

# HOW TO EDIT ARISTOTLE IN SYRIAC

Response to Arzhanov

DANIEL KING

CARDIFF UNIVERSITY

The history of textual criticism is full of sometimes surprisingly tortured debates over matters both of principle and of detail. Since textual criticism aims at a point closer to pure science than does most research in the humanities, opposing notions of what is the “right” analysis and what the “wrong” may from time to time achieve a certain pointedness that other, greyer, areas of research do not. Even textual criticism is not a pure science; an element of creative judgement is *sine qua non* in the application of its principles, yet its principles are based on sound logic, and false applications of them can feasibly be detected and unmasked.

As early modern readers of Horace discovered to their cost, to reconstruct an autograph out of a tradition that is already corrupt at its outset, is at the very least an exercise in searching out tracks in a wilderness. The oasis is there somewhere, but the tracks have been so far obscured by the sands as to be discoverable only by the most tortuous methods. Those methods, however, are not unknown—they are the basis of the criticism of textual traditions and the descent of manuscript readings, and so the trackless waste is not a hopeless place. Good principles must be followed, and the evidence permitted to draw us where it will. Moreover, most textual critics are a good deal terser in expression than is the norm in the meandering ways of the more discursive branches of

humanities. All the data is presented; but the reader must be willing to expend the effort to interpret it aright.

Some Syriac texts, especially the more technical philosophical translations from Greek originals, are so muddled that it is hard for the reader to judge whether an obviously faulty text should be ascribed to the incomprehension of an over-ambitious translator, the forgivable mis-readings of a confused scribe, or even the well-meaning but quite hopeless attempts at "correction" by the next tradent in the succession. In any case, the modern editor will be faced with some fundamental questions to which (s)he must offer some semblance of an answer, in order to produce something serviceable for future students. The only other option is simply to reproduce manuscripts, add a few remarks, and let readers make of the corrupt text what they will.

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I am wholeheartedly thankful to Yury Arzhanov for the comments and observations that he offers in the article preceding the present. To tell the truth, it is only when editing a text for publication that one comes squarely face-to-face with its problems and failings, and at this point the editor cannot avoid proposing solutions to these problems unless (s)he would prefer to go and do something else instead. This is why Arzhanov is especially well-suited to writing the article in question, since it arises not merely from his own reading of my edition of Aristotle's *Categories*, but out of preparations for his own digital edition. The issues that he raises emerged quite naturally and organically from his task, and for this reason they are to be welcomed. Through such a dialectical process, we may all progress.

It must be stated clearly, however, that Arzhanov's approach and aim as a textual critic are quite different to my own. He entered upon the business of producing a diplomatic edition of a corrupt ms, I of offering a critical edition. The former may be

used with profit by those sifting through the textual traditions of the Syriac and Arabic versions, and by those who have the ability silently on their own to correct the many and egregious corruptions in the text; the latter, however, may be used by those who rely on an editor to make reasonable judgments about what the author actually wrote. The diplomatic text is, in my view, a defunct goal in today's world of digitised and easily-available manuscripts; the critical reconstruction of a corrupt tradition, by contrast, is the only goal that can offer wider benefits in allied fields of research.

We are dealing then with a fundamental difference of principle. Arzhanov unfortunately often mistakes what are in fact matters of principle for mere issues of detail. He sincerely believes that he is critiquing some matter of detail because he has misconstrued the nature of that fundamental difference of principle that I am talking about. His article contains a number of fruitful corrigenda to my own edition, which I gladly receive and approve. But he too often appears to think that these testify to a failure of principle, or to confusion between different goals, whereas in reality the confusion is on his side of affairs. I shall lay out the evidence for this in the rest of this article, which thus presupposes at all points a close awareness of the issues raised by Arzhanov, and can only be read in close concert with his essay.

## 1. GUTAS ON THE PRINCIPLES OF TEXTUAL CRITICISM

Let us begin with Arzhanov's summary of Gutas's "principles"—they may not seem especially momentous comments, but their significance will become apparent. Arzhanov somewhat misrepresents the complexity of Gutas's system. What Gutas

says<sup>1</sup> is that “non vert Ar” indicates that the editor is sure that the word(s) were present in the Vorlage and were for some reason not translated; whereas “om Ar” is neutral, it indicates only word(s) that are not present in Ar—in these cases the editor does not pretend to know the reason. Hence “om Ar” is a neutral description of a collation of one text against another. I do not pretend nearly as much as Gutas to guess the intent of the translator but, in any case, “om A” in my edition is a similarly neutral expression, indicating that a word is missing in the ms by comparison with the printed page. This is also how it is used quite normally in classical text editions. Whenever in a very corrupt tradition an editor needs to make many conjectural emendations of this type, then naturally many “om” items will appear in the apparatus. This fact does not prejudge how those errors arose.

Arzhanov appears on more than one occasion to want rather more from the editor than I am willing to commit myself to. He always seeks clarity about the *cause* or *source* of a particular textual error or corruption, whether it be attributable to an otherwise unknown Greek variant, or to a translation error, or to a scribal error within the Syriac transmission. For him, it appears to be a failing of the edition not to commit itself to an explanation.

Yet the simple answer is that, in the vast majority of cases, the precise cause of corruption is unknowable. The edition lays out the data; editors may offer an hypothesis as to the precise cause of corruption only when they are reasonably certain; otherwise they limit themselves to the presentation of the data. That is what I have done. To have done otherwise would be to go beyond the data. Of course, future students with greater

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<sup>1</sup> D. Gutas, *Theophrastus On First Principles (Known as his Metaphysics)*, Brill, Leiden & Boston 2010 (Philosophia Antiqua, 119), p.99.

acuity than I will take this data and themselves suggest possible causes of variants.

In this connection, two points must be carefully noted and emphasised:

1) that I only make emendations on the basis of *extant* Greek readings *known* from Greek mss [i.e. I do not imagine a Vorlage that is otherwise not extant]. This is a crucial principle. Just as Gutas pointed out, having a good knowledge of the Greek tradition *does* help in the reconstruction of the original text of the Syriac translation, because it offers variants which sometimes allow us to gain insights into the mechanism of error within the Syriac tradition. Speculating that unknown Greek variants might once have existed as a means of explaining errors in the tradition that are more easily explained in other ways is fruitless. Examples are offered below.

2) that emendations to the text were made only in cases where I believed it was clear and obvious what the translator had written, and where the mechanism of corruption can be plausibly reconstructed. The fact that we have only a single ms witness to this text does not alter this principle.

## 2. TWO DIFFICULT POINTS

### i) The A/A\* sigla

This section of Arzhanov's article announces its own superfluity from the outset, for he writes: "...these two points may potentially lead the reader of the book to confusion." I suppose they may do so in the case of a reader prone to confusion of principles. But for the reader who is not easily confused, they will not. And I take it that a reader who has reached p.257 is probably not to be easily confused.

But, briefly, for the benefit of the confused: in the general commentary (pp.170-256) there is no need in principle to distinguish A from A\* since the commentary presupposes the reconstructed form of the text, which can simply be labelled A. This is what is supposed (by a degree of subjective editorial emendation) to have come off the translator's desk. In the textual notes (p.257-282), A\* needs to be used as an additional siglum to indicate the evident differences between the reconstructed autograph (A) and the readings of the ms (A\*).

To take briefly one of examples Arzhanov offers (p.498):

on p. 212 we read: "6b36-7 [80vb] A omits any equivalent for δόξει." Since A remains undifferentiated on these pages, it is unclear whether the commentary refers to the state of the manuscript (i.e. A\*) or to that of the translator's copy (i.e. A).

It is hard to conceive what could be unclear to him. "A" means only what it always means, viz. the reconstructed text of Anon as printed in the book. When I state that A omits any equivalent for δόξει, it means just that, that there is no equivalent in the edited text. I do not pretend to know the reason—hence there could be no A/A\* distinction here. Because 1) the ms text makes sense as it is, and because 2) I can see no obvious mechanism of scribal corruption, there is no call for any conjectural emendation. A glance at the apparatus will make this much clear. My commentary is a commentary on the edited text and requires the reader to have the text open. Of course, a critic is most welcome to disagree with the editor's judgement, but to say that "it leads to confusion in the analysis of stage 3" [to use Gutas's terminology] will not be true for any reader who follows the required logic. The principles here in question are sound and in no way confusing. What I am thankful for is Arzhanov's discovery of the occasional error—e.g. that the comment on p.178 relating to 1b8 ought to have been placed in the textual notes and not in the commentary.

It is clear enough that much of this arises rather from Arzhanov's personal preference for the "security" of a diplomatic edition than from any failure of principle on the part of my critical edition. The term "om[ittit]" has throughout a purely functional role. A critical text prints the editor's reconstruction. The apparatus offers the wording of the ms collated against it, hence, e.g., when "horse" must have been part of the translator's autograph (1b5), the apparatus must read "om." since the ms does not read "horse." Arzhanov's edition reads <ܠܗܝܬܐ>, because that is what a diplomatic edition does. But this approach offers no benefit over and above just digitising the manuscript, since the emendation is clear and obvious.

## ii) The relation of Anon to J and G

On p.497, Arzhanov asserts that I established a dependency relation between the three versions. Dependency is a rather slippery term and is not, in fact, one that I used. I described J and G as "revisions" of Anon. On p.80 I provide evidence for this, especially some interesting translational errors which J and G both took from Anon in the early passages of the text and corrected further on. Of course, there is no question that J and G are in many ways very different from Anon. Moreover, it is abundantly clear that they both used Greek copies of *Categories* different from that which provided Anon with his Vorlage (not being an edition of J or G, this book does not exhaustively treat the question of the Greek variants represented in them), and it is equally clear that they used many quite different expressions and translation equivalents. At the same time, I believe it well-established that they knew Anon and made use of it. This permits us to use the expression "revisions" but not "dependency" – it is not clear to me quite what the latter term would entail in Arzhanov's schema.

What is of far greater moment, however, is that this relationship—whatever one calls it—does not in the slightest degree imply that J and G have become, for the editor, “additional witnesses to the textual tradition represented by Anon that help us to reconstruct the ‘translator’s autograph’ deriving from it” (p.497). How could they possibly have any such role? It would as absurd as using an incunabulum of Catullus to reconstruct the archetype of the Verona manuscript. J and G have significance only insofar as they reveal to us the lines of the reception of Aristotle in the seventh/eighth century. They in no way contribute to the reconstruction of Anon’s autograph. They do not have that role in my edition; nor, I think, in his.

Arzhanov’s unnecessarily long foray into J and G and their specific characteristics concludes limply: “What a reader of the book finds is a combination of the analysis of Anon with that of J and G, which makes an *impression*...that J and G are mere witnesses of the Syriac text of Anon” (p.501, my italics). It is singularly unfortunate that Arzhanov falls under such a false impression. In truth, neither J nor G are anywhere used in any way to assist in the reconstruction of Anon, nor are they used as a source of “good variants.” To have used them in such a way, even a little, would have been a manifest transgression of principle, and I never entertained it for a moment.

Arzhanov’s own analysis of the nature of the relationship between Anon and JG is reasonably accurate, and is more-or-less in line with my own conclusions; the actual (non-)role of JG in my own efforts to reconstruct the text of Anon is, on the contrary, abundantly clear. Just how my edition “does not allow the reader to distinguish between their witnesses, both in the analysis of A/A\* and in reconstructing the Greek text behind it” (p.502) will remain a mystery to any reader willing to follow the logic of the case. The use of sigla allows the reader to make precisely those distinctions between witnesses that Arzhanov



seems to find so difficult. He can offer no single example where my reading of JG has affected my conjectural emendations of the text of Anon. All that is left is his misplaced “impression.”

It would be wholly otiose even to embark upon a defence of my using the Bekker referencing system for a text of Aristotle (p.502) – to have done otherwise would have been pretentious and unnecessary. In the same paragraph, he proffers the extraordinary suggestion that “the commentary...creates a number of difficulties for readers who do not have the Greek text of the *Categories* in front of them.” Indeed I would be shocked if a reader of the commentary *without* the Greek text in front of them did not encounter very many difficulties! Is a reader of a commentary expected now *not* to have access to the text itself?

### 3. NOTES ON THE SYRIAC EDITION

In this section, Arzhanov suggests that the “edition does not always provide sufficient information on ‘what is known’” (p.503). However, this criticism is otiose—the emendations concerned are minor matters such as punctuation and syame points – it is quite normal in Syriac editions not to note in the apparatus these minor editorial interventions.

He further suggests the improvement of using “angle brackets <...> that would denote additions made to the text of the manuscript” (p.504). In fact, such a method would be misleading. Angle brackets usually indicate the filling up of an actual lacuna in the ms (e.g. words that are hard to read or where there is an illegible part of the page). On the other hand, angle brackets to indicate conjectural additions would give them a significance over and above other sorts of conjectural emendation, such as deletions and reordering, which would then become down-graded as against additions. Arzhanov is again offering nothing more substantial than a description of

the difference between his own preferred ‘diplomatic’ method and my ‘critical’ method.

The section ends with a list of errors in the apparatus. These are welcome as corrigenda. They are not, however, errors of principle, as Arzhanov seems to think, and these errors would still be errors under any other principle of editing. They are corrigenda pure and simple, and can be corrected in any future edition.

#### 4. EMENDATIONS VS CORRECTIONS

Here we come to what may be the clearest evidence of how Arzhanov and I differ rather substantially in our approach to the critical treatment of a corrupt tradition. This evidence shows how he is both more reticent in his editorial method (for he prefers to print the ms rather than to offer emendations) and yet also more reckless (for he offers as possible causes of error some extremely improbable suggestions and on this basis retreats into a thoroughgoing skepticism).

A rather anodyne example will make the general point clearly enough. Arzhanov writes (p.528):

104.26 ~~ⲉⲡⲉⲓ~~ ms : ~~ⲉⲡⲉ~~ ed. — The editor’s variant is based on the fact that the transmitted Greek text has 2nd person sg. However, it remains possible that the Syriac translator 1) had another variant in his Greek copy, or 2) preferred to change it for some reason.

In fact, these two alternative suggestions are barely worth a moment’s consideration. All known Greek witnesses read ΕΡΕΙΣ (*you say*). Moreover, this is a standard piece of Aristotelian style which offers the translator normally no difficulty and he elsewhere always translates it correctly. Arzhanov’s suggestion (1) that the translator had a different Vorlage is extremely unlikely (ΕΡΕΙΣ > ΕΡΩ is a most unlikely error; and I do not believe in speculating Greek variants not otherwise attested in the tradition, although Arzhanov seems

happy to posit a new Greek variant for almost any reason to exculpate the Syriac scribes). The suggestion (2) that it was a *deliberate* alteration on the part of the translator is even more absurd, since no conceivable reason for this can be imagined and in no other location does he make any comparable alteration. Hence any editor would come to the same, rather obvious, conclusion as I did, viz. that this is most plausibly understood as a scribal error.

What should become at once apparent from this somewhat insignificant, but still telling, example, is that Arzhanov is much the *more* speculative editor of the two of us. I have opted for the most likely explanation, viz. scribal error, and emended the text accordingly; he speculates about unknown Greek variants and deliberate translational alterations, both without any evidence; and then decides to leave the text as it is on the basis of the consequent uncertainty regarding the source of the error. In my view, this is at once to over-speculate about the unknown and to surrender the responsibility of the careful editor.

Here follow a selection from the other examples that he offers (pp.508-512), demonstrating in each case the basic soundness of the principles of my edition, and the superfluity (sometimes absurdity) of his doubts:

104,33 ܡܫܠܚܐ ms : ܡܫܠܚܐ ed. The transmitted text stands in need of correction and the emendation is required for the grammar. The Gk text here is not in dispute and the mechanism of error is reasonably clear, viz. the translator, eager to preserve the Gk word order, translated the genitive τῶν δευτέρων οὐσιῶν at the start of the sentence; a later scribe had trouble with the resulting unnaturalness and has omitted the ܐ, thereby confusing where the “secondary substances” belong, in the foregoing or the succeeding sentence. Arzhanov’s contention that “the editor’s correction is...based on the assumption that the Syriac translator was trying to render the transmitted Greek text in a way that would be close to the Greek” is accurate. The

translator was no fool; in fact I generally proceed on the basis that he was very good at his job—anyone translating Aristotle would need to be, and this translator did not knowingly write nonsense. Errors are therefore, *a priori*, far more likely caused by an uncomprehending scribe than by a translator who certainly *did* understand what he was translating. Moreover, the *mechanism* of error is clear once one sees the (not-in-dispute) Vorlage—this is to follow Gutas’s principle of using our knowledge of the Greek text wisely and critically.

106,10 ܠܝܢܐ ms ܠܝܢܐ ed. Arzhanov’s objection is superfluous. The translator nowhere else writes ܠܝܢܐ, but always ܠܝܢܐ. The evidence is clear and the required emendation obvious, once it is established that the tradition is anyway corrupt.

106,24 ܠܝܢܐ ms ܠܝܢܐ ܐ ed. Contra Arzhanov, the grammar cannot stand without the particle. He offers ܐ as an alternative. Perhaps. Indeed, suggestions for alternative emendations to the ones I offered are welcome, but one or other is required.

110,11 ܐܡܐ ms ܐܡ ܐܡܐ ed. Again, extraordinarily, Arzhanov seems to think that positing a speculative and imaginary Greek variant would be a *better* explanation for the corrupt transmitted text than inner-Syriac scribal error. He suggests the evidence is not decisive – that there are other possibilities. Naturally no evidence will be wholly decisive in the case of a corrupt tradition, but the question at hand is one of the balance of probabilities. Any critic is more than welcome to differ in the application of the critical principles—but the principles themselves remain secure.

116,33 (not 32) ܐܠܐ ms ܐܠܐ ܐ ed. Arzhanov is wholly misguided in thinking that I am in any way using JG to emend Anon. I add “cf. JG” to alert the reader’s notice to a datum (*confer* simply means “take a look at”) – it in no way implies that I am using JG as a basis for the emendation. The reason for the

emendation is the same as in all the other cases, viz. that the text is corrupt. The Greek surely read *τούτων δὲ οὐδέν...*, and elsewhere this would always, by this translator, be rendered as ...ܐܠܐ ܕܐܝܢܐ ܕܐܝܢܐ. The mechanism of corruption is more than tolerably apparent. That “Anon was not consistent and modified the Greek original” (Arzhanov, p.509) is an absurd speculation based on no comparative evidence. On the contrary, without ܐܝܢܐ the Syriac text makes no sense; the intent of the overall sentence is abundantly clear; there is no possibility of a Greek variant (were one known) explaining the transmitted Syriac text. In all, only one sensible solution offers itself to any reader, namely that ܐܝܢܐ has dropped out in transmission (a ready and likely error for a scribe not accustomed to the material). The editor’s responsibility to insert ܐܝܢܐ is thus incontrovertible.

118,14 ܐܝܢܐ ܕܐܝܢܐ ms : ܐܝܢܐ ed. This is another instance of the same balance of probabilities. The mechanism of error, together with awareness of the translator’s style and the Greek Vorlage, combine to offer the editor a good idea of how the transmitted text has emerged. Arzhanov’s complaint appears to be that there is less explanation on the editor’s part than he would like. For each and every textual decision to be explicated in full would be a task beyond the confines of a single volume.

120,8 ܐܠܐ ms ܐܠܐ ed. Being in possession of the Vorlage allows the editor to perceive readily enough the mechanism of corruption. But Arzhanov’s predilections have again got the better of his critical sense, for he writes, “since the Syriac translator is not always consistent...it seems better to have the ms variant in the main text.” This is a non sequitur. On Arzhanov’s logic, since no translator is ever consistent and most are rather the opposite, we should never do anything but reproduce the text of a ms—why not just make it available online and retire? The translator’s inconsistency does not at all entail that we surrender the critical task and just print the ms text—the two things have nothing to do with one another.

140,5 ܠܐܡܬܐ ܡܬܬܬܝܢܐ ms ܠܐܡܬܐ ܡܬܬܬܝܢܐ ed. Arzhanov's suggestion is quite correct here, the emendation should have been ܠܐܡܬܐ ܡܬܬܬܝܢܐ. He is quite right to emend the transmitted text.

144,6 ܠܝܬܬܐ ms ܝܬܬܐ ed. The received text is not impossible, but requires us to take a very low view of the translator's capacity. Since there is a fault, far more likely that the culprit be a scribe who knew not the meaning of what he was writing, than the translator who clearly did.

150,23 ܠܡܬܬܬܝܢܐ ms ܠܡܬܬܬܝܢܐ ed. The ms reading cannot be a translation of ἀπόφασις—it would have quite another meaning. By contrast, Anon *always* uses the loan word for ἀπόφασις. There is a close visual similarity between text and conjecture, a clear mechanism of corruption, and the translator's normal equivalents are well-established. In my judgement, there is an almost zero chance that the transmitted text is what the translator wrote.

162,25 ܬܬܬܬܬܬܬܬܬܬܬ ms ܬܬܬܬܬܬܬܬܬܬܬܬ ed. Arzhanov admits that the proposed emendation is probably right, yet still refuses it entry into the critical text because 1) the Gk Vorlage could have been different from anything attested in the Greek tradition; 2) because the translator might have interpreted it thus. However, the transmitted text is nonsense and no translator who could even half understand Aristotle would have written ܬܬܬܬܬܬܬܬܬܬܬܬ. *Not* to place the emendation in the text would be a total abdication of editorial responsibility and a retreat to a quite different principle, namely that of reproducing the ms with only the most minor alterations, i.e. the diplomatic edition that Arzhanov prefers and which he prepared for Hunaynnet.

164,2 ܠܝܬܬܐ ms ܠܝܬܬܐ ed. Arzhanov accuses: "it would seem that by changing the Syriac text, the modern editor is correcting not the errors of textual transmission, but rather the Syriac translator of the 6th century." Not in the least. The editor is correcting what is manifestly a scribal error. Arzhanov's logic

is faulty. Correcting the scribe is precisely what is being done, for such is the editor's task. If Arzhanov would like to ascribe the error to incompetent translation, he had better find some good evidence first.

The comments about the use of square and angled brackets (p.512) shows a lack of understanding of how these instrumental work. Arzhanov expects that angled brackets would be used to indicate conjectural additions (I have explained above why this ought not to be the case) and seems to be under the illusion that square brackets are used to indicate textual conjectural additions as well. In fact, in line with most English cribs of Syriac texts, square brackets indicate words added to ease the flow of a translation and have nothing to do with the critical work on the Syriac text itself.

Throughout these examples, the paradox of Arzhanov's position is that his non-emendations require far greater speculation than my own conjectures—for *he* must always be forced either to invent a Greek variant without attestation to account for a strange Syriac translation, or else he must posit a translator who had little idea what he was doing and simultaneously a scribe or scribes who had a rather better idea. By contrast, I never make emendations based on imaginary Greek readings, and I emend what may be adjudged to be scribal errors wherever the mechanism of error can be discerned, minor errors in transmission being *prima facie* more likely than imagining a translator who knew even less Greek than he did Syriac.

## 5. CONTEXT OF ANON

"Anon turns out to be a highly unstable version that frequently changes its use of terminology and translation technique; its author (or authors) seems frequently to have had difficulties in grasping the precise meaning of the Aristotelian text; and it

seems to be possibly based on an imperfect copy of the Greek text" (p.513).

Not at all. There is no evidence that the translator's Greek copy was any more defective than any normal ms would have been. Arzhanov offers no evidence to the contrary. Moreover, as I demonstrated in the book, the translator had a good understanding of a difficult text. He did not yet have a stable tradition of Syriac renderings with which to work, and this is the explanation for his inconsistencies—they are the inconsistencies of an experimenter, not those of a confused tyro. This would suggest that, far from being the product of a pre-existing pedagogical tradition, this translator was working in something of a vacuum. He was inventing a field largely *de novo*, and was remarkably successful.

## 6. THE CASE OF J

Finally, Arzhanov offers a beneficial analysis of the tradition of Jacob's translation, something I only touched upon in the book by way of an appendix. A future analysis needs, however, to check again that the errors of MV are also found in BP. There are (see p.287) a few shared MV errors; this "should" mean that these errors are found throughout the tradition back to [ω] and hence "must" be in BP as well. If BP have any right readings where MV are in error, then the stemma will need further rethinking. But this is only a minor suggestion.

## SUMMARY

Housman pointed out that "people come upon this field bringing with them prepossessions and preferences; they are not willing to look all facts in the face, nor to draw the most probable conclusion unless it is also the most agreeable



conclusion.”<sup>2</sup> The easiest way to treat the unicum ms of the Syriac *Categories* would have been always to assume that the translator was at fault and that the text as transmitted, barring only the most obvious errors, is in all major particulars the text that came off the desk of the translator. Were it so, we could confidently reconstruct his Vorlage and also describe in some detail his technical failings. However, it was not so. The text was poorly treated in the years between original translation and extant copy. The translator, albeit that he was an experimenter and a pioneer, had a strong grip on his task and executed it remarkably well. This places the onus on any editor to make every effort to present to the wider reading public, and especially to editors of the Greek Aristotle, the actual words that the translator most likely wrote, and this requires more conjectural emendation than is found in many other Syriac texts. An editor who surrenders that responsibility might as well just publish an online digital copy of the manuscript.

Nonetheless the presentation of the material does not, in the event, rely upon this conclusion. The text presented in the edition is, as with any critical edition, a reconstruction based on editorial judgement. The data needed to analyse this editorial work is all present in the footnotes, tersely yet clearly laid out. Where there are errors in the edition, corrigenda are to be welcomed. They are not errors in the principles being applied. Editors such as myself also warmly welcome variant judgements on individual emendations—indeed this is how knowledge progresses.

But Arzhanov has mistaken one species of criticism for another. Having found my *critical* edition a difficult instrumentum for the realization of his own goal of producing a

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<sup>2</sup> A. E. Housman, “The Application of Thought to Textual Criticism,” *Proceedings of the Classical Association* 18 (1921), 67–84, at p.71.

*diplomatic* edition of the same text, he has misread both its principles and their application.