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Uprooting Byzantium. Ninth-Century Byzantine Books and the Graeco-Arabic Translation Movement*

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1. Rootless

This study examines the available historiographic approaches to the transition in Byzantine history that occurred in the period running from the middle of the seventh century to the early ninth century. This is the transition from the so-called—and poorly documented—"dark age" to the better-documented "Macedonian Renaissance" or (after Paul Lemerle) "premier humanisme Byzantin".[1] This period is characterised by two sharply polar phenomena: the massive adoption of a minuscule script in library production, which replaced the majuscule script,[2] and the second phase of the Iconoclast Controversy. A major outcome of the period has been the production of earliest secular manuscripts written in minuscule script.

Two accounts have been elaborated to explain the Macedonian Renaissance. We shall call them the "internalist" and "externalist" approaches.

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[1] On the limits of the notion of "renaissance" as applied to Byzantine literature, see Agapitos 2020, 5 and 7. See also Spieser 2017 for art history.

[2] For the introduction of the minuscule script, see most recently Ronconi 2021.

The internalist approach has been set forth in its fullest form in Paul Lemerle's *Le premier humanisme byzantin*.[3] According to this approach, changes in a given civilisation are driven by internal dynamics alone. Lemerle adopts a twofold strategy. He suggests that the discontinuity between the Dark Age and the Macedonian Renaissance is not so sharp. He selects one of the above-mentioned concomitant phenomena to explain the perceivedly renewed interest in secular culture. This phenomenon is the Iconoclast Controversy, which prompted otherwise torpid minds to search and interpret texts that might support either party.[4] Two key characters from both parties of the iconoclast controversy are selected, namely, the patriarchs Tarasios (died 806) and Nikephoros (died 828) among the Iconodules, and John the Grammarian (died before 867) and Leo the Mathematician (died after 869) among the Iconoclasts, whose cultural exploits—in particular those of Leo the Mathematician—are duly highlighted.[5] The other phenomenon—adopting the "new" minuscule script in book production—is readily explained as a consequence of the regain of interest in books and literacy. On close look, the internalist explanation advocated by Lemerle has an obvious drawback: his argument does not explain the revival of *profane* culture more than simply stating it as a fact.

By contrast, the externalist approach postulates the existence of a catalyst, and accordingly identifies the interaction with a nearby civilization as the cause of substantial changes in society and culture. For Byzantium, this can only be early medieval Islam:[6] the ninth-century "Byzantine Renaissance" resulted from the impact of the scholarly

[3] Lemerle 1971.

[4] See also Mango 1975, 44–45, and Treadgold 1979, 1253–1254.

[5] For Tarasios and Nikephoros, see Lemerle 1971, 128–135; for John the Grammarian and his nephew Leo the Mathematician, see Lemerle 1971, 135–146 and 148–176, respectively. Leo, however, changed sides as soon as the circumstances required it.

[6] Lemerle dismissed this view, which he called "le relais syro-arabe", at the very beginning of his *Le premiere humanisme Byzantin*: Lemerle 1971, 22–42 ("L'hypothèse du relais syro-arabe"). This chapter follows an introductory chapter (pages 9–21) that presents the "discontinuity" ("Interruption de la culture hellénique en Occident"). The English translation (cited among others in Gutas 1998, 178 n. 49) renders the crucial term "relais" with a colourless "Link".

activity in Arabic-speaking countries on the Byzantine intellectuals. When reading and copying Greek scientific and philosophical works, the Byzantines were merely reacting to an impulse coming from outside, for they wished to emulate the progress achieved in the nearby Caliphate—or maybe they just wanted to sell them the books.

This explanation has been lingering for more than two centuries, with subtle variations as to its exact formulation.[7] However, the externalist approach has been frequently supported by anecdotal material and by such poor an argument as can, at best, undermine it rather than confirm it. Bertrand Hemmerdinger offers an example of the tendency to transform anecdotes into argument. In a short article published in 1962, he argued in favour of the Arab roots of the first phase of Byzantine humanism on the grounds of a specific historical circumstance: an Arabic scientific embassy in Byzantium. This embassy prompted Emperor Leo V the Armenian (died 820) to gather books from all over the empire's provinces. Hemmerdinger writes:[8]

> Ce rapprochement [*scil*. linking the Arab scientific mission that Hemmerdinger has pointed out with the fact that 'à partir du 20 mai 814 (E. de Muralt, Essai de chron. byz., 1855), Jean le Grammairien réunit à Constantinople, sur l'ordre de l'empereur Léon l'Arménien, tous les manuscrits anciens qui se trouvaient dans l'empire'] permet

[7] The fact that this explanation had a character of *vulgata* is confirmed by what we read in Vogel 1967, 269 (our underlining): Theophilus (ruled 829–842) "was also anxious to make Byzantium the leading cultural force in the Orient, impelled in this ambition, perhaps, by thoughts of rivaling Baghdad where the Caliph al-Ma'mlūl (813–33), like his father before him, was seriously concerned to make translations of the Greek works preserved in Syrian monasteries or purchased from Constantinople available to Arab readers". We shall identify the source of this view at the end of the present paper.

[8] Hemmerdinger 1962, 67, whose finding is apparently forgotten by the author himself in the subsequent Hemmerdinger 1964. In this paper, Hemmerdinger smooths out the dark-age discontinuity: using Irigoin's 1959 paper (see below), he highlights the sizeable extent of the book production in coptic uncial, a script used in the Middle East, he recalls again John the Grammarian collecting books upon order of Leo V, he points out that Ḥunain Ibn Isḥāq had no problems in finding Greek books during his *iter Byzantinum* in 823–825, and he concludes "En 823-825, les manuscrits philosophiques abondaient à Constantinople" (p. 133).

> de dater la mission scientifique arabe avec la plus grande précision (avant et après le 20 mai 814). Cette mission faisait connaitre à Léon l'Arménien l'intérêt des Arabes pour la science grecque antique, et, bien qu'il fût lui-même un ignorant, devait lui inspirer le désir de ne pas laisser les rivaux de l'empire byzantin jouir sans partage de l'héritage intellectuel de ses grands ancêtres.

Several such anecdotes are staged in this period, both from the Byzantine and from the Arab side. They are surely important for reconstructing the history of the relations between the Byzantines and the Caliphate. These episodes may not be fictitious, but they must be taken cautiously, especially because an ideological bias may easily condition their interpretation.[9]

For this reason, Dimitri Gutas' 1998 reassessment of the "externalist" account was a welcome contribution to the debate. Gutas did not simply endorse the account, but strengthened it through data taken from the Byzantine manuscript production of the relevant period. Gutas claimed that the existence of most (if not all) scientific and philosophical manuscripts produced between 800 and 850 could be explained in socio-economical terms, either as a Byzantine response to the Arabic translations or as the result of the demand of manuscripts by the Caliphate, or both. It is not fortuitous, claims Gutas, that these Byzantine manuscripts contain exactly the same secular works that were translated earlier in Arabic. Gutas crucially exemplifies his view through a comparative list of works contained in Byzantine secular manuscripts and their Arabic translations. According to Gutas, the result shows a perfect correlation between the two and proves the validity of the externalist approach.

Discussing Gutas' reassessment after so many years may seem odd. Yet, as we reviewed the literature on the subject, we realised that his thesis has gained tacit acceptance among both Byzantinists and Arabists. Hoping to prompt further studies on the Byzantine-Arabs cross-cultural

[9] On Byzantine-Arab diplomacy as a vector for exchanging knowledge and books, see Eche 1967; Signes Codoñer 1996; Magdalino 1998; Gutas 1998, 83–95; Koutrakou 2007; Droucourt 2009; Mavroudi 2012 and 2015, 39–42.

relationship, the present paper tests for the first time Gutas' data. We shall show in Section 2 that Gutas' account is not corroborated by the data he sets out; our analysis of these data also shows that they have been collected inaccurately and interpreted tendentiously. Section 3 proposes a critical reassessment of the current narrative on the "Macedonian Renaissance". In Section 4, we shall uncover the historical and ideological bias lying behind the externalist approach advocated by Gutas and others before him.

2. Gutas' Thesis

Before tackling Gutas' thesis, we clarify our assumptions and our argumentative strategy. We first point out that the so-called "Macedonian Renaissance" is, to some extent, a historiographic figment that originates in the scant documentary record of the preceding period. The mere and inescapable fact that the documentary record is fragmentary entails that any "explanation" of this "renaissance" cannot but be conjectural. In such cases, what makes the difference between different historical accounts or explanations is less their adherence to historical reality—which cannot be checked in any way—than the quality of their argument: what is required is sound logic, a firm knowledge of primary sources, faithfulness to the proposals coming from other scholars, and an accurate and unbiased presentation of the evidence.

In light of the fragmentary nature of the evidence, refuting Gutas' account by proposing an alternative scenario would not do, for such a scenario would inevitably retain its status of conjecture and would be easily impugned by its opponents. Therefore, we shall not *refute* Gutas' thesis (which may well be partly or entirely *true* as far as historical reality is concerned) but *deconstruct* it by showing that it is grounded on an appraisal of the available evidence that is both inaccurate and deceitful. To this end, we will endorse one of the basic principles that regulated ancient dialectical debates: conceding as much as possible to the opponent. Accordingly, we shall deconstruct Gutas' thesis in the said way (1) by making exclusive use of documents and literature that were likely to be available to Gutas in 1998 and (2) by accepting the main assumptions of his thesis.

Let us now have a close look at Gutas' account. His 9-page-plus-one-table argument runs as follows.[10] A statement of the problem (175–176) is followed by a summary of what the "[s]tudents of Byzantium" have said about the period under scrutiny, namely, the time of the iconoclast controversy and of the introduction of the minuscule script: these are the so-called "dark ages" of Byzantium (176–178). This summary stresses two major transformations in the said period.

First, Gutas addresses the introduction of the minuscule script. In his view, the "uncial" script is "cumbersome" and, accordingly, uncial manuscripts are "more expensive than minuscule" manuscripts; parchment is more expensive than papyrus, whose "usefulness [outside of Egypt] was curtailed due its greater perishability in more humid climates" (176): "[d]ue to these circumstances, it is understandable that during this period […] there appears to be no book trade in Byzantium to speak of. Book production was laborious and costly; therefore, acquiring even a very modest private library of a few dozen books was beyond the means of most, if not all, rich intellectuals" (176–177).[11]

Second, "the major collections of books can be expected to have been in monasteries, in the libraries of high officials of Byzantine government (including the imperial library), and in private collections". In the "dark ages", "the production of secular literature had completely disappeared. Consequently, no manuscripts of secular content were copied; there was no demand for them, and there were no scholars and scientists demanding them" (177). The "gradual re-emergence of scholarly activity" gives the occasion for citing Lemerle's book; Lemerle is "in a general sense" right in his contention that "internal and innate factors" are necessary and that these "make [a society] receptive to such outside influences". Still, Lemerle is wrong in assuming "a hermetically

[10] All quotations for which we shall not provide a reference in the footnotes come from Gutas 1998, 175–186; we shall usually give the exact page range just after a quote or a group of quotes.

[11] This statement is corroborated by a reference to Wilson 1975, 4, but Wilson discusses examples from the whole Byzantine period. This discrepancy is partly concealed by the following parenthetical remark, placed where we have put the sign "[…]": "(and in this case, throughout the ninth century as well)" (176).

sealed society", for "the Byzantines were quite aware of the scientific and translation movement in Baghdad and it is obvious that it influenced the ninth-century renaissance in significant ways" (178).

Let us pause and comment on Gutas' account as just summarised, for this will allow us to have a first look at the quality of his argument.

[1] Gutas writes that the usefulness of papyrus was undermined by its perishability in more humid climates. This is surely true, but Gutas forgets that papyrus has been used for centuries in an indisputably humid place as Alexandria, which is located in a stretch of land between the sea and a lagoon. This notwithstanding, Alexandria hosted the most important library of the ancient world. The problem of humidity there was solved by periodically renewing the entire library. Therefore, this argument fails to explain the paucity of philosophical and scientific books in the early Byzantine period.

[2] Granting that uncial manuscripts are "more expensive than minuscule", this (along with the perishability of papyrus) does not explain the scarcity of manuscripts in early Byzantium and the ninth-century introduction of the minuscule script. Formulated in these terms—that is: uncial script, and outside Egypt—it applies to the production of books in Rome in the ages of Cicero or of Galen as well, where in spite of these limitations, books were abundantly circulating. Gutas forgets that goods (for instance, papyrus) are the object of trade and that people has been writing books in majuscule script for more than two thousand years before feeling the necessity to use the minuscule to this end. Moreover, the scant available evidence may not represent the actual situation in the early Byzantine period.[12] Consider the immense collection of books owned or read by patriarch Photius (died 893): we no longer read most of the works he refers to in his *Bibliotheca*.

[3] It is certainly true that in the period at issue, "the major collections of books can be expected to have been in monasteries, in the libraries

[12] Compare the remark in Treadgold 1979, 1257 n. 39 (with bibliography), to the effect that previous computations "overstated the rarity of books in the ninth century".

of high officials of Byzantine government (including the imperial library), and in private collections". However, this applies to any period of Byzantine history, and *mutatis mutandis*, to any pre-modern period: who else could own books apart from state or religious institutions and individuals?

[4] Gutas' claim that "the Byzantines were quite aware of the scientific and translation movement in Baghdad and it is obvious that it influenced the ninth-century renaissance in significant ways" begs the question: the very thesis he has set out to prove is here stated as something "obvious". As a matter of fact, contemporary Arabic sources (like, for instance, al-Jāḥiẓ's *Book of Annals*) can be found that praise the Byzantines for their achievements, but no one dared to use these sources to prove that the Arabs were in their turn imitating the Byzantines.[13]

Let us now resume our analysis of Gutas' argument. In order to make his point stronger, Gutas must preliminarily dismiss all historical reports that may go against his thesis. Therefore, he blames Byzantinists, particularly Paul Lemerle, for taking at face value the anecdote, transmitted in Theophanes Continuatus, about the Byzantine Emperor Theophilos and Caliph al-Ma'mūn competing for Leo the Mathematician.[14] This move is necessary since Leo is the main actor in Lemerle's narrative of the ninth-century Byzantine Renaissance, and thereby a major obstacle to Gutas' thesis. Accordingly, Gutas dismisses the anecdote on Leo as a "fairy tale" (180). Nevertheless, right after criticising Byzantinists for accepting the fabled anecdote about Leo, Gutas presents precisely one such anecdote, namely, the "report of an astrologer" (Stephanus) as one of the two sources from which the "only reliable evidence" (180) comes.

The anecdote depicts Stephanus coming to Constantinople from Baghdad and noting the decline of astrology and astronomy, which he wished to re-establish. Stephanus' rhetorical strategy is clear: presenting himself as the one who revived these sciences. We will discuss this

[13] Some of these witnesses are collected in Gutas 1998, 85–88.

[14] Theophanes Continuatus, *Historia* 190 (Bekker). See the discussion in Lemerle 1971, 150–154.

anecdote and explain its exact function within Gutas' narrative later. As for now, we recall that this anecdote repeats a widespread literary *topos* in Byzantine literature, which is rife with emperors and scholars who claim to have revived learning of all kinds after a period of complete neglect. To cite a few: in the early seventh century, Theophylact Simocatta presented emperor Heraclius (died 641) as the one who revived learning after a long period of neglect; the continuator of Theophanes says the same of the Cesar Bardas (died 866); the historian George Kedrenos writes the same of the later Constantine VII (died 959); the collection known as *Geoponica* stresses Constantine's role in the revival of learning (the author mentions rhetoric and philosophy) in comparison with the predecessors; in the eleventh century, Michael Psellos' presents himself as the one who revived philosophy after years of neglect; in the twelfth century, Anna Komnena does the same (citing Psellos) and ascribes to her own father, the emperor Alexios I (died 1118), the role of reviving philosophy and in general learning after it had vanished in the earlier period.[15] In short: Gutas dismisses the anecdote about Leo as a "fairy tale" while accepting the same kind of anecdote about Stephanus as realistic.

After discussing the anecdote about Leo, Gutas sets out a second—and main—piece of evidence: a tabular list presenting "[e]vidence from […] Greek secular manuscripts" "which survive from the first three-quarters of the ninth century". For, "in addition to being the major hard evidence for the ninth-century renaissance, they were for the most part written in the new minuscule hand in the context of a movement, aimed at transcribing the old uncial manuscripts, that is responsible for the preservation of most classical literature"."[A] brief look at the list makes it immediately apparent that the vast majority, indeed almost all of them, are scientific and philosophical" (181). The list, whose sources are given in a footnote, fills pages 182–183.

In Gutas' view, this tabular list provides decisive support for his own version of the "externalist" explanation. We are told, in fact, that the "table shows an almost perfect positive correlation between the works

[15] See Linnér 1983, 2.

translated into Arabic and the first Greek secular manuscripts copied during the first fifty years of the ninth century" (184), a statement backed up by a 1-page core argument (more on this later) and by a final remark stating that "[i]t seems clear that the correlation is causally related" (184). Thus, there are "two basic alternatives: either the Greek manuscripts were copied in imitation of or as a response to the Arabic translations of these works […], or they were copied because of specific Arab demand and under commission for these works" (184–185). A bipartite scenario (again, more on this later) follows that substantiates the disjunctive statement just read (185–186), followed in its turn by an afterthought (186). The conclusion is carefully worded: "[p]rovisionally, however, there are sufficient grounds to conclude that the Greco-Arabic translation movement was causally and directly related to the 'first Byzantine humanism' and also, through the Arabic scientific tradition in the Islamic world which fostered it, to the renewal of the ancient sciences in Byzantium after the horrors of the 'dark age'" (186).

We now analyze in detail the tabular list and the bipartite scenario mentioned above. These two items are the core of Gutas' argument—they will also be the core of our deconstruction.

Before presenting the list of manuscripts, we must preliminarily discuss its sources and how Gutas employs them.[16] He did not check any manuscript catalogues or secondary literature on the listed manuscripts. Gutas' main source (Jean Irigoin's seminal paper *Survie et renouveau de la littérature antique à Constantinople*) is read by him in a reprint collection, as several other items of secondary literature he cites, and simply cut-and-pasted (the manuscripts are also given in the same order as Irigoin's). A few obvious misunderstandings are induced by Irigoin's formulation of some pieces of information: there are blank spaces in the "Work" column of Gutas' table whenever Irigoin does not give any title; Gutas' attempts at guessing a title end in mistakes (see below); Paul of Aeginas' *Epitome medica* (Gutas does not mention the title of the work and leaves a blank space) is split into two "works";

[16] These sources are declared by Gutas (184, n. 65). These are Irigoin 1962, in particular, 289–290 and 298–299, supplemented by Allen 1893, Dain 1954, Irigoin 1957, Wilson 1983, 85–88.

Proclus' commentary of Plato's *Republic* is recorded twice although the two ninth-century manuscripts that carry this work are two tomes of one and the same edition; the false statement (regarding Damascius in ms. *Marc. gr*. Z. 246) "Comm. on *Parm*. = *De principiis*" corresponds to Irigoin's "commentaire sur le *Parménide* [*Des principes*]" (thus, according to Gutas, Damascius' commentary on the *Parmenides* and the *De principiis* are one and the same work); the indication "geographies, doxographies" is the result of the attempt at transforming the long list of authors in *Heidelb. Pal. gr.* 398 (see again below) into a couple of titles. Furthermore, a point of exactness is implicitly made in providing the folio numbers of the works in *Vindob. phil. gr.* 100 and in *Par. suppl. gr.* 1156, a detail that comes in fact from slavishly reproducing Irigoin 1957. Finally, Gutas did not realise that what he calls "the medical/biological compilation in *Par. suppl. gr.* 1156" (184) is just a collection of disparate fragments assembled in modern times (the manuscript comes from the Miller collection). As accuracy and reliability in collecting the available data are essential to corroborate a scholarly thesis, the above remarks are not secondary to our argument.

Let us now focus on the list of manuscripts. Since Gutas asserts that this list is the main evidence supporting his own thesis, for the readers' benefit we reproduce the list exactly as it is set out in his study, followed by a list of remarks. The asterisk in the table "means that though this particular book by an author is not mentioned in Arabic bibliographies and does not survive in independent ms tradition, other books by the same author on the same or related subject were translated into Arabic" (183, n. 59).[17]

[17] The sigla are *U*/*M* = Uncial/Minuscule; *F* = Flügel 1871–2; *GAS* = Sezgin 1967–2015; *DPA* = Goulet 1994–2017; *GAP* III = Fischer 1992.

Date	*U/M*	*Author*	*Work*	*Greek MS*	*Earliest attested Arabic transl.*
800–30	M	Theon	Comm. on Ptolemy's *Almagest*	Laurentianus 28, 18	"old transl." *F* 268.29, *GAS* V, 186
800–30	M	Pappus	Comm. on Ptolemy's *Almagest*	Laurentianus 28, 18	* *GAS* V, 175
800–30	U	Ptolemy	Almagest	Parisinus gr. 2389	transl. before 805; *GAS* VI, 88
800–30	U	Dioscurides	*Materia Medica*	Parisinus gr. 2179	tr. Steph. b. Basil; *GAS* III, 58
800–30	M	Paul Aegin.		Paris. suppl. gr. 1156	before 814; *GAS* III, 168
800–30	M	Paul Aegin.		Coislin. 8 and 123	before 814; *GAS* III, 168
800–30	U	Aristotle	*Sophistici Elenchi*	Paris. suppl. gr. 1362	before 785; *DPA* I, 527
813/20	U	Ptolemy	*Almagest*	Vaticanus gr. 1291	transl. before 805; *GAS* VI, 88
813/20	U	Ptolemy	*Almagest*	Leidensis B.P.G. 78	transl. before 805; *GAS* VI, 88
813/20	U	Theon	Comm. on *Almagest*	Leidensis B.P.G. 78	(see first entry above)
830–50	M	Ptolemy	*Almagest* and other works	Vaticanus gr. 1594	transl. before 805; *GAS* VI, 88

830–50	M	Euclid	*Elements*	Vaticanus gr. 190	before 800; ch. 6.3 above
830–50	M	Euclid	*Data*	Vaticanus gr. 190	ca. 850; *GAS* V, 116
830–50	M	Theon	Comm. on Ptolemy's *Canons*	Vaticanus gr. 190	before Ya'qūbī; *GAS* V, 174, 185
830–50	M	Theodosius	*Sphaerica*, etc.	Vaticanus. gr. 204	*GAS* V, 154–6
830–50	M	Autolycus	*Sphaerica*, etc.	Vaticanus gr. 204	*GAS* V, 82
830–50	M	Euclid		Vaticanus gr. 204	before 800; ch. 6.3 above
830–50	M	Aristarchus		Vaticanus gr. 204	*GAS* VI, 75
830–50	M	Hypsicles	*Anaphorica*	Vaticanus gr. 204	*GAS* V, 144–145
830–50	M	Eutocius		Vaticanus gr. 204	*GAS* V, 188
830–50	M	Marinus	Comm. on Euclid's *Data*	Vaticanus gr. 204	? but cf. Euclid
830–50	M	Aristotle	*PA*, *IA*, *GA*, *Long. vit*., *De Spir*.	Oxon. Corp. Chr. 108	ca. 800; *DPA* I, 475
ca. 850	M	Aristotle	*Physics*, ff. 1r–55v	Vind. phil. gr. 100	by 800 (ch. 3.2 above)
ca. 850	M	Aristotle	*De caelo*, ff. 56r–86r	Vind. phil. gr. 100	by 850 (ch. 6.3 above)
ca. 850	M	Aristotle	*De gen. et corr.*, ff. 86v–102r	Vind. phil. gr. 100	? but cf. *Physics*

ca. 850	M	Aristotle	*Meteorology*, ff. 102v–133v	Vind. phil. gr. 100	by 850 (ch. 3.2 above)
ca. 850	M	Aristotle	*Metaphysics*, ff. 138–201	Vind. phil. gr. 100	ca. 842; *DPA* I, 529
ca. 850	M	Theophrastus	*Metaphysics*, ff. 134r–137	Vind. phil. gr. 100	before 900
ca. 850	M	Aristotle	*Hist. anim.* VI, 12–17: ff. 13–14	Paris. suppl. gr. 1156	ca. 800; *DPA* I, 475
850–80	M	Ptolemy	[*Almagest*?]	Vat. Urbinas gr. 82	transl. before 805; *GAS* VI, 88
850–80	M	Plato	*Tetralogies* VIII and IX	Paris. gr. 1807	never translated in full(?)
850–80	M	Maximus Tyr.		Paris. gr. 1962	?
850–80	M	Albinus		Paris. gr. 1962	never translated(?)
850–80	M	Proclus	Comm. on the *Timaeus*	Paris. suppl. gr. 921	*
850–80	M	Olympiodorus	Comm. on Plato	Marcianus gr. 196	never translated(?)
850–80	M	Simplicius	Comm. on the *Physics* V–VIII	Marcianus gr. 226	*
850–80	M	Philoponus	*Contra Proclum*	Marcianus gr. 236	*GAP* III, 32, note 52

850–80	M	Damascius	Comm. on *Parm*. = *De principiis*	Marcianus gr. 246	never translated(?)
850–80	M	Alex. Aphrod.	*Quaest.; De an.; De fato*	Marcianus gr. 258	*DPA* I, 132–133
850–80	M	Proclus	*Comm. on the Republic*	Laurentianus 80, 9	*
850–80	M	Proclus	*Comm. on the Republic*	Vat. gr. 2197	*
850–80	M	Varii	*geographies, doxographies*	Palat. Heidelb. gr. 398	various translations
IX Cent.		Aristotle	*De interpr. 17a35–18a16*	Damascus	9th c.; DPA I, 514

This list calls for a preliminary remark, which pertains to the logic of confirmation. If we have to corroborate a thesis of "almost perfect positive correlation between the works translated into Arabic and the first Greek secular manuscripts copied during the first fifty years of the ninth century" (our underlining, as always in what follows), what we must do is to show that the first piece of evidence (such-and-such works were translated into Arabic) is a necessary and sufficient condition for the second piece of evidence (such-and-such secular manuscripts were copied, etc.). Gutas' table, and its author's intent, can at best show that the first piece of evidence is a sufficient condition for the second, that is, translation(x) → copying(x). However, to corroborate his thesis of "almost perfect positive correlation", Gutas should have proved that the arrow also points in the opposite direction, namely, that copying(x) → translation(x), or, by contraposition, that ¬translation(x) → ¬copying(x). Thus, Gutas should also have shown that if a work was not translated during the first fifty years of the ninth century, then it was not copied

either. As a matter of fact, the overall table does not even prove that translation has been a sufficient condition for copying, as we shall see in a moment.

Granting Gutas his use of the logic of confirmation, we shall now show that the above 43-token table, even if the most favourable reading of the evidence is granted, must be reduced to a handful of items.

1. At 184 we read that the "table shows an almost perfect positive correlation between the works translated into Arabic and the first Greek secular manuscripts copied during the first fifty years of the ninth century", but on p. 186 Gutas asserts that "[w]ith regard to the Greek manuscripts in the table that were copied during the second half of the ninth century, the evidence presents striking differences. The subjects covered are almost entirely philosophical, and the correlation with Arabic translations of the same works is only partial". Thus, according to Gutas himself, manuscripts copied from 850 onwards cannot count as supporting the stated "almost perfect positive correlation". The presence of these items in the list (ten out of forty-three) is hard to explain and is even detrimental to Gutas' thesis. The existence of these manuscripts, mostly preserving Neoplatonic writings that had not been translated into Arabic, suggests that, after all, the Byzantines had their own agenda. Moreover, according to the very four sources Gutas uses, all these manuscripts, with the possible exceptions of *Vindob. phil. gr.* 100 (*Diktyon* 71214) and of *Par. suppl. gr.* 1156 (*Diktyon* 53834), were part of one and the same copying campaign—the so-called "philosophical collection"—so that they can and must be eliminated from the list *en bloc*.[18]

2. Items that might support Gutas's thesis must be eliminated too. For instance, it has been well-known since about 1940 that *Vat. Urb. gr.* 82 (*Diktyon* 66549) is a much later exemplar, penned in imitative writing between the end of the thirteenth century and the beginning

[18] We also note that, according to Gutas' sources, Alexander of Aphrodisias' *De fato* was not translated into Arabic.

of the fourteenth century.[19] Furthermore, this manuscript does not contain "[*Almagest*?]" but Ptolemy's *Geography*. Independently of this inaccuracy, *Vat. Urb. gr*. 82 must be eliminated from the list.

3. Another problematic issue concerns the dates of the Arabic translations of the Greek texts listed in the table. The manuscripts in Gutas' list are intended to corroborate his thesis *directly*, that is, insofar as they are physical objects produced in the early ninth century. However, such specific manuscripts can have this role only if one can prove that the Arabic translations are not decidedly later than them. However, there are cases in which the chronological interplay between the production of a codex containing a given treatise and the completion of a translation into Arabic of the same treatise is less clear-cut than Gutas claims it to be. For example, from one of Gutas' sources, we learn that Theodosius' *Sphaerica* transmitted in ms. *Vat. gr.* 204 (*Diktyon* 66835), ff. 1r–37v, a manuscript dated to 830–850 by Irigoin-Gutas, has been translated by Qusṭā Ibn-Lūqā (died around 912)[20] after a request of caliph al-Musta'īn bi-llāh (died 866).[21] It is no surprise, then, that whenever the chronological data support his thesis—that is, whenever the Arabic translation of a text certainly predates the production of the Greek manuscript preserving that same text—Gutas transcribes in the table the year in which the translation has been carried out. By contrast, when the chronological data are uncertain or do not corroborate his thesis, Gutas generically refers to *GAS* without providing further details. For instance, as for the "little astronomy" preserved in ms. *Vat. gr.* 204, we learn from Gutas' sources that all known translations (Euclid's works are the exception) belong to the second half of the ninth century. As David Pingree puts it, "there is no evidence that [these treatises]

[19] Diller 1939 and 1966. Gutas, whose aim is, of course, to stretch out the list, was deluded by Dain 1954, 41; this manuscript is not mentioned in Irigoin 1962.

[20] On Qusṭā Ibn-Lūqā see Rashed 1984, XVI–XXII.

[21] See *GAS* V, 154–156. Even if one allows a slightly later dating for this manuscript, it is impossible to ascertain whether its production follows or precedes the Arabic translations of the text it contains.

were translated as a corpus", which points to a line of transmission independent of the one surfacing in *Vat. gr.* 204.[22]

4. The description of specific items in Gutas' list is problematic. This is the case of ms. *Heidelb. Pal. gr.* 398 (*Diktyon* 32479).[23] As said above, this manuscript must be eliminated from the list because of its later dating and its origin as part of the so-called "philosophical collection". Yet, even a cursory look at the way Gutas describes this item casts doubts over the reliability of the data presented in the list. From Gutas' table, we learn that the manuscript contains the works of various authors, particularly geographers and doxographers, and that these works have received "various translations", but Gutas does not indicate any source for his statement. He could not have indicated any, for none of the sources used by Gutas mentions translations of these works. Moritz Steinschneider writes that Philo of Byzantium's fifth book of his *Mechanikē syntaxis* (*On Pneumatics*) was translated into Arabic, but not his *De septem orbis miraculis*. Likewise, several works attributed to Hippocrates or included in the *corpus hippocraticum* have been translated into Arabic, but not—as far as we know—the pseudo-epigraphic letters contained in the Heidelberg manuscript.[24]

5. There is a further problem concerning the list. Since Gutas' point rests on manuscript production, the list should be keyed on manuscripts,

[22] The evidence is conveniently collected in Pingree 1968, 16, from which we quote.

[23] This manuscript contains Anonymus, Ὑποτύπωσις γεωγραφίας; Agathemerus, *Geographiae informatio*; ex [Aristotelis] περὶ σημείων; Dionysius of Byzantium, *Anaplus Bospori*, [Arrianus] *Periplus Euxini*; Eiusd. *Cynegeticon*; Eiusd. *Periplus Euxini*; Eiusd. *Periplus maris Erythraei*; Hannon, *Periplus*; Philo of Byzantium, *De septem orbis spectaculis*; Χρηστομάθειαι ἐκ τῶν Στράβωνος γεωγραφικῶν; Ps. Plutarch, *De fluviis et montibus*; Parthenius Niceanus, *Erotica*; Antoninus Liberalis; Hesychius, *De origine Constantinopolis*; Phlegon of Tralles, *Paradoxa et Macrobii*; Eiusd., *Olympia*; Apollonius Paradoxographus; Antigonus Paradoxographus; *Epistulae* Hippocratis, Themistoclis, Diogenis, Bruti Romani. For a description of *Heidelb. Pal. gr.* 398, see Stevenson 1885, 254–257.

[24] Steinschneider 1897, 107.

not on works counted as items. Since manuscripts usually contain several works, which in some cases were manifestly copied as a *corpus*, it is obvious that keying the list on works aims at inflating the number of its items. A case in point is *Vat. gr.* 204, which must count as one item and not as seven.[25]

After the operations just described, the table can be set out as follows:

Date	*U/M*	*Author*	*Work*	*Greek MS*	*Earliest attested Arabic transl.*
800–30	M	Theon / Pappus	Comm. on Ptolemy's *Almagest*	Laurent. 28, 18	"old transl." *F* 268.29, *GAS* V, 186 / * *GAS* V, 175
800–30	U	Ptolemy	*Almagest*	Paris. gr. 2389	transl. before 805; *GAS* VI, 88
800–30	U	Dioscurides	*Materia Medica*	Paris. gr. 2179	tr. Steph. b. Basil; *GAS* III, 58
800–30	M	Paul Aegin.		Paris. suppl. gr. 1156	before 814; *GAS* III, 168
800–30	M	Paul Aegin.		Coislin. 8 and 123	before 814; *GAS* III, 168
800–30	U	Aristotle	*Sophistici Elenchi*	Paris. suppl. gr. 1362	before 785; *DPA* I, 527
813/20	U	Ptolemy	*Almagest*	Vat. gr. 1291	transl. before 805; *GAS* VI, 88

[25] As said, these treatises form the so-called "little astronomy", which all early Greek manuscripts transmit as a *corpus*: see *e.g.* Mogenet 1950.

813/20	U	Ptolemy / Theon	*Almagest* / Comm. on *Almagest*	Leidensis B.P.G.78	transl. before 805 *GAS* VI, 88 / (see first entry above)
830–50	M	Ptolemy	*Almagest* and other works	Vat. gr. 1594	transl. before 805; *GAS* VI, 88
830–50	M	Euclid / Theon	*Elements*, *Data* / Comm. on Ptolemy's *Canons*	Vat. gr. 190	before 800; ch. 6.3 above; ca. 850; *GAS* V, 116 / before Ya'qūbī; *GAS* V, 174, 185
830–50	M	Theodosius / Autolycus / Euclid / Aristarchus / Hypsicles / Eutocius / Marinus	*Sphaerica*, etc. / *Sphaerica*, etc. / *Anaphorica* / Comm. on Euclid's *Data*	Vat. gr. 204	*GAS* V, 154–156 / *GAS* V, 82 / before 800; ch. 6.3 above / *GAS* VI, 75 / *GAS* V, 144–145 / *GAS* V, 188 / ? but cf. Euclid
830–50	M	Aristotle	*PA*, *IA*, *GA*, *Long. vit*., *De Spir*.	Oxon. Corp. Chr. 108	ca. 800; *DPA* I, 475
ca. 850	M	Aristotle / Theophrastus	*Ph*, *Cael*., *De gen. et corr.*, *Meteorology*, *Metaphysics* / *Metaphysics*	Vind. phil. gr. 100	by 800 (ch. 3.2 above); by 850 (ch. 6.3 above); ? but cf. *Physics*; by 850 (ch. 3.2 above); ca. 842; *DPA* I, 529; before 900

ca. 850	M	Aristotle	*Hist. anim.* VI, 12–17: ff. 13–14	Paris. suppl. gr. 1156	ca. 800; *DPA* I, 475

We are left with a 14-item list. We now proceed to carry out the following operations:

6. Mss. *Vat. gr.* 1291 (*Diktyon* 67922) and *Leid.* B.P.G. 78 (*Diktyon* 37735) are listed as carrying the *Almagest*, whereas they contain Ptolemy's *Handy Tables*, which were not translated into Arabic according to Gutas' sources.[26] Accordingly, these two manuscripts must be eliminated from the list.

7. *Vat. gr*. 1594 (*Diktyon* 68225) is an item included in the "philosophical collection" by one of the sources Gutas availed himself of in compiling the list.[27] Consequently, this manuscript must be eliminated from the list because it falls under the domain of operation (1) above.

8. The fragments of Paul of Aeginas, listed by Gutas as two items, come from one and the same manuscript.[28] The Aristotelian fragment in *Par. suppl. gr*. 1156 was part of *Vindob. phil. gr.* 100,[29] but since no sources available to Gutas state this explicitly, we shall keep these two items distinct.

9. In his own core argument on p. 184 (this is placed *after* the list; more on this argument below), Gutas is categorical that what especially counts are "really" scientific works. Consequently, the fragment of

[26] The Leiden manuscript also contains, penned in a *minuscule* of the late ninth to the beginning of the tenth century, Theon's "little commentary" on the *Handy Tables* (what Gutas, following Irigoin, calls "*Canons*"), not his commentary on the *Almagest*: Tihon 1978, 105–106.

[27] Wilson 1983, 85. See also Leroy 1978, 44–45.

[28] Skimming the standard catalogues Devreesse 1945 and Astruc & Concasty 1960 would have sufficed to avoid the splitting.

[29] See Irigoin 1957, 8–9.

Aristotle's *Sophistici Elenchi* in *Par. suppl. gr*. 1362 (*Diktyon* 54019) must be eliminated from the list.[30]

10. Likewise, and despite some ambiguities in Gutas' wording (a date "ca. 850" lies on the border between the relevant and the non-relevant time intervals), we must assume that the only remaining philosophical item(s), namely, *Vindob. phil. gr*. 100 + *Par. suppl. gr*. 1156, are irrelevant to Gutas' argument and must be eliminated from the list. One may concede, though, that Aristotle's writings collected in *Oxon. Corp. Christ*. 108 (*Diktyon* 48635) may be considered as "scientific".[31]

After the indicated operations, the list contains seven items, only four of which have mathematical or astronomical content, and it reads as follows:[32]

Date	*U/M*	*Author*	*Work*	*Greek MS*	*Earliest attested Arabic transl.*
800–30	M	Theon / Pappus	Comm. on Ptolemy's *Almagest*	Laurent. 28, 18	"old transl." *F* 268.29, *GAS* V,186 / * *GAS* V,175
800–30	U	Ptolemy	*Almagest*	Paris. gr. 2389	transl. before 805; *GAS* VI, 88
800–30	U	Dioscurides	*Materia Medica*	Paris. gr. 2179	tr. Steph. b. Basil; *GAS* III, 58

[30] The indication "before 785; *DPA* I, 527" we read in the list refers, according to Gutas' source, to the Syriac translation, not to the Arabic translation.

[31] However, the manuscript also contains the *De iuventute et senectute*.

[32] The sigla stand for the following works: Theodosius, *Sphaerica*, *De habitationibus*, *De diebus et noctibus*; Autolycus, *De sphaera mota*, *De ortibus et occasibus*; Euclid, *Optica*, *Phaenomena*; Aristarchus, *De magnitudinibus et distantiis solis et lunae*.

800–30	M	Paul Aegin.		Paris. suppl. gr. 1156 Coislin. 8 and 123	before 814; *GAS* III, 168
830–50	M	Euclid / Theon	*Elements*, *Data* / Comm. on Ptolemy's *Handy Tables*	Vat. gr. 190	before 800; ch. 6.3 above; ca. 850; *GAS* V, 116 / before Ya'qūbī; *GAS* V, 174, 185
830–50	M	Theodosius / Autolycus / Euclid / Aristarchus / Hypsicles / Eutocius / Marinus	*Sph.*, *Hab.*, *Di. noct.* / *Sph. mota*, *Ort. occ.* / *Opt.*, *Phaen. Magn.* *Anaphoricus* / Comm. on Apollonius' *Conica* prolegomena to Euclid's *Data*	Vat. gr. 204	*GAS* V, 154–6 / *GAS* V, 82 / before 800; ch. 6.3 above / *GAS* VI, 75 / *GAS* V, 144–145 / *GAS* V, 188 / ? but cf. Euclid
830–50	M	Aristotle	*PA*, *IA*, *GA*, *Long. vit.*, *Juv.*, *De Spir.*	Oxon. Corp. Chr. 108	ca. 800; *DPA* I, 475

This list contains some inaccuracies:

a) Writing "? but cf. Euclid" by the side of Marinus' *Prolegomena* to Euclid's *Data* means that no document attests to an Arabic translation of Marinus' work. So, this item should also be removed.

b) There remains an asterisk in the list, by the side of Pappus' commentary on the *Almagest*: "* *GAS* V,175". The reference is to the

GAS entry for Pappus, where no mention is made of any translation of his commentary on the *Almagest*.[33] In general, asterisks in the lists often highlight a lack of correspondence between manuscripts and translations.

c) Euclid's works preserved in *Vat. gr*. 204 are *Optica* and *Phaenomena*.[34] Writing "before 800; ch. 6.3 above" (that is, p. 148) by their side is problematic, for the provided date can refer only to the *Elements*. Again, the title of Hypsicles' work as transmitted in *Vat. gr.* 204 is *Anaphoricus*, not *Anapahorica*. Gutas also ascribes a work that never carried the title *Sphaerica* to Autolycus, and deems Marinus' writing a "commentary" on Euclid, whilst this is, in fact, a short isagogical tract.

After these corrections, we now discuss Gutas' core argument (184), which is opened by the following sentence: "[t]his evidence can be interpreted by taking into consideration the following factors". These are:

a) "[A]ll the works copied […] are scientific in nature" with the exception mentioned in point (9) above, which has allowed us to eliminate a manuscript from the list.

b) "we have absolutely no information that any Byzantine scholar" of the period "was either interested in or had sufficient training and mathematical knowledge to be able to study these works", a statement that is little more than a truism—for it refers to a period for which we have little or no information on any kind of intellectual activity—and which is backed up by the above-mentioned story of the astrologer Stephanus visiting Constantinople and finding an intellectual waste (more on Stephanus just below).

[33] Only Books V and VI of Pappus' commentary survive.

[34] *Vat. gr*. 204 contains one of the two extant recensions of each of these works; the other recension is, in both cases, witnessed by the late eleventh-century manuscript *Vindob. phil. gr*. 31 (*Diktyon* 71145).

c) Stephanus himself "transmitted demonstrably some astrological knowledge from Baghdad to Constantinople". Nevertheless, the "above" discussion referred to by Gutas (that is, the one carried out on p. 180) rests on the Stephanus' role only. In addition, we are told that on the authority of the historian of science David Pingree "an astrological technique described in a work by Theophilus was used in 792 by Pancratius, the astrologer of Constantine VI, to cast a horoscope" (181; more on this just below). Readers willing to accept Gutas' main argument will probably regard an anecdote on a single astrological technique used to cast one horoscope as conclusive evidence. By contrast, we will point out in a moment the evident limitations of the scant evidence provided by this anecdote.

d) "[A]ll of these texts" (of course with "possible exception[s]") "had been translated into Arabic, etc." This is an evident *petitio principii*, for one cannot use the "almost perfect positive correlation" to explain the "almost perfect positive correlation".

All in all, leaving aside the manuscript list and its shortcomings, Gutas' core argument amounts to two truisms, a circular statement, and a single piece of evidence: Pancratius' horoscope of 792, where he used a technique described in a work written by some other astrologer.

We may concede that a single horoscope can be used to explain why Byzantine intellectuals were eager to read Euclid, the "little astronomy", and the *Almagest*, but let us look closely at what David Pingree says in the article where Gutas finds the pieces of information about Pancratius' horoscope. Pingree had his own agenda, which in some respects is similar to Gutas':[35] in a nutshell, Pingree advocated a "loop" circulation of astronomical and astrological knowledge from Hellenistic Greece and Babylonia to India and the Persian empire, and then back to Byzantium and Western Europe by the intermediation of medieval Islam and the Medieval Latin translations. Every civilisation contributed its own share.

[35] Pingree's reaction to "hellenophilia" in the history of science can be read in Pingree 1992.

Thus, one is likely to find in Pingree's studies arguments and evidence supporting Gutas' thesis. However, Pingree's eagerness for sweeping statements and scholarly romancing suggests a more sceptical approach to his results. The two characters of Pingree story as endorsed by Gutas are Theophilus and Stephanus: the former, "al-Mahdī's astrologer" (180),[36] is useful to Gutas (180–181) insofar as he was an associate of Stephanus, the author of "an apology for astrology written in the 790s in Constantinople" (180). Neither Pingree nor Gutas says that any source links Stephanus with the Abbasid court. As a matter of fact, we know next to nothing of Stephanus, who has been credited with the authorship of a vast amount of pseudepigraphical works.[37]

In introducing his discussion of Stephanus, Pingree (1989, 237) writes: "We now must address the question of how an interest in scientific texts, and particularly in astronomy and astrology, came to be implanted in Byzantium".[38] However, only astrology will be treated by Pingree in what follows, a discipline that does not figure in Gutas' 43-item list of manuscripts. Even granting this, Pingree corroborates his statement with a discussion that bristles with conjectures. He ascribes a treatise to Stephanus on flimsy grounds; he starts his discussion of the only piece of evidence certainly to be ascribed to him (a "short defence of astrology as a Christian science") with the following statement: "Stephanus states that he has come from Persia—presumably he means by this Baghdad—to this happy city [scil. Constantinople] only to dis-cover that the

[36] Gutas should have clarified whether we have to believe his source in the main text (180), which states that Theophilus was "Hauptastrologe al-Mahdī's" (*GAS* VII, 49), or his source in footnote 56 of the subsequent page, who asserts that "[w]e know that [Theophilus] served as military advisor to al-Mahdī" (Pingree 1989, 237). The issue is settled by the common (secondary) source of all later biographical sketches of Theophilus, namely, Franz Cumont's account in *CCAG* V, 229–231; see also Tihon 1993, 190–192. Al-Mahdī ruled from 775–785.

[37] There is a surprising number of Stephanus involved in scholarly activities in the seventh-eighth centuries; see Wolska-Conus 1989, and the clear synthesis—which also refutes Pingree's main argument for ascribing any profound expertise in astronomical matters (namely, the alleged construction of astronomical tables adapted to the Byzantine world era and to Roman months) to Stephanus—in Tihon 1993, 185–190.

[38] See also the quotes that follow from 238 and 239.

astronomical and astrological parts of philosophy have been snuffed out in it". After several "presumably" and "probably", we finally discover that the link Theophilus–Stephanus–Pancratius, in virtue of which "an interest in scientific texts, and particularly in astronomy and astrology, came to be implanted in Byzantium", is just a conjecture, which Gutas restates as a fact. By the same token the "astrological technique" of Theophilus used by Pancratius is entirely Pingree's conjecture.

On these grounds, Pingree concludes: "With Stephanus, then, we have astrology and astronomy restored to Byzantium, historical astrology introduced from the East, and the mathematical art so stoutly defended as a Christian science that even the archbishop of Thessalonica [scil. *Leo* the Mathematician] felt free to follow it". This conclusion, let us repeat, is grounded on the sole documentary evidence of a single horoscope. What *we* can conclude is that all of this story, if freed from Pingree's conjectures, is a matter of relations between Hellenistic Greece – Sassanian Iran – Byzantium: to quote Pingree again,[39] "the astrology they [*scil*. the four treatises composed by Theophilus] represent originated in the Hellenistic period, was transmitted to Iran, and returned via Baghdād and Syria to Byzantium". From what Pingree says, we may only gather that the route passed through Baghdad and Syria just because these are located between Iran and Byzantium.

Nota bene: we are not claiming that a real transmission process through Baghdad and Syria has never occurred; we claim that the evidence adduced by Pingree does not corroborate this thesis. In any event, since these anecdotes concern isolated enterprises of specialists, we cannot see how this story can be related to the translation movement and to Gutas' suggestion that the ninth-century Byzantine "renaissance" originated from an input coming from outside.

After Pingree's conjecture, it is now time to go back to the last part of Gutas' argument. Gutas offers (185–186) two socio-cultural explanations, which he calls a "financial" and a "sociological" explanation, "both [of which] may have been operative". Gutas' argument is expressly formulated

[39] Pingree 1989, 236.

as a sequence of conjectures.[40] The gist of the "financial" explanation is that "to supply Arab demand" "for manuscripts of secular Greek works" "would be a lucrative enterprise"; "[n]ews of the demand would certainly travel fast", and "would easily reach Asia Minor and Constantinople" (185). This explanation is problematic for the following reasons.

We first note that supplying the *early ninth-century* foreign customers of manuscripts with texts written in the new minuscule script (which*, by definition*, they were totally unaccustomed to) could only be financial suicide. As a matter of fact, the extensive searches carried out by the Arabic translators who allegedly triggered the renewed interest in copying manuscripts could only have started before the introduction of the minuscule script and could only have begun from the Middle East. Second, according to the "financial" explanation, the ninth-century philosophical and scientific manuscripts written in minuscule had been produced "to supply Arab demand". If this were the case, and since Gutas' list only includes extant manuscripts, either these manuscripts travelled to Baghdad and then came back to Constantinople, or they were master copies of other (now lost?) manuscripts that took the route to Baghdad. There is only one way to test this point: comparing the Arabic texts of the translated scientific and philosophical works with the Greek texts witnessed by the manuscripts listed in the table. Gutas is aware of this problem, for he writes: "nor have the Greek manuscripts of the ninth and tenth centuries been investigated to ascertain whether they have been used for translation into Arabic" (178–179).

As a matter of fact, the relevant Greek manuscripts were investigated in this sense, and the results are unfavourable to Gutas' main hypothesis. Such investigations have shown that there were plenty of Greek manuscripts in the Middle East,[41] that the most natural place where Arab

[40] This is well highlighted by extracting the modal modifiers in the argument: "relatively clear … in general lines … would have … would be … there is no reason why … should not … would certainly … would expect … would easily reach … would be to interpret … would also be very close to the truth". In addition to this, even Pingree (quoted for rescue, as we shall see in a moment) prints a "seems to have been due" that speaks for itself.

[41] On the manuscript production in the script called "coptic uncial", see Irigoin 1959 and Hemmerdinger 1964.

translators could find Greek manuscripts were Palestine and Syria,[42] and that the Arabic translations of *all* Greek scientific works listed by Gutas fit the rule of marginal areas as to their location in the textual tradition of these works.[43] This means that the Arabic translations had access to layers of Greek text possibly more ancient than, and certainly independent of, the Greek texts witnessed by the direct tradition, or at least by the direct tradition carried by the ninth-century manuscripts listed by Gutas. Accordingly, the Arabic translation constitutes a separate (and farther rooted) branch in the stemma summarising the entire tradition of a given Greek text.[44] This is first and foremost true of the *Elements*,[45] but Gutas might also have checked Euclid's *Data*,[46] the Euclidean blank space in his list that he should have filled with "*Optica*",[47] Autolycus' treatises,[48] and Hypsicles' *Anaphoricus*.[49] Major mathematical authors that cannot figure in Gutas' list provide striking instances of complete independence

[42] Crucial in our perspective is the testimony of the Banū Mūsā, who coordinated the Arabic translators of Apollonius: one of them travelled to Syria in search of manuscripts of the work; see Toomer 1990, 620–629, in part. 626–627. See also, for the period that precedes the translation movement, the evidence adduced in Mango 1991 and Cavallo 1995a and 1995b.

[43] This rule is discussed in Pasquali 1952, 159–160. Gutas might have read in Goulet 1994–2017, I, 458, that the same phenomenon applies to Aristotle's *Rhetorica*.

[44] Gutas had apparently missed Crubellier 1992—entirely relevant to the Theophrastus-item in his own list—a paper he happened to have discovered in Gutas 2010, where (see page 65) such a kind of deeply-branched stemma is presented as students of ancient Greek mathematics were accustomed to since several decades.

[45] Knorr 1994, who also summarises the late nineteenth-century debate between Martin Klamroth and Johan Ludvig Heiberg, the editor of the *Elements* (Gutas cited only the works by Sonja Brentjes, on 148 n. 69). As for the *Almagest*, see Kunitzsch 1974 (cited by Gutas on 148 n. 71), 15–71, and Toomer 1984, 3, respectively. The latter notes that the Arabic tradition frequently confirms the reading of *Vat. gr.* 180 (*Diktyon* 66811), a tenth-century witness that does not carry the slight recension we read in the other branches of the direct tradition; these branches are represented by *Par. gr.* 2389 (*Diktyon* 52021) and by *Vat. gr.* 1594 and *Marc. gr.* Z. 313 (coll. 590; *Diktyon* 69784), respectively.

[46] Thaer 1942.

[47] Rashed 1997 (but see below), cited by Gutas at 148 n. 70. The blank space should also be filled with "*Phaenomena*".

[48] Mogenet 1950, 170–181.

[49] De Falco & Krause 1966.

between the Greek and the Arabic traditions: cases in point are crucial authors like Archimedes, Apollonius, or Diophantus.[50]

Combining these two remarks, the manuscripts that may support Gutas' "financial" explanation reduce to

Date	*U/M*	*Author*	*Work*	*Greek MS*	*Earliest attested Arabic transl.*
800–30	U	Ptolemy	*Almagest*	Parisinus gr. 2389	transl. before 805; *GAS* VI, 88
800–30	U	Dioscurides	*Materia Medica*	Parisinus gr. 2179	tr. Steph. b. Basil; *GAS* III, 58

As Gutas emphasises manuscripts penned in the new minuscule script, no manuscripts support Gutas' "financial" explanation.

As to the "sociological explanation", its gist lies in the "awareness by Byzantine intellectuals of the scientific superiority of Arabic scholarship and the wish to emulate it" (185). This statement is taken to be corroborated by an identical statement by David Pingree and by further recalling that *four centuries later* "numerous Arabic and Persian scientific works were translated from Arabic into Byzantine Greek" (186). The statement might have been corroborated more effectively by mentioning Bertrand Hemmerdinger, who in 1962 proposed more or less the same explanation as Gutas' and who is cited by Lemerle.[51]

As a matter of fact, the "sociological explanation" is an excellent approximation of a statement that no evidence can corroborate. Can "awareness by Byzantine intellectuals" of anything be corroborated by any evidence apart from an explicit statement by some Byzantine intellectual? As we have argued at length, the data set out by Gutas do

[50] See *GAS* V, 121–136; Toomer 1990; Sesiano 1982 and Rashed 1984, respectively.
[51] Citation in Lemerle 1971, 16 n. 8.

not prove the point. We cannot enter the mind of a Byzantine or Arab scholar of the period to determine his motivation.

Let us explain this with an example. A Greek epigram found in the *Palatine Anthology* suggests that Leo the Philosopher (died after 869) owned a copy of Apollonius' treatise on conic sections.[52] Around the same period—the first half of the ninth century—this very same work was translated into Arabic. Did Leo's interest in this work originate from similar interests in the Islamic world, or is it the other way around? Or were Leo and the mathematicians in the Caliphate independently interested in this work because of its status as a reference work? Can any document provide an answer to these questions?

If the "sociological explanation" cannot be corroborated by any evidence, *a fortiori* no manuscript list can corroborate it.

3. After and beyond Gutas

In the previous section, we discussed Gutas' thesis on the grounds of the evidence available when *Greek Thought, Arabic Culture* was written. We now present evidence that has become available after 1998, or that has been thoroughly discussed after that date. In this section, whose content is more technical, we shall not deal with Gutas' thesis.

The documentary record has not been greatly enriched in quantity or in quality during the last 25 years, but what has been put to scholarly attention may contribute to improving the quality of the discussion.[53] Our remark above about the Arabic tradition of mathematical, astronomical, and philosophical writings constituting a branch independent of, and possibly farther rooted than, the direct Greek tradition has been confirmed to various degrees[54] by studies on Euclid's *Elements*, *Data*, and *Optica*,

[52] *AP* IV 578.

[53] See in the first place Magdalino 2006, 17–54, and Martelli 2016 for a state of the research on the two Stephanus who are relevant in our perspective. An important clue is the palimpsest *Vat. sir*. 623 (*Diktyon* 69457), *rescriptus* in 886, which contains parts of Ptolemy's *Handy Tables* in majuscule script and fragments of an Arabic translation of Theon of Alexandria's "little commentary" on the *Handy Tables*: D'Aiuto 2003; Tihon 2011, 41–47; Tihon 2021; Giuffrida, Németh & Proverbio 2023.

[54] The main difficulty, apart from the very different structure of the two languages,

and on Aristarchus' treatise,[55] and by the editions of Aristotle's *Int*., *GC*, *Metaph*., *Po.*, and of Theophrastus' *Metaphysics*.[56]

Very recent studies strongly suggest that some translators from Greek into Arabic looked for exemplars written in majuscule: "Ḥunayn semble avoir eu pour coutume de traduire des manuscrits qu'il tenait pour anciens. C'est un indice de la plus haute importance, et dont les éditeurs de textes grecs devront tirer toutes les conséquences, pointant vers le fait que ses exemplaires grecs de traduction n'étaient pas des manuscrits proto-byzantins, mais des manuscrits tardo-antiques".[57]

A further contribution to the discussion comes from the following considerations, which concern aspects that were outside the focus of previous studies. First, the strategy of the scholars who wished to

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lies in the fact that we often have access to recensions only. This is certainly true of Apollonius' *Conica*, of Diophantus' *Arithmetica*, of Euclid's *Optica* and *Data* (see references below), and, among his other treatises, of Aristotle's *Cael*., *Mete*., *EN*: for the latter, see Goulet 1994–2017, suppl., 285, 325, 192–194, respectively.

[55] Vitrac 1998 and 2001 (add Rommevaux, Djebbar & Vitrac 2001); Sidoli & Isahaya 2018 (but philologically unreliable); Kheirandish 1998 (the author concludes, contradicting the claim in Rashed 1997, that we have access to a text that is both a revision and a conflation of the two Greek recensions: see the pages mentioned in the summary, at 103–105); Berggren & Sidoli 2007 (Noack 1992, 37–45, is not informative, and for this reason it is not cited in Section 2 above), respectively.

[56] Weidemann 2014; Rashed 2001 (whose argument at 84–92 for locating the translation exemplar in Constantinople is plausible, but nothing more; also read Marwan Rashed again, in Goulet 1994–2017, suppl., 304–312, esp. 305: "[i]l est probable, pour un certain nombre de raisons stemmatiques et historiques, que Ḥunayn acquit à Byzance (plutôt qu'en province) un manuscrit contenant la *Physique* et le *De generatione et corruptione*") and 2004; Rashed 2019 (the edition, in collaboration with Oliver Primavesi, is in progress; the Arabic translation is an independent branch of family β; Rashed's main argument in this paper shows that an ancestor of the Greek model of the translation into Arabic—and not the model itself, as Rashed has it—was damaged and had such-and-such codicological features); Tarán & Gutas 2012 (who show that the exemplar of translation was in majuscule); and Gutas 2010 (who postulates an exemplar of translation in minuscule on the grounds of just *two identical* translation mistakes *likely* to arise from a Greek misreading οι → α, which in its turn is *more likely* to happen in minuscule than in majuscule; one of these readings is marked by "ut vid."), respectively. Further information on the Syriac and Arabic translations of several Aristotelian treatises can be found in Goulet 1994–2017, suppl.

[57] Quote from Förstel & Rashed 2020, 214; see also Rashed 2019.

smooth out the discontinuity after the Byzantine "dark ages" (hence, no need for any "explanation" of an alleged "renaissance", etc.)[58] has so far mainly consisted in showing that scientific matters were somehow practised before and during the alleged discontinuity. However, recent studies suggest that they were not actively practised until the eleventh century:[59] as far as the scant documentary evidence goes, one may well

[58] On smoothing out such alleged discontinuities in Byzantine intellectual history, see most recently Ronconi (forthcoming) and in particular section 2b on the "Arab connection".

[59] See Tihon 2017 for an informed and well-balanced assessment of the astronomical activities in the period, with a discussion of the scholia (Tihon changed her overall assessment with respect to her 1993 paper); Acerbi 2018, 156–159, for a deconstruction of the mathematical achievements of Leo the Mathematicians. These studies show the weakness of the reconstruction in Magdalino 1998, 208–213, and Magdalino 2006, 33–89, who uses astrology to remove the scientific discontinuity while leaving the door open for the "relais syro-arabe" and concludes that "the road to Baghdad became inextricably associated, in Byzantine intellectual life, with astrology and Iconoclasm" (1998, 213). However, Magdalino makes his case partly rest on chronological material (this means that this material is neither astronomical nor astrological; see just below) and on an assessment of the scant evidence grounded on the methodological principle of framing a tangle of conjectures corroborated by incidental coincidences. Finally, recall that Magdalino develops an insight first put forward in Alpers 1988, 354–359. As for one of the *pièces de résistance* of Magdalino's construction, namely, the astronomical scholia placed on ff. 1r–2r and 95v of *Vat. gr*. 1291 (at least three different hands, dated to the middle and end ninth century; the scholia carry internal chronological elements that point to their being composed in 704–815 and 830), these are codicological units heterogeneous to the rest of the manuscript and to each other: Spatharakis 1978, to be completed with Janz 2003, 172–174. These short directive texts of disparate contents are edited in Mogenet 1969, who ends his article with this statement (1969, 91): "nous nous trouvons en présence de traces d'une activité astrologico-astronomique, à Constantinople vraisemblablement, de la part d'anonymes, des professeurs sans doutes, qui, à leur manière, transmettent le flambeau de la culture par delà les bouleversements du VII^e^ siècle et éclairent d'une vague lueur ce que, trop facilement, l'on continue d'appeler les *dark ages* du moyen âge byzantin". Mogenet's uninterrupted soft-pedalling (underlined) speaks for itself. It remains that one has to have Theon's and Stephanus' commentaries on Ptolemy's *Handy Tables* at hand in order to compose a collection of texts that, to a large extent, heavily depend on these commentaries (as Mogenet shows), and for the rest compile the definitions Heraclius prefaced to Stephanus' commentary on Ptolemy's *Handy Tables* (these definitions, edited in Heiberg 1907, CXCI–CXCII, amount to about one-

speak of a slow but steady growth of scientific activity occurring from the late eighth century to the early eleventh century, but not more than that.

On the one hand, thus, there was hardly any discontinuity in scientific matters. On the other hand, however, if the sciences were scarcely and sparsely practised, the problem of "explaining" the existence of scientific manuscripts produced in Constantinople in the first half of the ninth century becomes urgent. A facet of the problem is that it is extremely difficult to ascertain whether a given manuscript is an exemplar of first transliteration or not. This means that the textual tradition of a given text should be investigated so as to understand whether one or several transliterations occurred, and in what period—and so as to state clearly whether any claim in this sense is supported by the extant evidence or not (the latter will most often be the case).[60] Such an investigation is important since it may well be that the absence of profane-yet-not-scientific manuscripts copied in the first half of the ninth century is a distortion arising from the fact that such early copies actually existed but got discarded whenever copies of them were taken. Likewise, the relative dearth of eighth-century profane (majuscule) manuscripts could be a depletion phenomenon originating from the transition to the minuscule script: antigraphs written in majuscule were regarded as no longer useful and discarded accordingly.[61]

It is reasonable to suppose that manuscripts in good conditions were selected to serve as models of transliteration, and this explains

third of the whole sequence on ff. 1r–2r of *Vat. gr.* 1291) and very elementary material usually found in Easter Computi (what Mogent did not see, while seeing astrology almost everywhere, apparently to account for the triviality of the contents of most of these scholia).

[60] This analysis is almost never done, though (an exception is Tarán & Gutas 2012). See the discussion in Ronconi 2007, 125–142, and do not forget Browning 1960 for a caveat on late transliterations and the remarks in Lemerle 1971, 120 n. 40.

[61] A case in point is the Euclidean palimpsest *London*, *BL*, *Add.* 17211 (*Diktyon* 38926), ff. 49–53 (7th–8th c.), which contains fragments of Book X of the *Elements*. On a not so clearly defined practice of "destroying" [verb (δια)φθείρω] books alluded to by Photius, see Treadgold 1978. The depletion thesis was put forward in Dain 1949, 115; it is criticised in Ronconi 2007, 20–24 and 168–169.

why late majuscule manuscripts were doomed to disappear. Conversely, the depletion phenomenon explains why we have two manuscripts of Ptolemy's *Handy Tables* that can be assigned with certainty to the first half of the ninth century and two others that were copied towards the end of the century:[62] a "text" entirely made of numerical tables and their titles is much less sensitive to the selection effect induced by transliteration. In the context of the early ninth-century modes of production, a book containing just numeral letters and texts in *Auszeichnungsschrift* can only be penned in majuscule, so the distinction of minuscule/majuscule simply does not apply. Consequently, if the emphasis is put on the transition to the "new" script and the consequent enlargement of the book market, witnesses of Ptolemy's *Handy Tables* can hardly count as evidence. Still, as remarked by Timothy Janz,[63] one of these four manuscripts, namely, the above-mentioned *Vat. gr*. 1291, copied soon after the reign of Nikephoros I (802–811), is almost certainly the apograph of a now-lost (and possibly deliberately discarded, as just suggested) model transcribed during the reign of Constantine V (740–775).

This brings us to the core of our final reflection, which the following question can summarise: if what has been said is a plausible suggestion, how are we to explain that very early scientific manuscripts did not disappear, like so many other profane manuscripts did?[64] Well, because

[62] These later exemplars are *Laur*. *Plut*. 28.26 (*Diktyon* 16207) and *Marc*. *gr*. Z. 331 (coll. 552; *Diktyon* 69802).

[63] Janz 2003, 164–167. The date of *Vat. gr*. 1291 has been debated; the point are the changes of hand in the *Royal Canon*: an obvious change of hand occurs after Nikephoros I, and a less obvious one after Constantine V. Janz's paper seems to have settled the issue. Relevant previous literature includes Spatharakis 1978, Wright 1985 (who developed an observation by Ševčenko, 1992, 279). As Janz (2003, 160–161), rightly remarks, the astronomical data in the illuminated circular table on f. 9r of *Vat. gr.* 1291 can be used for dating the table itself, not the production of the manuscript. On this table, see Van der Waerden 1954 and Tihon 1993, 194–200.

[64] From our perspective, it is disappointing that Photius declares (545.13–14 Bekker) that he did not include summaries of common-use profane works and of those items that we might consider as school-textbooks in his *Bibliotheca*; see the factual analysis of Photius' work in Treadgold 1980. Still, the very fact that he declared that he excluded these works means that their accessibility was taken for granted. Thus, Photius did not summarise Nicomachus' *Introductio arithmetica* (which we read in about 100

they were copies intended for conservation—and this also explains their very small number: the "market" for conservation exemplars is exceedingly restricted.[65] This is somewhat confirmed by the fact that manuscripts like *Vat. gr.* 190 (*Diktyon* 66821), *Vat. gr*. 204, *Vat. gr*. 1594,[66] three of the above-mentioned witnesses of the *Handy Tables* out of four,[67] and so many manuscripts of the "philosophical collection"[68] do

manuscripts, none of them prior to the eleventh century), but he did summarise the lost *Theologoumena arithmeticae* of the same author (*codex* 187). Photius clearly states that "in our day, in geometry, arithmetic and the other sciences, as you know as well as I do, there are many among our acquaintances who have no less exact knowledge, I dare say, than the son of Hermias (for you of course know the skill of Ammonius in those fields), and none of the propositions that Nicomachus piles up together in his work on numbers would be obscure to them" (145a36–41): see again Treadgold 1978, whose translation we use.

[65] For scientific manuscripts, this remark is also made in Tihon 2017; for the "philosophical collection", see Westerink 1990, 123, Rashed 2002, 715, and Acerbi 2020b, 300–303, for *Vat*. *gr*. 1594, which belongs to both categories. For the manuscripts of the *Handy Tables*, this was clearly stated already in Usener 1898, p. 364, who referes to *Laur*. *Plut*. 28.26, according to him copied "iussu ac sumptibus aut ipsius imperatoris aut viri alicuius tunc primatis".

[66] With the tiny exception of *Vat. gr.* 1594, which contains a handful of corrections by a late tenth-century hand: Acerbi 2020b, 260.

[67] The exception is *Leid*. B.P.G. 78, but the sparse exegetical activity on this manuscript that can be assigned with certainty to the eighth and ninth centuries only comprises material attached to Ptolemy's chronological tables (the *Royal Canon*): these synchronisation tables are edited, together with the later scholia, in Usener 1898, 392–410 and 447–453; two further scholia are edited in Tihon 2011, 172 e 182; a synthesis of the chronological data that can be extracted from the scholia is found in Tihon 2011, 30–31 (dates 615/6, and a series from 775/6 to 812), or in Usener 1898, 364; for a discussion in our perspective see Acerbi 2020a, n. 17 at 589–590. Other chronological tables in *Leid*. B.P.G. 78, ff. 52r–53r (how to find the weekday of an assigned date) do not figure in the other early witnesses of the *Handy Tables*; they are almost certainly those mentioned by the emperor Heraclius in his supplementary chapters to Stephanus of Alexandria's commentary on Ptolemy's *Handy Tables*: text in Usener 1914, 311.4–6. These tables of the *Leidensis* are accompanied by a scholium that assumes 840/1 as a convenient epoch (nothing is said, contrary to custom, about the fact that this is the current year). The special tables for the latitude of Constantinople that Stephanus added to the *Handy Tables* (Usener 1914, 310.11–17) are contained only in *Laur*. *Plut*. 28.26 and in *Vat*. *gr*. 1291 (these are Tables **B** in Tihon 2011, 65 and 72).

[68] A lively debate has recently sparked about the very existence of the "philosophical collection": see Ronconi 2012 and 2013; Marcotte 2014; Cavallo 2017; Bianconi & Ronconi 2020.

not bear *any* sign of use prior to the twelfth century.[69]

But why were scientific manuscripts selected for conservation? A possible answer brings into play the other "concomitant phenomenon" mentioned at the beginning of this paper, namely, the Iconoclast Controversy. Among the reasonable criteria for selecting profane conservation exemplars, there are their being (1) imposing;[70] (2) illuminated and hence beautiful; (3) possibly incomprehensible so as to confirm that the imperial power is in full command of most arcane wisdom; (4) and generally related to such crucial issues as the control of time and celestial phenomena. Moreover, if one had to select illuminated manuscripts during the second iconoclast wave (814–843), there could have been no safest choice than scientific manuscripts, enriched by hundreds of totally harmless geometric diagrams; or a manuscript entirely made of totally incomprehensible tables, a codex that in the eyes of an outsider would have appeared as an aniconic book of wonderfully outlandish icons. And here we are: one exemplar of Euclid, one of the "little astronomy", a couple of *Almagest* and Theon's commentary thereon,[71] and a couple of *Handy Tables*. No need to read them, and hardly any need to open them unless in particular circumstances.[72] There

[69] Another example of this phenomenon is the Euclid in *Laur. Plut*. 28.3 (*Diktyon* 16184), penned *ca*. 960 by Efrem (Perria 1999) and bearing no sign of early scholarly activity. This is to be compared with *Vindob. phil.* gr. 31, a scholarly edition of Euclid set up towards the end of the eleventh century and enriched with an imposing and multi-layered apparatus of scholia (Pérez Martín 2017).

[70] Readers are urged to try to hold *Laur. Plut*. 28.18 (*Diktyon* 16199) using one hand only.

[71] Ms. *Laur*. *Plut*. 28.18 contains only Theon, *in Alm*. I–IV and VI, and Pappus, *in Alm*. V–VI, but a complete two-tome edition circulated as far as the end of the thirteenth century and was included for some time in the library of Pope Boniface VIII: Acerbi & Vuillemin-Diem 2019, sect. 8, *passim*. We remark that Gutas' list matches fairly well, as far as contents are concerned, the list of the Greek manuscripts in the Papal library: some items are, in fact, materially the same manuscript (certainly *Laur*. *Plut*. 28.18, and possibly *Vat*. *gr*. 204, *Marc*. *gr*. Z. 226 [coll. 615; *Diktyon* 69697], and *Marc*. *gr*. Z. 258 [coll. 668; *Diktyon* 69729]: see again Acerbi & Vuillemin-Diem 2019, sect. 8). This might not be coincidental after all. It may be that the selection criteria of conservation exemplars were the same in the East and in the West, unless one considers the Papal library as a mere repository of embassy gifts.

[72] A magnificent "stemmatic brother" of *Vat*. *gr*. 1594, namely, *Marc*. *gr*. Z. 313, was

has been no need, then, for an Arab intermediary in the production of these manuscripts because —and sadly so— there has been no scientific renaissance.

4. Winding up: The Ideological Bias

In Byzantine intellectual history, two concomitant phenomena have rightly attracted scholarly attention. Between the eighth and the tenth century, a massive effort to translate Greek scientific and philosophical works into Arabic was carried out.[73] Around the same period, particularly in the ninth century, a number of still extant scientific and philosophical manuscripts were copied; this was backed up by a relatively restricted number of scholars credited with an interest in scientific matters and, more generally, in literary writings of the classical era. Are these events related? According to Dimitri Gutas we must answer this question in the positive and in a clear-cut way: the former is the cause of the latter.

In his consequential *Greek Thought, Arabic Culture*, Gutas buttressed the long-standing thesis that the ninth-century "Byzantine Renaissance" resulted from an external input. According to Gutas' scenario, the Byzantine scholars of this period wished to emulate their Arab homologues or simply to provide the Caliphate with the manuscripts Arab scholars were looking for. As we have shown, however, Gutas' scenario is grounded on inaccuracies and on a problematic assessment of the available evidence.

Reviewing Gutas' scholarship on Byzantium, we found that ideology was a driving motive in some of his proposals. In recent publications, Gutas has repeatedly argued that the modern prejudice that sees Byzantium as an obscurantist society, inimical to science and philosophy, is not a prejudice but a historically sound and perfectly appropriate

probably used as an embassy gift and served (maybe by intermediation of an apograph) as a model for the Greco-Latin translation of the *Almagest*: see most recently the discussion, with bibliography, in Acerbi & Vuillemin-Diem 2019, 125–128, 144, and 162–163.

[73] For an overview of the translation movement, see also D'Ancona 2005, 180–258.

assessment.[74] The reason is that the Byzantines were Christians, and Christians, by nature, cannot philosophise or apply themselves to science.[75] Unsurprisingly, Gutas calls the "orthodox" (*sic*) approach to science "cultural schizophrenia", and on this basis he argues that Byzantium was as an essentially Christian society inimical to science and philosophy.[76] In short, according to Gutas, the Byzantines merely preserved the classics for the later generation of Renaissance scholars;[77] modern scholars who think otherwise do so out of political correctness.[78] Unsurprisingly, Gutas has sometimes exacerbated his harsh judgement: not only must the Byzantines be dismissed as mere transmitters of Greek writings, butthey must be blamed for failing to preserve more of the works that went lost between Late Antiquity and the ninth century.[79]

Gutas' approach in *Greek Thought, Arabic Culture* differed from the just-mentioned negative appraisal of Byzantium. Whereas the latter exemplifies, so to say, a "diachronic" kind of prejudice against Byzantium, which considers the Byzantine civilisation as a mere bridge between the classical world and modernity, *Greek Thought, Arabic Culture* exemplifies a different, "synchronic" prejudice. According to this approach, Byzantium must be evaluated compared to the developments in contemporary neighbouring cultures. If, according to the diachronic prejudice, Byzantium is only seen as a repository of the classical past, according to the synchronic prejudice, Byzantium is only considered as reflecting developments that are not its own, but were triggered by an external catalyst. This is more than evident in Gutas' narrative. According to him, Byzantium was an intellectual wasteland, and the few good things that the Byzantines produced (like the ninth-century scientific and philosophical manuscripts) must be considered as induced by cultural developments in neighbouring civilisations rather than the result of Byzantine efforts. According to Gutas' "financial

[74] See *e.g.* Gutas & Siniossoglou 2017, 295.
[75] See *e.g.* Gutas & Siniossoglou 2017, 292–293.
[76] Gutas 2012, 249.
[77] See *e.g.* Gutas & Siniossoglou 2017, 295.
[78] See *e.g.* Gutas & Siniossoglou 2017, 271.
[79] See *e.g.* Gutas 2018, 31.

explanation", these manuscripts were simply produced to be sold on the market. The present paper shows that these views, which eventually result in uprooting Byzantium, are unfounded.

Recent research allowed a different understanding both of Byzantium in itself and of Byzantium in comparison with neighbouring civilisations,[80] and disproved the approach described so far as purely ideological.[81] Precisely these studies allow us to differentiate in a clear way between Byzantium and the modern perception of it. By contrast, the results of Gutas' biased approach are there for all to see. Leaving aside inaccuracies and methodological flaws, the amount of manipulations therein calls for a new—and ideologically unbiased—appraisal of the relationship between Byzantium and the Caliphate. Since these were not isolated or hermetically sealed societies, they must have had a cross-cultural relationship. While leaving to future scholars the task of assessing the nature of this relationship, the present paper shows that the data presented by Gutas to identify the Islamic roots of the so-called ninth-century Byzantine "renaissance" do not prove the point.

By the same token, we would like to address students of Byzantium as well. Scholars who appeal to the manuscript evidence from this period in order to support the idea of a strong discontinuity between the ninth century and the earlier period should be careful in avoiding the collateral damage consequent to adopting the ambiguous notion of "renaissance". As the ninth-century manuscripts discussed in this paper bear little or no trace of use by contemporary scholars, employing these artefacts as evidence of a cultural renaissance in Byzantium is problematic.

Let us conclude with a historiographic remark. When reconstructing the historical origin of Gutas' thesis, it was amusing to note how the *topos* of preterition dominates this scholarly debate: no one mentions the names of their opponents. On the first page of their analysis, neither Lemerle nor Gutas refers to earlier literature. Lemerle introduces the thesis of the "relais syro-arabe" by means of an impersonal "[o]n s'est depuis longtemps demandé si …", and the reader must await five full pages before being provided with a clue allowing the guess that the

[80] See for instance Mavroudi 2015 and 2020.

[81] See the essays collected in Lazaris 2020.

polemical target is Bertrand Hemmerdinger. Gutas simply erases any trace of his predecessors; he just mentions "the theories that had been proposed about Arab influence" in his short rebuttal of Lemerle's thesis.[82] But who advanced first the thesis rehearsed by Gutas? Apparently, it was Edward Gibbon (died 1794). In his *The History of the Decline and the Fall of the Roman Empire*, he writes:[83]

> In the ninth century we trace the first dawnings of the restoration of science. After the fanaticism of the Arabs had subsided, the caliphs aspired to conquer the arts, rather than the provinces, of the empire: their liberal curiosity rekindled the emulation of the Greeks, brushed away the dust from their ancient libraries, and taught them to know and reward the philosophers, whose labors had been hitherto repaid by the pleasure of study and the pursuit of truth.

Gutas' thesis shows how pervasive Gibbon's views still are in modern narratives on the Middle Ages.[84] Apparently, some modern scholars lend credence to Gibbon or, like Gutas, presented Gibbon's view as an innovation of their own.

[82] Quotations from Lemerle 1971, 22, and Gutas 1998, 178, respectively.

[83] Gibbon 1788, ch. LIII, 512.

[84] On this topic, see Runciman 1976.

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