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CHANGING TRENDS IN THE HISTORIOGRAPHY OF MESOPOTAMIAN MATHEMATICS

—AN INSIDER'S VIEW—

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CHANGING TRENDS IN THE HISTORIOGRAPHY OF MESO-POTAMIAN MATHEMATICS —AN INSIDER'S VIEW—

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To the memory of the founding fathers:
OTTO NEUGEBAUER, FRANÇOIS THUREAU-DANGIN, SOLOMON GANDZ, **and** *KURT VOGEL*

TABLE OF CONTENTS

I. The heroic era, 1930 to 1940 2

II. The triumph of translations, 1940 to 1975 14

III. Fresh start from sources through new approaches, 1971 onwards 26

IV. The situation 33

Postscript 38

Bibliography and abbreviations 39

Habent sua fata libelli. So had C. P. Snow's *Two Cultures*, published in 1964 and containing lectures held in 1959. Dealing centrally with the attitude of rich toward poor countries and with the importance of planned technological development, it was only remembered for its secondary, introductory aspect: The mutual fear and distrust between literary and scientific culture.

The reason is obvious: Those who shared the central concern of Snow's book did not need his kind of argument for doing so. The secondary aspect, however, struck a strong-sounding chord on the mental and cultural keyboard of 1959-1964.

This chord—the reticence and anxiety of humanists when confronted with natural science and mathematics, and the contempt or ignorance of the latter regarding the insights and concerns of humanist scholarship—also explains much of what happened to the study of Mesopotamian mathematics from 1945 to c. 1980 (even disciplines have their fate, indeed)[1]. The split of the two cultures is what marks off this intermediate period from the initial phase where the existence of advanced Babylonian mathematics was discovered; fading of the split, on the other hand, is what characterizes the latest years.

In the following, I shall trace the general tendencies which have characterized the development of the field since 1930, basing myself on this certainly only approximate periodization.

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[1] To some extent, of course, all history of science is ridden by the same dichotomy: Is history of science to be done and judged as *history*, or does it belong within the realm of the sciences. Logically, one would perhaps opt for the former answer; according to the down-to-earth sociology of the pay-roll and the institutional affiliations of most historians of science, however, most historians of science are *scientists*.

Yet, even if the problem is shared by all history of science, it becomes more outspoken when the philology and history of the period involved gives outsiders the impression of an occult science, as it is the case of Assyriology.

I. THE HEROIC ERA, 1930 TO 1940

Two preliminary remarks should be made in order to clear away misunderstandings. Firstly: the following is not concerned with Babylonian *astronomy*, nor with cuneiform astronomical calculation. To some extent, the reason for this restriction is pragmatic—the existence of complex astronomical calculation was discovered long before the existence of sophisticated mathematics *strictu sensu*. The primary reason, however, goes deeper: Babylonian astronomical calculation is a late occurrence, which only emerged a thousand years after the culmination of genuine mathematical interest, and which was carried by a professional environment of a different character than that which had created Babylonian *mathematics*.[2]

Secondly: Making 1930 the starting point does not mean that nothing was known about Babylonian mathematics before that date. Knowledge of the sexagesimal place value system used in mathematical texts, the mixed decimal-sexagesimal absolute value system of economical texts, certain metrologies, tables of reciprocals, squares and cubes, and instances of land mensuration and other practical computations—all this became known from the 1850s onwards[3].

In the late 1920s, Neugebauer entered the field, thus inaugurating the rise of the *specific* study of Babylonian *mathematics*, understood as a moment in the unfolding of mathematics rather than as an aspect of

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[2] The rather few mathematical texts which we know from the late period, it is true, were written by and for members of the astronomical environment, a context which seems to have influenced the mathematical mode of thought.

[3] A survey of publications from the period 1854 to 1929 with relevance for the understanding of Mesopotamian mathematics will be found in Friberg 1982: 1-36.

Assyriology or in relation to Classical lore (»Plato's number«, etc.). Whatever one may think with hindsight of the principle, this turn was obviously necessary to crack those complex texts which were now taken up. This appears with great clarity if one compares Neugebauer's analysis (1929) of texts concerned with the partition of trapeziums with the first attempts at translation of the same texts made by Carl Frank (1928). Neugebauer's prophetic conclusion (p. 79f) should be quoted:

> Man darf wohl sagen, daß in den vorliegenden Texten ein gutes Stück babylonischer Mathematik zutage liegt, unsere nur alzu dürftigen Kenntnisse dieses Gebietes um wesentliche Züge zu bereichern. Ganz abgesehen von der Verwendung von Dreiecks- und Trapezformel sehen wir, daß komplizierte lineare Gleichungs*systeme* aufgestellt und gelöst werden, daß man ganz systematisch Aufgaben *quadratischen* Charakters stellt und zweifellos auch zu lösen verstand—und all dies mit einer Rechentechnik, die der unseren völlig äquivalent ist. Bei einer solchen Lage der Dinge bereits in altbabylonischer Zeit wird man in Hinkunft auch die spätere Entwicklung mit anderen Augen anzusehen lernen müssen.

In a postscript added in print. Neugebauer acknowledges the decisive role of H. S. Schuster in the interpretation of the text[4]. In the following (second) fascicle of *Quellen und Studien*, Neugebauer (1930) and Schuster (1930) each had an article dealing (in Neugebauer's case among other things) with Old Babylonian and Seleucid[5] solutions of second-degree problems, respectively. Already in the first fascicle, Neugebauer and Struve (1929a) had investigated the Babylonian way of dealing with circles, circular segments, and truncated cones.

[4] According to what I was told in 1985 by Kurt Vogel, Schuster was in fact the first to discover the Babylonian solution of second-degree problems.

[5] The Old Babylonian period goes from 2000 B.C. to 1600 B.C. (the mathematical texts seem to come from the later half of the period); the Seleucid period goes from 312 B.C. to 64 B.C.

To which extent these publications mark a watershed is revealed by an slightly ironical remark made by Neugebauer in the preface to MKT I (p. v):

> Wenn ich sage, daß es *von Anfang an* meine Absicht gewesen wäre, eine Edition aller erfaßbaren mathematischen Keilschrift-Texte zu veranstalten, so soll das heißen, daß diese Arbeit zwar nie ihre Grundtendenz verändert hat, um so mehr aber ihren Umfang. Das erste, bereits 1929 »Druckfertige« Manuskript erfaßte nur die ca. zwei Dutzend Tabellentexte aus Hilprechts Publikation BE 20,1, die drei Londoner Texte BM 85 194 und BM 85 210 aus CT IX, BM 15 285 aus RA 19 (Gadd), die beiden Pariser Texte AO 6456, AO 6484 (TU 31 und 33), und schließlich die sechs Texte aus Frank SKT. Das war nicht die hälfte der jetzigen Kapitel I bis III und Kapitel V.

Apart from the corpus of tables, even this early list consists of texts which had not been interpreted before—among which those which Schuster, Struve and Neugebauer dealt with in *Quellen und Studien* in 1929-30. That is, already the supposedly »print-ready« manuscript from 1929 was a decisive leap forward—yet the impetus created by this initial breakthrough made the leap look like a quite modest step in the perspective of 1935.

It is characteristic that Schuster's and Neugebauer's articles (and a number of others) were published in the newly founded *Quellen und Studien zur Geschichte der Mathematik, Astronomie und Physik*, and not in the Assyriological literature. Schuster could also point out (1930: 194) that Thureau-Dangin, in the first publication from 1922 of the Seleucid tablet under discussion, had only identified its contents as »opérations arithmétiques«. However, Thureau-Dangin (who in fact had contributed decisively since the mid-nineties to the knowledge of Mesopotamian metrology and computational techniques) immediately took up the challenge, and investigated texts of similar mathematical complexity in the *Revue d'Assyriologie*. But still, and in spite of the immediate audience of the *Revue* and a sometimes more precise reading of the texts, the approach was the same: Babylonian mathematics was related by

Thureau-Dangin no less than by Neugebauer to categories of later mathematics.

Why was the breakthrough produced at Neugebauer's seminar at Göttingen University and not by a competent philologist like Thureau-Dangin, whose interest in the matter was conspicuous, and whose mathematical competence turned out in the 1930s to be fullyy sufficient? Paradoxically, the answer to this question has to do with that very complexity of cuneiform writing which would make one expect the philologist to be best fit for the task.

Firstly, most cuneiform signs are plurivalent. They may carry one or several logographic meanings (not necessarily related), to which comes one or more groups of phonologically related syllabic readings. Specific text types have their particular usages, which reduce the ambiguity—but only when the characteristic usages have been discovered.

To this we can add, secondly, the terminology itself. Like all technical terminologies, Babylonian mathematical terminology was ultimately derived from daily language—but often technical meanings cannot be guessed from general meanings, even when these are known. They have to be derived from the mathematical procedure used—which itself is hard to get at, if one is does not understand the terminology[6].

Without having observed the processes directly, one may surmise that only scholars with thorough mathematical training (and, certainly, with a level of cuneiform competence approaching that of many Assyriologists) would possess sufficient creative phantasy to crack the codes from the numbers in the tablets (numbers mostly written in a sexagesimal place value system without indication of absolute place and hence ambiguous) and a rudimentary understanding of the connecting words. Once this step was taken, Assyriologists with less exhaustive mathematical competence would be able to join in and improve readings by philological means.

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[6] Further explanation and exemplification in Høyrup 1990: 43-45.

This was what happened, and the 1930s were dominated by a passionate though correct and very polite race between Neugebauer and Thureau-Dangin, whose philological level was supreme, whose interest in mathematics was longstanding, and whose understanding of mathematics and knowledge of its early history proved entirely adequate[7]s from Neugebauer's hand culminated in the publication of the *Mathematische Keilschrift-Texte* I-III in 1935-1937, while Thureau-Dangin's decisive achievements were published in article form, not least in the *Revue d'Assyriologie*, which he directed together with Vincent Scheil. His *Textes Mathématiques Babyloniens* from 1938 was presented as an attempt to »mettre des documents à la disposition des historiens de la pensée mathématique« (TMB, xl) at a more accessible price than the MKT (von Soden 1939: 144). For this reason, the philologically »inconvenient« method of transcribing Sumerograms into Akkadian was adopted—for precise philological purposes, the reader was referred to original publications (at times Thureau-Dangin's own, at times those in MKT).

Important contributions to the field were also published by Kurt Vogel and Solomon Gandz, both of whom had their main interests elsewhere, and both of whom brought their distinctive perspective. In spite of this, however, and in spite of the different starting points of Neugebauer and his collaborators on one hand and Thureau-Dangin on the other, a coherent approach to the history of Mesopotamian mathematics emerged and came to monitor the way both general historians of mathematics and Assyriologists saw the matter for decennia.

First of all, the field was seen specifically as "Babylonian mathematics«. What little was known about practical computation in the Sumerian third millennium disappeared from view (presumably as »not really mathematics«[8]). »Genuinely« mathematical texts were only known from

[8] In MKT I, a number of presumed Ur III (21st century B.C.) tables of reciprocals had been listed. Still, the mathematical substance of these was evidently soon exhausted, and as long as mathematical procedures and techniques were asked for, only the late, multi-place tables were subjected to further investigation.

the Old Babylonian and the Seleucid period (with one or possibly a few exceptions, which might be a bit younger than Old Babylonian). None the less, »Babylonian mathematics« stood forward as one immutable entity. Schuster (1930: 194) had found it important to point out that his investigation of a Seleucid tablet gave insight »in die mathematischen Kenntnisse [...], die noch zur Griechenzeit in Babylonien existierten« and demonstrated »Kontinuität der orientalischen Tradition von sumerischer Zeit bis weit in den Hellenismus«, while on the other pointing to a conspicuous change in mathematical terminology taking place between the Old Babylonian and the Seleucid period (originally distinct operations losing their proper designation and thus—we may add—perhaps their proper identity)[9]. At the end of the decade, the former conclusion had become a trivial matter of fact—maybe because of its agreement with the stereotype of an »immutable Orient«. The latter observation had largely come to be neglected—it regarded only the »history of terminology«, seemingly a purely philological and somewhat pedantic concern.

The separation of the *history of mathematics* from the *history of terminology* is a particular instance of another characteristic of the resulting ruling approach: The separation of philology and mathematics, and the exclusive reading of the sources for their *mathematical* content.

This had not been Neugebauer's intention. To the contrary, he had claimed (1932: 222) that the transcription of Sumerograms into Akkadian

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The above statement does not mean that *nobody* looked at older mathematical techniques. For one, F.-M. Allotte de la Fuye, who had produced important publications on such subjects for decades, continued to do so. But his text material and his results were not understood as belonging to the history of (Babylonian) mathematics.

[9] Neugebauer (1932a: 6) had been even more cautious; he presented the existence of a Sumerian prehistory to Old Babylonian advanced algebra as a hypothesis which was close at hand but unsupported by positive evidence. An important part of the same article is also dedicated to terminological differences and changes, and the statement that »sich das *inhaltliche* Niveau [from c. 1700 to c. 300 B.C.] nicht sehr erheblich verändert hat« is characterized as »selbstverständlich nur eine Aussage 'in erster Näherung'«.

destroyed the »*fundamentale sachliche Rolle der Ideogramme:* daß sie nämlich volkommen wie mathematische Symbole wirken«. But precisely his emphasis on *this* aspect of the relation between terminology and mathematical thought was taken as justification for a translation into modern mathematical symbols, and thus as a reading as *modern mathematics*—in particular when the statement was read by others who knew neither Sumerian nor Akkadian but trusted the translations and the mathematical commentaries.

Rather unreflecting[10] reading, if not *as* then *through the categories of* more recent mathematics was in fact what characterized the main workers in the 1930s albeit with important shades, and what distinguishes no less the average picture of Babylonian mathematics which emerged and was accepted.

It is often claimed that Neugebauer was the most modernizing of all. This is more than a half mistake, a mistake which is due to careless reading of MKT. In commentaries to the texts, it is true, unrestricted use is made of symbolic algebra; but the aim is to show that the computational procedures employed by the Babylonian calculators are correct (or, at rare occasions, mistaken). The mathematical commentary is not claimed to map the ideas or methods of the Babylonians[11]. When

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[10] A distinction between »unreflecting« and »critical« reading through the categories of more familiar mathematics is important. Explanation always has to represent the categories which are to be explained by others which can be supposed to be known, *and which are necessarily different*. »Unreflecting« translation of categories is »one-to-one«, while »critical« translation will be »network-to-network«. In itself there is nothing wrong in describing a problem »I have added the measuring number of the side and the area of a square, and the result was 110« as »an equation«; this is in fact *the closest we can get* in terms of familiar notions. But an explanation which stops at this point, instead of discussing the *particular* character of the »equation«, the way it differs from and the way it is similar to a modern equation in x and y, is no explanation but a *replacement* of an ancient by a modern conceptual structure.

[11] A few cases can be found where Neugebauer is mislead himself and takes the justification to be the only possible interpretation. So in his (1932a: 21f), a commentary to problem #3 of the tablet AO 8862. In the discussion of the same problem in MKT (I, 120), however, the mistake is eliminated.

making general statements, Neugebauer would normally take care to put the terms *Algebra* and *algebraisch* in quotes (so 1932a: 24, and MKT III, 79). His idea was that Babylonian mathematics was *numerical*, springing from the use of sexagesimal computation and fertilized by the advantage offered by ideographic writing (MKT III, 79). He warned against overrating Babylonian mathematics, which he felt contained »an keiner Stelle etwas, was als unerwartete Glanzleistung angesehen werden müßte«, when only »die ungeheure Schwierigkeit und Langsamkeit der Entwicklung der *allereinfachsten* mathematischen Grundbegriffe, vor allem einer wirklichen Rechentechnik« had been overcome (*ibid*, 80).

Regarded closely, Neugebauer's »modernization« in the MKT thus reduces to the application of *numerical* conceptualization. Even though his restorations of damaged texts shows him to have been very sensitive to the terminological distinction between different »additive« and different »multiplicative« operations (cf. below), he understood the operations as *addition and multiplication of numbers*.

This does not apply, it is true, to Neugebauer's more popular lectures on *Vorgriechische Mathematik*[12]. Here it is stated, e.g., that Babylonian mathematics »ihrem ganzen Niveau nach eine algebraische Stufe erreicht hat, die erst zu Beginn der neueren Geschichte wieder errungen worden ist« (Neugebauer 1934: 172). One of the texts which he had discussed very cautiously in his article from 1929 is also presented in a way which makes it impossible to distinguish *justification through* from *interpretation as* algebra, and it is stated quite bluntly that »die Formulierung ist zwar hier noch eine geometrische, aber die Ausrechnung selbst ist nichts als ein rein algebraisches Bestimmen von Unbekannten auf Grund gewisser gegebener Relationen« (1934: 179).

On this point, Thureau-Dangin's stance was identical with the attitude which Neugebauer had expressed in the context of popularization. So, in an article on "L'Équation du deuxième degré dans la

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[12] Another exception is his article on »geometric algebra« (1936), to which I return below.

mathématique babylonienne" he tells his interpretation through symbolic algebra to be a reconstruction of demonstrations not given in the text but evidently lying behind (1936: 28). In his introduction to the TMB he also speaks about algebra without hesitation or qualification, while suggesting that the kind of algebra involved is similar to the one later taught by al-Khwārizmī—a position which is made more explicit in an article on "L'Origine de l'algèbre" (1940: 301).

Thureau-Dangin's concept of »algebra« was completely numerical. Second-degree problems were formulated so as to deal with plane figures, he claimed (1940: 302), simply because

> une figure plane, telle notamment qu'un triangle, un carré ou un rectangle, donne facilement lieu à une équation du second degré, mais les problèmes qu'en tirent les Babyloniens ne relèvent pas plus de l'algèbre géométriques que, par exemple, les problèmes indéterminés que traite Diophante dans son livre VI et dont il emprunte les éléments au triangle rectangle. Il s'agit dans les deux cas de problèmes purement numériques.

So far, Thureau-Dangin's position was thus more or less shared either by Neugebauer the high-level popularizer or by Neugebauer the meticulous scholar. A point where they differ is in Thureau-Dangin's repeated reference to the »method of false position« (1938; 1940: 316f), which Neugebauer seems never to have mentioned.

Like the comparison with al-Khwārizmian rhetorical algebra, this is an illustrative instance of Thureau-Dangin's tendency to read Babylonian mathematics through the concepts and categories of other pre-modern mathematical cultures—a tendency which he shares with both Gandz and Vogel, while Neugebauer followed the maxim *Hypotheses non fingo* in this question as closely as possible.

Gandz contributed to the field in various ways, not least through his competence as a Hebrew scholar. His contribution to the *profile* of the field, however, was a monographic article on "The Origin and Development of the Quadratic Equations in Babylonian, Greek, and Early Arabic Algebra" (1937). As suggested by its title, the article claimed that both

Greek and al-Khwārizmian algebra descended from the corresponding Babylonian discipline. »Greek algebra« not only meant the algebra of Diophantos (more precisely, that modest part of Diophantos' *Arithmetic* which is concerned with determinate second-degree problems) but also the »geometric algebra« of *Elements* II—an issue to which I shall return below.

Making use of earlier work by Vogel, Gandz introduced a comparative classification, which has remained influential, not least because it brought some order to the vacillating identification of Babylonian problems with modern symbolic equations. At the same occasion, however, it disseminated the belief that this classification as well as its formal expression corresponded directly to what was found in the Babylonian (and Greek and Arabic) texts[13].

Algebra was thus established as a discipline which the Babylonians had created, with the Medieval algebra type as the example through which the term was primarily understood. Mathematical fields and concepts without pre-Renaissance antecedents were mentioned occasionally as a characterization of one or the other Babylonian text but with much greater caution. »Logarithms« turn up in MKT I, 362, in the statement that a reverse compound interest problem »der sache nach« asks for a solution which »*mit* $n=\log_2(K/a)$ *irgendwie äquivalent sein muß*«; but it is argued very clearly in the following pages that this does not correspond to the Babylonian procedure; the same rejection of the

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[13] The problematic nature of this belief can be illustrated on Euclidean material. Even *if* we accept the theses that, e.g., *Elements* II, prop. 5 should be read, firstly, as *algebra*, and, secondly, as *an equation* and not as an algebraic identity, how do we know that the statement »if a straight line be cut in equal and unequal segments, the rectangle contained by the unequal segments of the whole together with the square on the straight line between the points of section is equal to the square on the half« (transl. Heath 1926: I, 382) is to be translated into $x+y=a$, $xy=b$, and not into $ax-x^2=b$? Indeed Heath, in his commentary, gives the latter equation as his main interpretation and the former only in passing. In certain Babylonian problem, the situation is definitely no better.

logarithmic interpretation is given in (Neugebauer 1934: 198). Nor is »theory of numbers« postulated directly,—only, reticently, as »eine Art elementarer Zahlentheorie«, which is then referred to »Pythagorean« arithmetic (MKT III, 80).

A substantial share of the Babylonian mathematical problem texts are concerned with practical problems involving metrological conversions, norms for work etc. and with the determination of volumes. Considerable effort was devoted not only by Thureau-Dangin (who had been interested in such matters since the beginning of his career in the late 19th century) but also by Neugebauer to the analysis of the precise technical meaning and the techniques of these texts.

All in all, the reading of the Babylonian texts »as mathematics« was thus no uncritical identification of Babylonian mathematics with »immature modern mathematics«. Yet the tendency to concentrate on the mathematics of the texts, necessary as it probably was as a »first approximation« if the code should be cracked[14], invited to extrapolation: From unreflecting characterization by means of modern mathematical concepts to interpretation in terms of these. This is what came to characterize the following period. Before we leave the thirties we shall,

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[14] And also, it should be remembered, by the lack of obvious connections between the sophisticated mathematical texts and what else was known about Babylonian culture: »Man darf [...] nicht vergessen, daß wir über die ganze Stellung der babylonischen Mathematik im Rahmen der Gesamtkultur praktisch noch gar nichts wissen« (Neugebauer 1934: 204).

It was understood that the texts which we possess are training problems, constructed backwards from the solution, and thus school exercises. But texts elucidating the structure, curriculum and ideology of the Babylonian school have only been published since the late 1940s. In 1934 Neugebauer was fully right in maintaining that only a negative conclusion could be attained: Babylonian mathematics was *not* a child of astronomy and astrology, and not born from religious concerns.

Even the relation between »practical« mathematical problems and real computational practice was difficult to specify at a time when tables of practical (»igi-gub«) constants were unknown (the first were to be published in MCT).

however, look closer at an important spin-off of the discovery of Babylonian mathematics, *viz* the idea that Greek »geometrical algebra« was nothing but Babylonian numerical algebra in geometrical dress (necessitated by the discovery of irrationals).

The idea that *Elements* II should be understood as *algebra* was not new. It had been formulated explicitly by Zeuthen (1886: 5ff), in his interpretation of Apollonios' *Conics* and has antecedents far back[15]. On this background, the discovery of Old Babylonian second-degree »algebra« invited the evident and still open-ended conclusion that »man in Hinkunft auch die spätere Entwicklung mit anderen Augen anzusehen lernen [muß]« (Neugebauer 1929: 80).

How these other eyes should look at things was later specified by Neugebauer (1936: 250) as follows, after he had presented Zeuthen's concept with approval:

> Die Antwort auf [...] die Frage nach der geschichtlichen Ursache der Grundaufgabe der gesamten geometrischen Algebra [i.e., the application of an area with deficiency or excess], kann man heute vollständig geben: sie liegt einerseits in der aus der Entwicklung der irrationalen Größen folgenden Forderung der Griechen, der Mathematik ihre Allgemeingültigkeit zu sichern durch Übergang vom Bereich der rationalen Zahlen zum Bereich der allgemeinen Größenverhältnisse, andererseits in der daraus resultierenden Notwendigkeit, *auch die Ergebnisse der vorgriechischen »algebraischen« Algebra zu übersetzen.*

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[15] According to al-Nayrīzī's commentary to the *Elements* (ed., transl. Besthorn & Heiberg 1893: II,i, 27), already Hero had begun proving the theorems of book II »by means of analysis«, which is at the very least a step in the direction toward an algebraic interpretation (depending, of course, of our definition of that term, but in agreement with Viète's understanding of his own accomplishment as a redemtion of *analysis*).

In the 13th century, Jordanus de Nemore modelled his whole reconstruction of Arabic algebra after *Elements* II and the corresponding propositions of the *Data* (cf. Høyrup 1988: 332-36). In his case, the idea that *Elements* II was a metatheoretically more satisfactory version of *al-jabr* is thus indubitable.

Hat man das Problem in dieser Weise formuliert, so ist alles Weitere vollständig trivial und liefert *den glatten Anschluß der babylonischen Algebra an die Formulierungen bei Euklid.*

—not least, thus Neugebauer in the following passage, because Babylonian »'algebraic' (i.e., numerical) algebra« was »translated« into geometry already in the Babylonian sources: E.g., the problem $xy=a$, $x+y=b$ into a problem concerned with a rectangle with given area and given sum of length and width, i.e., into the simplest version of »application with deficiency«.

As we have seen above, the thesis was taken over as trivially unproblematic by Gandz (1937). This is how its further career began.

II. THE TRIUMPH OF TRANSLATIONS, 1940 TO 1975

The heroic epoch can be taken to have ended around the beginning of the Second World War. Admittedly, another important collection of texts, some of them unprecedented (the igi-gub-tablets and the tablet Plimpton 322 with its »Pythagorean triplets«), was published by Neugebauer and Sachs in 1945 as *Mathematical Cuneiform Texts*. Yet from around 1940 »everybody« knew that Babylonian mathematics was as described by Neugebauer and Thureau-Dangin. With few exceptions, Assyriologists finding a tablet containing too many numbers in place value notation would put it aside as »something for Neugebauer«, while mathematicians and general historians of mathematics would know all they wanted from the translations contained in MKT (TMB only rarely except in Francophonic areas) or, all too often, from the few examples rendered in German, English or symbolic translation in the secondary literature[16].

Since the secondary literature was more prone than the (generally cautiously formulated) text editions to subscribe to modernizing readings of the texts and would neglect all references to the terminology and its development, it was soon conventional wisdom that »Babylonian mathematics« could be treated as *one thing* from Old Babylonian through Seleucid times[17]; that Babylonian mathematics could be adequately described in terms of symbolic algebra and other recent mathematical techniques[18]; and finally that Greek »geometric algebra« *was* really a geometricized algebra derived from the Babylonian prototype.

Van der Waerden's *Science Awakening* (1962), probably the most influential work of all, was explicitly intended (among other things) »to explain clearly *how Thales and Pythagoras took their start from Babylonian mathematics but gave it a very different, a specifically Greek character*« (p. 5).

Van der Waerden's presentation of »Babylonian algebra« (pp. 63-75) is still undogmatic as far as modernization is concerned. Admittedly, along with a number of moderately straightened translations of texts it

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[16] In early years not least Neugebauer 1934 (reprinted 1969) and Gandz 1937; later also Neugebauer 1969 (1st ed. 1952) and van der Waerden 1962 (1st Dutch ed. 1950, with English transl. 1954). Vogel 1959 and Vajman 1961 have (undeservedly) been much less influential, in Vajman's case because of the language in which the book was written, in Vogel's perhaps because its appearance in a series of high-school textbooks veiled its qualities.

[17] Neugebauer (MKT III, 5 n.20) had explained his choice of what he considered as »*sachlich* adäquaten« instead of literal translations by the observation that »wer terminologiegeschichtliche Studien an Hand einer *Übersetzung* machen will, dem ist doch nicht zu helfen«. *If* this was read at all, then only as a statement that »terminologiegeschichtliche Studien« were irrelevant to the study of the history *of mathematics*, and that translations could thus safely be relied upon.

[18] Even though a few writers have maintained, basing their understanding upon one or two simple examples borrowed from the secondary literature, that Babylonian mathematics contained nothing but empirically established numerical schemes. Familiarity with only a modestly broader sample of translations taken from MKT or TMB would have prevented the mistake.

brings translations into symbolic algebra. At the same time it suggests, however, that the thought process behind a particular solution »is expressed better by [a certain intuitive argument ascribed to a hypothetical 'elementary school teacher'] than by the elaborate algebraic transformations, which Neugebauer gives« (p. 67); it also conjectures (pp. 71f) that fundamental algebraic identities »like $(a-b)(a+b) = a^2-b^2$« can have been found by means of geometric diagrams, while still maintaining that

> we must guard against being led astray by the geometric terminology. The thought processes of the Babylonians were chiefly algebraic. It is true that they illustrated unknown numbers by means of lines and areas, but they always remained numbers. This is shown at once in the first example [of the preceding], in which the area xy and the segment $x-y$ are calmly added, geometrically nonsensical.

The tendency to replace the Babylonian texts by modern mathematics becomes more outspoken and much less reflecting if we go to general histories of mathematics[19]. Here, furthermore, practically oriented mathematics disappeared from view apart from rudiments: interest in the (symbolically expressed) formulae for areas and volumes, and succinct statements that mathematics was used for this or that practical purpose. The sexagesimal place value system is a recurrent *pièce de résistance*, but the restricted role of this system and the existence of other, unambiguous notations used for practical purposes is bypassed in silence.

An early example is Hofmann's *Geschichte der Mathematik* (1953). According to this book, slopes are measured by their »Rücksprung (cotg)«, while no word is wasted on that absence of a general notion of *angle* which had been pointed out time and again by the original

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[19] The same strengthening of the tendency can be noticed in a large article on "Die Algebra der Babylonier" (Goetsch 1968), which builds exclusively on translations and, even more, on the mathematical commentaries of original editions (see, e.g., p. 118), and whose only reserve against symbols arises when the author does not understand that Neugebauer's *justifications* should not automatically be understood as *interpretations* (p. 103). The form of the article is illustrative of the general expectation as to how the history of Babylonian mathematics was to be dealt with.

workers. Equations are presented in symbols without a word as to their original, verbal formulation, and evidence from all ages is presented without distinction. Neugebauer's idea of the function of ideograms as operators is taken over, but now referred to those *practical* problems where ideograms can surely be maintained to serve as mere technical abbreviations. Perhaps because of the formalization of which the secondary literature makes use, perhaps because mathematics is thought of as identical with formalization in the century of Hilbert and Bourbaki, it is finally stated that the rich material gives us an interesting insight »in die *formale Höhe* der babylonischen Mathematik« (emphasis added), while workers closer to the original texts had rather been impressed by the contentual level which was reached *in spite of* the absence of formalization[20].

Carl Boyer's *History of Mathematics* (1968) is less concise, more factually precise and much richer in details and examples. Yet al-Khwārizmī's classification of mixed quadratic equations, translated into symbols, is stated to be the classification used »in ancient and Medieval times«, and we are informed that »all three types are found in Old Babylonian texts« (p. 34f), and that the type »$x^2+q=px$« »appears frequently in problem texts, where it is treated as equivalent to the simultaneous system $x+y=p$, $xy=q$«, without any attempt being made to explain that this is a symbolic interpretation of something different (nor of course that the texts in question contain no hint of the idea of one form of the problem being equivalent to another formulation). Elsewhere, the tablet Plimpton 322 (the table based on Pythagorean triplets) is told to have »deep mathematical significance in the theory of numbers« (p. 37).

There is no reason to go on with detailed exemplifications, even though more analysis of other works would enrich the picture with shades. Eves (1969: 31), e.g., explains that Babylonian algebra (which is

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[20] It is immaterial for the present purpose that the presentation is also ridden by actual mistakes.

taken for granted) is a »rhetorical, or prose, algebra«. Kline (1972: 8f), in an otherwise reasonable exposition, manages to explain that the problem of finding »a number which, added to its reciprocal, yields a given number« is »a fundamental problem of the older Babylonian algebra«, and that the problem of finding two numbers with given sum and product was »reduced« to this form (the original text of the tablet YBC 6967 shows that it is rather the opposite reduction which takes place, the number and its reciprocal being understood as the sides of a rectangle, the area of which is explicitly spoken of as such—see MCT, 129, but not the translation). Etc.

That the authors of general histories tend to believe in the secondary literature written by specialists (and to overemphasize that modernizing aspect of the specialists' exposition with which they are familiar at the cost of qualifying remarks) should not cause bewilderment. It is more amazing that the same trend can be found in the specialist literature itself, and that Assyriologists took over the modernizing interpretation.

The first, and perhaps the most amazing instance is Neugebauer's and Sachs's *Mathematical Cuneiform Texts* from 1945. Evidently, the superficiality encountered in the expositions in general histories is as far removed from this careful volume as at all possible. The relations between practical mathematical problems and technical practice are carefully investigated; and far from pretending that everything Babylonian looks like anything else irrespective of chronology, the volume contains a chapter by Albrecht Goetze where linguistic differentiation is used to distinguish localities and time of origin *within* the Old Babylonian epoch.

When it comes to *mathematics*, however, the tendency to interpret unreflectingly through modern concepts is indubitable. Plimpton 322 is taken, not precisely as an expression of »eine Art elementarer Zahlentheorie«, as the reticent words of MKT III are quoted, but as »a text of purely number theoretical character«, and as »investigation of the fundamental laws of numbers themselves« (p. 41).

As it will be remembered, the relevance of the concept of logarithms had been rejected both in Neugebauer (1934) and in MKT. But as a commentary to an inversion of the table of »powers« (rather, »repeated products«, since this is what is stated in the »direct« table) it is said in MCT (p. 35) that

> We now have an Old Babylonian tablet which answers the question: to what power must a certain number *a* be raised in order to yield a given number? This problem is identical with finding the *logarithm* to the base *a* of a given number«.

In the end of the discussion it is then stated that

> In a comparison with our concept of logarithm, the only missing element is the selection of a common base and the tabulation for constant intervals, which would be needed if the tables were to be used for practical computations in general. It is accordingly clear that the Old-Babylonian mathematicians were very close to an important discovery but failed to take the final, essential step.

Forgotten is, firstly, that the »tabulation for constant intervals« is not just one element of a modern table of logarithms but *the only element*—no table of decadic logarithms bothers to tell the logarithm of 10, 100, etc., which on the other hand is *the only thing* listed in the Babylonian table, merely with base 2 instead of base 10. Forgotten is, secondly, a question which would probably have been asked by Neugebauer 10 years earlier: Was »practical computation in general« what the author of the table was after? Forgotten, finally, the question whether logarithms are really *transcendentally* important or only important in the context of Early Modern to contemporary mathematical theory and computational techniques.

When Neugebauer, the paragon of translators, and Sachs, the »scientific humanist«, are thus bound by the spell of their own conceptual translation, it is only to be expected that modernizing interpretations were as a rule also accepted by Assyriologist in general when dealing (on rare occasions) with mathematical texts[21].

So far, everything seems to agree with an almost Kuhnian scheme: After an initial phase where methodological and philosophical problems are amply discussed follows another where scholars do »business as usual«, convinced by the success of the first generation that it was right—more firmly convinced, indeed, than this generation had dared to believe itself—boiling the methodological message down to a simplified textbook version while refining and extending actual results. Eventually, even the founding fathers become convinced[22]. One aspect of the process, however, falls outside this general logic of the development of knowledge though under the more general heading *menschliches, allzumenschliches*. Notwithstanding the principle *nihil nisi bene* it has to be mentioned, since much of what happened to the field would else be unexplainable.

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[21] I shall restrict myself to a single reference: the unreserved use of symbolic algebra in Gundlach & von Soden 1963. The reason to pick out precisely this thoughtful publication is that von Soden was almost the only scholar at the time to point out the dangers inherent in unreflective modernization—thus in a slightly later publication (1973: 28) on »language, thought and concept formation in the Ancient Orient«: »Die Mathematikhistoriker setzen die babylonischen Ausrechnungen m.E. vorschnell in uns gewohnt Gleichungen, noch dazu oft mit allgemeinen Zahlen, um und werden dadurch der Andersartigkeit des mathematischen Denkens im alten Orient nur unzureichend gerecht« (von Soden 1974: 28). In spite of the authors' own doubts concerning the procedure, there *was* no other way to present Babylonian mathematics at the time.

Apart from the Susa texts (on which below) and a smaller bunch of tablets from Tell Harmal (Baqir 1950, 1950a, 1951; Goetze 1951), only very few new texts were published between 1945 and 1970. That they were treated according to the tradition which had been established by Neugebauer, Sachs and Thureau-Dangin goes more or less by itself, and calls for no supplementary commentary in the present context.

[22] As I discussed the process with my colleague Michel Olsen he commented that this was exactly what also happened within the field of structuralist text analysis.

As mentioned above, Neugebauer was not given access to the mathematical tablets from Old Babylonian Susa, for reasons which I have not traced. Instead, the task was entrusted to the historian of mathematics Evert M. Bruins in collaboration with the Assyriologist Marguerite Rutten, who took care of copying and—so it appears—was main responsible for transliterations. Bruins was responsible for the mathematical commentary and apparently for most of the Akkadian transcription from Sumerograms and for the translation into French[23]. The outcome of this collaboration appeared in 1961 as *Textes Mathématiques de Suse* (TMS), after ten years where Bruins had informed about one or the other tablet in various articles.

The tablets are of extreme importance for the understanding of the higher levels of Babylonian mathematics, in particular the »algebra«. They are difficult, and at times very different from anything known beforehand. Bruins has thus had an indubitably difficult task, and he should be praised for finding sometimes ingenious interpretations. On other occasions, however, his transcriptions into Akkadian contradict the most elementary rules of the Akkadian vocabulary and grammar; he overlooks that two consecutive problems on a tablet are different and spins a long story out the existence of two different solutions to what he believes to be one problem; in a standard construction he takes an Akkadian possessive particle *-šu*, »its«, for a Sumerian šu, »hand«; etc.[24].

According to normal rules and experience, others should have continued work on the texts, confirming sound conclusions and eradicating obvious mistakes. This never happened; instead, the

[23] »Apparently«, since the preface only states that the translation (which seems to encompass everything between copying and mathematical commentary) was made in cooperation (p. xi). It is obvious, however, that much in the translation into French and even in the transcription into Akkadian has been derived backwards from the mathematical commentary; the transliteration, on the other hand, is relatively free of this backward influence.

[24] For documentation, I shall only refer to (Høyrup 1990: 299-302, 320-327). The *-šu*/šu-mistake, not mentioned there, is TMS, p. 52.

interpretations remained almost fully unchallenged until a few years ago, and the fanciful mathematical commentary was accepted by eminent scholars without specialized knowledge of Akkadian, and even by many Assyriologists, who may have been as scared by the mathematics as other scholars by the cuneiform script. Both groups, of course, were entitled to believe that everything was sound as long as those who should have done so did not object.

The reason that almost nobody objected is obvious from what happened to the sole scholar who tried to do so. Wolfram von Soden made a review (1964), which was precise but quite gentle in tone. In 1963 Karl-Bernhard Gundlach and he had also dared to disagree with another one of Bruins's interpretations. As a result, von Soden was submitted to almost 30 years of defamation, expressed in a language and with a self-assurance which nobody is expected to use in scholarly discourse unless his cause is impeccably sound.

Neugebauer and others who had dared to disagree fared no better[25], and appear to have decided to ignore the pest. This might have been a sensible strategy (and was indubitably sound for their mental health), if Bruins had not had free access to publication channels—principally in the journal *Janus*, of which he was the main editor, and whose deficit he paid. As things were, however, his verbal violence and his assurance were liable to deceive everybody who was not extremely familiar with the matters in question *and* with all relevant earlier publications[26].

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[25] Bruins could never agree with himself whether it was Neugebauer or Derek Price who should have been caught in the Plimpton collection trying to break off a piece from Plimpton 322 in order to make the counter-evidence to his theory disappear. He told the story regularly but with changing protagonist. A third variant—less obviously absurd—can be found in Bruins 1984: 118.

[26] Once Bruins discovered that he had made a mistake he would cite in future publications himself for the correct opinion and make somebody else responsible for the erroneous point of view—preferably the one who had pointed out his mistake. This can be exemplified by the sequence (Powell 1976: 432), (Bruins 1978), (Høyrup 1982: 32 n.6), and (Bruins 1984: 134 n.5). In the first of these, Powell had pointed out that two mid-third millennium texts solve the same mathematical problem, one

Bruins was thus widely held to be a highly competent scholar with a most difficult temper, and he was able to maintain his status as an expert almost to the end. While the relation between Neugebauer and Thureau-Dangin can be taken as an exemplary instance of the functionality of the norms of the scholarly community, the Bruins phenomenon shows their possible dysfunctionality. Owing to the general conviction that nobody advances devastating criticism without support in strong arguments or indisputable facts, Bruins could retain his monopoly on the interpretation of the Susa tablets almost up to the present date, thus delaying advances in the field for decades.

I shall return from the turbid waters of individual behaviour, generally influential though they may be, to broader issues. The first challenges to the orthodoxy of the postwar period turned up between the late 1960s and the mid-1970s, not from within but from outside the field: they were formulated by scholars who knew considerably to much less about Babylonian mathematics that the fathers of orthodoxy, but who were more alert to metatheoretical questions than the disciples of these (and as alert as the fathers had been in the 1930s). The issue was the combined question of »Babylonian algebra« and »geometrical algebra«.

In 1969 a reprint of Neugebauer's *Vorgriechische Mathematik* from 1934 appeared. In this work, we remember, Neugebauer had been much more explicit on the algebraic interpretation than in his text editions. Michael Mahoney, particularly well read in the history of that algebra which was »a creation of the seventeenth century—AD!« (1971: 375), took

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correctly and another wrongly, and based his interpretation on analysis of the error; in the second, Bruins rejected Powell's interpretation of the first tablet without noticing that his own interpretation was contradicted by the second; in the third, I permitted myself to mention this neglect in a footnote; in the fourth, Bruins accuses Powell of having overlooked the existence of the two parallel texts (and identifies them wrongly).

David Fowler commented upon this example with the words »I could put together a similar sequence over the Rhind papyrus $2/n$ table«.

advantage of the occasion to ask in an essay review in which sense »Babylonian algebra« could be taken to be *algebraic*. Distinguishing the mere *algebraic approach* from *algebra as developed from Viète to Descartes*, he argued that only the former term characterized the Babylonian type of mathematics, which (in the reading of the texts that had been established in MKT, TMB and MCT) contained only recipes for numerical procedures. He made a plea (p. 377)

> to wield Ockham's razor when dealing with Babylonian mathematics and not to assign to the Babylonians any concept, or form of mathematical thought, for which there is no explicit documentation, nor even need.

Apart from the choice of the term, Mahoney was broad-minded concerning the idea of a Greek »geometrical algebra« inspired from Babylonia, maintaining (p. 371) that

> the theory can marshall a great deal of indirect evidence in its support (neither it nor its opponents have anything like direct evidence). Moreover, like most good theories, it explains phenomena it was not originally intended to explain. For at the same time that it reveal continuity, it throws discontinuity into sharper focus«.

Others were more sanguinary. A first attack had been launched by Arpád Szabó (1969: 455ff). Granted »daß es eine 'babylonische Algebra' wirklich gegeben hat—wovon O. Neugebauers Forschungen uns überzeugen möchten« (p. 457), he rejected as extremely implausible that the Greeks should have known about it—and if they had, he doubted that they would have borrowed it. Instead he argued for an autochtonous development of insights like those of *Elements* II,5, suggesting a starting point in the kind of geometry told about in Plato's *Menon* (82B-85E).

An even stronger rejection of the »monstrous, hybrid creature, a contradiction in terms, a logical impossibility« was formulated by Sabetai Unguru (1975: 77), in the context of a general attack on modernizing interpretations of Greek mathematics. The argument does not really involve Babylonian mathematics—which is claimed to belong to an »*arithmetical* stage [...] in which the reasoning is largely that of elementary arithmetic or based on empirically paradigmatic rules derived from successful trials taken as a prototype« (p. 78), on the faith of Abel Rey's book on Greek mathematics from 1935, certainly less well-informed on Babylonian topics and much more speculative than both Neugebauer and van der Waerden.

Neither Szabó nor Unguru were really concerned with Babylonian mathematics, so there may be no particular reason to blame them for treating the subject superficially or for speaking from mere hearsay. None the less, their interventions made it clear to historians of mathematics in general that the orthodox interpretation *was* an orthodoxy and no necessary plain truth—not least Unguru's sharply formulated intervention, which was answered by a no less sharp retort by Hans Freudenthal (1977), by a venomous commentary by André Weil (1978), and by a gently reasoned reply from van der Waerden (1976), which together could not but arouse attention. They thus inaugurated the beginning of a third phase in the study of Babylonian (now rather Mesopotamian) mathematics, which was to reintegrate it into the general pattern of the study of cultures and into the broader context of Mesopotamian culture—while making perhaps the Babylonian calculators less interesting for mathematicians as »the first of our kind«

III. FRESH START FROM SOURCES THROUGH NEW APPROACHES, 1971 ONWARDS

With few new sources at hand, and reliance upon already existing translations into modern languages and symbols, the active study of Babylonian mathematics had come to a virtual standstill around 1970, and the received conceptions and translations could be conveniently used by historically oriented teachers of mathematics almost like one of those »finalized« [or »post-paradigmatic«] sciences which Böhme, van den Daele and Krohn have singled out (1973). The incipient revitalization of the subject during the 1970s can be connected to the activity of three scholars: Marvin Powell, Denise Schmandt-Besserat, and Jöran Friberg.

Marvin Powell finished a PhD-dissertation in 1971 on *Sumerian Numeration and Metrology;* the following year he published a major article on "Sumerian Area Measures and the Alleged Decimal Substratum". Evidently, Assyriologists had never stopped working on metrological questions, which are unavoidable if written evidence is to be used as source material for economic and social history; the novelty in what Powell did was to take up systematic study of the topic in a way which would ultimately connect it again to the global field of mathematical thought and techniques. In this way he brought the badly neglected third millennium back into focus, and demonstrated to historians of mathematics the necessity to work with philological precision on original texts—not least texts not directly to be characterized as »mathematical«. He showed that Mesopotamian mathematics had to be seen *in historical development:* Sumerian third-millennium mathematics was different from, but at the same time foundation for the mathematics of the second-millennium Babylonians—including the mathematics which was written in more or less corrupted Sumerian by Babylonians during the second and first millennium.

These points were brought home even more clearly by the other strand of his work from the early 1970s, which culminated in an article on »The Antecedents of Old Babylonian Place Notation and the Early History of Babylonian Mathematics« (1976). Here, he succeeded in pushing back the firm *terminus ante quem* for the creation of the sexagesimal place value system to the mid-21st century B.C. Only slightly older mathematical exercise texts, on the other hand, were shown to presuppose ideas which were to go into the place value system *without* as yet possessing the tool. In this way Powell could make plausible a connection between the invention of the place value notation and the needs of the particular, tightly administrated Sumerian economy of the 21st century (»Ur III«).

In the same article Powell analyzed a number of mathematical school exercise texts (i.e., genuine *mathematical problem texts*, as this term has been used since the 1930s) from the mid- to late third millennium, thus opening up a new vista even for the received conception of what the history of Mesopotamian mathematics should be about.

Schmandt-Besserat's contribution to the process of revitalization was of a completely different character. Herself a Near Eastern archaeologist, she discovered that a system for recording based on small clay calculi (»tokens«) in varied shape and magnitude and previously only noticed in late fourth millennium Susa had been widely used in the Near East since around 8000 B.C. A number of proto-cuneiform signs seemed to be pictures of tokens—a set of very early token forms (large and small cones and spheres, the most common types from the very beginning) re-emerging as sexagesimal number symbols: 1, 10, 60, 3600.

The discovery, which was speedily and efficiently published[27] and for this reason and because of its striking character soon widely known,

[27] First in *Syro-Mesopotamian Studies* (1977) and soon in *Discovery* (1977a) and *Scientific American* (1978).

brought nothing immediately to the study of the history of Mesopotamian mathematics—nothing was changed in the interpretation of the proto-literate number signs. This task was left to Jöran Friberg.

Friberg, a mathematician, started to look at cuneiform, proto-cuneiform and proto-Elamite mathematics, computation and metrology in the later 1970s. His first important discoveries, (inefficiently) published in (1978) and (1979) and indeed primarily spread through personal interaction during the first years, changed the whole understanding of the earliest numerical and metrological notations[28]. In the best tradition of the *ingénu* Friberg took a second look at the corpus of published proto-cuneiform and proto-Elamite tablets[29]. Most of these contain numerical or metrological notations, often accounts with single contributions and total. Through analysis of the summations he was able to demonstrate that the conventionally established interpretation was partly a myth, and to single out in the proto-cuneiform material a number of metrological sequences »integrating quantity and quality«[30], together with a number notation containing the steps 1, 10, 60, 120[31]. The same signs were used

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[28] Once again, Aisik Vajman should have been mentioned, if only his earlier works on the same matters had not been even more badly published, and *not* backed by personal contacts. Nobody outside the Soviet Union (and few scholars there) seems to have taken serious note of them before Friberg.

[29] »The proto-literate period« in Mesopotamia, to which the proto-cuneiform script belongs, is dated approximately 3300 B.C. to 2900 B.C. (according to a compromise between not too firmly established calibrated radiocarbon dates and stratigraphic evidence). Proto-Elamite writing was used in the Iranian region during the second half of this period. It appears to have been inspired by the invention of writing in Mesopotamia, but makes use of a different inventory of signs; the metrologies, however, are largely but not fully identical.

[30] That is, for instance: the area 3 iku is denoted by threefold repetition of the sign iku; in our metrology, on the contrary, three hectars are written »3 ha«, with separation of quantity (»3«) from quality (»ha«).

[31] It has later turned out that the proto-cuneiform accounting tablets make use of *two different* counting systems used for counting objects belonging to different categories: One, sexagesimal, with the steps 1, 10, 60, 600, 3600, and 3600; another, »bisexagesimal«, containing the steps 1, 10, 60, 120, 1200, and 7200 (Damerow & Englund 1987:

in the various sequences, but with different mutual ratios; there was thus no longer any reason to believe that Schmandt-Besserat's tokens demonstrated the existence of a pure number system back to 8000 B.C.—the tokens could just as well have stood for specific measures of grain, as held indeed by Schmandt-Besserat in later publications.

Friberg (1979: 33-43) was also able to decipher a complex computation text (probably a school exercise) from the later proto-literate period, inaugurating thus the study of the mathematical techniques of this period (500 years earlier than any genuinely mathematical text analyzed before).

In the 1980s, non-Assyriologists have continued to play a role for the changing profile of the field. At the same time, Assyriologists have taken new interest, perhaps simply because new texts were found and could be published[32], perhaps in the wake of the discoveries and developments of the 1970s[33]. Only the latter inspiration, however, can be counted as a genuine exemplification of »changing trends«.

Much of what falls under this heading has been connected to the Berlin Workshops on »Concept development in Babylonian mathema-

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126f, 133f, 165).

[32] Soubeyran published and discussed a collection of mostly mathematical texts from Mari in (1984), while 10 mathematical problems from Tell Haddad have been published and discussed by al-Rawi & Roaf (1984). The discovery of Ebla has brought three texts with mathematical contents (analysis and previous publication history in Friberg 1986).

Several new texts have been located by Friberg, cf. below.

[33] So, critical reflection on Schmandt-Besserat's thesis led Lieberman (1980) to investigate the Sumerian use of two different ways to write numbers (»curviform« and »cuneiform«) throughout the third millennium and connect it to a conjectural use of tokens as a computation device (Lieberman did not know about Vajman's and Friberg's work, and therefore accepted the identification of tokens with sexagesimal numbers). Whiting (1984) analyzed Powell's evidence and some supplementary texts in an attempt to push backward the *ante quem* of the place value system, but neglected to observe his distinction between the prerequisite idea of sexagesimal regularization and extension and the establishment of a place value system *strictu sensu*.

tics«, an endeavour to which Marvin Powell and Jöran Friberg have been attached regularly, together with Peter Damerow, Robert Englund, Hans Nissen, and others[34]. I have also had the pleasure myself to belong to this informal group.

Two members, Peter Damerow and I, had brought questions and ideas inspired by cognitive psychology, by the sociology of knowledge, and by anthropology into the field already before the formation of the group. Peter Damerow, primarily a psychologist, took up the study of early numerical notations and arithmetical techniques (tokens, Egyptian and early cuneiform writing) as an approach to historical genetic epistemology and to historically oriented philosophy of knowledge. The first outcome was published in (1981)[35]. Soon afterwards, Peter Damerow joined the »Uruk Project« directed by Hans Nissen, and undertook the computerization[36] of a complete edition of proto-literate tablets from Uruk; together with Robert Englund, an Assyriologist also engaged in investigation of the administrative system of the Uruk III period, he analyzed the complete numerical and metrological evidence in the proto-literate tablets and was thus able to confirm and complete the results of Vajman and Friberg (Damerow & Englund 1987). Afterwards, Damerow and Englund (1989) applied the same method to Proto-Elamite material from the locality Tepe Yahya, thus again confirming and completing conjectures and preliminary results of Vajman and Friberg on this topic. Together, Damerow, Englund and Nissen (1988a, 1988b) have drawn up the resulting picture of the emergence of writing and numerical notations, while Robert Englund has been able to demonstrate that the

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[34] I persist in disregarding astronomy—for which I apologize to Hermann Hunger, who participated in several workshops. I also omit what a number of regularly participating »general discussants« have contributed from their general competence as historians of science or as Assyriologists: Kilian Butz, Jean-Pierre Grégoire, Wolfgang Lefèvre, Johannes Renger, Jim Ritter, Arpád Szabó, Sabetai Unguru, as well as everybody who only participated once.

[35] A more refined analysis has been published (with considerable delay) as Damerow 1988.

[36] Described in Damerow, Englund & Nissen 1989a.

specific administrative calendar used during Ur III for the computation of rations, fodder and work obligations was used already in the proto-literate period.

My own first contribution (1980) was part of a larger comparative investigation of the interplay between institutional and social context and mathematical mode of thought. I shall leave the word to Jöran Friberg (1982: 137[37]):

> [In] »an inter-cultural investigation of the role that the existence of an institutionalized teaching of mathematics may have played for the evolution and inner organization of mathematical thinking«, H. follows [...] the gradual development of mathematical ideas and principles, and the changing role of the profession of scribes and teachers of mathematics, from the proto-literate period in Mesopotamia (when there are clear signs of efforts to establish coherence and uniformity in the numerational and metrological notations), via the school of scribes in the Ur III period (when there was no room in the curriculum for »l'art pour l'art«) to the proud and self-conscious Old Babylonian mathematicians (in a time of far-reaching individualization of the economic and social life), and finally to the time of the militaristic Kassites and their successors (when mathematical traditions were kept alive only through the efforts of a few »families of scribes«).

The specific aim of the chapter dealing with Mesopotamia was thus to delineate the historically changing character of Mesopotamian mathematical thought and to relate it to its use by a particular professional group and through this to the broader context of the history of Mesopotamian social structure and ideology.

Soon afterwards, a random question asked by Peter Damerow drove me into another direction: a reinterpretation of the terminology and (as a consequence) the substance and techniques of Old Babylonian »algebra«. Through a method which can be characterized as »structural

[37] Strictly speaking, Friberg does not report my original publication but a slightly later Danish essay.

semantics«[38] I was able to show that the operations spoken of in the Old Babylonian texts could not possibly be genuinely arithmetical operations with numbers[39]. Instead, the texts seemed to describe analytical operations on geometric figures, whose character can be described as »naive« like those of Plato's *Menon*, but whose substance is of course much more sophisticated.

The outcome, which was first fully described in a fairly illegible publication from (1984)[40], put the question of the conjectural Babylonian inspiration behind Greek »geometric algebra« in new light, since this inspiration would be precisely of the type which Szabó had suggested (cf. above).

While Damerow's intervention as well as my own were thus governed by questions and methods different from those of earlier times, Friberg has demonstrated how far established questions and methods in stubborn and bold combination can carry. One facet of his work has been a continuation of the reading of the texts »as mathematics« unhampered by too many metatheoretical scruples. But while the orthodoxy of 1940 to 1970 à la (Goetsch 1968) would do so without reference to the original text, basing itself at best on a translation, Friberg respects the cuneiform original more scrupulously than the best philologist. His view of what pertains to mathematics has been as broad as that of the orthodoxy is narrow, and encompasses every text and every publication concerned with matters numerical, metrological, and computational.

One outcome of this was a *Survey of Publications on Sumero-Akkadian Mathematics, Metrology and Related Matters (1854-1982)* from (1982), an extensive annotated bibliography of which I have made ample use while preparing the present paper, and which has set the stage for a new

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[38] So I later found out—but my real inspiration for the method was vaguely structuralist text analysis.

[39] The texts distinguish sharply between two different operations both traditionally translated as »addition«, similarly two different »subtractive operations«, no less than four »multiplications«, and two different »halves«.

[40] A more readable exposition was published recently as Høyrup 1990.

delimitation of the field in better agreement with the place and function of Mesopotamian mathematical activity in its own historical context[41]. Another result has been the discovery and analysis (and, to some extent, the publication) of a number of new mathematical tablets, some of them from periods which hitherto had been completely devoid of mathematical texts[42]. Finally, Friberg has provided new insights through his analyses of a large number of texts—once again, mostly unpublished as yet.

Marvin Powell, the final regular member of the workshop circle contributing actively to the field, has continued his work on the development of metrologies in their technical and philological context.

IV. THE SITUATION

What has then been achieved since 1971? What I have listed appears to be an array of disconnected approaches, and it may perhaps seem improbable that it should be possible to distinguish any *trend*, in spite of the opportunity offered by a more or less regularly held workshop.

In order to find out whether a trend *can* be distinguished we may look at the outcome of the seemingly disparate approaches of the 1970s and 1980s concerning specific problem fields.

Firstly the emergence of mathematics in the Near East. The approaches of Schmandt-Besserat, Friberg, Damerow, Englund, Nissen

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[41] Unfortunately but for reasons of space, his recently published article "Mathematik" (1990) in *Reallexikon der Assyriologie* was not allowed to cover the subject-matter as broadly.

[42] Friberg 1981; Friberg, Hunger & al-Rawi 1990a.

and to a lesser extent myself, and the dialogue between these approaches, has made it possible to see the function of the token-system and its role in the emergence of script, mathematical notations and mathematical conceptualizations in the light of general cognitive psychology and anthropological state formation theory, and to integrate the insights thereby obtained with what else is known about the specific development of social structure and culture in early Mesopotamia and its Near Eastern surroundings.

A similar integration of mathematics into general history and culture has been achieved for later periods. A first condition for this to happen was that the myth of timeless Babylonian mathematics be exploded—which, again, could only be done if texts illustrating development and change could be discovered and analyzed. This was done by Powell as far as the third millennium is concerned, and by Friberg, in part together with Hunger, as regards the Late Babylonian epoch. Powell also inaugurated the investigation of *development* in his analysis of third millennium texts and of the changing character of metrological systems, while Friberg's and Hunger's work on Late Babylonian material has demonstrated how new text types and new techniques connected to the new metrology had come into existence. My own analysis of »algebraic« texts has brought back into focus the large difference between the Old Babylonian and the Seleucid terminologies (which was pointed out by Schuster (1931: 159f) and Neugebauer (1932a: 6f) but since then forgotten as unimportant); it has furthermore allowed the conclusion that the differences in terminology reflect different conceptualizations.

However, integration into general history and culture presupposes more than this. Here it has been of extreme importance that Assyriologists do not any longer automatically consider everything which looks mathematical as »a matter for Neugebauer« but as a legitimate part of their own field containing important information on society and culture. An as yet unpublished example is Karen Nemet-Nejat's work on "Cuneiform Mathematical Texts as a Reflection of Everyday Life in

Mesopotamia"[43]. Englund's analysis of the administrative calendar and (to a lesser extent) his research in Ur III administrative procedures, Powell's investigations of the relations between metrology and agricultural practices has added other facets to the »onionology« of mathematics, to borrow a term coined by Ignace Gelb (1967: 8) for the study of low-status but vital subjects like the distribution of onions, as opposed to the more celebrated interest in gods and myths.

The outcome of my own work on »algebra« has been described by Horst Klengel[44] as a parallel to what has happened to the study of Mesopotamian law: Instead of being analyzed in terms of its relation to Roman Law—once so to speak the embodiment of the very Idea of Law—it has come to be seen as an expression of Mesopotamian culture and mode of thought, and concerned with the problems of Mesopotamian society.

Only one problem field has not given rise to a crystallization involving several approaches: until now, I have been alone in recasting theories about the transmission of Babylonian mathematical knowledge and techniques to later cultures (with appurtenant transformation) and about the relation between practitioners' mathematics, scribal mathematics and »scientific« mathematics[45]. In so far as this makes part of a trend it is not inside the small community of students of Mesopotamian mathematics but due to my interaction with scholars from neighbouring fields.

Apart from that, it should be clear that the development over the last 10 years does expresses a trend, *viz* away from the attitude that the mathematical knowledge of the Babylonians should be studied as *a step in the ladder leading to*, and hence from the *perspective of* modern mathe-

[43] Drafts of this work were presented at the 1988 Berlin Workshop, but the author had taken up the subject independently of the Berlin collaboration.

[44] In private conversation, and thus not quoted verbally.

[45] In this connection I disregard van der Waerden's work on *Geometry and Algebra in Ancient Civilizations* (1983), since the fundamental perspective, though certainly a recast, is not that of Babylonian mathematics.

matics, and toward the position of a multi-dimensional anthropology where mathematics is primarily studied in relation to its historical context[46], and where the distinction between »external« and »internal« causation is regarded as only relative.

Thus seen, the »new trend« in the historiography of Babylonian mathematics is not specific to this field. It can be found in many quarters of the history of science, and in the humanities in general. It is therefore not so strange that the historiography of Mesopotamian mathematics has performed what Michael Mahoney hoped for (but doubted could be done from extant sources): bring about the »transition from mathematics to mathematical thought« (1971: 378). Indeed, Mahoney's hopes coincided with the motivation of many scholars, including some of those who took up new approaches to Babylonian mathematics.

That it *could* be done, in spite of Mahoney's pessimism, depended on the real interdisciplinarity of the group which was engaged in the task—not only as a collectivity but also individually (which is the presupposition that collective interdisciplinarity can work): negation of the mutual segregation not only of *the two* but of *the N* cultures was essential.

Neither Neugebauer nor Thureau-Dangin respected the segregation. In their time, however, the source situation was still so that Neugebauer (1934: 204) was forced to conclude that »wir über die ganze Stellung der babylonischen Mathematik im Rahmen der Gesamtkultur praktisch noch gar nichts wissen«, as quoted above. Afterwards, as the source situation

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[46] This does not prevent that categories of modern mathematics can be used when needed as analytical tools (cf. note 9)—even if we stop asking as our fundamental question »how the equations of the Babylonians looked« we may still take notice that a problem »I have added [the measuring numbers of] the side and the area of a square, and the result was 110« shares essential features with modern equations, and can be termed no more adequately by a single word. We still need an Archimedean point from where to describe the world, and purist who refuse to speak about »algebra« and insist, e.g., on »numerical mathematics« are mistaken—»numbers« are no more transhistorically immutable than »algebra«, as shown by Vajman, Friberg and Peter Damerow.

improved, orthodoxy took care that historians of mathematics did not discover or bother, while Assyriologists got no opportunity to tell. It has only been since 1970-80 that an improved source situation, disrespect for »cultural« boundaries within the scholarly world, and the combination of individual and collective interdisciplinarity have created the breakthrough to those new questions which could now be answered. The process has also revealed the distinction internal versus external to be a sociological accident just as much as an absolute cognitive category.

The new approaches have certainly brought about new insights on many levels. They have probably also created a void, or at least a loss: The orthodox picture of Babylonian mathematics allowed mathematicians the illusion that they could overcome the two-culture split without leaving home: »we mathematicians got our own humanities, Greek and Babylonian mathematics—they look precisely as mathematics, and are written in x and y«. Once the new picture has been discovered by general historians of mathematics and has gone into the textbooks, it will perhaps be less easy to use Babylonian mathematics as historical staple food for such justifications of present-day mathematics. May the new insights contribute instead to overthrowing all 2- and N-culture distinctions in earnest.

POSTSCRIPT

As a rule, historians of science are quite sceptical when a practitioner from a scientific field undertakes to write its history. I shall not repeat the weaknesses which are liable to threaten such an undertaking, merely point out that they evidently also menace the undertaking when the field in question belongs within the domain of the history of science. I am clearly not the one to judge whether my attempt to analyze the changing orientations of a field within which I work myself has been severely distorted by the necessary false consciousness of a participant; what I do know is that the preceding essay is a hybrid: not an unpolluted insider's report, because of the Fall from innocence caused by the author's attempt to apply a voyeurist, metatheoretical and sociological point of view; nor on the other hand a real historical or sociological account, since any participant's belief that he can step outside the process is illusory.

In any case: *I* learnt quite a bit about a field with which I felt to be familiar through looking at what used to be (good or bad) *theory* under the aspect of *primary sources*. It is a pleasure no less than an obligation to express my gratitude to the Greek Society for the History of Science and Technology for giving me the occasion to do so.

It is also obligation as well as pleasure to thank those participants in the conference who reacted to my talk and to the preliminary written version of the present paper—in particular to David Fowler, who also had the kindness to correct the English of a number of passages.

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