Wilbur R. Knorr on Thābit ibn Qurra

A Case-Study in the Historiography of Premodern Science

by

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Abstract

There was a widespread belief among historians of science of my generation that high competence with regard to content and languages alone can guarantee better, more reliable results than can good philology combined with high competence in history or the other human sciences. In my case-study of Wilbur R. Knorr's analysis of several medieval Arabic and Latin texts on the balance, or steelyard, I highlight a variety of factors that compromised time and again his understanding and interpretation of his chosen texts. I conclude that a greater openness to more complex historiographical assumptions and more sophisticated methodological approaches as well as a greater willingness to contextualize documents in numerous dimensions before coming to conclusions about their specific meaning is crucial if we are to correct and improve upon work such as Knorr's analysis of the *Kitab al-qarastun*, ascribed to Thābit ibn Qurra, and the *Liber de canonio*. The way forward is to enhance and temper philological analysis with solid analysis of scientific content within its relevant contexts.

About the Author

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In what follows, I present arguments for the need to go beyond both the positivist and the postmodernist approaches to the history of science in Islamicate societies. While positivist research practice often was and is focused exclusively on scientific content, postmodernist practice often avoids the analysis of this content and focuses instead on a narrow language of contexts. I think that good historical practice needs to aim at a solid analysis of scientific content within its relevant contexts. Analysis of content without paying attention to the conditions of and motivations for its creation can at best be the very first step in our labor. Context analysis without interest in the nature of scholarly practices and their results loses its basis and transmutes all too often into specious respect for a different culture. In my view, the professional goal of our activities is not the subjugation of historical objects to the power of our own worldviews and academic profiles. Our academic self-representation and legitimation, if taken seriously and honestly, should aim instead at our being competent seekers for reliable and trustworthy interpretations of the material that we study. Truth about the past in this sense, however, can only be established if we consider the objects of our research as self-contained, valuable products constructed by independent human beings of earlier times who, though they differ from us in their knowledge and values, remain worthy of our respect, appreciation, and our honest effort to discover them and their worlds. Even as the hybrid cultural creations that many of them are, these works are always more than simple containers of yet another past which we happen to esteem more or less because of the stories that we tell about our own history.

I will make my arguments on the basis of a book published 31 years ago by a senior and serious scholar of good repute. I chose this example because it is a very elaborate technical product, which leaves no doubt that its author, Wilbur R. Knorr, spent much time and labor to work out his positions. Yet Knorr's *Ancient Sources of the Medieval Tradition of Mechanics: Greek, Arabic and Latin Studies of the Balance* [1982] is characterized by the following three flaws which are found all too often also in current publications:

(1) the undue impact of prior beliefs and prejudices on questions, arguments, and conclusions;

(2) the deleterious role of methodological limitations in the choice of an interpretive framework; and
(3) lack of expertise.

I am also moved to write about Knorr's study because it forced me to confront some of the problems resulting from our standard approach to the analysis of Arabic scientific texts written in the ninth century or of Arabic translations of Greek texts executed in this period, that is, to face the problems that arise when we treat these works as isolated texts without any contextualization.

At the same time, an analysis of this book serves to highlight that many of our biases are deeply anchored in our education and training, in the political alignments and ideological commitments of our teachers and, thus, in our own academic values, convictions, and beliefs. Such deep-seated biases are often very difficult to recognize, even more difficult to acknowledge as severe shortcomings, and extremely difficult to overcome because of the demands that changes in working practices have necessitated. As Dagmar Schäfer remarked in a conversation about issues of contextualization:

> It is already a difficult endeavor to read, translate, and understand a complex medieval text in any language. It is much more difficult to analyze its textual contexts, if these contexts are unknown and the relevant texts unpublished. But it is nearly impossible to investigate the entire non-textual contexts in which the medieval text, which is the primary goal of study, was created.

In the case of Arabic, Persian, and Ottoman Turkish manuscripts, the challenges do not merely include the search for manuscript copies in sometimes almost inaccessible libraries and then overcoming problems with handwriting, dating, and the interpretation of content. It also means breaking away from the traditionally sanctioned habit of analyzing a historical sequence of texts, starting either in antiquity or in ninth-century Baghdad, which considers them only in relation to one another, by turning first and foremost to a study of contemporaneous authors, their works, and their networks. This has been undertaken so far only in fairly limited ways, my own work included, of course.

1. Knorr's working practices

In order to understand Knorr's analysis of a number of Arabic and Latin medieval texts on the steelyard, one should remember the goals of textual studies 30 years ago, the values attached to ancient and medieval sciences and mathematics, and the methods taught and valued in that period. Editions of Greek, Latin, or Arabic scientific texts aimed to (re)produce the genuine text of an author on the basis of critical comparisons among the extant copies and their errors. Interpretations of a text's content proposed to establish its scientific or mathematical results, preferably, but not exclusively, in a more recent language than that of the text itself. Chronology and authorship were additional important issues that were pursued, the first prevalently within the oeuvre of the individual author, the second mostly within the specific text at hand or in comparison with other texts on the same topic. Once these tasks were completed, the historian's duty ended. Of course, there were always colleagues who invested time and effort in the study of larger disciplinary or institutional themes. But such questions were certainly considered to go far beyond the investigation of a single text.

This methodological stance was not new in the 1980s; it has been well established since the 19th century. What emerged in the 1970s and 1980s was the conscious and explicit opposition between two research positions and their respective goals: the study of content alone by the so-called internalists and the study of the external conditions of the sciences, including mathematics, by the so-called externalists. Most historians of mathematics and almost all historians of ancient and medieval mathematics and the exact sciences subscribed strongly to the internalist position and rejected or even disparaged the pursuit of externalist inquiries. As my discussion of Knorr's work will show, the strong belief in the exceptionality of the sciences in comparison to other domains of human society did not merely prevent the study of the mechanisms that created interest in, and support for, scientific problems in any given society. It did not attend to the study of textual content or of the subsidiary questions of chronology and authorship beyond the simplest understanding of how a text was produced, read, and reproduced. How producers and readers of texts communicated through such texts within their immediate environments and how they created meaning were issues only accepted much later in the history of science.

Hence, in contrast to today's much broader array of methodologies available for the study of texts, the conditions in which Knorr worked in the 1980s were more restricted. Even if he had wished to approach the issue of authorship, which is central to his study of the texts on steelyards, in a different manner, he could have done so only by contravening practices current at the time for the study of scientific and mathematical texts by classicists and medievalists. While Knorr had shown in earlier works on ancient and medieval geometry, Archimedes, and Euclid that he was willing to reshuffle beliefs held earlier, his iconoclastic tendencies did not include issues of methodology. In this respect, he was a representative of the dominant approach of his time and day—he was clearly an internalist. This basic methodological stance underlies his entire analysis and all of his arguments in *Ancient Sources of the Medieval Tradition of Mechanics*.

1.1 *A caveat* Those who read my critique of Knorr's results and my different interpretation of the same texts must take into account the enormous changes in working practice, values, and goals that have taken place between 1982 and 2020 in order to avoid an anachronistic reading of Knorr's book. My main critique does not in fact center on the conceptual and methodological differences between then and today, although my new interpretative results are clearly the product of these changes. Instead, it focuses on those misinterpretations or even clear missteps that belong to the internalist framework of textual studies. It is in regards to these points that I will argue that Knorr's interpretive and analytical shortcomings were caused by unquestioned assumptions and beliefs about authorship, the development of ancient and medieval mathematical texts, and the relative qualities of ancient and medieval scholars of the mathematical sciences as well as of modern historians of science and mathematics from the US, Europe and the Middle East. As my analysis indicates, Knorr too often broke the "rules" of an internalist textual study because of his larger desires, prejudices, and assumptions. Additional interpretative difficulties were the direct result of his limited control of the Arabic language and his failure to subject his decisions about the merits of different interpretive options to critical examination of the criteria by which these decisions were made.

2. Issues of authorship

As I have said, the central problem of Knorr's study comes to the fore in his determination of the authorship of several Arabic and Latin texts (complete as well as fragmentary) on the steelyard translated or written in the ninth, possibly 10th, and 13th centuries. His analysis yielded three conclusions concerning the texts ascribed to Thābit ibn Qurra (d. 901):

(1) the Arabic *Kitāb al-qarasṭūn* (Book of the Steelyard) was not written by this Sabian scholar;
(2) the Latin *Liber karastonis* (Book of the Steelyard) is rightfully seen as his work but of a different textual tradition than the *Kitāb al-qarasṭūn*; and
(3) an addition (*ziyāda*) to MS Beirut, St Joseph University, 223, one of the two manuscripts available to Knorr, was not of Arabic origin.

In regard to the *Liber de canonio* (Book on the Beam), an anonymous Latin text on the steelyard, Knorr confirmed earlier evaluations by Duhem [1905], Moody and Clagett [1952] that this text was a translation of an ancient

Greek ancestor. Knorr's final, main conclusion states that all the Arabic texts which he analyzed and the *Liber de canonio* were but fragments of one major ancient Greek text on balances and that its author was Archimedes in his youth.

In order to settle the questions raised by Knorr for the Arabic and Latin texts, I studied carefully each and every claim and its demonstration in order to understand their soundness and to decide what further work was needed. The only point that I excluded from my analysis concerns the issue of the young Archimedes: I am not an expert in Archimedean studies, let alone of the young Archimedes, whose writings are not extant. The result of my analysis is that only one of Knorr's conclusions is valid, namely, the one about Thābit's authorship of the *Liber karastonis*. All other conclusions are insufficiently backed by primary source evidence or rest on faulty or one-sided arguments.

Problems that played an increasing role in my own investigations, but that understandably were not addressed in a comparable manner by Knorr, concern our beliefs about authorship—beliefs that have changed substantially in the last 30 years. In the 1980s, we believed that an ancient or medieval text ascribed to a concrete person could be either the work of this person, or the work of someone else ascribed to this person by mistake, or a forgery. The idea of multiple authorship, for instance, where many people contribute to the production, transformation, and dispersion of a text, while only one, if any, is named as its author, had not yet been put forward. In its more complex form, this concept of multiple authorship allows for a group production of a text at a specific time and location as well as a series of subsequent contributors, previously often thought of as "mere" commentators or interpolators, to a living text. Such an understanding of the concept of multiple authorship turns out to be fruitful for reformulating Knorr's claim that Thābit had not composed the *Kitāb al-qarasṭūn* as a question of the sense in which Thābit had contributed to the content and form of the extant text and, hence, the sense in which he could be credited or not with authorship. This type of question, as I will show below, is not an effort to hide the fact that Thābit was not the originator of the *Kitāb al-qarasṭūn* in its entirety. Indeed, it is clear that Thābit was not the sole author of this text. But against any such traditional sense of authorship, we must note that this text would not have come into being without Thābit. It is, in fact, a new product and, hence, his role in producing it deserves proper recognition as such.

2.1 *Knorr on the* Kitāb al-qarasṭūn Knorr's rejection of Thābit ibn Qurra's authorship of the Arabic *Kitāb al-qarasṭūn* is directed against

Khalil Jaouiche, the modern Arabic editor and translator of this text [1976]. Jaouiche had accepted Thābit's authorship on the basis of the three manuscripts known then, i.e., MSS London, BL, India Office Library, 461; Beirut, St Joseph University, 223; Cracow, Jagielonska University Library, Mq 559. He had had access, however, only to the first of them, which prevented him from discussing the appendix found only in the Beirut copy. In regard to the *Liber de canonio*, Jaouiche challenged its interpretation as a Latin translation of an ancient Greek text. He held that this text was written after the *Kitāb al-qarasṭūn* and was willing to grant a Byzantine origin. Jaouiche also rejected the false translation of the preface of the *Liber karastonis* provided by Duhem [1905] and accepted by Moody and Clagett [1952], and presented his own and better reading of this part of the Latin translation, which he achieved in cooperation with a medievalist. Duhem's false reading of the preface stipulated another ancient text, a so-called *(Liber) causae karastonis*, as the original text behind the *Liber karastonis*. Knorr accepted Jaouiche's rejection of this so-called *(Liber) causae karastonis* but upheld Duhem's overall reading of the preface as correct. Knorr ignored Jaouiche's proposal of a Byzantine origin of the *Liber de canonio* and focused entirely on his question of whether the author of this text could have been a Latin scholar of the 13th century.

Knorr's differentiated replies to the interpretive positions of his predecessors suggest—in addition to the existence of conceptual issues regarding authorship, originality, commentators, and the like—the role of beliefs about the relative merits of scholarly works written by ancient Greek, Byzantine, and Arabic-writing scholars. I will return to this point [see p. 125, below]. The fact that Knorr did not recognize Duhem's clear and, in some instances, even simple mistakes in translation in the case of the *Liber karastonis* [1905] and his striking disregard of Jaouiche's arguments about the *Liber de canonio* may reflect some puzzling biases. His rhetorical treatment of both groups of colleagues is slightly different. Duhem, Moody, and Clagett are mostly treated with respect, with only a few strong expressions of criticism such as "Duhem's view being irrelevant". Jaouiche, in contrast, is more often described in strong or emotive terms, the latter carrying a negative subtext in the internalist framework: for example, "but Jaouiche's denying the existence of any such Greek text", "although he is emphatic", "Jaouiche's desire", "Jaouiche wishes to assign". A few times, Knorr also misrepresents Jaouiche's claims or ignores the explanations given in his footnotes, while this is not the case as far as Duhem, Moody, and Clagett are concerned. Although I know from my own experience that such missteps in reading occur more easily than one might wish, such small differences in treatment may indeed be more than simple accidents.

Knorr's method for clarifying the problems of authorship was to study the philological and scientific features of texts taken by themselves. No other approach was considered necessary at the time. In the case of the *Kitāb al-qarasṭūn*, his focus was on matters such as proofs, arguments, and methods. As I have already indicated, Knorr was trained as a classicist and historian of mathematics but not as an Arabist. His limited expertise in Arabic along with his prior beliefs about the mathematical capabilities of Thābit ibn Qurra as opposed to Archimedes, Greek writers of late antiquity, and post-ninth-century writers in Arabic had a clear, negative impact on his interpretation. I will provide a few examples to confirm this in the following two sections.

For now, I will note that, surprisingly, Knorr did not undertake a study of philological features of this text, either to establish arguments against Thābit's presumed authorship or to determine features that might have spoken in favor of its character as a translation from Greek. Somehow he was satisfied to rest his case for authorship and character on the analysis of a limited range of mathematical statements, a few proofs, and a few perceived mistakes, which, like it turned out, were mostly his own. Knorr gives no reason for his limited exploration of the text. He was clearly inconsistent in his working practice in this book, since his main arguments concerning the *Liber de canonio* are taken from a philological analysis, as I will show below. Beyond this internal inconsistency of methods and conclusions, this lack of any justification for the differences in his analysis of the various texts at issue contravenes the standards for research of his own time.

Granted, a traditional philological approach to the issue of authorship, which Knorr knew and practiced well in his other papers and books, would have provided him initially with additional arguments for a Greek ancestry of the *Kitāb al-qarasṭūn* because it uncovers Graecisms in the Arabic text. But a proper, comprehensive philological analysis of this work would have alerted Knorr that his belief in a single text as a predecessor of the *Kitāb al-qarasṭūn* was in all likelihood erroneous since these Graecisms do not occur in the same manner in all parts of the text but differ in kind and frequency.

2.2 *Knorr on the* Liber de canonio In contrast to the analysis of the *Kitāb al-qarasṭūn*, the confirmation of Duhem's, Moody's, and Clagett's identification of the ancestor text of the *Liber de canonio* as an ancient Greek text rests primarily on the analysis of its philological properties. When summarizing the theorems of the *Liber de canonio* and the addition (*ziyāda*) in the Beirut manuscript, Knorr discussed certain of their aspects but not with the same comprehensiveness as in the case of the *Kitāb al-qarasṭūn*. For instance, he paid only very little attention to the text's axiomatic-deductive structure; its references to definitions, axioms, or proofs in its proofs; and the lack of physical arguments, which are, however, an important key for understanding, for example, the relationship of these two texts to the one presented in the *Kitāb al-qarasṭūn* or for understanding the relationship between the first and the second part of the *Liber de canonio*.

Knorr's philological analysis identifies a number of Graecisms, a single Arabism, and a number of philological features that could be identified as either of the two and so are undecidable. Given this, he finds it more plausible to consider the undecidable cases as favoring an ancient Greek ancestor. This result is surprising since a brief glance at the Latin text uncovers without any difficulty many more Arabisms than Knorr recognized. It shows too that Graecisms, Arabisms, and undecidable forms are unevenly distributed throughout the complete text. Graecisms are concentrated in the first half, while Arabisms dominate the second half. Undecidable forms can be found in both parts. Although Knorr allowed at the beginning of his discussion for the possibility of some other identification of the source text—for instance, its translation in Sicily, or even an Arabic ancestor (because of the existence of similar theorems at the end of the Beirut manuscript of the *Kitāb al-qarasṭūn*)—he does not spend time weighing such alternatives seriously. The purely rhetorical character of these alternative interpretations is of a piece with Knorr's failure to see the contradiction between another of his claims, namely, the largely correct, if slightly too general, assertion that Latin translations of Greek texts (and texts composed in Latin) prefer singular verbal forms over plural forms in contrast to Arabic translations of Greek texts and texts newly composed in Arabic—and the numerous plural verbal forms found in the second half of the *Liber de canonio*.

It is very difficult to believe that Knorr did indeed miss all 28 instances of a first person plural in six printed pages of Latin text. However, there is no indication in his text to suggest that he intentionally misconstrued the argument. Hence, I am inclined to think that he was in fact blinded by his belief in the ancient Greek origin of the *Liber de canonio* and simply did not see the many plural forms in its second part. There are other, indisputable Arabisms in the *Liber de canonio* and here it is much easier to understand why he missed them. Recognizing them presupposes a much broader familiarity with Arabic translations of Greek mathematical texts than Knorr had. Such intimate familiarity with unpublished Arabic translations of Greek mathematical texts is requisite if one is to render properly the pair "greater"/"smaller" in Greek, Arabic, Greek-to-Latin, and Arabic-to-Latin texts. Greek texts and Greek-to-Latin as well as certain Arabic translation texts only use one pair corresponding to "greater"/"smaller": respectively, «μείζων»/«ἐλάσσων», "maior"/"minor", and «aʿẓam»/«aṣghar». In other Arabic translations and Arabic texts derived from them, there appear three pairs corresponding to different types of objects. The three pairs are «aṭwal»/«aqṣar» (longer/shorter) for lines, «akbar» or «akthar»/«aqall» (bigger or more/smaller or less) for areas, solids, or numbers; and «aʿẓam»/«aṣghar» (greater/smaller) for numbers and angles or similar magnitudes. In Arabo-Latin translations, these three pairs are represented as a rule by the following two pairs: "longior"/"brevior" and "maior"/"minor".

Since the *Liber de canonio* mixes "longior" with "maior" and "brevior" with "minor" in its second part, it is impossible that the direct predecessor of this part was an ancient Greek text. It is also unlikely that it was a pure Arabic translation of such an ancient Greek text. In my experience, the mixing of these terms occurs predominantly in commentaries, editions, or newly composed texts. This philological phenomenon goes beyond idiosyncratic usage by an individual. It reflects the coming into being of different sets of technical vocabulary during the process of translation, their social acceptance by the scholarly community, and their merger into one technical language with several options to express one and the same point. Thus, the second part of the *Liber de canonio* suggests strongly that its basis was an Arabic text, which may have derived from an earlier Arabic translation of an ancient Greek text or a newly composed Arabic text in which such usage was accepted. Whether a Byzantine intermediary was situated between this Arabic basic text and the Latin final product cannot be decided on the basis of this and other Arabisms in the second part of the *Liber de canonio*.

But what of the first part? The overwhelming presence of Graecisms in it might seem to contradict this. But after a closer look at Latin translations of Arabic and Greek texts in the 12th and 13th centuries, four possible explanatory hypotheses compete with each other:

(1) there was indeed a Byzantine intermediary between the Arabic ancestor of the *Liber de canonio*, whose producer paid significantly more attention to Greek style and grammar in the first part than in the second;

(2) the Arabic ancestor was translated in Sicily by a trilingual translator who paid significantly more attention to Greek style and grammar in the first part than in the second;

(3) the first part of the *Liber de canonio* represents a Graecisized Arabic-to-Latin translation, while the second part was left in the original form of translation;
(4) the first and second parts derive from a single source translated by two translators or from two different sources translated by one or two translators.

The second part of hypothesis 4 may easily be excluded by virtue of the consistency in content, procedures, types of arguments, and sources used or referred to in both textual parts. This consistency militates against the existence of two different texts that were intentionally or accidentally fused to form one new text by one or two translators. The remarkable philological differences between the two parts appear then to be results of a difference in the intention of either one or two authors translating a single text. The overall philological properties of this single text favor the hypothesis of a single translator at work.

This raises the question: Which of the two parts was philologically altered? When we consider the two language components in the Latin text and the historically possible cultural environments (Sicily, Iberian Peninsula, southern France, Crusader states) of the translation, it seems more plausible to assume an editorial modification towards a more "Greekish" appearance than one which would increase an "Arabicizing" outlook. The distribution of the two language components also supports the hypothesis that the modification of the translated text consists in the introduction of the Greek terms and forms. Reworking a text from its beginning instead of starting with such changes in its middle sounds not merely practically more plausible, it also makes more sense with regard to the effect such a change may have meant to achieve.

A further argument for abandoning hypothesis 4 altogether and privileging hypothesis 3 instead comes from properties of other Latin texts translated from Arabic. At least two Arabic-to-Latin texts, one a translation (Theodosius, *Sphaerica*), the other a compilation (Euclid's *Elements*, labelled Adelard III by Clagett and ascribed to Robert of Ketton by Busard and Folkerts [1992]), are well-known examples of the use of Graecisms in Arabic-to-Latin translations. Translations made from Arabic at Sicily are few and not known for this, so far as I know, and Byzantine intermediaries of Arabic-to-Latin texts are not known at all. Hence, until more material is found, the most likely interpretation of the fascinating philological contrast and its uneven distribution through the text of the *Liber de canonio* is hypothesis 3 [Brentjes and Renn 2016].

2.3 *Knorr and the question of context* As I have emphasized, Knorr was an internalist. Hence, contextualization, even in the limited form of textual contextualization, was not something that he would have pursued as a means necessary for putting the analysis of authorship on solid footing. The fact that social, cultural, epistemic, and other contexts were not considered to the degree that they are today led Knorr, as it had other, previous scholars who studied the *Kitāb al-qarasṭūn*, to ignore the explicit statement in one of the two manuscripts that he worked with according to which the extant text of the *Kitāb al-qarasṭūn* had been dictated by Thābit b. Qurra. Such a statement generally indicates a teaching text. The *Kitāb al-qarasṭūn*, in the form in which we have it, is thus not the result of Thābit's research or of his editing one or more Arabic translations of one or more Greek short texts on the balance. It is rather a text that Thābit prepared for classroom work. This is important for several reasons. First, given that historians of education and codicology in Islamicate societies claim that such dictation, teaching certificates, or auditing certificates and the like can be found only in literary texts from the late 10th century onwards and in texts on religious matters after the early 11th century, the *Kitāb al-qarasṭūn* would appear to be one of the earliest, if not the earliest, extant document for formal teaching activities [Gacek 2005, 55; Witkam 2012, 157–160]. Given the fact that it is the only Arabic or Persian text on mechanics known so far that carries this kind of information and that such statements indeed become more prominent only in later centuries, remaining always much less a feature of scientific literature than of religious and literary texts, there is no reason to suspect falsehood in these references to teaching. I, at least, cannot think of any good reason for explaining such falsehood. Hence, in absence of arguments and evidence to the contrary, I consider as a true report the claim that Thābit dictated the *Kitāb al-qarasṭūn*.

We know next to nothing about the teaching of the mathematical sciences in the ninth century. Thus, the statement that Thābit had dictated the *Kitāb al-qarasṭūn* is most welcome. Not only does it confirm that the mathematical sciences were taught in the ninth century outside the court and beyond its patronage, it also suggests through its similarity to later such statements in texts taught at the *madrasa* or in mosques that the methods of teaching that we are aware of may already have been in place during the ninth century. That is, Thābit's statement confirms that it was the practice in his time to write out a complete text which was then to be read out loud to students who were to copy it meticulously and who then received confirmation from their teacher if their note-taking was correct. Indeed, a fourth manuscript, which I found in the late 1990s in the Biblioteca Laurenziana at Florence, Or. 118, contains a different form of such teaching statements, e.g., an audition certificate. An audition certificate signifies exactly what I just summarized, i.e., that the teacher read his text to students who listened to him and wrote down (correctly) what he had said. Since I do not see any good reason for assuming that either form of the two teaching statements is a falsification, we may be fairly certain that the *Kitāb al-qarasṭūn* is the result of Thābit's holding classes on the steelyard. This means that the specific character of the *Kitāb al-qarasṭūn* as a teaching text will have impacted its form and structure. We should, therefore, expect and look for explanations, a less rigid axiomatic structure, different types of demonstrations, and other didactic devices. This means that parts of the text considered by Knorr as interpolations may now be understood, for example, as remainders of oral explanations by Thābit given to his students when reading his prepared text to them [1982, 8–9, 63–72, 78–87]. Other oral features appear to exist when the text is studied from such a perspective.

A second aspect of the identification of the *Kitāb al-qarasṭūn* as a teaching text is that we may now also understand other textual features as a reflection of the manner in which Thābit presented the text in class and not as interpolations in the classical sense. One instance is the appearance of postulate-like statements after the first theorem and not at the text's beginning. Knorr proposed to consider these two statements plus a subsequent theorem as an interpolation. One explanation for this decision is his disagreement with Jaouiche's choice to understand their presence as a misplacement through copying, which induced Jaouiche to emend the manuscript text [see Knorr 1982, 63n15; Jaouiche 1976, 146–147, 171]. Another reason seems to have been the absence of these two statements plus the subsequent theorem from the *Liber karastonis*, where this difference between the two texts is obviously understood not as a decision made by Thābit but as an indicator for the "better" or "purer" quality of the Arabic text that forms the basis of the *Liber karastonis*. A third reason will have been Knorr's lack of access to the Berlin and Florence manuscripts of the *Kitāb al-qarasṭūn*, since these two texts confirm the presence of the two postulate-like statements at exactly the same place where they are found in the London manuscript. Had he known this, Knorr might have chosen a more cautious interpretation of the text's provenance and its circumstances. Instead, Knorr buttressed his interpretation with a rash as well as inconsistent identification of the two postulate-like statements with two postulates in the pseudo-Euclidean *Kitāb fi l-mīzān* (Book on the Balance) [1982, 79, 81]. I say rash because his judgment is exclusively based on the similarity of content and does not take into account the substantial differences in their formulation as well as expression. I say inconsistent because he modifies his first evaluation of the parallelism of the two texts by the later qualification that none of them depends on the other but that "they must be viewed as independent translations from closely related, if not identical, sources" [1982, 81].

In contrast to Knorr, I have the impression that the apparently misplaced postulate-like statements were indeed presented in class by Thābit after the first theorem. In addition to the didactic character of the work, this idea is based on the differences in content, concepts, and terminology that demarcate boundaries between different parts of the text. The internal philological analysis of the text regarding its possible relationship to Greek predecessors reveals these borderlines in regard to specific Graecisms. It is thus most likely that Thābit presented successively different bits and pieces from Greek texts on the steelyard to his students. The partially incomplete nature of these pieces suggests that the idea of discussing them in this way with his students may have been a consequence of the fact that Thābit had come across several Arabic translations of Greek fragments, which he wished to interpret. This fits well the preface of the *Liber karastonis*, where he reports in a continued discussion with an unnamed friend on his engagement with faulty and partially incomprehensible translations or copies of collections of theorems on the balance and his efforts to solve the problems of transmission. Moreover, it is not at all true that all Greek texts on theoretical geometry start with their axioms, postulates, or definitions placed exclusively at their beginning. Archimedes, for one, often introduces such new statements after he has already proven theorems. Euclid did the same in book 10 of the *Elements*. Knorr was well aware of this textual practice of ancient Greek scholars. As I see it, his insistence on the interpolated character of these two postulate-like statements in the *Kitāb al-qarasṭūn* is symptomatic of the presumptions that he brought to his conclusions concerning Thābit's authorship of the *Kitāb al-qarasṭūn*.

2.4 *Advantages of contextualization* As is well known, contextualization may occur in different ways and on different levels. I will begin my discussion with textual contextualization. Contextualization of this sort is the lowest possible level and should be acceptable to most students of past scientific or mathematical texts. A textual contextualization provides clues for understanding parts of a scholar's working practice and intellectual environment in addition to those which can be derived from the analysis of the particular text being studied.

A systematic check of all of Thābit's published works confirms that he was not a solitary writer. He exchanged letters on scholarly themes with colleagues and friends. He wrote short introductory texts for courtiers and a more general public, a feature that whoever attached the title to his treatise on Aristotelian natural philosophy and metaphysics made explicit. He composed at least one other sufficiently difficult text, this time on astronomy, as the result of repeated discussions with friends and students.[1] Thus, identifying the *Kitāb al-qarasṭūn* as one part of a complex project on the study of the balance, which was headed by Thābit b. Qurra and included friends, students, and apparently his patrons, the Banū Mūsā (ninth century), and not as an isolated single text created in antiquity and translated into Arabic, is very plausible. Other remainders of this project are the *Liber karastonis*, the extract of Thābit's text on the properties and causes of the equal-armed balance produced by ʿAbd al-Raḥmān al-Khazīnī (d. after 1130) in Merv (then northeastern Iran, today southern Turkmenistan), and perhaps, but not likely, a further text attributed to Thābit also called *Kitāb al-qarasṭūn*. Thābit's efforts to understand various, partly contradictory and faulty Greek fragments on the balance and to transform them into a consistent explanation of the conditions of equilibrium of an unequal-armed balance formed the center of this project, as the content of these texts along with the preface and prologue of the *Liber karastonis* and several remarks in the treatise on the properties and causes of the equal-armed balance shows.

A higher level of contextualization concerns issues beyond the texts of one author. Such contextualization can produce further insights into the sociocultural nature of authorship and the intellectual interests shared among different groups of scholars in a certain period and location. This at least is the case for the discussions on equal- and unequal-armed balances and the issues of weights that took place in Baghdad in the ninth century. It also applies to scholars in the 10th and 11th centuries and helps to explain the presence of such texts and intellectual interests in western and northeastern Iran in the early 12th century. However, only a few of the contextual elements of Thābit's and other texts on balances and weights that are at the heart of these two claims are new discoveries. In and of themselves, they were known to individual researchers since the 19th century. But they were never brought together nor questioned for their relevance regarding

[1] Sabit ibn Korra 1984, 12, 20–21, 24, 243–247, 278–284, 321–328, 353–355, 365–367, 380–381; Lorch 2008, 43, 47, 49, 51.

the issue of Thābit's authorship. To these long-known elements belong facts noted on specific copies of manuscripts:

(1) one of the manuscripts of the (pseudo-Euclidean?) *Kitāb fi l-mīzān* once belonged to the Banū Mūsā;

(2) in the 10th century, this manuscript came into the possession of the astronomer/astrologer of the Buyid court, ʿAbd al-Raḥmān al-Ṣūfī (d. 986);

(3) it was finally copied by another scholar of the mathematical sciences in the 10th century, Aḥmad b. Muḥammad b. ʿAbd al-Jalīl al-Sijzī;

(4) the Banū Mūsā owned the only extant copy of the Arabic translation of book 8 of Pappus' *Collectio*;

(5) Thābit edited the anonymous Arabic translation of the other (pseudo-Euclidean?) text on issues of weight, this time specific weight, with the title *Kitāb fi l-thiqal wa'l-khiffa* (Book on Heaviness and Lightness).[2]

Other long-known facts concern translations, newly written treatises, and patronage of Greek texts related to balances and weights. Among them are:

(6) Qusṭā b. Lūqā's (d. *ca* 912/3) translation of Hero's *Mechanics* around 860;

(7) Qusṭā's text on weights used in medicine for an unnamed patron in Baghdad with medical interests (identified in some manuscripts as one of the Banū Munajjim); and

(8) Sanad b. ʿAlī's (ninth century) treatise on the unequal-armed balance.

Finally, since Josef van Ess' magisterial oeuvre, *Theologie und Gesellschaft im 2. und 3. Jahrhundert Hidschra* [1991–1997], we know not only that there was a vivid debate on the meaning of the balance in the Qur'an among religious scholars of the eighth and ninth centuries but that in particular Muʿtazili authors were also interested in the question of why an unequal-armed balance needed only a small counterweight to balance a much heavier body [van Ess 1991–1997, 3.64].

What do such long-known contextual data signify for the issue of authorship of the extant *Kitāb al-qarasṭūn*? They buttress the claim that Thābit was embedded in an environment interested in how equal-armed and unequal-armed balances function and what ancient Greek authors had to say on this

[2] Woepcke 1851, 225, 232; MS Paris, BnF, Arabe 2457; Jackson 1970, 113, A78; Ahlwardt 1893, 5.353 no. 6.

and other mechanical questions, as well as in who collected manuscripts of translations of such works and who received commissions for writing summaries of these issues. In this sense, they imply that the attribution of the *Kitāb al-qarasṭūn* to Thābit b. Qurra is not at all implausible. Furthermore, the data show that the interest in this particular text and its topics continued into the 10th century. The preservation and acquisition of texts from the libraries of leading scholars of the ninth century was an important part of the scholarly practices in the mathematical sciences during the 10th. Finally, these larger contextual data draw attention to the explicit sociocultural nature of a theoretical text and its genesis.

Finally, I wish to stress that contextualization is indeed beneficial for solving even such classical questions as that of authorship. In order to develop my own position on whether Thābit might be the author of the *Kitāb al-qarasṭūn*, not only did I compare this work with the *Liber karastonis* and the treatise on the properties and causes of an equal-armed balance, I also compared it with all published mathematical and astronomical works attributed to Thābit as well as with Qusṭā's translation of Hero's *Mechanics*, the anonymous translation of book 8 of the *Collectio* by Pappus, the extant Arabic fragment of the *Problemata mechanica*, Archimedes' works and their Latin translations by William of Moerbecke, and the Greco-Latin translation of the *Elements*. The goal of this extensive comparative analysis was to determine philological properties of the various texts in order to find at least preliminary answers to three questions:

(1) Did Thābit b. Qurra write all his works in a consistent style with a stable vocabulary?

(2) Do other texts contain the philological and conceptual idiosyncrasies of the *Kitāb al-qarasṭūn* and do they form clusters in regard to content, time, or origin (author, translator)?

(3) Which parts of the language of the *Kitāb al-qarasṭūn* are shared by other translators or editors of mechanical and related mathematical texts, and do such relations reveal the existence of parallel or even competing technical languages that can be linked with some caution to identifiable groups of translators or authors during the course of the ninth century?

Knorr's denial that Thābit was the author of the *Kitāb al-qarasṭūn* was a denial of authorship in the classical sense: he took for granted that the text had but a single author and maintained that it was not Thābit. But once one allows that a text can have multiple authors where one is singled out above the others as explained above, it is clear that Thābit was indeed the text's author.

Thus, the medieval attribution of the *Kitāb al-qarasṭūn* to Thābit b. Qurra is to be accepted not merely as an expression of beliefs held then but also as a result of my systematic analysis of the text's features. Thābit, that is, compiled the *Kitāb al-qarasṭūn* from several fragments translated from Greek into Arabic, fragments which represent different Greek scholarly traditions—Aristotelian, Archimedean, Heronian, and possibly mixtures of those with other school or commentary literature. Philological, symbolic, conceptual, methodical, and demonstrative particularities leave no doubt that it was not a single ancient Greek text translated into Arabic (perhaps by the anonymous colleague to whom Thābit refers in the preface of the *Liber karastonis*) as one of Knorr's many contradictory hypotheses would have it [1982, 37, 48]. The fact that the *Kitāb al-qarasṭūn* shows undeniable traces of numerous Greek traditions highlights the usefulness of the larger concept of multiple authorship. It also illuminates, as said before, that Thābit respected the forms of the fragments that he encountered when he compiled this particular text. (When he later reworked it into a text now lost but translated from Arabic to Latin by Gerard of Cremona, he no longer respected these forms but changed them quite substantially.) The *Kitāb al-qarasṭūn* is, thus, not an extract of earlier works that summarizes their main content. Still, it is true that Thābit was not the immediate author of any of the parts of the *Kitāb al-qarasṭūn*, as is shown by the omissions and oddities in some parts of the *Kitāb al-qarasṭūn* such as the incomplete proof of theorem 2 or the circularity in the proof of theorem 5. A second argument comes from its comparison with the *Liber karastonis*. There, Thābit expresses his frustration with the difficulties that he encountered in the translations and copies, their proofs and explanations, and describes some of his efforts to understand the ancient texts.

On the other hand and against Knorr's belief that both works represent different textual traditions, a stepwise comparison of the elements that both texts share leaves no doubt that the *Liber karastonis* is a carefully modified, edited, corrected, at times simplified version with explanations of the *Kitāb al-qarasṭūn*. Thābit clearly invested much effort into improving the extant Arabic version. It is in this altered text that he dealt with the deficits of the Greek ancestors of the *Kitāb al-qarasṭūn* by deleting two of the postulate-like statements and one theorem, introducing a new theorem and modifying the proof of its subsequent theorem, while he left these parts unchanged in the compilation. He also added explanatory statements and numerical examples within the various theorems, which can be easily traced. In addition to these clearly visible mathematical and methodical interferences into the previously compiled *Kitāb al-qarasṭūn*, Thābit's personal voice is also much clearer and stronger in the *Liber karastonis*. Hence, Thābit's role as an author differs between the two texts, although he wrote neither of the two fully on his own. This difference is reinforced by Thābit's explicit claim to authorship in the case of the *Liber karastonis*, while the claim to authorship in the *Kitāb al-qarasṭūn* comes from his students who wrote the text down in his class.

This comparison also shows very clearly that both texts constitute a textual unity and elucidates Thābit's working practice and concerns. It is this feature of interconnectedness and continuous dialogue, already visible to some degree in the elements surrounding the *Kitāb al-qarasṭūn*, and the leading role of Thābit in this continuous debate with unnamed contemporaries that allow us to attribute not merely the *Liber karastonis* but also the *Kitāb al-qarasṭūn* to Thābit as an author who chose their individual elements and decided how to present and share them and in which manner to interact with them.

The contextualized philological analysis of the *Kitāb al-qarasṭūn* reveals several other important features. This text shares central parts of its vocabulary, style, and grammar with many of the texts for which Thābit's authorship is accepted. Conspicuous terminological idiosyncrasies are shared with a translation of the *Almagest* made in the 820s as well as with early and late ninth-century translations of Aristotle's *Meteorology* and *Physics*. Symbolic idiosyncrasies link the *Kitāb al-qarasṭūn* to the environment of Hero's *Mechanics*. The *Kitāb al-qarasṭūn* and the translations just referred to agree in a specific and, in mathematical and astronomical texts, not widely spread choice of words for drawing or generating the path of a moving object. This idiosyncratic expression is "cutting out (or through) space or distance". Its particular context in the *Kitāb al-qarasṭūn* is that of producing a sector of a circle. It appears in the same form and with the same mathematical meaning in the extant fragment of the *Problemata mechanica*. This does not merely suggest that the *Problemata mechanica* was translated probably in the early ninth century and belonged to the collection of texts on mechanics available to the Banū Mūsā and Thābit b. Qurra before the 870s. The differences in detail between the two texts also show that Thābit did not copy directly from this Aristotelian text.

The same applies to the two texts on the balance and on heaviness and lightness attributed to Euclid. They share philological, conceptual, and representational elements with the *Kitāb al-qarasṭūn*, all without being identical. There is even sufficient reason to assume that Thābit worked with an older version of the *Kitāb al-mīzān* than the one extant today or with some other, very similar fragment. Thus, by comparing in this way the

Kitāb al-qarasṭūn with a substantial number of other mathematical texts translated, edited, or newly written during the ninth century, we confirm that the enriched and enlarged concept of multiple authorship applies well to this text. Thābit clearly used Arabic translations of Greek fragments, which he fused without remedying their shortcomings. But he also had access to a broader range of such texts and preferred certain formulations of principally similar subject matters to others. If he reformulated some of them on his own—which is possible but difficult to prove—he took care not to deviate recognizably from the language of his source(s). Textual fidelity was thus an important aspect of Thābit's authorship but had a different, richer nature than we tend to suppose.

The central force of my critique of Wilbur Knorr's arguments against Thābit b. Qurra's authorship of the *Kitāb al-qarasṭūn* is that we must engage in meaningful textual contextualization. This means considering all the other types of data available in the text at stake, along with the manuscripts containing its copies as well as their meaning for this text, if we wish to understand the working practices, values, and goals of the scholar to whom the text is ascribed. Furthermore, such contextualization yields insights into the sociocultural richness of the very concepts of authorship and textual fidelity for the text under analysis and, thus, may not merely answer specific questions but also help us to gain deeper insights into the scholarly climate and practices in a given culture at a given location and time. In the case of Thābit's connection to the *Kitāb al-qarasṭūn* and its Greek components, such contextualization also works against our importing any prejudices and generalizing assumptions about the value of different scholarly cultures and the capabilities of their members.

3. Peculiarities of Knorr's analysis

One outstanding peculiarity of Knorr's analysis of authorship is his continued modification of claims and positions, unaccompanied by any final decisions regarding which of his various ideas he considers at the end to be the most plausible. It makes it difficult for the reader to understand the relevance of individual arguments for or against each of these ideas. Moreover, these idiosyncratic oscillations obstruct the clarity of the proofs for or against Thābit's or young Archimedes' authorship.

A further methodological problem follows from Knorr's basic assumptions about authorship in general and for the case of the *Kitāb al-qarasṭūn* in particular. These assumptions summarized above cohere with his overlooking alternative hypotheses to his idea that the *Kitāb al-qarasṭūn* is an edition of an Arabic translation of a single ancient Greek text. And once set on this unilinear track, his particular readings of individual passages of the Arabic text were almost predetermined.

4. Issues of expertise

In this section, I will present evidence that Knorr's analysis did not merely suffer due to the limitations of his methods and his commitment to an internalist reading but was adversely affected as well by his misunderstanding of some of the Arabic words, expressions, or grammatical features. His analysis also suffered because of his limited engagement with the problems that he saw in the mathematical content of the *Kitāb al-qarasṭūn* and the *Liber karastonis* as well as with the issue of the relation between the content of these two texts, the *Liber de canonio*, and the appendix of the Beirut manuscript. It is not always clear whether his omitting to study these points more closely and his misinterpretation of some of them was due to his biases, which I will discuss in the last section, or whether they also were due to his difficulties with Arabic.

One case where the issue of philological competence played a decisive role is the interpretation of the *Liber de canonio* as a Latin translation of an ancient Greek text. As I have argued, one reason for Knorr's not seeing the many Arabisms in the second part of the *Kitāb al-qarasṭūn* is his limited familiarity with unpublished Arabic translations of Greek mathematical texts and their philological peculiarities. This kind of shortcoming applies to all of us. But not all of us are equally aware of the possible implications of our limited knowledge for our analysis and conclusions. Knorr certainly was more confident than I am. Another, and greater, part of his denial of Arabisms in the *Liber de canonio* (except for one) is his blindness to the many instances in which the second part contains verbal forms of a first person plural as well as conventional formulas for stating that something was proved or would be done similarly and so forth. I doubt that this astonishing fact reflects Knorr's philological problems, although one cannot exclude this entirely. It is more likely that it is the professional blindness that many of us will have experienced in our own work, a blindness which prevents us from seeing things in a text because we are so bound by our biases or questions as to overlook them.

4.1 *Philological misunderstandings and misrepresentations* Cases of true misunderstanding of Arabic occur when Knorr identifies phrases or sentences as corrupt or false against either classical grammar or medieval dictionaries. Their interpretation as simple philological errors remains nonetheless difficult since they are occasionally also part of his misrepresentations.

For example, there is his discussion of one of the passages in the Beirut manuscript of the *Kitāb al-qarasṭūn* which he took to be scholia. The London manuscript, the only other text available to Knorr, does not contain this part. Neither does the third manuscript, originally in the possession of Berlin's State Library but preserved since the final stage of WWII in Cracow. However, the shorter Florentine version of the *Kitāb al-qarasṭūn* presents this part after claiming that the *Kitāb al-qarasṭūn* had ended [MS Florence, Biblioteca Laurenziana, Or. 118, f 72a]. Thus, this new copy may indeed support the view that this particular passage is a scholium. It also offers some valuable alternative readings for weighing Knorr's interpretations of the corresponding passage in the Beirut text.

Knorr's main quest is, as in the case of the *Kitāb al-qarasṭūn*, for the (unknown) author of this passage. He admits that no clear evidence can be found in the text itself for providing a definitive answer. But he feels that

> certain awkward or unclear expressions...together with problems of its logical ordering, recommend viewing it to be a translation, rather than an original composition. [1982, 68–69]

In alleging the logical problem and construing one of the expressions as awkward, Knorr shows that he misunderstood the Arabic here. The so-called "unclear expression" likewise highlights his limited familiarity with Arabic scientific literature. I will discuss this "unclear expression" momentarily. As for the problem of logical order, Knorr describes it as the failure to point out that "the problem of balancing the unevenly divided weighted beam" is "a logical consequence of (the) general principle of equilibrium for the weightless beam" [1982, 68]. The alleged lack of logical ordering is the product of Knorr's misinterpretation of «wa-dhālika annahu» as «wa-dhālika innahu» and his literal understanding of this expression as "and that is what it...". But «wa-dhālika innahu» does not exist, while «wa-dhālika annahu» means "this is the reason why", "because" or "since". It also can be translated simply as "which means" or "to say it more precisely" or simply "namely" or "to wit". Thus, there is no logical problem here. Read correctly, the Arabic text makes clear that the problem of the material beam can be treated on the basis of the knowledge provided for the immaterial beam with the additional consideration of the role of the beam's materiality. There is no need to treat this formulation "as an inadequate translation" [1982, 69].

Knorr's first type of "awkward expressions" occurs in two instances of stating—incorrectly, according to Knorr—"the condition of parallelness of the beam". According to Knorr's discussion in the main body of his book, the text expresses the equilibrium as obtaining when "it (*sc.* the scale-pan) is parallel to the horizon with the beam" [1982, 69]. In the appendix, however, the passage is translated as "if it is suspended at the end of the smaller part, it is too small to make the beam parallel to the horizon..." [1982, 187]. Ignoring here the rendering of « aqṣar » as "smaller" rather than "shorter" and the reading of « qaṣara ʿan » as "being to small to make" rather than as "being unable to, failing to reach..."—these two different possible translations reflect two different "identities" of the verb « qaṣara »—the translation in the appendix is in principle correct. Thus, I fail to understand Knorr's lengthy discussion of a deviating and false rendering of this as well as a second expression of analogous kind and their description as "awkward", "clumsy", or "ungainly". Neither is it clear to me why he chastised the two Arabic expressions by writing "But of course it is the beam, not the counterweight, which can be parallel" [1982, 69–70]. Had he forgotten his translation in the appendix or did he believe so strongly in his intended result, i.e., in the fact that we have here "again an imprecise rendering of an absolute expression from the Greek" that he sacrificed this translation?

Furthermore, the incorrect statement detected by Knorr in the expression "it (*sc*. the scale-pan) is parallel to the horizon with the beam" reflects difficulties in understanding the function of the preposition « bi » in two Arabic phrases [Knorr 1982, 186] which I translate as follows:

اذا علقت بطرف القسم الاقصر قصرت عن ان يوازي بالعمود الافق

> If it [*scil*. the scale-pan] is suspended at the endpoint of the shorter arm, it fails to make the beam parallel to the horizon.

ثم يتعرف وزن ما يحتاج اليه مع وزن الكفة لموازاة الافق بالعمود

> Then the weight is to be learned [i.e., determined], which is needed together with the scale-pan for the parallelism of the beam to the horizon.

Knorr took both instances to signify that the author of the scholium speaks "in each instance of the counterweight, the scale-pan" and expresses equilibrium as obtaining when "it (sc. the scale-pan) is parallel to the horizon with the beam" [1982, 69]. The second passage, however, does not concern the weight of the scale-pan alone but a sum which is responsible for the equilibrium, namely, the weight of a part of the material beam together with the weight of the scale-pan. In short, Knorr's description of the problem is inadequate. But is it correct to interpret even the first passage as meaning that "it (the scale-pan) is parallel to the horizon with the beam"? As my translation indicates, I do not think that this is a correct reading of the Arabic phrase. Knorr goes astray here because he mistakes the meaning of the preposition « bi ».

In classical Arabic, « bi » indicates *a connection with something* or *the object with which something happens* [Fischer 1972, 136: §§294, 294.1]. Knorr obviously selected the first meaning, though the second is the appropriate one. Furthermore, in one of its sub-forms, « bi » is called the « bi » of transitivity. This means that it either transforms an intransitive verb into a transitive one or strengthens the transitivity of an already transitive verb. It is this grammatical function that « bi » has in the two instances given above. Hence, « bi'l-ʿamūd » in both cases can either be translated simply as a part of a genitive construction, viz. the parallelism of the beam to the horizon, or as an object that is made parallel to the horizon. Given the minor differences between the two formulations, I have given both in my translation. In sum, the Arabic of these formulations is not faulty. And there is no cause to mark them as the product of a bad Arabic translator or to speculate about the existence of a Greek ancestral text.

4.2 *Identifying diacritical marks* Other philological problems with the passage just discussed in the previous subsection concern the identification of the letters in an Arabic word without diacritical points [1982, 183]. The lack or misplacement of diacritical points is a constant technical problem of medieval texts in Arabic script. It is not always easy to ascertain the correct placement of these points and, thus, to identify the verb and its grammatical form. Mistakes are easy. Their avoidance necessitates in difficult cases careful reflection and at times tedious comparison with other, similar formulations within the same text or, if one encounters a particularly ambiguous statement, with other texts.

In the two cases within one sentence that I will present here, the difficulty rests not merely in the lack of diacritical points but in the changes evident in the text in the Beirut manuscript. Knorr could not fully comprehend these changes, since he was not aware of the Florentine manuscript. Nonetheless, his first choice of diacritical marks should at least have made him suspicious of the passage since, in order to make sense of the text, he had to assign the verb that he settled upon a meaning which is not supported by our lexica. Moreover, he clearly recognized that the reference to some previous theorems (where his forced translation of this verb appears) posed a problem in so far as the Beirut text refers to theorems which were not yet presented. But rather than make allowances for a problematic Arabic text, he took this feature to signify that the passage was interpolated from an Arabic translation of a different Greek text where it actually had made sense.

وذلك انه اذا اخذ عمود متساوي الغلظ فقسم بقسمين مختلفين على نقطة وجعلت المعلاق فانه يتهيا بالاشكال التي قد علمت ان يوخذ مقدار الثقل الذي اذا علق بطرف القسم الاقصر اعتدل العمود اذا علق بمعلاقه على موازاة الافق.

[Knorr 1982, 182]

> And that is that if there is taken a beam, uniform in thickness, and it is divided into two different parts at a point and this is made its suspension, then it results from the theorems which have just been learned that there can be taken the quantity of weight, which, if it is suspended at the end of the smaller part, the beam is in balance if it is suspended from its suspension in parallel to the horizon. [Knorr 1982, 183]

I have highlighted the two verbs without diacritical points and their interpretation by Knorr in red text and the reference to previous theorems in dark red text. In both verbs, three letters are unidentified in the Arabic text. In the first, Knorr chose to interpret them as «y», «t», and «y». In the second, he opted for «y», «kh», and «dh». In this latter case, he knew that the alternative was «y», «j», and «d». In the first, he does not present any alternative reading, which in my view is given by «n», «b», and «n». Knorr's reading of the first verb is «yatahayya'u», which means literally "it is prepared" or "it is ready". But this does not fit the context as can be seen in his translation above. Hence, he altered it to "it results" [1982, 84–185]. My alternative identification of the consonants yields «nabihnā» for the first verb, which means "we note". This modifies the translation meaningfully without overstating the content of the Arabic verb. This new translation is, however, only possible thanks to my access to a fourth Arabic text preserved in Florence.

In the case of the second verb, the Arabic text available to Knorr allows for two possible readings: «yu'akhudha» meaning "to take" or «yujada» meaning "to exist" or "to be found". The text transmitted in the Florentine manuscript [see MS Florence, Biblioteca Laurenziana, Or. 118, f 72a,4–6] offers a substantial variant to the Beirut text and thus opens the way for an altogether different understanding of this passage.

فانه نبهنا ان يجد بالاشكال التي عملها ابو الحسن مقدار الثقل الذي اذا علق بطرف القسم الاصغر اعتدل العمود اذا علق بمعلاقه على موازاة الافق.

> ...then we note that the quantity of the weight, which equilibrates the beam in parallel to the horizon, if it (i.e., the weight) is suspended at the end point

> of the smaller part (and) if it (i.e., the beam) is suspended in its suspension, is found with the theorems, which Abū l-Ḥasan has produced.

It is easy to see that the verb of the second passage together with «an» appears much earlier in the Florentine text than in the Beirut version and has, thus, a different point of reference. This difference in placement implies a different understanding of this passage and, by virtue of further deviations between the two texts, allows us to solve the problem of the referent of the previous theorems. The Florentine variant specifies that these are theorems which Abū l-Ḥasan, i.e., Thābit b. Qurra, had produced. This explanation shows, moreover, that the passage came into being in all likelihood after Thābit b. Qurra had compiled and taught the *Kitāb al-qarasṭūn* or at least the parts prior to theorem 2.

The problems that I have just addressed not only indicate Knorr's struggles with classical and middle Arabic, they also highlight the difficulties of interpreting such passages on limited textual bases. They warn us to be more cautious and to avoid drawing over-grand conclusions from too small features, a failing of mine for many years in my studies of the Arabic *Elements*. This insight into my own shortcomings has convinced me of the need to contextualize documents textually at the very least.

5. Issues of prior beliefs

Our explicit beliefs and deep-seated prejudices can be a persistent obstacle in our research. They guide our interpretations and conclusions and, thus, typically mislead us in our study of texts, images, or material objects. The way to limit their impact is well known today—critical reflection. In comparison to today's attention to historical epistemologies, there was not so much awareness of the importing of modern notions to historical sources at the time when Knorr wrote his book; not, at least, among historians of premodern mathematics and other exact sciences. At that time, we believed, myself included, that we could and ought to be objective and neutral and that, if we did introduce values, they should work in favor of the people and the works that we studied. We did not believe that our scholarship included and inevitably brought to bear values that we did not question but took for granted. Thus, for example, we believed without question that doing science for science's sake was the most noble and, indeed, the only right way of doing science. Likewise, we also assumed that good science relied on objective, rational, and verifiable principles, methods and theories; and that in contrast to other domains of human activity, science was free of biases and subjectivity. One consequence of these beliefs was a scholarly practice that privileged the study of theoretical themes, primarily in texts, by scholars deemed first-class, in periods and regions regarded as leading intellectual centers. All other products, scholars, periods, and regions were more or less overlooked with the exception of astronomical, mathematical, and geographical material on timekeeping, the determination of the *qibla* or direction to Mecca for prayer, and related religiously sanctioned problems.

5.1 *The putative superiority of ancient Greek geometers* Knorr's claims against Thābit's authorship and in favor of a single ancient Greek ancestral text with the young Archimedes as its author are anchored in two beliefs, the first of which I will discuss in this section and the second in the next. The first is that Hellenistic geometers were intellectually superior to medieval scholars writing in Arabic. The second is that mathematical texts developed or evolved from a higher, more advanced level to a lower, more elementary level as a result of the explanatory and exemplifying interpolations introduced over centuries of teaching those texts. The belief in the intellectual superiority of ancient Greek scholars was first formulated by humanists. It was particularly rampant during the 19th century when claims to astronomical or mathematical creativity by scholars from Islamicate societies were greeted, for instance in France, with disbelief or even derisive laughter, as Charette has argued in his analysis of the respective positions among European writers about the exact sciences in Islamicate societies [1995, 101–142].

In the course of the 20th century, especially since the 1960s, historians increasingly began to argue for the innovative and creative achievements of medieval scholars from Islamicate societies. Other historians, in particular classicists and European medievalists, continued, however, to uphold the older "sandwich thesis" according to which the only or major role that scholars from these societies had played consisted in their translating ancient Greek texts and preserving them in this way for their later translation into Latin.[3] Knorr's belief in the intellectual superiority of Hellenistic geometers was less crude in that he recognized that from the 10th century onwards there existed gifted masters of theoretical geometry among the scholars from the classical Islamicate societies.

In the case of the *Kitāb al-qarasṭūn*, Knorr's conviction derives from his profound familiarity with Archimedes' works and his superficial understanding of Thābit b. Qurra's oeuvre. The mere fact that Knorr did not try to

[3] Sabra termed this position as straightforwardly "reductionist" [1987, 224–225].

compensate for his limited exposure to Thābit's works by a careful analysis of at least all treatises by Thābit that contain aspects relevant to the various issues discussed by Knorr in the *Kitāb al-qarasṭūn* indicates the guiding power of his belief in the higher quality of classical and Hellenistic mathematical works. His constant willingness to ascribe all kinds of perceived or actual shortcomings in the Arabic text to translators at large or to Thābit b. Qurra in particular, and to use these failings as indicators of the mishandling of Greek source texts, further manifests the power of this belief. Likewise, Knorr's seizing on the contradictory and inconsistent treatment of individual points in the *Kitāb al-qarasṭūn* as hints of an Archimedean background or as evidence of Thābit's "pedantic" or "pedestrian" but "competent" work as a geometer is yet a third instance of his biases at work.

The clearest cases of Knorr's biases and their interpretive consequences appear in his analyses of theorems 3–5. But, before I turn to them, I must draw attention to his inconsistency in formulating his main interpretations, something which I have already mentioned. He changes these formulations often and in a substantive manner, as I will show below. Again, it is unclear why he proceeded in this manner, given the difficulty in supposing that he did not understood the differences between his various statements.

The inconsistency and contradictory manner of Knorr's presentation of his belief in the Greek origin of the *Kitāb al-qarasṭūn* surfaces on numerous occasions. He oscillates between stronger and weaker forms of this belief. This would not have been a problem if he had expressed his uncertainty clearly and presented the arguments in a manner clarifying the problems that he saw in regard to any of his proposed positions. But he does not do so. Instead, he leaves the reader with the impression that he either remained unaware of these variations or did not recognize the methodological problems that they entail.

Knorr starts his discussion by allowing that Thābit had composed "his *Kitāb al-qarasṭūn*" or at the very least the proof of theorem 5 [1982, 31, 33]. Then, he proceeds to the claim that, in this proof, the relationship between this text, the Latin *Liber karastonis*, the *Liber de canonio*, and the short appendix (*ziyāda*) in the Arabic text of the *Kitāb al-qarasṭūn* in MS Beirut, St. Joseph University, 223 shows that the *Kitāb al-qarasṭūn* must have been written by "an author different than Thābit" and that Thābit "prepared an improved edition of this prior treatment and appears to have had access to the theorems in the Beirut appendix to guide his effort" [1982, 37]. He repeats this when he claims:

> The Arabic manuscripts [of the *Kitāb al-qarasṭūn*] appear to derive from a work by one of Thābit's colleagues; it is not impossible that it was a prior draft on the *qarasṭūn* made by Thābit himself. It depended in an important way on materials translated from a Greek work not now extant. [Knorr 1982, 48]

But, after discussing some of the material, he no longer hesitates to offer a strong form of his thesis of its Greek origin: "...the Greek source, of which the Arabic manuscripts of *K. Qar*. are an edited translation,..." [1982, 86]. A few pages later, he goes a step farther and writes: "This strengthens our view that *K. Qar*. presents to us the edited remnant of an Archimedean work" [1982, 93].

Accordingly, Knorr takes the position that the text extant in the two copies of the *Kitāb al-qarasṭūn* which were known to him (Beirut, London) is but a single text derived from another single text by way of translation from Greek into Arabic and editing in Arabic. Two men were the main contributors to this textual sequence: Archimedes and Thābit b. Qurra. The other persons whom he touches upon during his discussion, Thābit's "anonymous colleague" and the author of the possibly pseudepigraphic Euclidean fragments of *On the Balance* and *On Heaviness and Lightness* have faded into the background.

5.2 Knorr on the devolution of mathematical texts The second belief that shaped Knorr's analysis and, hence, his arguments and conclusions concerns whether there was in the main a single direction of development in mathematical texts during antiquity and the Middle Ages. Rommevaux, Djebbar, and Vitrac [2001] have already described Knorr's view [1996] of this in their analysis of his article about Heiberg's edition of Euclid's *Elements*. They concluded, in somewhat different words, that Knorr believed that in ancient Greek texts there was in general a devolution which went from more complex or advanced mathematical works to simpler and longer ones where (almost) every simple step of the original has been spelled out. According to their analysis, Knorr saw this line of development as a result of the use of texts in teaching and of continuous editing and commenting. In his book on Greek, Arabic, and Latin texts on the balance, Knorr does not formulate this belief explicitly. But, as my analysis of his treatment of a part of the proof for theorem 3 in the *Kitāb al-qarasṭūn* shows, it was one element that guided his choice between two alternative interpretations.

Knorr's strong thesis about the Archimedean origin of the *Kitāb al-qarasṭūn* and its fragmentary textual nature is based on a mixture of beliefs about how mathematics developed in ancient and medieval times as well as about the mathematical skills of ancient Greek and early Abbasid scholars. In addition, he develops and proposes to justify it, as I will show below, through references to a number of Archimedean works and an analysis of some of the mathematical as well as a few philological features of the two copies of the *Kitāb al-qarasṭūn* available to him [1982, 47–48, 76–86]. These beliefs and his selective working practice precluded considering interpretations of material alternative to his strong thesis. In short, his conclusions are not always derived from an open-minded investigation of what was available to him in the 1980s. In several instances, no firm conclusions can be drawn from the evidence presented by Knorr, sometimes not even from the broader evidence that I have collected. The processes that led to the texts extant today may have evolved in more than one way. The material available does not allow us to determine one historical sequence of steps. Indeed, it is possible to conjecture one set of steps as a sequence taking the one or the other direction or as a parallelism of events, independent or not from each other. Instead of allowing for questions that could not be answered fully or problems that must be left unresolved, Knorr wished to do the impossible—to reconstruct a fully lost text of which we possess no more than a small selection of titles provided by Heron and Pappus. In his effort to reach this goal, Knorr did not attend to the extant texts and determine their individual features with care and caution.

Lest one think that this is peculiar to Knorr alone, I must confess that I have been told on several occasions by a friend and colleague that I was trying to achieve too much in my analysis of the Arabic translations and editions of Euclid's *Elements* made during the ninth century. Having believed for more than a decade in the medieval narrative of two main bodies of translations and editions, one undertaken in the early ninth century by al-Ḥajjāj b. Yūsuf b. Maṭar (d. after 827), the other by Isḥāq b. Ḥunayn (d. 911) in cooperation with Thābit b. Qurra, I tried to compile a collection of fragments of al-Ḥajjāj's work. Eventually I was forced to admit that all the extant texts of these two different traditions of the ninth century derive, in the case of books 3–9 at least, from the work of only one of these translators, given that they share idiosyncratic vocabulary and mistakes. Many of our specific beliefs about the work of these three scholars and their terminology stand in need of revision as well. Whether it will be possible to determine the translator or editor of this interrelated set of texts remains an open question: I suspect that I may never be able to sort things out in a manner that will allow me to formulate at least a credible hypothesis. Such differences in research goals, as illustrated by the differences between Knorr's and my own aims, signal fundamental changes in epistemic values over time, even within the lifetime of a single scholar.

5.3 *Theorem 3 of the* Kitāb al-qarasṭūn

Theorem 3 describes a weightless beam with one weight appended to its extremity, and with two equal weights on the other side, one of them at the extremity and the other one closer to the fulcrum. Assuming that this configuration is in equilibrium, the theorem states that these two weights can be replaced without disturbing the equilibrium by a weight of the same amount as the two taken together, positioned at the midpoint between them [Jaouiche 1976, 152–155].

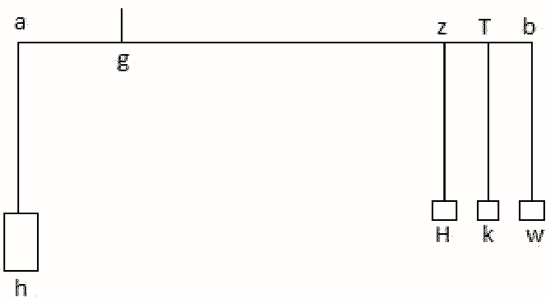

Figure 1. Theorem 3
(the weightless beam)

In the two manuscripts available to Knorr, there are differences in how the proof of this theorem begins. I summarize the relevant steps as follows:

The London manuscript

(1) For w (it is the case that) (part of h)$:w = bg:ga$
(2) For H (it is the case that) (part of h)$:H = zg:ga$
(3) $w = H \ \rightarrow$ (part of h)$:w = zg:ga$
(4) Now add $\rightarrow h:w = (bg + zg):ga$
(5) Equally (it is the case that) $h:(w + H)$, since $w + H = 2w$, $= (bg + gz):2ga$
(6) Also: $= ½(bg + gz):½(2ga)$
(7) As for $½(bg + gz)$, this is gT
(8) As for $½(2ga)$, this is ga
(9) $\quad \Rightarrow h:(w + H) = Tg:ga.$

The Beirut manuscript

(1) For w (it is the case that) (part of h)$:w = bg:ga$
(2) For H (it is the case that) (part of h)$:H = zg:ga$
(3) Now add → all of $h:(w + H) = (bg + gz):2ga$
(4) Equally (it is the case that) (part of h)$:(w+H)$, since $w+H = 2w$, $=$ ½$(bg + ga)$; this is gT.
(5) As for ½$(2ga)$, this is ga.
(6) $\Rightarrow h:(w + H) = Tg:ga$.[4]

Anybody who encounters such a divergence needs to decide which of the two versions is most likely the earlier one. There are no strict criteria to apply, only rules of thumb deriving from the search for mistakes, contradictions, modernizations, interpolations, philological peculiarities, and comparison with similar passages within the given text as well as with other works of an author, translator, or commentator. Often such investigations clarify the sort of divergence that one sees here in theorem 3. Occasionally, though, the best one can offer is an informed guess about which is earlier. But is this the case here?

A quick comparison between the two step sequences reveals two key differences. First, step 3 of London is missing in Beirut and step 4 in Beirut differs in form and content from that in London. Second, the last part of London's step 4, all of steps 5 and 6, and the beginning of step 7 are missing in Beirut. This second difference leaves no doubt that Beirut has lost a part of its text. So, is this also the case for the first difference? There is here, however, no clear sign to show which version has lost text. Yet, one would seem to be a modification of the other. The question only is, then: Which version modifies the other?

Knorr decided it was Beirut that preceded London, i.e., that the London version modifies the Beirut version. What are his arguments? He claims that, in addition to some minor differences, the texts of the Beirut and London manuscripts are separated by one subtle but substantial difference. He believes that the move from steps 1 and 2 in the Beirut manuscript to step 3 necessitates the silent assumption of a lemma about the addition of proportions. He finds the same move in Archimedes' theorem 1 of *Conoids and Spheroids* and he calls Beirut's step 3 "daring" [1982, 59].

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4 My use of the letters here corresponds literally to the Arabic. In the following, however, I will stick to Knorr's usage of Latinized Arabic letters as is the convention among historians of science in Islamicate societies.

For the sake of clarity, I will quote here the entire passage where Knorr presents this evaluation. Before I do so, however, I have to point out that a mistake of copying is part of this quote. In the Arabic text of the proof of theorem 3 (called by Knorr "proposition IV" after the *Liber karastonis*) in appendix E, Knorr correctly gives the verb in Beirut as «jumiʿa» and in London as «jamaʿnā», which he correctly translates as "to combine" with their specific grammatical forms [1982,194–195]. In the passage that I will quote now, however, he transformed the Arabic verb «jamaʿa» into «jaʿala», translating the latter incorrectly as "to compose" [1982, 58–59]. In the quote, I highlight in red important interpretative sentences.

After pointing out that step 3 of London is missing in Beirut, Knorr writes:

> the texts now come back in agreement, save for a key difference at the end of the next line:
>
> | (London) | (Beirut) |
> |---|---|
> | And if we compose, the ratio of weight E to weight W is as the ratio of the ratio $bg + gz$ to ga. | And if they have been composed, the ratio of *all* of weight E to weight WH is as the ratio of $bg + gz$ to *twice* ga. |
>
> Here the differences are minor: "compose" and "have been composed" are a matter of scribal differences («jaʿalna» and «juʿila»). The appearance of "all" in Beirut is important, in that it alludes to the procedure by which the subsequent proportion has been derived: namely, by adding parts of E which have been viewed as counterbalanced separately by W and H. We note also the appearance of "twice" in Beirut, missing from London. These discrepancies result from two rather different modes of "composing" the ratios. To see this, let us write E_w for the part of E counterbalanced by W, and E_h for that part balanced by H. Then, $E_w : W = bg : ga$ and $E_h : H = zg : ga$. In the London ms. we are to introduce the substitution $H = W$ in the second proportion. Since the denominators of our two proportions are now identical, we may add the numerators, obtaining $(E_w + E_h) : W = (bg + gz) : ga$. This step is not stated in this form, but as $E : W = (bg + gz) : ga$, it being obvious that E is the sum of the parts E_w and E_h. In the Beirut ms. an operation of a subtly different sort is performed. On the same initial terms, $E_w : W = bg : ga$ and $E_h : H = zg : ga$, it is at once deduced that $(E_w + E_h) : (W + H) = (bg + gz) : (ga + ga)$, that is, $E : W = (bg + gz) : 2ga$. Is this justified? As it happens, the step, daring in appearance, is actually covered precisely by the theorem on proportions which Archimedes proves as *Conoids and Spheroids*, 1—we only need the condition that the four numerators or the four denominators are in proportion, e.g., $W : H = ga : ga$. This is manifestly true here since $W = H$. [Knorr 1982, 58–59]

Once Knorr "recognizes" in the variant presented in the Beirut manuscript an Archimedean ancestor unknown to scholars in Abbasid Baghdad, he reverses the argument. He now presents the silent application of the Archimedean lemma just diagnosed as a marker for an Archimedean character of the *Kitāb al-qarasṭūn* and supports this argument by referring to the subsequent theorem:

> The automatic assumption of a lemma on proportions of this sort, proved and applied only in Archimedean works not available to Arabic scholars, is reminiscent of the Archimedean features we have perceived in *K. Qar*. VI. [Knorr 1982, 58–59]

Thus, in seeing in the Arabic *Kitāb al-qarasṭūn* the remainder of a single text of Archimedean provenance, Knorr ignores the circularity of his argument and overlooks the fact that other readings are possible and more plausible, if one abandons the Archimedean thesis and tries to understand the Arabic text on its own terms. This might entail, for instance, searching for hints that the slow, step-by-step procedure of London constituted the original version, while Beirut's allegedly daring recourse to Archimedes' *Conoids and Spheroids* theorem 1 was the result of editing London's text. Or it might involve asking whether the proof given in the *Liber karastonis* contributes to the understanding of this small textual difference.

If one tries to understand the Arabic text on its own terms and looks to its language, it becomes evident that Knorr's understanding of the two variants of the proof of theorems 3 in the Beirut and London manuscripts is predicated on three more simple mistakes that are relevant for answering the question of which variant is the older of the two. The first of these two mistakes Knorr shares with Jaouiche. Both did not recognize that the letter «waw» in one occasion signified the mathematical symbol of one of the weights as it does on other occasions in this theorem. As a result, Knorr's translation is incorrect.

The phrase in question is «idhā kānā mithlay waw». Its correct translation is: "since the two are twice the same as *waw*", i.e., $W + H = 2W$. Jaouiche [1976, 155] understood the phrase to mean «lorsque ces deux derniers sont égaux», i.e., $W = H$. He overlooked the «waw» and ignored the fact that the form of the dual of «mithl» (the same in this phrase) is a *status constructus* (*mithlay*) due to the following «waw», not a *status indeterminatus* («mithlayni») as demanded by his translation. Knorr [1982, 59] rendered this phrase as "since they are equal", thus agreeing tacitly with Jaouiche.

The paragraph in full where this expression occurs runs as follows:

London

وكذلك تصير نسبة ه الى و ح مجموعين اذا كانا مثلي و كنسبة ب جـ جـ ز مجموعين الى مثلي جـ ا ونسبة نصف ب جـ جـ ز الى نصف مثلي جـ ا. فأما نصف ب جـ جـ ز فهو جـ ط. وأما نصف مثلي جـ ا فهو جـ ا.

> And equally, the ratio of E to W, H, the two being added, since the two are twice the same as W, will be as the ratio of bg, gz, the two being added, to twice the same as ga (as well as) the ratio of half of bg, gz to half of twice the same as ga. As for half of bg, bz, this is gt. As for half of twice the same as ga, this is ga.[5]

Beirut

وكذلك نظير نسبة ه الى و ح مجموعين اذا كانا مثلي و كنسبة نصف ب جـ جـ ا مجموعين فهو جـ ط. واما نصف مثلي جـ ا فهو جـ ا

> And equally, the corresponding of ratio E to W, H,[6] the two being added, since the two are twice the same as W, (is) like the ratio of half of bg, ga, the two being added, this is gt. As for the half of twice the same as ga, this is ga.

The text in the two Arabic manuscripts differs in the third word. MS London uses «taṣīru» (will be, becomes, *vel sim.*). MS Beirut has «naẓīr» (same, like, corresponding, equivalent *vel sim.* or in correspondence to, in return of, for, *vel sim.*). Knorr correctly suggests considering the spelling in the Beirut ms. as a scribal mistake [1982, 59n4].

Knorr's second mistake in this passage comes in his translating «kadhālika» as "and for that (reason)" [1982, 59]. This translation of «kadhālika» is simply false from the semantic point of view. But, surprisingly, Knorr also misinterprets the content of this sentence. Step 5 of the London variant and the garbled step 4 of the Beirut manuscript are clearly not the consequence of their respective predecessors. In the London variant, step 5 is the result of the multiplication of both denominators W and ga by 2 and the argument that $2W = H + W$. It is here that Knorr's previous mistake concerning

5 I did not add the twice missing «majmūʿayn» (the two being added) after bg, gz in the second half of the passage, since such an elliptical mode of speaking was not uncommon in Arabic mathematical texts of the period. The corresponding passage from Beirut shows, however, that the term was not missing in an earlier stage of textual transmission.

6 *scil.* what corresponds to $E:(W+H)$, i.e., $(bg+gz):2ga$. The Arabic word «naẓīr» is, however, a scribal mistake for «taṣīru».

«waw» impedes a proper understanding of the garbled sentence in the Beirut manuscript. But, in trying to interpret the sentence as he has read it, Knorr makes a third simple mistake by explaining the obvious loss of steps in Beirut as due to *homoioteleuton* [1982, 59–60]. But in so doing, Knorr is forced to emend the Arabic *ga* to the English *gz*, i.e., "the half of $bg + gz$", which appears twice in quick succession:

> (Beirut) And for that (reason) the equivalent of the ratio of E to $W + H$, since they are equal, is as the ratio [of the half of $bg + gz$ to half of twice *ga*. As for] the half of $bg + gz$, it is *gt*, and as for the half of twice *ga*, it is *ga*. [1982, 59–60, Knorr's emphasis]

The Arabic text has, however, "the half of $bg + ga$" [1982, 194.13 (middle column)]. Thus, it is necessary to assume two losses. The first occurred between $bg+$ and *ga*. It consists of "*gz* to half of twice". This is not due to a *homoioteleuton*. The second loss occurred between *ga* and the description of an addition followed by "this is *gT*". It consists of "as for the half of *bg*, *bz*". This *is* the result of a *homoioteleuton*. The complete ancestral text of Beirut in this passage would then be almost identical to the one in London:

> Equally it is the case that $h : (W + H)$, since $(W + H) = 2W$, $= \frac{1}{2}(bg +$ $[gz) : \frac{1}{2}(2]ga)$; [as for $\frac{1}{2}(bg + gz)$,] this is gT.

When we ponder the significance of this restored passage within the entire part of the Beirut text for the question of which of the two variants is the younger, we find an additional argument for the Beirut manuscript's being the one which was modified. The point is that this step fits perfectly well into the slow procedure of the London text but is superfluous in the Beirut variant, since in the Beirut text step 3 has already produced the proportion $h : (H + W) = (bz + gz) : 2ga$, which is the purpose of step 5 in the London text and of the restored form of step 4 in the Beirut manuscript.

The mistakes which Knorr makes in interpreting these two sentences result, on the one hand, from his problems with Arabic and his extending the semantic range of Arabic words too broadly and, on the other, from his identification of supposedly Archimedean features in the Arabic text and his unwillingness to investigate alternative interpretations.

My conclusion that the Beirut manuscript is more recent than the London manuscript is reached without any preconceived notion about the character of the *Kitāb al-qarasṭūn*. It is also confirmed by the philological congruence of London's steps 7 and 8 and the latter's equivalence with Beirut's step 5. The agreement between these steps and their elementary content also contradicts Knorr's assumption that the first step in Beirut must, by virtue of its supposed boldness, represent the older textual stage. The elementary character of this part of the proof, where the author found it necessary to state that ½$(bg + bz) = gT$ after he had just stipulated this and that ½$(2ga) = ga$, does not support Knorr's characterizing the start of Beirut theorem 3 as a "daring" step. Rather, one must either explain away the later steps as an interpolation or abandon the idea that Beirut's first step is Archimedean and prior to London's elementary building up of the proportions needed. If one values a minimalist invasion into a transmitted text to "make it fit" some idea of correctness, one will have difficulty seeing any credible alternative to considering the text in the London copy as the earlier stage of the proof, with the constraint that it contains certain features that are clearly the result of copying, e.g., the disappearance of the stipulation that bz, gz or two other quantities need to be added.

This interpretation of London's priority is strengthened by the fact that the proof of this proposition in the *Liber karastonis* proceeds like that in the Beirut manuscript. The *Liber karastonis* is, as I have said, a clearly recognizable edition of the *Kitāb al-qarasṭūn* by Thābit b. Qurra [see Moody and Clagett 1952, 96, 98]. Accordingly, it seems more likely that Beirut's variant is a modification of the original text introduced by some copyist on the basis of Thābit's edited Arabic version of his compilation of Arabic translations of Greek fragments on the steelyard, a compilation which is only extant in Latin translation as *Liber karastonis*.

As I have already mentioned, Knorr's interpretation of the textual differences in the manuscripts of theorem 3 confirms what Rommevaux, Djebbar, and Vitrac have already learned from their analysis of his article on Heiberg's edition of Euclid's *Elements* about his view of the development of ancient and medieval mathematical texts [Rommevaux, Djebbar, and Vitrac 2001, 244–246]. His unwarranted interpretation of step 3 in the Beirut manuscript as a marker of an Archimedean ancestry reflects partly this belief in a "downhill" change in mathematical treatises. In the case of theorem 3, the reason for this change is purportedly due to editorial work:

> While the discrepancies between the manuscripts are minor, they are nevertheless instructive. Most important are the differences occasioned by the slightly different conceptions of the "composition" of the proportions. The Beirut ms. adopts a rather more sophisticated method, reminiscent of a technique peculiar to Archimedes. But the London ms. is here quite correct, despite the changes made. These changes are thus not inadvertent, but deliberate, the work of an editor who perceived a step in his text, assumed there without explicit justification, and so sought to clarify it by making minimal changes. It is evident here that the manuscript tradition of *K. Qar*. represented by the Beirut ms. must be

> prior to that represented by the London ms., since the former adopts a proof technique not likely to have been familiar to an Arabic editor. [Knorr 1982, 60]

The third and the last sentences in this quotation highlight Knorr's beliefs about Archimedes' exceptional methods and about the dependence of Arabic scholars on ancient Greek methods and theories. The quotation also confirms my analysis of the shortcomings in Knorr's reasoning and the fact that they are due to such prejudices. Note too that Knorr's reasoning is circular. He looked at this brief passage, decided that Beirut is more sophisticated, recognized the method in Archimedes' *Conoids and Spheroids*, which had not been translated into Arabic, and concluded that the Beirut variant must be the older textual level derived from an Archimedean text most likely unknown to an Arabic editor. My analysis of theorem 3 indicates, in contrast, that no Archimedean predecessor is warranted and that the "daring", "peculiar", "Archimedean" technique actually seems to have been introduced by a later copyist on the basis of Thābit's modifications of this proof in his revision of the *Kitāb al-qarasṭūn*.

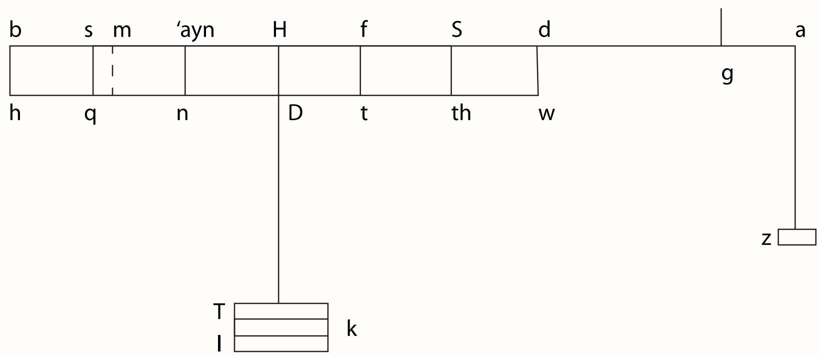

Figure 2. Theorem 4
(the weightless beam)

5.4 *Knorr on Theorems 4 and 5* Theorem 4 (see Figure 2) states that equilibrium is not disturbed if a uniformly distributed weight on an (immaterial) balance is replaced by an equal weight suspended from the middle point of that distributed weight. The proof is a sophisticated demonstration *ex contrario* using Archimedean-style techniques of proof [Jaouiche 1976, 156–165].[7]

[7] Figure 2 represents the first part of the proof. For the diagram of the second part, see Jaouiche 1976, 160–161.

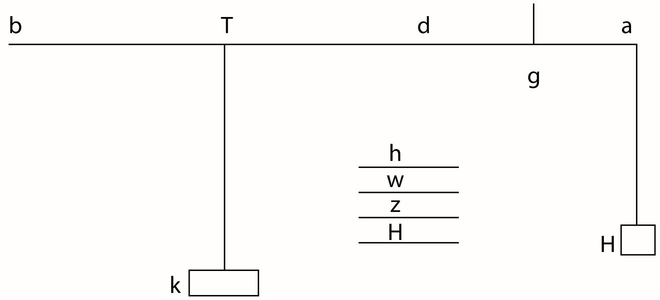

Figure 3. Theorem 5
(the material beam)

Theorem 5 (see Figure 3) treats a balance with a material beam. It determines, in the form of a problem, the weight that must be attached to the shorter end of a material beam that is not suspended from its middle point, in order to keep the beam in equilibrium. The proof explicitly refers to calculation techniques of practitioners. It also makes explicit use of Theorem 4 [Jaouiche 1976, 164–169].

Knorr's treatment of these two theorems represents a second example of the impact of his beliefs and prejudices on his description, analysis, and interpretation of this Arabic text. Knorr describes the first of these two proofs as standing "firmly in the tradition of the finest Archimedean convergence arguments" [1982, 53]. Three elements are particularly emphasized: the division of the beam into equal weights by parallel sectioning, the procedure of distributing an assembly of equal weights at equal intervals and then aggregating them at the midpoint of the whole interval, and, finally, the application of the Archimedean axiom in an indirect proof of the exhaustion type. He states: "we find among known Archimedean works several places which provide exact models for portions of *K. Qar*. VI [i.e., theorem 4—SB]" [1982, 53]. Indeed, as already pointed out by Jaouiche and acknowledged by Knorr, the proof of theorem 4 possesses clear similarities with methods and arguments made by Archimedes in his *Quadrature of the Parabola* and in *Plane Equilibria I* [see Jaouiche 1976, 94–101; Knorr 1982, 53n6]. Knorr does not take this immediately as a proof of Archimedes' authorship of this proof. He acknowledges that the two texts are not known to have been translated into Arabic. This was the belief commonly shared in 1982. He also discovers subtle differences between the extant Archimedean texts and their procedures and the proof of theorem 4 [1982, 54–55]. He concludes:

> All these observations thus compel us to recognize the author of the Arabic *K. Qr*. VI [i.e., theorem 4—SB] as a master of the application of formal geometric techniques in the analysis of mechanical theorems. [Knorr 1982, 55]

But Knorr sees himself faced with a major conundrum when comparing the proof of this theorem with that of theorem 5: "This impression, as we have seen, is yet utterly belied by the uninspired treatment of the Arabic *K. Qar*. VIII [i.e., theorem 5—SB] [1982, 55]." In his analysis as well as later comments, he labels the proof of theorem 5 with a string of very negative terms, the denigrating force of which he reinforces several times by adding qualifiers such as "lamentably uninspired" or "remarkably inept" [cf. 1982, 55, 31–33, 37, 53, 55].

I will give examples of the sort of language that Knorr chooses in evaluating theorems 4 and 5 by several quotes because they elucidate his prejudices and their impact on his analysis. I begin with a quotation concerning theorem 5:

> In the Arabic version the proof of this rule [i.e., the rule for the calculation of the counterweight—SB] is clumsy and confused.... This outline [of the proof—SB] only begins to suggest the labored line of this proof. Each step, however patent, is justified in detail. Yet the essential idea—that the weight F equals $W_a - W_b$ so that the extended portion $A - B$ can be replaced by F suspended at its midpoint—is virtually submerged in a flood of trivia. The wonder is that this proof, so inexpertly conceived, should still be quite correct and that the text has suffered not even a single scribal error.
>
> But if the essential line of the proof is here obscure, nevertheless the author's procedure is entirely clear. He has constructed the proof by working backward from the formula. This is in striking contrast to the approach in *L. Can*. and the Beirut appendix which derive the rule from the two or three essential aspects of the problem. It would thus appear that the author of the Arabic proof had before him a statement of the computational rule without its proof and set out to verify it by means of a safe, "brute-force" method. While such inelegance can be found in Thābit's work, one still begins to doubt that he could have been responsible for such an ill-framed method. [Knorr 1982, 33]

Knorr defends his claim that Thābit's mathematical methods were "inelegant" with the following comment:

> In his treatment of the quadrature of the parabola, for instance, Thābit plods through fifteen lemmas on arithmetic summations before coming to the properties of the parabola. The determination of the area takes five propositions.... So inelegant was Thābit's method that his grandson took up the problem for his family's name's sake to devise a better proof.... Ibrāhīm clears up the whole matter in four propositions. By a comparable method Archimedes had required two lemmas on the parabola, one on summation and four propositions. [Knorr 1982, 33n3]

Knorr is not bothered by the fact that despite its lengthiness Thābit's determination of the area of the parabola is of a much higher degree of difficulty and complexity than the proof of theorem 5, which indeed is simple but neither "inept" nor "ill-framed". Knorr's comparative claim of "inelegance" is misplaced. Given Knorr's own mathematical skills, his evaluations of Thābit's mathematical skills in his treatise on the quadrature of the parabola and of the character of the proof of theorem 5 are hardly accidental. They are either an intentional misrepresentation of the respective degrees of difficulty or mere sloppiness.

There are other cases of lack of care, at times serious ones, in Knorr's book: for instance, when he argues for the ancestry of the *Liber de canonio* and the appendix to the Beirut manuscript in relation to the *Kitāb al-qarasṭūn* on grounds of the content of the first two texts. I will return to this below. For now, I observe that Knorr's assessment of Thābit's mathematical capabilities is hardly compelling, since he does not analyze these capabilities on the basis of *all* of Thābit's extant texts, texts which use, like the proof of theorem 4, methods of exhaustion, partitions, and the axiom attributed to Eudoxus and Archimedes.

The same negative evaluation of the proof of theorem 5 (which he calls *K. Qar*. VIII) appears in Knorr's summary of his (equally false) analysis of the corresponding content in the *Liber karastonis*, the *Liber de canonio*, and the appendix to the Beirut manuscript:

> The rules proposed in *L. Can*. III and in *K. Qar*. VIII are in essence the same. Yet the Arabic proof of *L. Qar*. VII is remarkably inept, apparently the effort by an editor who knew the rule and by proceeding backward from the rule to the givens of the problem attempted in a most cumbersome way to compose its proof. By contrast, the proof adopted in the Latin version, *L. Kar*. VIII, is well framed, much in the manner of *L. Can*. [Knorr 1982, 37]

This denigrating language is repeated in Knorr's account of theorem 4:

> ...the Arabic version [of the proof of theorem 4—SB], for all its length and complexity, is as precise and as tightly conceived as it could be. It is firmly in the tradition of the finest Archimedean convergence arguments. While this has already been recognized by commentators on Thābit's work, the consequent paradoxes have not been appreciated. First, how could the author of the inept proof of the Arabic *K. Qar*. VIII [i.e., theorem 5—SB] have come up with such a profoundly accurate proof in VI [i.e., 4—SB]? Further, if that author were an Arabic scholar, perhaps Thābit, what could his technical model for the proof have been? [Knorr 1982, 53]

This last quotation does not merely show Knorr's contrasting evaluation of the proofs of theorems 4 and 5, it also expresses very clearly his beliefs about the fundamental differences between ancient Greek and medieval Arabic scholars. The author of the proof of theorem 5 is only conceived as an Arabic scholar. There is no reflection on how the analysis might change if a Greek author of this proof is assumed. But if the author of the proof of theorem 4 was perhaps an Arabic speaker, he must, in Knorr's view, have used an ancient Greek, technical model for his work. An independent invention of the proof by an author of the ninth century who wrote in Arabic is apparently unthinkable for Knorr. Again, for Knorr, that the author of the proof of theorem 4 might also have been the author of that of theorem 5 seems unthinkable. This is remarkable because at the end he ascribes the entire text to the young Archimedes. He can do so only by proposing that the Arabic text was derived from an incomplete Greek text and that the extant proof of theorem 5 was produced by its unknown Arabic translator.

This perception of such a substantial difference in quality between the two proofs leads Knorr to the following move, which, like many others of his ideas and arguments, is highly problematic for its patent dependence on biases and falsehoods:

> How are we to account for this radical discrepancy? We appear required to assume two authors for the Arabic *K. Qar.*: the one a geometer of amazing insight, who could draw freely from ancient technical works inaccessible to others in late antiquity and the Middle Ages; the other a competent but pedestrian commentator. While any number of ancient and medieval commentators known to us still were capable of translating a technical text and explaining its difficult points, so fitting the latter description, there is none to name, not even Thābit, who might fit the former. Only a century after Thābit do we come upon this sort of formal but creative geometer. But such a level of expertise seems unlikely as early as the 9th century, the first generation of Arabic scholarship in the formal tradition of geometry. This problem leads us to the view that what we have in the Arabic *K. Qar.* VI [i.e., theorem 4—SB] is the translation from a Greek mechanical writing. It is still a problem to determine who might have produced such a work, since the few writers from the later Hellenistic period who concern themselves with formal geometric methods manifest little originality. We return to this question later.
>
> But given such an ancient work, any one of the Arabic scholars we have named was fully able to produce an accurate translation. This accounts completely for the technical idiosyncrasies of the proof of *K. Qar.* VI [i.e., theorem 4—SB], and also for its length and complexity. An Arabic editor, by contrast, would certainly have striven to produce a simpler and shorter treatment of his own. As for the weaknesses we have detected in *K. Qar.* VIII [i.e., theorem 5—SB], we may recall that the most direct logical order for the treatment of the weighted beam is to establish the replacement theorem (*K. Qar.* VI) [i.e., theorem 4—SB]

> and then use it for the determination of the counterweight, as in *Liber de canonio*. What we have in *K. Qar*. VIII is merely an alternative expression of the result in *L. Can*. III. Presumably, the Greek manuscript containing the proof of the replacement theorem was defective, missing the theorems of *L. Can*., or possibly bearing them out of place—perhaps as an appendix, as they appear in the Beirut manuscript of the Arabic *K. Qar*., but still asserting the solution of the counterweight in its alternative form. This would appear as a corollary whose proof would be "obvious" in the context of the complete work, but far from clear within the defective manuscript. The Arabic translator would thus be required to provide his own proof for *K. Qar*. Apparently, the first attempt to produce such a proof, as we have it in the Arabic *K. Qar*. VIII, was correct, but far from perceptive. Faced with this Arabic edition of the *qarasṭūn*, Thābit set out to improve it, revising the theorem on the counterweight (*L. Kar*. VII and VIII) to good effect, but abridging the proof of the replacement theorem (*L. Kar*. VI) in a way that misconstrues a key feature of the argument. [Knorr 1982, 55–56]

This passage is saturated with highly problematic statements that are not grounded in a careful analysis of Arabic texts extant from the ninth century nor formulated in view of what was argued about the mathematical proficiency of scholars like Thābit b. Qurra in the 1970s by other historians of mathematics. It is not clear why Knorr believed that Thābit was incapable of applying formal geometric techniques, since he knew Thābit's text on the parabola [1982, 33]. Despite the fact that he considered this proof "inelegant" because it needed almost thrice as many lemmas and propositions as Archimedes, who needed twice as many as Thābit's grandson, this perceived lack of elegance does not entail that Thābit did not master the design and proof of a correct exhaustion method and the application of the so-called Eudoxus-Archimedes axiom. Seemingly characteristic for Knorr's working practice here is the fact that he did not consider Thābit's three other treatises which use exhaustion methods and the Eudoxus-Archimedes axiom, i.e., Thābit's works on parabolic bodies of revolution, on two lines that meet each other when they include an angle different from a right one, and on the trisection of an angle. Lack of familiarity with Arabic manuscripts does not excuse Knorr's unfriendly evaluation of Thābit's skills as a geometer. These texts namely were known to be extant in MS Paris, BnF, Arabe 2457 long before Knorr's study of the *Kitāb al-qarasṭūn*. He did not even have to work with this collection of texts in manuscript form since the three treatises had been published or studied in Suter 1916–1917, 16–17; al-Dabbagh 1966; and Hogendijk 1981. While Knorr may not have had access to al-Dabbagh's thesis in Russian, he was familiar with the two other works. Hence, at least one comment with an argument to the effect that the methods used by

Thābit in the three works do not warrant praise for their expertise but only disparagement as the work of a "competent but pedestrian commentator" would have been in order. But do the studies of other colleagues support such a negative evaluation of Thābit's treatises and his skills as a geometer?

An investigation of all published mathematical works of Thābit b. Qurra that focuses on his use of the Eudoxus-Archimedes axiom and the method of exhaustion makes it clear that Thābit fully deserves to be recognized for his talent as a geometer. Such an investigation also shows that he used the axiom in most cases and the method in all cases in a different manner than that in the *Kitāb al-qarasṭūn*.[8] The one case of identical usage of the axiom is found in postulate 5 of Archimedes' *On the Sphere and Cylinder* [Sabit ibn Korra 1984, 184; Heiberg and Stamatis 1972–1975, 9], a text which Thābit knew. In the other cases, Thābit uses the axiom in the form of theorem 1 in book 10 of Euclid's *Elements* [Heiberg and Stamatis 1972, 72.2–3]. In contrast to the equidistant partition of the thick segment mounted at a beam, Thābit partitions the diameter of the segment of a parabola or a paraboloid according to the sequence of odd numbers beginning from 1 [Sabit ibn Korra 1984, 184–185, 195].

As for the methods of exhaustion, Thābit uses in his other works variants of what Dijksterhuis has baptized the "method of approximation" [1956, 130–133: cf. Jaouiche 1976, 95]. In the *Kitāb al-qarasṭūn*, the method used is the variant that Dijksterhuis has labeled the "method of compression". Jaouiche [1976, 94–101, 135–137] has argued convincingly that it is the form also found in theorem 16 of Archimedes' *The Quadrature of the Parabola*. This Archimedean work was, however, not translated into Arabic, as far as we know.

This brief survey brings to light that the technical elements of the method of exhaustion in Thābit's published mathematical works differ from the method in the *Kitāb al-qarasṭūn*. Hence, one may conclude that there is no direct, immediate link between these two methods and the aforementioned texts. Thus, the conclusion to be drawn is that, given the existence of other partitioning methods in Thābit's works as well as another form of the Eudoxus-Archimedes axiom and his use of the method of approximation, Thābit was a competent geometer who was capable of working with advanced concepts and methods which are known to us, but perhaps

[8] Sabit ibn Korra 1984, 70, 149, 184–185, 195, 239–240, 334n7, 342n21, 343n32, 343–347n3, 345nn61–63, 348n85, 351–353n13, 353n15.

not to him, from Archimedes' works. If the information about a text on centers of gravity ascribed explicitly to Archimedes in works of the 10th century is correct, and so far there is no reason to doubt its veracity, we may suppose that Thābit may have known Archimedes' *Plane Equilibria*. But this would mean that he was capable of understanding Archimedean reasoning and techniques, and applying them to new problems. Rozenfel'd, in his evaluation of the first two works, goes beyond this: as he sees it, Thābit's work on paraboloids demonstrates that in comparison to Archimedes' *On Conoids and Spheroids* (theorem 22) Thābit "solved the more complicated problems of determining the volumes of cupola with straightened and indented cusps" [Sabit ibn Korra 1984, 344–345]. Since Knorr confirms his familiarity with Jushkevitch's *Les mathématiques arabes*, his downplaying of Thābit's geometrical skills without any discussion of this counter-evidence is inappropriate at best.

But did Thābit apply Archimedean methods independently to the problem discussed in theorem 4 as Jaouiche surmised? This seems unlikely, not because of any doubt about Thābit's mathematical abilities but because, as a philological analysis shows, there are a few Graecisms in this theorem. It is thus possible that Thābit worked with an unknown or as of yet undetermined Greek text on the balance in Arabic translation. The limited number of such Graecisms and the lack of mistakes in the Arabic text suggest that Thābit might also have edited this Arabic translation. Analysis of the Arabic theorem 4 does not, however, support Knorr's speculation that an Arabic translator was responsible for the length and complexity of the proof as well as those features perceived by Knorr as idiosyncrasies. Knorr's other speculation concerning the role of the anonymous Arabic translator in completing a fragmentary proof of theorem 4 and invention of the proof of the rule for the counterweight (theorem 5) is equally unfounded. The extant text of the *Kitāb al-qarasṭūn* does not provide any evidence for it. On the contrary: there are components in both proofs that connect them with each other and suggest that they derive from the work of a Greek scholar. In addition, the proof of theorem 5 shows traces of Thābit's interference, while a comparison of this proof with the corresponding theorems 7 and 8 in the *Liber karastonis* indicates that Thabit's willingness to alter the text of the translated Greek fragments was very limited when he compiled the *Kitāb al-qarasṭūn*.

Two elements connect the proofs of theorems 4 and 5:

(1) the repeated physical arguments in Aristotelian language, and
(2) the use of theorem 4 in the proof of theorem 5.

Knorr overlooked (1) and seems to deny (2) in the quotation given above [see p. 155], where he highlights the use of theorem 4 only for the *Liber de canonio*. In all likelihood, Thābit did not introduce the physical arguments into the proof since they disappear completely in the *Liber karastonis*. While this observation is of limited use in the case of theorem 4 because it is unclear who the author was of the proof in the extant form of the *Liber karastonis*, it applies to theorem 5 and its two corresponding theorems 7 and 8 in the *Liber karastonis*. In effect, Thābit strengthened the purely geometrical character of the treatment of the steelyard when he transformed the *Kitāb al-qarasṭūn* into the Arabic text of the *Liber karastonis*. In this transformation, he almost completely eliminated any physical argument. If this observation based on the comparison of the two texts reflects correctly Thābit's conceptual goals, then it is not very likely that he would have introduced the prominent physical arguments in the proofs of theorems 4 and 5. It is more plausible to assume that they were a genuine part of the Greek ancestor text of the two theorems. If this is a correct evaluation of the two theorems, then this shared peculiar feature of the two proofs speaks for one author of both. Whether this author was the inventor of the two proofs or an editor of two proofs invented by two different scholars cannot be decided in the absence of good evidence. The fact, however, that the goal of theorems 3–5 consists in determining the quantity of the counterweight needed for balancing a material beam implies rather one inventor than two of the two latter theorems.

Knorr's evaluation of the proof of theorem 5 rests on three claims that are evident in the various quotations that I have adduced:

(1) This proof is so simple that it cannot be part of an Archimedean heritage.
(2) The theorems found in the *Liber de canonio* were originally part of a single Greek text that also contained theorems 3 and 4. (This is another of Knorr's false conclusions, as I will show.)
(3) Someone else created the proof of theorem 5: an inept, pedantic scholar who encountered the rule for the counterweight without a proof. Although Knorr does not say this explicitly, his argumentation makes it clear that in his view the proofs of theorems 4 and 5 could not have had one and the same author. He oscillates between ascribing this proof to Thābit, the anonymous translator into Arabic, and some other unspecified Arabic author.

To address these claims, I will begin with the fact that Knorr, following Jaouiche [1976, 166–169], has misunderstood and misrepresents the simple proof of theorem 5.[9] This proof may be summarized as follows, with L signifying length and W or w, weight; z and h, auxiliary quantities; and H, the counterweight (see Figure 3 on p. 152):

> Material beam ab, suspended at point g, part $gb >$ part ag. Cut off ag from gb; this is bd; $L_{bd} \times W_{ab}$. Let the result be h. Let $h : L_{ab}$ be w; let $w \times L_{ab}$ be z; let $z : 2L_{ga}$ be H.
> I say: H is the magnitude of the heavy body that, if it is suspended in point a, balances the weight of the beam parallel to the horizon.
> $h = L_{bd} \times W_{ab}$ and also $h = L_{ab} \times w$
> [MS Mq 559, f 223v.12]: because $h : L_{ab} = w$
> $L_{bd} : L_{ab} = w : W_{ab}$,
> $w = W_{bd}$.
> But this is so because the thickness of the segment *bd* of beam *ab* together with all the beam is equal among each other and the substance of the whole is one. Hence, the heaviness of all of its parts (*ajzāʾ*) is equal among each other [i.e., the weight of each part is the same—SB].
> Also $z = w \times L_{ab} = H \times 2L_{ag}$
> and $z = w \times L_{ab} = H \times 2L_{ag}$ because $z : 2L_{ag} = H$.
> $H : w = L_{ab} : 2L_{ag}$
> bisect *bd* at point T.
> $L_{gT} = ½\, L_{ab}$, because $ag = gd$
> $L_{Tg} : L_{ag} = L_{ab} : 2L_{ag}$.
> But we had explained that $L_{ab} : 2L_{ag} = H : w$.
> $L_{Tg} : L_{ag} = H : w$.
> If we now imagine that H is a weight suspended at point a and if k is a heavy body with weight w suspended at point T and we imagine ab as a straight line or as a straight beam without weight, so that the heaviness of H counterbalances the heaviness of k, then the weight of beam ab is balanced parallel to the horizon given the preceding fundamental statement (*aṣl*) [i.e., theorem 4—SB].
> But w, as we explained, is the weight of segment *bd* of the beam, if we gave weight to the beam *ab*. The heaviness of the beam's segment *bd*, if we imagined it [i.e., the segment—SB] suspended in point T, so that it counterbalances the heaviness of H suspended at point a, will equilibrate the weight of beam *ab* parallel to the horizon.
> Likewise, we also imagine it [i.e., the segment *bd*—SB] spread out and expanded in evenness and connectedness in its attachment between the two points b, d. It is clear that segment *gd*, (which is) also part of the beam, counterbalances segment *ag* of it because the two are equal to each other in length and thickness and substance and in sum are equal to each other in weight.

[9] See MS Cracov, Jagielonska University Library, Mq 559, ff. 223r.10–224r.15.

> The entire part gb thus counterbalances the beam ag and the weight H. Hence, the weight of beam ab will be parallel to the horizon. QED

This summary contradicts clearly Knorr's claim that

> (e)ach step, however patent, is justified in detail. Yet the essential idea—that the weight F [i.e., w—SB] equals $W_a - W_b$ [i.e., W_{bd} —SB] so that the extended portion $A - B$ [i.e., bd —SB] can be replaced by F suspended at its midpoint—is virtually submerged in a flood of trivia. [Knorr 1982, 33]

There is no "flood of trivia" but merely two auxiliary quantities h and z, which structure the proof neatly and thus look like didactic devices, and three explanatory statements that repeat things as given in the exemplum, one of which is repeated once. Neither should one call these very short justifications of the type "because $x = y$" detailed; nor is the "essential idea" "obscured", as Knorr would have it, since it is explicitly stated in the passage "But w, as we explained, is...". Having wavered above in my description of Knorr's evaluation of Thābit's mathematical skills as either sloppiness or intentional denigration, I think that his excessively negative evaluation of the proof of theorem 5 is intentionally misleading. The proof of theorem 5 is simple, no doubt, except for two points—the use of theorem 4, which at least the inventor of the proof fully understood and who is thus not rightly described "inept", and the use of physical theory in order to make the transition from the immaterial beam as proved in theorem 4 to the material beam discussed in theorem 5. But even in its simple parts, theorem 5 is well structured in that it uses the didactic device of auxiliary quantities and is to the point.

Knorr's strong condemnation of the proof of theorem 5 was predicated on a misunderstanding of several of its elements. He did not recognize the didactic device, which explains an apparently absurd feature in the formulation of the rule, namely, the immediate sequence of a multiplication and a division by the same quantity. Neither did he see that the proof's claim "But we had explained that $L_{ab} : 2L_{ag} = H : w$" is actually false. The proof does not explain why the factor $L_{ab} : 2L_{ag}$ is correct for obtaining H from w. It merely justifies the product $w \times L_{ab} = H \times 2L_{ag}$ with a reference to the labels provided in the exemplum. This lack of a true justification of the definition of the counterweight is something Thābit apparently chose not to correct in compiling the various fragments that constitute the *Kitāb al-qarasṭūn*. But he remedied this mistake later by introducing a new theorem in his text extant today as the *Liber karastonis*, namely, theorem 7. There are other elements in this proof that Knorr misunderstood but I will abstain from discussing them too in order to focus more closely on Knorr's claims about the dependence of the *Kitāb al-qarasṭūn* on the *Liber de canonio*.

6. On the relation of the *Kitāb al-qarasṭūn* and the *Liber de canonio*

Knorr's claim that theorem 5 (rule and proof) is derived from theorem 3 in the *Liber de canonio* is also false. First, the forms of the rule as expressed in these two texts as well as in the appendix to the Beirut manuscript do not agree, contrary to what Knorr suggests [see p. 163, below]. Second, theorem 3 of the *Liber de canonio* is proved with explicit references to axioms and theorems in Euclid's *Elements* because it works with similar triangles. It is, thus, on a higher level of mathematical complexity than the proof of theorem 5. All physical arguments of the proof of theorem 5 are missing in theorem 3 of the *Liber de canonio* as is the didactic device of theorem 5. Third, the *Liber de canonio* splits the rule proved as a package in theorem 5 into two parts. Theorem 1 proves the proportion for the weight of the material segment *bd*, while theorem 3 deals with the proof of the proportion for the counterweight. Moreover, theorem 3 justifies in its first part this proportion with the help of similar triangles and so avoids, or perhaps repairs, the mistake of theorem 5 of the *Kitāb al-qarasṭūn*. Hence, it makes no sense to assume that theorem 5 (rule and proof) was designed on the basis of the *Liber de canonio*.

It is difficult to understand what motivated Knorr to make such an ill-considered claim, if not his desire to understand these texts as remnants of one and the same ancient Greek source composed by the young Archimedes. That this is not another instance of sloppiness can be seen in the manner in which Knorr rewrites the rule for the counterweight according to theorem 5, the appendix to the Beirut manuscript, and the *Liber de canonio*. The resulting statements are equivalent to, but different from, their original forms in the three texts.

Knorr's translation of the prescription for the counterweight

Be the material beam divided into two segments a and b. Then L and W with their indices denote the length and weight of the respective segments. W without an index labels the counterweight.

Kitāb al-qarasṭūn

$W = (L_a - L_b) \times (W_a + W_b) : (L_a + L_b) \times (L_a + L_b) : 2L_b$ [1982, 31]

Addition (*ziyāda*) after the text in MS Beirut

$W = ½ (L_a + L_b) \times (W_a - W_b) : L_b$ [1982 18]

Liber de canonio

$W = (W_a - W_b) \times (L_a + L_b) : 2L_b$ [1982 18]

I will now give a literal presentation of this rule in the three texts. The letters "N" and "P" stand for "numerus" and "productus", both belonging to the set of Arabisms of the *Liber de canonio*.

Prescription of the counterweight as expressed in the three source texts

Kitāb al-qarasṭūn
$W = (L_a - L_b) \times W_{ab} : L_{ab} \times L_{ab} : 2L_b$ [Jaouiche 1976, 166–167]
ziyāda, MS Beirut
$W = ½\, L_{ab} \times W_{a-b} : L_b$ [Knorr 1982, 160]
Liber de canonio
$(W_a - W_b) \times N\{L_{ab}\} = P$ and $P : N\{L_{2b}\} = N\{W\}$ [Moody and Clagett 1952, 68–69]

The comparison between these two sets of formulas shows that the formulations in the three texts contain no additions due to their different labelling of the various parts of the steelyard. Furthermore, it shows that the *Kitāb al-qarasṭūn* is recognizably distant from the two variants in the *ziyāda* to the Beirut version and the *Liber de canonio*, while the latter two show structural similarities without being identical. Knorr's idea that the variant in the *Kitāb al-qarasṭūn* was derived from an Arabic version of the *Liber de canonio* is thus plainly unwarranted.

7. Knorr's lack of precision

Cases of a clear lack of care in Knorr's analysis appear always when he speaks of literal coincidence between parts of different texts. The example that I have chosen to back up this judgment is closely connected to the discussion of the proof of theorem 5 and its relationship to the *Liber de canonio*. It deals with the relation between this Latin text and the Arabic *ziyāda* to the *Kitāb al-qarasṭūn* in the Beirut manuscript. Knorr suggested that the *ziyāda* was derived from a larger Greek text, a text which, according to him, was also the source of the theorems found in the *Liber de canonio*. His first argument rests on a putative "literal coincidence" of the enunciations of theorems 1–3 of the *Liber de canonio* and the last two theorems and the corollary to proposition 4 (3b) of the *ziyāda* [1982, 15–17]. This claim is, however, far too grand. While the enunciations describe the same content and so do indeed possess shared features, they are not in literal agreement.

Knorr's second argument states that "(w)hile the proofs do not agree literally as texts, their arguments are the same, step for step in the same order" [1982,15–16]. This too is too grand a claim, since it obliterates important differences between the proofs. In my discussion of Knorr's concept of "literal coincidence", I will consider only the enunciations of the different theorems since this is Knorr's point of reference.

7.1 *Relation of the* Liber de canonio *and the* ziyāda

Study 1

Theorem 1, *Liber de canonio*

> *Si fuerit canonium symmetrum magnitudine, et substantie eiusdem, et dividatur in duas partes inequales et suspendatur in termino minoris portionis pondus quod faciat canonium parallelum epipedo orizontis, proportio ponderis illius ad superhabundatiam ponderis maioris portionis canonii ad minorem, est sicut proportio longitudinis totius canonii ad duplam longitudinis minoris portionis.* [Moody and Clagett 1952, 64]
>
> If there is a beam of uniform magnitude and of the same substance, and if it is divided into two unequal parts, and if at the end of the shorter segment there is suspended a weight which holds the beam parallel to the plane of the horizon, then the ratio of that weight, to the excess of the weight of the longer segment of the beam over the weight of the shorter segment, is as the ratio of the length of the whole beam to twice the length of the shorter segment. [Moody and Clagett 1952, 65][10]

Theorem 3 of the *ziyāda*, Beirut

> اذا كان عمود متساوي الغلظ متشابه الجوهر وقسم بقسمين مختلفين وعلق من طرف القسم الاقصر ثقل فاعتدل العمود على موازاة الافق فانه تكون نسبته الى ثقل فضل القسم الاطول على القسم الاقصر كنسبة نصف طول العمود جميعه الى طول القسم الاقصر.
>
> [Knorr 1982, 146]
>
> If there is a beam, (which is) equal in itself in thickness, equal in itself in substance, and it is partitioned in two different parts and a weight is suspended at the end of the shorter part so that it balances the beam in parallel to the horizon, then its ratio to the weight of the surplus of the longer part over the shorter part is like the ratio of half of the length of the beam in its entirety to the length of the shorter part.

These two enunciations represent the same content. They are closely related but not identical. They differ in their statement of the second part of the proportion and they show some differences in language. The *ziyāda* does not speak of magnitude but thickness. It uses a second term, « mutashābih », for

[10] Moody and Clagett translate the Latin "minor" and "maior" by "shorter" and "longer". A literal translation would be "smaller" and "greater" or "larger".

describing the property of the substance, which is not present in the Latin text. Instead of saying that the weight suspended at the end of the shorter part of the beam makes the beam parallel to the place of the horizon, it prescribes that it is of such a kind that the beam balances itself in parallel to the horizon. In view of my earlier argument on the Arabisms in the second part of the *Liber de canonio*, let me point out here that it is only the Arabic text that speaks of *shorter* and *longer* parts of the beam. The Latin text speaks of *smaller* and *greater* or *larger* parts. Unfortunately, in his translation, Knorr obliterates this important terminological difference. He chose to translate "shorter" by "smaller" and "longer" by "greater", thus following Moody and Clagett [1982, 139, 141]. He does the same in the remaining theorems [cf. 1982, 143, 147, 149, 153, 155, 159, 161].

The two terms «mutasāwin» and «mutashābih» used for describing the quality of the beam in terms of thickness and matter mean both "equal" and "similar" in Arabic. There is a clear preference in Arabic mathematical text for using the first for equal and the second for similar. Thus, Knorr translated them in this manner [1982, 139]. In the given context, it is clear though that similarity is not meant literally but in the sense of having the same property. This ambiguity reflects the use of «ἴσος» and «ὁμοίος» for respective terms in Greek. It is, however, useful to remember that the *Kitāb al-qarasṭūn* does not use «mutashābih» or its verb at all in the sense meant here, i.e., for equality or evenness, but exclusively in the sense of "similar" [Jaouiche 1976, 146, 148]. Neither does the *Liber karastonis* [see Moody and Clagett 1952, 108, 110, 112].

Study 2

Theorem 2, *Liber de canonio*

Si fuerit proportio ponderis in termino minoris portionis suspensi, ad superhabundantiam ponderis maioris portionis ad minorem, sicut proportio longitudinis totius canonii ad duplam longitudinis minoris portionis, erit canonium parallelum epipedo orizontis. [Moody and Clagett 1952, 66]

If the ratio of the weight suspended at the end of the shorter segment, to the excess of the weight of the longer segment to the weight of the shorter one, is as the ratio of the length of the whole beam to twice the length of the shorter arm, then the beam will hold parallel to the plane of the horizon. [Moody and Clagett 1952, 67][11]

[11] The second mention of weight in the second term of the proportion is supplied by Moody and Clagett. The Latin text does not have it.

Theorem 4, *ziyāda*, Beirut

وجعلت نسبة الثقل الى ثقل فضل القسم الاطول على ثقل القسم الاقصر كنسبة نصف طول العمود كله الى طول القسم الاقصر فان العمود يعتدل على موازاة الافق.

[Knorr 1982, 154]

If there is a beam, (which is) equal in itself in thickness, equal in itself in substance and partitioned in two different parts and (if) a weight is suspended at the end of the shorter part and the ratio of the weight to the weight of the surplus of the longer part over the weight of the shorter part is made like the ratio of half of the length of all of the beam to the length of the shorter part, then the beam equilibrates itself in parallel to the horizon.

Again, the content of both theorems is the same and the two enunciations are similar but not identical. Their difference is greater than in the previous case because the *Liber de canonio* does not repeat the description of the properties of the beam and the suspended weight, and so has to integrate the latter into the description of the proportion. It differs from the *ziyāda* also in regard to the placement of the term "weight" in the description of the second term of the proportion. The *Liber de canonio* uses the term only once, that is, after the surplus and before the longer part. The *ziyāda* uses it twice, once before the surplus and once before the shorter part. While the formulation in the *Liber de canonio* is imprecise but comprehensible, the formulation of the *ziyāda* is comprehensible but false. It is most likely the result of a scribal error as may be the sloppy form of the *Liber de canonio*. Again, it is only the Arabic text that uses "shorter" and "longer", while the Latin text works with "smaller" and "greater" or "larger".

Study 3

Theorem 3, *Liber de canonio*

Atque ex hoc manifestum est, quoniam si fuerit canonium symmetrum in magnitudine et substantie eiusdem, notum longitudine et pondere, et dividatur in duas partes inequales datas, tamen possible est nobis invenire pondus quod, cum suspensum fuerit a termino minoris portionis, faciet canonium parallelum epipedo orizontis. [Moody and Clagett 1952, 68]

But from this it is evident that if there is a beam, symmetrical in magnitude and of uniform substance, whose length and weight are known, and which is divided into two given unequal parts, it is still possible for us to find the weight which, when suspended from the end of the shorter segment, will make the beam hold parallel to the plane of the horizon. [Moody and Clagett 1952, 69]

Porism, *ziyāda*, Beirut

وهنالك استبان انه اذا كان عمود متساوي الغلظ متشابه الجوهر يقسم بقسمين مختلفين وننقص من القسم الاطول مثل القسم الاقصر ويضرب نصف طول العمود في وزن ثقل فضل القسم الاطول على القسم الاقصر وقسم ما اجتمع على طول القسم الاقصر فان ما خرج من القسمة يكون ثقلا اذا علق بنقطة طرف الاقصر اعتدل العمود على موازاة الافق.

[Knorr 1982, 160]

And herewith it is clarified that if a beam, (which is) equal in itself in thickness, equal in itself in substance, is partitioned in two different parts and we take away from the longer part the same as the shorter part and half of the length of the beam is multiplied by the weight («wazn») of the weight («thiql») of the surplus of the longer part over the shorter part and that what results is divided by the length of the shorter part, then that what comes out from the division is a weight («thiql») [that], if it is suspended in the point at the end of the shorter [part], balances the beam in parallel to the horizon.[12]

The main difference between these two propositions is caused by their different format. The Latin statement presents the task in the form of a problem. The prescription of how to determine the weight sought follows afterwards. The Arabic statement is formulated as a porism and so consists of the prescription of how to find this weight. This difference signals clearly that the Latin text belongs in genetic terms to a later developmental stage than the Arabic text. In order to evaluate the overall relationship in language, we must consider the statement of the prescription as given in the *Liber de canonio*.

Statement of theorem 3 of the *Liber de canonio*

Hoc est, ut sumamus superhabundantium ponderis maioris portionis ad minorem, et multiplicemus eam in numerum longitudinis totius canonii, et productum dividamus per numerum longitudinis duple minoris portionis, et quod exierit est numerus ponderis quod, suspensum a termino minoris portionis, faciet canonium parallelum epipedo orizontis. [Moody and Clagett 1952, 68]

The method is to take the excess of the weight of the longer segment over that of the shorter, and to multiply this by the number representing the length of the whole beam, and then to divide this product by the number representing twice the length of the shorter arm; and what results is the number representing the

[12] The Arabic text printed by Knorr has a few minor, probably scribal, errors: «wazn» before «thiql», the shift in tense and person between the verbs.

> weight which, if suspended from the end of the shorter arm, will make the beam hold parallel to the plane of the horizon. [Moody and Clagett 1952, 69]

Four points come to light when comparing these two passages from theorem 3 in the *Liber de canonio* with the Arabic porism.

(1) There is the small difference of the numerical factor used by the two (2 in the denominator *versus* ½ in the numerator) and the order of the two terms at the beginning of the prescription is changed.
(2) Of more substance is the addition of “numerus” in the Latin text, since this is conceptually improper.
(3) The repeated use of “symmetrum in magnitudine” (commensurable in magnitude where size is at issue) in the Latin text cannot be found in the Arabic version, which regularly uses « mutasāwī l-ghilaẓ » (equal/even in thickness). In contrast, the use of « mutashābih al-jawhar » (literally: equal/similar in substance) is not precisely reflected in the Latin formulation of the example but can be found elsewhere in the *Liber de canonio*.
(4) There is the probably insignificant difference between the two texts regarding the standard concept of the plane of the horizon *versus* the horizon *simpliciter* and the perhaps slightly more important difference in the verb used for expressing the parallelism, i.e., “facere” as opposed to « iʿtadala ».

These four points appear to be of minor relevance when compared to the philological coincidence of the two texts which is clearly visible despite their formulaic differences as a problem and a porism. But the differences listed confirm what can be easily discovered by comparing the proofs, namely, that none of the two texts is a translation of the other.

In sum, in the light of our studies of the relations between the enunciations of the theorems in the *Liber de canonio* and the *ziyāda* of the Beirut manuscript, Knorr’s claim of their “literal coincidence” is clearly too strong or, as one says in German, “The wish was the father of the thought.” I accordingly regard Knorr’s claim as an instance of a lack of care in carrying out his analyses.

8. Instead of conclusions

Here is not the place to identify in further detail the steps that contributed to Knorr’s misconstruals time and again of the *Kitāb al-qarasṭūn*, its scholia, the *ziyāda* in the Beirut manuscript, and the *Liber de canonio* as bits and pieces of a single, coherent, ancient Greek text on the steelyard, whose author was, in Knorr’s view, none other than the young Archimedes. Still, I hope that I have illuminated the dangers that arise from interpreting *any* text, whether highly technical or more narrative, without carefully exploring its content as well as its various contexts. Additional difficulties impeding an analysis that does justice to the extant textual and other material arise from the biases that typically guide our own perceptions of language, images, and values. A third type of problem results from the limitations of our own philological, scientific, mathematical, philosophical, and historical skills and knowledge. Humility is always the better path to truth than hubris in the case of a mathematical text or to a well-balanced evaluation in the case of any other type of text, because, as we all know, even here pride comes before a fall. In short, self-critical control is not only needed in regard to our beliefs and convictions but also towards our own scholarly abilities.

In consequence, for example, to date there is available no truly micro-historical study of any subject matter in the history of the sciences in any Islamicate society. Today, many historians of science or philosophy in Islamicate societies feel compelled to situate their topics much more explicitly in a chain of predecessors or even in a chain of predecessors and successors. This practice applies primarily to scholars and topics from the classical period of Islamicate societies, i.e., to the time before *circa* 1200. The price paid for this is akin in principle to that paid by Knorr, where his attention to the contemporaries of a scholar is visibly less than that to the scholar's predecessors and successors. This is not to say that it cannot be worthwhile to study the place of a scholar in some chain of ideas. But this always entails a substantial loss of insight into the intellectual environment of the scholar studied if such contextual considerations are not also taken into account.

Such work has to face a series of immensely more difficult questions:

- How can we recover information about the intentions, purposes, goals, or values of historical scholars?
- How can we unveil or penetrate the views that different groups of people held on the sciences of their times and then proceed to determine whether these groups engaged one another in a supportive or hostile manner and what that meant for other groups in their environment or their society at large?
- How can we move from such local studies to understanding the regional or even the bigger picture?

But these will have to wait, at least in historical studies of science in Islamicate societies, until we have a series of well-researched micro-histories of a broad range of topics at our disposal and learn which questions we need to ask beyond the clarification of authorship and which methods and theoretical fundamentals we need to develop. The one central point that my analysis of Knorr's book as well as the group of texts that he had studied brings to the fore, in addition to those three which I have discussed (issues of concepts, methodologies, and methods; issues of expertise and its lack; issues of beliefs and assumptions), consists in the insight that the narrowly defined set of questions that Knorr studied in order to produce a history of the steelyard in antiquity and the Middle Ages does not suffice for reaching this goal. At the very best, it is a preliminary preparation of the ground from which to start. Many other questions need to be raised and serious efforts made to answer them. Among them, contextual issues will be of primary importance.

In the present case, these contextual issues will concern the transfer and potential transformation of the material steelyard from Byzantine times to the Umayyad and then to the Abbasid dynasties. Unfortunately, only two specimens seem to be known from either of these two periods. More material is available for weights. Hence, we need ideas about how to link the study of weights and their specific properties to the study of the steelyard. We will also require a better knowledge of the development of long-distance trade in the Abbasid Empire, the emergence of merchant communities and their impact on Abbasid trade policies as well as scholarly patronage. We need to try, following studies in other areas of the history of science, medicine, or technology in other pre-modern societies, to understand what issues of authorship meant to scholars in the ninth or any other century and which functions the category or title of author had for the production of texts, the teaching of the sciences, or the pursuit of a successful career in the administration, at an educational institution, or at court.

There are many more questions that we need to address in studying scientific and other texts. This article certainly is not the place to formulate more of them, let alone most of them, except for one, since the *Kitāb al-qarasṭūn* contains one explicit statement pointing in this direction: How did scholars of the Greco-Arabic sciences and the practitioners of more practical domains of knowledge such as surveyors or calculators relate to each other and communicate with one another? Was there a spillover between these two spheres of knowledge? It seems to me that we are now poised not only to raise these kinds of questions but also to revise our concepts of what knowledge meant in the classical period of Islamicate societies and thus to question any facile belief in the dominance of Greco-Arabic theoretical knowledge over all other forms of knowledge.

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