common that they are instances of manipulation of language at the level of expressions and phrases, not of inference. And this is where we may expect to find a main source of

[25] SE 4, 165b31–2: μανθάνουσιν οἱ ἐπιστάμενοι, τὰ γὰρ ἀποστοματιζόμενα μανθάνουσιν οἱ γραμματικοί. A similar but slightly different argument is cited in Plato’s *Euthydemus*, 276c2–7: “When the grammar teacher used to dictate to you, which of the children were learning what was dictated, the clever ones (οἱ σοφοί) or the ignorant ones? – The clever ones, Kleinias said. – So it was the clever ones who were learning and not the ignorant ones, and so you did not answer Euthydemus correctly just now”. Kleinias had just been made to accept the claim that the ignorant “learn” (μανθάνειν), not the “clever”/“wise” (οἱ σοφοί). As Kirwan 1979, 40, points out, this argument does not trade on equivocation in the use of the verb μανθάνειν (as Aristotle’s example does), but in the use of σοφοί, which may mean both “apt at learning, clever” and “knowing, learned”.
[26] See SE 6, 168a23–8: “Of the linguistic fallacies, the ones are by equivocation, for example homonymy, ambiguity in definition, and similarity of form (for we are accustomed to assume that all these things signify something), the others are combination and division and accent, which arise because the word or account, when altered, is not the same”.
[27] SE 4, 166a7–9: ἆρ' ὅ τις γινώσκει, τοῦτο γινώσκει; καὶ γὰρ τὸν γινώσκοντα καὶ τὸ γινωσκόμενον ἐνδέχεται ὡς γινώσκοντα σημῆναι τούτῳ τῷ λόγῳ.

the error in accepting the disreputable as a *premiss*. For premiss acceptance is characterized by the fact that it operates upon immediate epistemic considerations which are not the result of further inference. And so it is reasonable that a part of Aristotle's account of argumentation from false ἔνδοξα is given in terms of the use of language and linguistic convention, which inform the practice of premiss acceptance.

But it is not just equivocation in the formulation of premisses which makes for false ἔνδοξα. Eristic premisses may also be false ἔνδοξα by simply resembling acceptable principles. Several of the seven non-linguistic types of fallacy illustrate this phenomenon. Fallacies generated by "using a particular expression without qualification or in a certain way, and not in the proper sense" (166b37–8) may seem acceptable because of a false but true-seeming general principle, namely: that to be something or in some way is the same as to be in an unqualified sense (167a2–3), or, conversely, that not to be something or in some way is the same as not to be in an unqualified sense (167a4–5). In some cases, Aristotle claims, fallacious arguments based upon such principles are easily detected, but in others they often escape detection (167a10–20). This is the case when an attribute is ascribed in a particular respect, but would seem to follow without qualification, or when it is not easy to see which of two attributes belongs in a proper sense, as when opposites (ἀντικείμενα) may be predicated of one and the same thing (167a14–20).

Fallacious argument by recourse to the consequent (παρὰ τὸ ἑπόμενον) occurs because people tend to believe that the consequent is convertible, i. they think that if B necessarily follows from A, A will necessarily follow from B (167b1–3). Interestingly, in illustrating this fallacy Aristotle gives examples from two very different contexts: one which he calls "syllogistical", which concerns Melissus's argument that the universe is unbounded, and another from cases of deception in the formation of judgements based upon sense-perception (167b4–20). The assumption that the consequent necessitates its antecedent is a mistake characteristic of the observation of regularities and inference from signs. Examples of mistakes from observation: when one infers that bile is honey because both are yellow, or that, if the earth is wet, it necessarily has rained (167b5–8). If the notion of false ἔνδοξα can be applied to such examples of reasoning, then it extends well beyond only the dialectical context to include true-seeming but false principles quite generally.

The remaining two examples, from forensic and philosophical contexts, suggest a wider application of the notion of false ἔνδοξα beyond dialectical contexts in the strictest sense. Aristotle's forensic example of fallacious argument from the consequent – that the man who dresses finely and is seen walking at night is an adulterer – belongs, as Aristotle says, to "proofs by sign" (αἱ κατὰ σημεῖον ἀποδείξεις) in rhetorical arguments (167b8–12). Aristotle elsewhere distinguishes the probable (τὸ εἰκός) and sign as types of propositions (προτάσεις) and claims that the probable is an endoxical proposition, and that the sign in a demonstrative proposition which is either necessary or endoxical (An Pr. B 26, 70a3–b6). But this does not present a difficulty for my interpretation of fallacy of the consequent as a false ἔνδοξον, for in the *Sophistical Refutations*, Aristotle is seeking to show how even certain legitimate principles may be used to generate fallacious argumentation. And inference from the probable and from signs, though reasonable, certainly can.

There remains the interpretation of Melissus' claim – the universe in unbounded since it is ungenerated – in light of a principle which seems to be acceptable but is not. It is of course unlikely that the false principle involved in this claim was endoxical in the sense that it was believed by all or the majority. The principle must, rather, be one of those things which seems true to those reputed to be wise. Aristotle gives it as "if the universe did not come to be, then it has no beginning, so that it is unbounded" (167b16–18), and he replies by saying that if the universe (or anything), in coming to be, has a beginning, it does not follow that what has a beginning comes to be. Aristotle's objection to this principle seems itself somewhat eristical.[28] Nevertheless, the discussion of this particular ἔνδοξον is clearly relevant to more than strictly dialectical contexts, as Aristotle is at pains in other works – i.e. in *Physics* Γ 4–8 – to show why it is unacceptable (and thus false). Still, recognizing how these works are based upon the discussion of endoxical or, in Aristotle's view, pseudo-endoxical propositions can help us understand why his procedure in them is sometimes strikingly "dialectical".

4. Conclusion

Both linguistic and as well as extra-linguistic fallacies are relevant mechanisms for making propositions seem acceptable when they are not so, i.e. as the causes of false or phony ἔνδοξα and fallacious argumentation. They are sources of error in regard to a statement's acceptability – and thus are sources of eristically fallacious argumentation, even when the arguments they appear in are "valid" in the sense of a συλλογισμός.

[28] Aristotle's objection to the principle: "That would be like if it would necessarily follow for a hot man to have a fever if the man who gets a fever is hot" (SE 5, 167b18–20). It is not clear to me how this is relevant to the principle under discussion, even if it is an example of fallacy by the consequent.

This conclusion is important, for Aristotle has been accused of wrongly attributing formal defects of argument to a "material" fallacy (equivocation) (Kirwan 1979, 35–46). The accusation is based upon the correct thesis that equivocation need not formally invalidate an argument or inference, and the further assumption that Aristotle's discussion of linguistic fallacies is motivated by the concern to show how they cause the acceptance of refutational arguments as valid when they are not so. But Aristotle explicitly recognizes a class of eristic arguments which are valid in the sense of a συλλογισμός, but nevertheless dialectically "unsound": real συλλογισμός upon the basis of things which seem to be, but are not, ἔνδοξα pertinent to the subject at hand (fake ἔνδοξα: Top. A 1, 100b23–25; merely apparent relevance of the premisses: SE 8, 169b20–23). If we take this statement to articulate (albeit negatively) criteria for the soundness of dialectical arguments and the overall integrity of dialectical procedures, then Aristotle may be exonerated of the charge of having himself committed a "formal fallacy" by making misleading forms of expression out to be the causes of formally defective, or invalid, argument (see Kirwan 1979, 38 ff.).[29] The criterion of validity is, in any case, inappropriate for evaluating the dialectical soundness of an argument, and irrelevant to semantic and syntactic mechanisms of deception on the propositional level.

The study of what makes certain unacceptable premisses seem acceptable takes an important place in Aristotle's theory of eristic argument. I have presented an account of why this is so. Aristotle's concept of eristic argument is dependent upon his concept of dialectical argument insofar as eristic is conceived as *counterfeit* dialectic. Dialectic is conceived as deductive argument upon the basis of ἔνδοξα; eristic is argument from premisses which seem to be ἔνδοξα but are not, or apparently deductive argument from real or merely apparent ἔνδοξα.[30] As I have argued in the first section of this article, the lack of commitment to ἔνδοξα in eristic reflects the lack of a co-operative morale and rule-governed procedure which Aristotle sometimes imputes to dialectical debate in general in determining it as argument from ἔνδοξα. An aspect of this morale is that one introduce only those propositions as premisses which are reputable and acceptable in the sense of Aristotle's concept of ἔνδοξα (seeming true to all, most, or the wise, etc.) – or at least more reputable and acceptable than the conclusion sought. Given the importance of the concept of ἔνδοξα for Aristotle's characterization of

[29] This does not directly address Kirwan's further criticism of Aristotle's classification of fallacies, which must be defended through an interpretation of Aristotle's argument for the reduction of the thirteen types of fallacies to *ignoratio elenchi* (SE 6).

[30] See Top. A 1, 100a29–b26; *SE* 2, 165b3–4 and 7–8; and especially *SE* 11, 171b3–12 and 171b34–7, noted above.

premiss-taking in dialectic, it is understandable that Aristotle provides in his theory of eristic argument an account of how certain propositions seem to be, but are not, acceptable.

Considering the sophistication of this theory of deception by false acceptability, it is likely, at least, that the epistemic capacity for *grasping* “the nature of the false” in eristic principles is not minimal. In fact, because Aristotle tends to consider eristic arguments fair game in the absence of something better, eristic imposes a significant burden upon those who would resist the inferences made from eristic premisses. And the terrain between logic, semantics and grammar which must be covered in order to debunk fake ἔνδοξα is rather something for those who can appreciate “finer points” than for those who are capable of comprehending “even just a little” (see Top. A 1, 100b29–101a1).[31]

[31] Pieter Sjoerd Hasper, Klaus Geus, Marko Malink and Christof Rapp made comments on earlier drafts of this paper and in so doing bettered my argument much, which I most gratefully acknowledge.

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CHAPTER 11

POETICS OF ERRORS

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1. ARE ERRORS ERRATIC?

Scribal errors and corrupt readings have their own logic and poetics. Encoded in them is fragmented yet firm evidence about the unique literary ancestry of the original text and its intellectual lineage. Their occurrences reflect coherent and systematic patterns; metaphorically speaking, they enfold the unique 'intellectual DNA' of their authors, and enclose the inherent cultural traits of their times and habitats. Containing information about ethno-confessional background and educational pedigree of the men of letters who composed the *Vorlage*, they further betray hereditary genes of those (often anonymous) scribes who diligently — or sometimes not so diligently — copied its consecutive redactions time after time, century after century. Furthermore the constellations of corrupt fragments attested in different manuscripts-allographs reveal the fossilised idiosyncratic imprints of each and every copyist upon the fabric of the (once pristine) protograph; it can be even argued that scribal errors contain the collective memory of its fluctuating transmission through space and time. Indeed, the language of scribal errors and corrupt readings can be regarded as a clandestine but faithful witness to the true nature of the *original*. When appropriately assessed, it may facilitate the reconstruction of the authentic features of the (no longer extant) *Ur-Edition*. The same can be said about errors occurring in the process of translation from language to language (e.g. Hebrew to Greek, Greek to Latin, Hebrew to Latin, Greek to Old Church Slavonic, etc.);[1] this is especially true when one examines the processes of text-transmission within Biblical and para-Biblical (apocryphal) Judaeo-Christian corpus. Of particular interest in this connection is the classical trilingual knot of *linguae sacrae* used in the intercultural spread of the Scriptures (i.e. Hebrew, Latin and Greek), the monopoly of which was successfully challenged in the 9th century by the last *lingua sacra* of Europe, Old Church Slavonic. The situation became even more intricate when the process of translation from one language into another (e.g. from Greek to Old Church Slavonic) was further complicated by the parallel existence of two

[1] For the typology of lexical and syntactic errors in Slavonic translations of the 9th to 15th centuries, see Thompson [1988: 351–380]. See also the discussion in Slavova [1999: 36–44], Totomanova [2008: 407–513, 545–573, 591–607, 611–630], and Fahl & Fahl [2008: 213–244].

Slavonic alphabets, Glagolitic and Cyrillic (at least during the first two decades of the 10th century).[2] While it has been generally accepted that the Glagolitic alphabet preceded the Cyrillic, it is still unclear when exactly the Cyrillic script claimed its ultimate victory over Glagolitic. There is, however, a possibility for detecting the original script used by authors, compilers and translators of the Old Church Slavonic/Bulgarian protographs of many of the texts from the period; due to the different numerical values of one and the same letter in these two scripts, the Glagolitic and the Cyrillic, the manuscripts copied in the period of transition from Glagolitic to Cyrillic contain transparent errors in conveying numbers.[3] This is due to the fact that each of the letters of the Glagolitic alphabet designates consecutive numbers, while this is not the case with Cyrillic script, in which some of the letters do not have a numerical value (see the chart below). Thus, while the second letter of the Glagolitic alphabet (боукъı) designates number 2, its counterpart in the Cyrillic alphabet does not have a numerical value; this in turn means that the third letter in the Glagolitic alphabet will designate number 3, while its Cyrillic counterpart will be used to denote 2; the fourth letter of the Glagolitic alphabet will designate 4, but its Cyrillic counterpart will denote 3, etc. Hence, on the basis of the straightforward assessment of the predictable discrepancies between numerical values of one and the same letter within the two alphabets — an assessment combined with the evaluation of various readings of numbers in different manuscripts — one can decipher the language of errors and detect the correct scribal characteristics of the protograph. The epistemological simplicity of this approach, however, along with its success rate rests on the preliminary painstaking extraction of relevant empirical data from all available surviving witnesses, combined with exhaustive text-critical analysis of various editions. In order to demonstrate the potential scope of this methodology, I will apply its strategies to the analysis of mistakes related to numbers in one specific text — the apocryphal *Book of the Secrets of Enoch* (aka *2 Enoch*),[4] as attested in the medieval literary heritage of *Slavia Orthodoxa*.[5] This

[2] Linguistic evidence suggests that at the end of the 9th and the beginning of the 10th century in Preslav literary centre (north-eastern Bulgaria) Glagolitic script was used simultaneously with Cyrillic; see Slavova [1999: 35–46].

[3] See Panaiotov [2001: 258–259], Slavova [1999: 37–38, 42–44], Totomanova [2008: 410–420, 429, 434, 449, 456–457, 510–511, 607–610].

[4] The apocryphal Enochic corpus was embedded in Jewish apocalyptic tradition from the Second Temple period; see Stone [1976: 414–452; 1980], Greenfield and Stone [1979, 89–103], Himmelfarb [1983; 2010], VanderKam [1984], Nickelsburg [2001], Schäfer [2004: 233–274], Reed [2005], Boccaccini [2005], Boccaccini and Ibba [2009]. Originally composed in either Hebrew or Aramaic, it survived in three versions: Ethiopic (*1 Enoch*), Slavonic (*2 Enoch*) and Hebrew (*3 Enoch*). The intellectual ancestry of the *2 Enoch* is that of a multilingual para-Biblical hypertext; its Greek *Vorlage* was based on either Hebrew or Aramaic original, and was fostered by Septuagint-related tradition, which was further influenced by Christian ideology of the Byzantine Commonwealth. Its Slavonic protograph appeared most probably in Bulgaria in the 10th century. Still, opinions

case study, I believe, is representative of the potential epistemological framework of the innovative methodology, which I outlined above.

2. EPISTEMOLOGY OF ERRORS: THE CASE OF THE APOCRYPHAL *BOOK OF THE SECRETS OF ENOCH*

The linguistic analysis of the text indicates that its Slavonic *Vorlage* must have been written originally in Glagolitic script, and only later converted to Cyrillic. Indicative in this respect is the shift between particular numbers in various recensions, and especially the alteration of six to five, due to the different numerical value of the letter 'E' (***есть)*** within the two scripts; while the numeral equivalent of the letter 'E' (***есть***) in Cyrillic alphabet is **5** (е҃), in Glagolitic the same letter has the numeral value of **6**. Thus, when taken to the western side of the fourth Heaven, Enoch sees, according to some of the versions of the apocryphon, five large gates through which the sun sets; according to other versions, however, the number of these gates is six. This kind of discrepancy between various redactions suggests that the

about its origins have differed widely; some scholars deny the existence of the intermediary Greek version, arguing that *2 Enoch* was a direct translation from a Hebrew or Aramaic protograph, while others conclude that its author was a Hellenised Jew from Alexandria who composed the text in Greek. The latter was suggested by Morfill and Charles [1896], who were the first to draw the attention of western scholarship to *Slavonic Enoch* and to publish the text in English translation with extensive commentaries; recensions of *2 Enoch* were further published by J. H. Charlesworth [1983: 91–221] and H. F. D. Sparks [1984: 169–362]. For more details, see A. Pennington's Introduction to her translation of the shorter recension in H. F. D. Sparks' edition [Pennington 1984: 321–326] and F. Andersen's introductory notes to his translation of the longer recension in Charlesworth [1983: 91–100]. Further on the *Book of the Secrets of Enoch* in Slavonic apocryphal tradition, see the discussion in Popov [1880: 66–139], Sokolov [1899, 1905, 1910], Bonwetsch [1896; 1922], Schmidt [1921: 307–312], Ivanov [1925: 165–191], Meshcherskii [1963: 130–147, 1964: 91–108], Navtanovich [2000: 204–241, 387–392], Vaillant [1952], Petkanova [1982: 49–63, 350–352], Santos Otero [1984: 147–202], Böttrich [1991: 35–42; 1996; 1997: 222–245], Alexander [1998: 101–104, 116–117], Panaiotov [2003: 279–283], Orlov [2004:3–29; 2007], Reinhart [2007: 31–46], Badalanova Geller [2010].

[5]The historiographic formula *Slavia Orthodoxa*, together with its counterpart *Slavia Romana* (also referred to as *Slavia Catholica*), was introduced by Picchio [1984]; the terms reflect the 'division of historical Slavdom into two main areas belonging to the jurisdiction of the Eastern Orthodox Churches (*Slavia Orthodoxa*) and to that of the Roman Church (*Slavia Romana*)' [*ibid.*: 1]. Following Picchio's methodology, I approach the institutionalised partition of Central and Eastern Europe between Rome and Constantinople as a *sui generis* linguistic phenomenon; Latin was to function as the *lingua sacra* in *Slavia Romana*, while in *Slavia Orthodoxa* this role was played by Old Church Slavonic. I further argued that, along with *Slavia Romana* and *Slavia Orthodoxa,* another set of terms, reflecting the confessional identity of 'other' (Jewish or Muslim) religious communities should be taken into consideration, with special emphasis on their respective *linguae sacrae*; hence my argument for *Slavia Judaica* and *Slavia Islamica* [Badalanova 1994; 2001; 2002]. The linguistic differentiation between *Slavia Romana* and *Slavia Orthodoxa* (i.e. Latin versus Old Church Slavonic) had a major impact upon future cleavage between the respective cultural traditions: 'within each of these two main areas of civilisation, the self-identification of the Slavs with certain cultural and linguistic systems was directly affected by the ideological and linguistic models that the ecclesiastical organisations introduced into their spiritual patrimony' [Picchio 1984: 3]; see also Picchio and Goldblatt [2008: 66–85]. The fact that the spiritual patrimony of *Slavia Orthodoxa* was anchored by Old Church Slavonic explains why *2 Enoch* was not attested in apocryphal heritage of *Slavia Catholica* and remained a specific product of *Slavia Orthodoxa* exclusively.

terminus ante quem for the translation/compilation of the Slavonic protograph of *2 Enoch* was the period when the transition from Glagolitic to Cyrillic script took place.

On the other hand, there is a widespread misconception regarding the distribution of the two different schemes of the numbers of heavens employed in celestial cosmography of the apocryphal *Book of the Secrets of Enoch*; it is maintained that in the longer recension the number of heavens is *ten*, whereas in the shorter recension the heavens are *seven*. A survey of MSS containing both the longer and shorter recensions shows that in the longer recension the number of heavens is either *seven* or *ten*, whereas in the shorter recension the heavens are usually *seven* (although in some isolated cases they may be *five*); the latter observation was briefly underlined in Iatsimirski's *Bibliographical Review of South-Slavonic and Russian Apocryphal Literature*.[6]

The reason behind these conflicting readings is rather complicated; taken into consideration in this (certainly not only graphic) puzzle of fluctuating numbers of heavens should be various small but significant details reflecting the evolution of Slavonic writing systems. First, it should be noted that in the Glagolitic alphabet the number 7 was marked by the letter ***живѣте***; however, the connection between the letter ***живѣте*** and the number 7 was disturbed in the process of transition from Glagolitic to Cyrillic, since in the Cyrillic alphabet the same letter (rendered as ***Ж***) did not have any numeral value. In order to mark the number 7 (employing Cyrillic characters), the scribes used another letter, ***ʒемлꙗ*** [***З***]. In the Glagolitic alphabet, however, the numeral value of this letter [i.e. ***ʒемлꙗ***] was 9. The number 9, on the other hand, was rendered in Cyrillic alphabet by the letter Θ ***(Θита)***, which occurs at the end of the alphabet. As for the number 8, it was marked in Cyrillic by the letter **И** (***Иже***) which in Glagolitic had the numeral value 20; however, its phonetic twin **I** (***Iota***) the 10th letter in both the Glagolitic and the Cyrillic alphabet, had the numeral value of 10; this is also true for the numeral value of this same letter (ι) in Greek alphabet. In the light of all these variations, it is hardly surprising to have different numbers of heavens in various manuscript traditions from different periods and, perhaps, from different scripts. One possibility is that the actual 7th letter in the Greek alphabet, η, which corresponds phonetically to Glagolitic and Cyrillic **I** (***Iota***), was once used to mark the number of heavens in the now lost Greek *Vorlage*; during the process of its translation into Slavonic, the scribe converted

[6] In the account presented by the version entitled 'О Еносе что был на пятом небеси и исписал 300 книгъ' ['About Enoch who was in the 5th heaven and wrote 300 books'] — briefly mentioned by Popov [1880: 106], Sokolov [1910: 1; part 1 in his Commentaries] with a reference to Pypin [1862: 15]), and Iatsimirskii [1921: 81–82] — the number of heavens is five (which parallels the number of heavens in *The Apocalypse of Baruch*).

the actual 7th letter of the Greek alphabet, η, into either Glagolitic or Cyrillic using its phonetic twin **I** (***Iota***); and since the latter has a numeral value of 10 in both Glagolitic and Cyrillic scripts, the number of heavens was also emended from 7 to 10.[7] This will also explain why the compiler ended up with three extra heavens to describe — an odd detail, which was obviously interpolated in Enoch's monologue against the traditional logic of the narrative of his celestial journey, in which the numbers of heavens is usually seven.[8] As a result, the scribe had to insert pieces of additional information into the account of visionary's encounter with God; here follows the fragment concerned:

> And those men <i.e. the angels> lifted me up from there, and they carried me up to the 7th heaven [воʒдвнгоста мѧ ѿт8д8 м8жїе ѡ҆́нъı на ʓ҃ Н҃бо]. And I saw there an exceptionally great light [н҆ вндѣх т8 свѣтъ велнкъ ѕѣло], and all the fiery armies of the great archangels [н҆ вон ѡ҆́гненнн велнкнх а҆рхагг҃лъ], and the incorporeal forces [беʒплотнъıх снлъ] and the dominions and the origins and the authorities, the cherubim and the seraphim and the many-eyed thrones [н҆ гсдтва, начала, н҆ властн, херȣвнмн, н҆ серафнмн, прстлн н҆ многоѡ҆́чнтїн]; and nine <var. five > regiments and the shining *otanim* stations [ѳ҃ полъковъ, свѣтлостоанїѧ Їѡ҆́аннтское]. And I was terrified, and I trembled with great fear [н҆ оу҆боахсѧ, н҆ вострепетах страхомъ велнкнмъ]. And those men picked me up and let me into their <midst> [н҆ поаша мѧ м8жїе ѡ҆́нъı, н҆ ведоша мѧ въ слѣдъ нх]. And they said to me: 'Be brave, Enoch! Do not be frightened!' [н҆ гл҃аша ко мнѣ. держай е҆ноше не бойсѧ!] And they showed me the Lord, from a distance, sitting on His exceedingly high throne [н҆ покаʒаша Гсда нʒдалече, сѣдащаго на Прстолѣ своем превнсоцѣ]. For what is on the 10th heaven, since the Lord is present there [что оу҆бо е҆стъ ʒане Гсдь т8 пребъıваетъ на ї҃ мъ Н҃бсн]? And on the 10th heaven is God [на Н҃бѣ ї҃ мъ е҆стъ Бг҃ъ], and it is called in Hebrew language *Aravoth* [е҆ѵрейскнм ѩ҆ʒъıкѡм а҆раватъ нарече тсѧ]. And all the heavenly armies came and stood on the ten steps, corresponding to their rank [н҆ всн вон нбснъıн вост8пнша стоах8 на ї҃ степенн по чнн8 нхъ], and they did obeisance to the Lord [н҆ покланах8сѧ Гсдвн]. And then they went to their places in joy and merriment and in immeasurable light, singing with soft and gentle voices, while presenting the liturgy to him gloriously [н҆ пакн вост8пах8 на мѣста своа, в҆ радостн н҆ веселїн, н҆ въ свѣтѣ беʒмѣрнемъ поюще пѣснн малнмн н҆ кроткнмн гласъı, а҆ славнн сл8жаще е҆м8]. <…> And when I had seen all this things, those men said to me: 'Enoch, up to this point we have been commanded to travel with you' [е҆гда вндѣх всѧ сїѧ, рекоша ко мнѣ м8жїе ѡ҆́нъı, е҆ноше, доʒде намъ с тобою е҆сть повеленно соп8тшествоватн]. And the men went away from me, and from then on I did not see them anymore [н҆ ѿндоша ѿ мене м8жїе ѡ҆́нъı, н҆ ктом8 не вндѣх нх]. But, I remained alone at the edge of the seventh heaven [н҆ а҆ʒъ ѡ҆́стах е҆дннъ на концн ʓ҃-го Н҃бсн]. And I became terrified [н҆ оу҆боахсѧ]; and I fell on my face and I said in myself [н҆ падох на лнцн своемъ н҆ рѣхъ в себѣ]: 'Woe to me! What has happened to me?' [оу҆въı мнѣ. что мѧ ѡ҆́брѣте?] And the Lord sent one of his glorious ones, the archangel Gabriel [посла Гдсь е҆днаго ѿ славнъıх своих а҆рхагг҃ла Гаврїнла]. And he said to me [н҆ рече ко мнѣ]: 'Be brave, Enoch! Do not be frightened [держай е҆ноше не бойсѧ]! Stand up, and come with me and stand in front of the face of the Lord forever!' [востанн пред лнцем Гсдннмъ въ вѣкн, востанн пойдн со мною] <…> And Gabriel carried me, like the leaf carried up by the wind [н҆ восхнтн мѧ Гаврїнлъ, ѩ҆ко же лъıстъ восхнщаемъ вѣтромъ]. He moved me along and put me down in front of the face of the Lord [н҆ поставн мѧ пред лнцем Гсднъıмъ]. And I saw the eighth heaven, which is called in the Hebrew language

[7] See also the discussion in Forbes and Charles [1913: 442, footnote XXI. 6].

[8] The model of 'seven heavens' is likewise represented in other apocryphal writings (such as *The Ascension of Isaiah*, *The Sea of Tiberias*), and in erotapocritic tradition. In some texts (e.g. *The Discussion Between the Three Saints*) each heaven is allocated to a different Biblical patriarch; thus ***Снть*** (Seth) is in the First Heaven, in the Second is ***Аʒарь*** (Azariah), in the Third – ***Еновь*** (Enoch), in the Fourth – ***Ное*** (Noah), in the Fifth – ***Аврамь*** (Abraham), in the Sixth – ***Нсакъ*** (Isaac), and in the Seventh – ***Нѩковъ*** (Jacob). A similar model of the sevenfold heavens (which are paralleled by the seven earths and/or the seven compartments of hell) is attested in oral tradition.

> *Muzaloth* [видѣхъ и҃-е Нб҃о, е̂же нареҷется е̂ѵрейскимъ ꙗ̂ꙁыкомъ М8ꙁалоѳъ], the changer of the seasons [премѣнитель временемъ], of dry and wet [с8хоти, и̂ мокроти], and the 12 zodiacs [двaнадесѧтим ѕодїамъ], which are above the seventh heaven [е̂же с8ть верх8 ꙁ҃-го Нб҃си]. And I saw the ninth heaven [и̂ видѣхъ ѳ҃-е Нб҃о], which in the Hebrew language is called *Kukhavim* [е̂же по е̂ѵрейск8 ꙁовемъ К8хавъıм], where the heavenly houses of the 12 zodiacs are [и̂деже с8тъ домове небеснїи ꙁодїамъ дванадесѧтимъ]. <…> And on the tenth heaven, *Aravoth* [на десѧтомъ Нб҃си А҆равоѳъ], I saw the view of the face of the Lord, like iron made burning hot in a fire [видѣх видѣнїе лица Гсдна, ꙗ̂ко желѣꙁо раꙁж҃жено въ ѡ̂гни, и̂ и̂ꙁнесенно, и̂ и̂скръı п8щающи, и̂ ж҃жетъ]. Thus even I saw the face of the Lord [Тако а̂ꙁъ видѣхъ лице Гсдне].[9]

There are some significant details in the above quoted text that deserve special attention. Only here are the 'surplus heavens' given special names; this is rather symptomatic, since the scribe does not mention any of the first seven heavens by name. He is anxious to designate only 'the superfluous ones' (i.e. the eighth, the ninth and the tenth). In other words, only those heavens which appear to be incompatible with the (otherwise) dominant scheme of the seven heavens are defined by special appellations. As pointed out by Andersen, these particular passages (which are also missing from all other text-witnesses of *2 Enoch*), are clearly interpolations. On the other hand, it is quite intriguing that the designations of the three additional heavens (i.e. *Muzaloth*,[10] *Kukhavim*[11] and *Aravoth*[12]) are in fact 'domesticated' Slavonic renditions of otherwise 'genuine Hebrew words'.[13] This specific detail betrays the scribe's attempt not only to iron out the problematic details concerning the troubling deviations from the conventional patterns of heavenly topography, but also to revive the dormant memory of the 'Jewish lineage' of the *Slavonic Enoch*. Indeed, according to the description found in Babylonian Talmud *Hagigah* 12b,

> *'Araboth* is that in which there are Right and Judgment and Righteousness, the treasures of life and the treasures of peace and the treasures of blessing, the souls of the righteous and the spirits and the souls which are yet to be born, and dew wherewith the Holy One, blessed be He, will hereafter revive the dead. Right and Judgment, for it is written: Right and Judgment are the foundations of Thy throne. Righteousness, for it is written: And He put on Righteousness as a coat of mail. The treasures of life, for it is written: For with Thee is the fountain of life. And the treasures of peace, for

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[9] The description of the three additional heavens is attested in only two texts:
1). MS № 13.3.25 (fols. 93a–125a) from the Academy of Sciences Collection (St Petersburg), which is a Bulgarian redaction, copied in the 15th–16th century in Romania. It forms the basis of the English translation of the longer recension of *2 Enoch* (MS ***J***) produced by F. Andersen [1983: 135–136].
2). Poltava MS (fols. 1–25) from the Khludov Collection of the State Historical Museum (ГИМ, Собрание Хлудова); copied in 1679 in Poltava. According to Meshcherskii [1964: 93], it is a 'poorly copied, full of scribal errors version of an earlier Moldavian-Bulgarian MS' which is 'rather close in its content to MS № 13.3.25 from the Academy of Sciences Collection (St Petersburg)'. The MS was first published by A. Popov in 1880 in Vol. 3 of the *Transactions of the Historical and Archaeological Society of the University of Moscow* [1880: 67, 75–83, 89–139]. The edition of Popov was used as a primary witness to the text of the longer recension in the translation of *2 Enoch* into English (by Morfill and Charles, and later by Forbes and Charles) and into German (by Bonwetsch).

[10] That is, ***М8ꙁалоѳъ*** (= Mazzalôt).

[11] That is, ***К8хавъıм*** (= Kokabîm).

[12] Rendered in Slavonic sources as either ***А̂раватъ*** or ***А̂равоѳъ*** (= Aravôt/Arabôt).

[13] See the discussion in Andersen [1983: 134–137, and especially footnotes 20a, 20d].

it is written: And called it, 'The Lord is peace'. And the treasures of blessing, for it is written: he shall receive a blessing from the Lord. The souls of the righteous, for it is written: Yet the soul of my Lord shall be bound up in the bundle of life with the Lord thy God. The spirits and the souls which are yet to be born, for it is written: For the spirit that enwrappeth itself is from Me, and the souls which I have made. And the dew wherewith the Holy One, blessed be He, will hereafter revive the dead, for it is written: A bounteous rain didst Thou pour down, O God; when Thine inheritance was weary, Thou didst confirm it. There [too] are the Ofanim and the Seraphim, and the Holy Living Creatures, and the Ministering Angels, and the Throne of God; and the King, the Living God, high and exalted, dwells over them in *'Araboth*, for it is said: Extol Him that rideth upon *Araboth* whose name is the Lord (Ps. 68: 5).

As pointed out by Jastrow, in rabbinic texts the lexeme *'Arabôt* functions as 'a poetic name for heaven'. In actual fact, in Hagigah 12b it denotes 'the seventh heaven', in which dwell righteousness and justice.[14] As for the Hebrew lexeme *Kokabîm*, it is used as a common term designating stars, planets and zodiac signs.[15] The lexeme *Mazzalôt* has a similar semantic coverage: it means 'planet', 'constellation', and even 'luck'.[16] Instead of solving the puzzle of perplexing cultural processes behind the 'domestication' of *2 Enoch* in *Slavia Orthodoxa*, this captivating lexicographic evidence raises even more challenging questions. What does 'the language of errors' say about the scribe who copied this manuscript? What was his intellectual background? Did he know Hebrew? And if so, what was the source of this knowledge? Shall we consider the discrepancies in his manuscript as witnesses to a bilingual scribal tradition, or to the lack of such a tradition? One thing is clear: the questions raised by 'scribal errors' and 'corrupt readings' make us aware of our own epistemological deficiency in grasping these phenomena.

[14] See Jastrow's *Dicionary of the Targumim, the Talmud Babli and Yerushalmi, and the Midrashic Literature* (Vol. 2) [1950: 1113].
[15] See Jastrow, *op. cit.* (Vol. 1) [1950: 619].
[16] See Jastrow, *op. cit.* (Vol. 2) [1950: 755].

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GLAGOLITIC	NUMERICAL VALUE	CYRILLIC	NUMERICAL VALUE	LETTER'S NAME
Ⰰ	**1**	а	**1**	аꙁъ
Ⰱ	**2**	б	—	боукы
Ⰲ	**3**	в	**2**	вѣдѣ
Ⰳ	**4**	г	**3**	глаголи
Ⰴ	**5**	д	**4**	добро
Ⰵ	**6**	е	**5**	естъ
Ⰶ	**7**	ж	—	живѣте
Ⰷ	**8**	ѕ	**6**	ѕѣло
Ⰸ	**9**	ꙁ	**7**	ꙁемлꙗ
Ⰹ	**10**	і, ї	**10**	і
Ⰻ	**20**	и	**8 (10)**	иже
Ⰼ	**30**	—	—	г'ервь
Ⰽ	**40**	к	**20**	како
Ⰾ	**50**	л	**30**	людье
Ⰿ	**60**	м	**40**	мыслите
Ⱀ	**70**	н	**50**	нашь
Ⱁ	**80**	о	**70**	онъ
Ⱂ	**90**	п	**80**	покои
Ⱃ	**100**	р	**100**	рьци
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About the Authors (in alphabetical order)

Markus Asper studied Classics and Philosophy at Freiburg and Vienna. He did his PhD work at Freiburg on the Hellenistic poet Callimachus and his habilitation on Greek science writing. After being assistant professor at Konstanz, he moved to Princeton's Institute for Advanced Studies in 2003 and, in the following years, held assistant professorships at Penn State and New York University. Currently, he is Professor of Classics at Humboldt University at Berlin. Asper has published on Hellenistic poetry and ancient Greek science and its literatures.

Hagan Brunke a is mathematician, physicist, and assyriologist. He graduated in physics from Technische Universität München with a thesis on string theory and obtained his PhD in mathematics in 1998 at Ludwig-Maximilians-Universität München with a thesis on knot theory. While holding positions as a research assistant at university and as a project manager in software development, he studied assyriology with aegyptology as a minor at Ludwig-Maximilians-Universität München where he obtained his PhD in assyriology in 2008 with a thesis on Sumerian food. Since then he has been working as a researcher at the Institute for Mathematics at Humboldt-Universität zu Berlin and the Institute for Ancient Oriental Studies at Freie Universität Berlin, partially within the TOPOI excellence cluster. At present, his main research interests are Sumerian economy, especially of the Ur III period, and Mesopotamian mathematics.

Florentina Badalanova Geller finished her doctorate at the Moscow State University in 1984 and from 1985 held tenured positions at the Bulgarian Academy of Sciences and University of Sofia as Associate Professor of Anthropology and Folklore Studies. In 1994, she accepted an appointment at the University of London where she taught Slavonic Studies until 2004, when she moved to the Royal Anthropological Institute; she became Honorary Research Associate at University College London in 2005. From 2007–2009 she was visiting scholar at the Max Planck Institut for the History of Science (for total 13 months), and from 2010 she was appointed as Guest Professor at the FU in the Topoi Excellence Cluster, where she has been teaching courses on "Anthropology and the Bible", "Religious art and Iconography", "Unholy Scriptures: Apocryphal Heritage", "Ethno-Medicine", and "Quran in vernacular". Her main research interests are in vernacular receptions of Abrahamic faiths (Folk Bible), anthropology of religion and para-biblical literature, religious iconography, ethnomedicine, ethnolinguistics and ethnohermeneutics.

Mark Geller studied at Princeton and Brandeis Universities in the USA before coming the University College London in 1976, where he is Professor of Semitic Languages, and since 2010 has been on secondment to the FU Berlin as Gastprofessor for the History of Science. He has held visiting fellowships from the Alexander von Humboldt-Stiftung, Netherlands Institute for Advanced Studies (NIAS), and Max-Planck Institute for the History of Science, and has an honorary doctorate from the New Bulgarian University. His main current research interests are in history of ancient science and ancient Babylonian medicine.

Klaus Geus is an ancient historian, philologist and geographer. In 2009 he was appointed full professor of the historical geography of the ancient world at the *Freie Universität Berlin*. Geus works in the areas of ancient sciences (especially geography, astronomy and metrology) and ancient historiography (especially Herodotus and Polyaenus). He is also interested in the spread and distribution of Greek knowledge to other ancient civilizations and regions (especially the Carthaginians, Armenia, the Red Sea and the Horn of Africa). He has published more than a dozen books and hundred papers and articles. With Mark Geller, he set up the Dahlem Seminar for the History of Ancient Sciences at Berlin in 2010.

Colin Guthrie King studied philosophy and astronomy in the US, graduated in Classics and philosophy in Berlin, Zürich and Paris, and completed a doctoral dissertation in 2009 at the Humboldt-Universität zu Berlin under the supervision of Prof. Dr. Christof Rapp (title of dissertation: “Aristotle’s Endoxa: A study in epistemology and the theory of argument”). King is interested in the history of ancient philosophy and science, and works on issues within and across the fields of logic, theory of argument, epistemology, and theory of science. His main concentration is Aristotle and Peripatetic philosophy, as well as the reception and study of Aristotle in German philosophy in the 19th Century.

Graeme Lawson studied music and archaeology at Newcastle University, England, in the early 1970s, completing his doctoral thesis in music archaeology at Cambridge in 1981. Building on his lifelong interests in the biological sciences, his particular aim has been to evidence the origins and map the evolution of musical and related behaviours through archaeology's material record. Since 1993 he has been a Fellow of Cambridge's McDonald Institute for Archaeological Research, and since 2009 has collaborated as Senior Fellow with colleagues at Berlin's *Excellence Cluster TOPOI: the Formation and Transformation of Space and Knowledge in Ancient Civilizations*. He has published many papers and technical reports in the archaeological literature, covering both musical and acoustical topics. His long-term scientific programmes include the early medieval musical burials of Southeast England and the microscopy of surfaces of European musical bone pipes, from the High Middle Ages to remote Prehistory. Alongside the development of new methodologies in archaeoacoustical research, he has made a particular study of the role of music in archaeological thought during the 18th and 19th centuries. Amongst his current high-profile projects in the UK are studies of the musical and poetical significance of the tomb of the 'Prittlewell Prince' (Essex, Anglo-Saxon, early 7th century AD) and of the finds and environs of High Pasture Cave (Island of Skye, Iron Age, 4th century BC). Amongst current co-editorships he is working with TOPOI colleagues on the publication of *Sound, Political Space and Political Condition*, a TOPOI conference held at Deutsches Archaeologisches Institut, Berlin, in 2011, and has contributed to 'Klangräume', part of the special TOPOI exhibition *Jenseits des Horizonts: Raum und Wissen in den Kulturen der alten Welt* at Pergamon Museum, Berlin (June-August 2012).

Alexandra von Lieven studied Egyptology, Comparative Religion and a few terms Assyriology/Near Eastern Archaeology, Classical Archaeology and Indology at Tübingen, where she also obtained her PhD degree in 2002. Until 2008, when she finished her Habilitation, she was assistant at the Egyptological Seminar of Freie Universität Berlin. She held a Visiting Professorship in winter term 2008/09 at Tübingen and a ten month Research Fellowship at the Lichtenberg-Kolleg Göttingen in 2010/11. Since 2009, she is a Heisenberg-Fellow of the Deutsche Forschungsgemeinschaft (DFG) at FU Berlin. Her main research interests include Ancient Egyptian religion, history of the sciences, Greco-Roman Egypt and music archaeology, areas in which she has published extensively since 1997.

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Irina Tupikova studied astronomy at the mathematical faculty of St. Petersburg University (Russia). She obtained her PhD degree in Mathematics and Physics and worked as Senior Scientific Advisor in Celestial Mechanics at the Institute for Theoretical Astronomy (St. Petersburg). Since 1996 Tupikova is working as a professional astronomer at the Lohrmann Observatory of the TU Dresden. She has published extensively on perturbation theory in celestial mechanics but has also – due to grants and fellowships at the Max-Planck-Institute for the History of Science and the TOPOI Excellence Cluster in Berlin – strong interests in the history of ancient astronomy and mathematics. With Pietro Omodeo she has published MPIWG preprint 422: *Ptolemy on Geocentrism: Diverging Argumentative Strategies and Epistemologies.*

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