Discussion Notes

Response to the Review
by Silvia Luraghi of Greek Prepositions:
From Antiquity to the Present

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This is a necessary response to the “review” of my 2010 book, Greek prepositions: from antiquity to the present (Oxford), published in the last issue of the JGL (11.2, Autumn 2011) by Silvia Luraghi of Pavia. Silvia Luraghi’s central criticism is that in my book (Bortone 2010) I did not consider her recent publications on the same subject. She proclaims that my “reason for doing so escapes understanding”. There was a serious reason, and I had been foolishly kind in not making it public before. I shall have to put it on record now. I will also show that her other complaints are groundless.

In 2000, I completed a doctorate at Oxford on “Aspects of the history of Greek prepositions”, which was then formally deposited at the Bodleian Library. In 2001, I presented part of my work at a conference in Paris. In the audience was Silvia Luraghi, who told us afterwards that writing a book on the history of Greek prepositions was her dream. Since she admitted knowing neither Medieval nor Modern Greek, I wondered how she could achieve that (the Medieval and Modern periods were what I had especially emphasised in my doctorate, both because they were the most informative phases and because Ancient Greek had already been extensively written about). I simply replied to Luraghi that I had just finished writing such a book, and I briefly showed her a copy of my unpublished doctorate. She was dismayed. She then insistently requested
a copy of it. There was something in her tone that made me uncomfortable, so I evaded her request. She asked me again in an email, admitting that she “needed” my doctorate for what she wanted to write. She pointed out that she had discovered that I had given copies of it to others, so I had to give in. I replied cordially and I sent her a copy. She emailed me acknowledging that she had received it (20 March 2002), and asked me if I could provide her with more data. I never heard from her after that.

In 2003 I was asked to peer-review an article on the Greek preposition *metá* that Luraghi had submitted to a major journal. Virtually every word in the abstract of her article repeated things I had pointed out in my doctorate. To my further surprise, her article covered also Medieval Greek and Modern Greek. Elements of my (then unpublished) work quietly appeared throughout the article with no reference to me. A paragraph in Luraghi’s article that started with “I will argue” rephrased, from beginning to end, what I had said in my doctorate (e.g. Bortone 2000: 222). Even if, by some unlikely chance, Luraghi had really received the same training, done the same work, found the same facts, and reached independently the same conclusions as I had, years before her, she had an ethical duty to acknowledge that I had done all of this first and I had written it first—as she knew, since she had read it all in my doctorate. But in her article my name only made two token appearances in connection with marginal points. Her article also contained several statements that presented as original suggestions facts known for centuries, such as that, in Ancient Greek, the genitive with prepositions has both ablative and partitive meaning. To such statements Luraghi regularly attached bibliographical references only to herself. Even on subjects much debated in recent years, such as case syncretism, the references were only to her own publications.

Luckily, the editor of the journal already had a copy of my doctorate, and having seen the numerous and extensive parallels between my doctorate and the article submitted by Luraghi, he recognised, as he wrote to me, “just how extensively she duplicates and/or draws on your material and without attribution”. He described Luraghi’s submission of that article as “very disturbing”, and thanked me for having averted the “travesty” that publishing Luraghi’s article would have been. The journal did not publish Luraghi’s article in any form.

At the time, I decided, naively, not to publicise this incident. I simply resolved to ignore Silvia Luraghi’s publications from then on, even when I reworked my doctorate into a book. Luraghi, however, went on to publish her article, virtually unchanged, in a different journal, not exclusively dedicated to Greek (Luraghi 2005). *That article is one of the articles that, in her “review”, she chastises me for not referencing in my book.*

Much as I try, I do not see how I could be expected to quote in my book people who repeat my own ideas as theirs; furthermore, if I had done so, I would have
given the impression that it had been me who had been inspired by Luraghi, or that the content of her article preceded my work, both of which are false.

Nonetheless, Luraghi's complaint, in her “review” of my book, that I ignored her recent publications, has induced an innocent scholar to think, and state, that she had dealt with certain aspects of metá before me (Bubenik 2011: 295), as if I had followed on from Luraghi's work without properly acknowledging it—exactly the reverse of what had happened.

Luraghi's “review” even attempts to justify her assertion that I should have cited her recent publications by claiming that “more exhaustive reference to the existing literature could have prevented Bortone from various errors”. I will return to that claim shortly.

In 2003 Silvia Luraghi published a book on Greek prepositions. She included (e.g. Luraghi 2003: 41) remarks on the semantic development of prepositions in Medieval and Modern Greek that made no reference to me and my pioneering work in the area. She also wrote (Luraghi 2003: 187), as she had done in the article that the first journal rejected, that the preposition diá developed a beneficiary meaning in Medieval Greek—but without quoting me, although in 2002 she had even emailed me saying “I would like to ask you a few things on the use of diá in Byzantine and Medieval Greek, which I do not understand from your book. You say that at a certain moment diá acquires the meaning of Beneficiary”. In the course of Luraghi's entire book, my work only got a couple of incidental and insignificant mentions, notably in small-print endnotes such as: “There are no comprehensive studies regarding the development of prepositions in Byzantine Greek. Jannaris (1897) has some data, but is largely outdated. Some information can be found in Browning (1983) and Horrocks (1997); see further Bortone (2000).” Luraghi also put in her book a drawing that I had made and included in my doctorate (Bortone 2000: 183, Luraghi 2003: 166); she referenced it in such a way that readers again thought—as I saw in an unpublished paper I peer-reviewed recently—that Luraghi was the first source. Luraghi concluded her book (Luraghi 2003: 315) stating, just as I had done in my doctorate, that the main trend shown by Greek proper prepositions is that they lose spatial senses and lose dimensionality. She did not reference my work on this, although my work was earlier than hers, and although I had demonstrated those conclusions (among others) with much more extensive, more systematic, more original evidence. The very last paragraph of her book (Luraghi 2003: 332–333) summarized the very thrust of my doctorate, but with no reference to my work. This is the book of hers that, in her “review”, she claimed that I had no reason not to quote.

As a result of her claim, even a reviewer who liked and praised my book (all reviewers other than Luraghi did) has highlighted, in a new journal edited by Luraghi, the fact that my book ignored Luraghi's book and Luraghi's article as serious omissions on my part (Bubenik 2011: 293 and 294).
Luraghi’s book, moreover, just like her article that the first journal rejected, was full of observations made by other previous scholars presented as if they were new insights or new discoveries made by her. Even for simple facts pointed out in the most common Greek grammars and dictionaries and already known to generations of students, she only gave references to her own earlier publications. The terminology was modern, but the content was mostly very old, despite phrases like “I will show” appearing everywhere (e.g. p. 37: “in Ancient Greek, the plain dative ... can also express Cause, as I will show”; p. 233: “as I remarked in Luraghi (2000b), hupó, both with the dative and with the genitive, frequently co-occurs with intransitive verbs of active voice ... such as ... thnéiskein”; p. 326: “As I have remarked in §1.1.5 and 1.2.9, abstract nouns often trigger abstract interpretations of otherwise local expressions”). Throughout, she tended to reference herself constantly—and, often, nobody else (e.g. Luraghi 2003: 31, 32, 33, 34).

In a work that she published later still (Luraghi 2006)—another article that, in her “review”, she complained that I did not reference in my book—Luraghi talked again about Modern Greek prepositions, and about the phenomenon I had spotlighted in my doctorate: proper prepositions being replaced by improper ones. Again, my name was not quoted anywhere in the text of that article, although it was left in the bibliography—the return of the repressed, one might say.

Luraghi’s “review” of my book also hugely emphasises the very few works (by other authors) that I did not consider in my book. But figures speak for themselves: my book was roughly the same size as Luraghi’s, and while hers had a bibliography of seven pages, mine had a bibliography of over thirty-one, so any charge on Luraghi’s part that my bibliography was woefully short is clearly disingenuous. And it is easy to show that those other missing authors are not what bothered her: almost all the works on Ancient Greek prepositions and cases (published before her book) that Luraghi criticises me for not citing, are not cited in her book either (Conti 2002, Martinez Vazquez 2000, Martinez Vazquez 2000b, Martinez Vazquez 2001, Martinez Vazquez 2002, Pompeo 2002, Theophanopoulou-Kondou 2002).

Similarly, in her “review”, Luraghi tells us what she thinks I failed to spot in the semantic development of diá, and then declares: “this development is described in virtually all reference grammars and dictionaries (e.g. Chantraine 1961, Kühner & Gerth 1898, Schwyzter & Debrunner 1950)”. So it is; one therefore marvels at the fact that, in her book, Luraghi describes that very same development in detail (Luraghi 2003: 176–185), but she makes no reference whatsoever to any such previous work by anybody else, and only quotes herself repeatedly as if she had discovered everything herself.

Let us come to the “errors” that Luraghi intimated that I would have avoided by citing her publications. She attacks me, telling the readers: “he leaves com-
pletely unsurveyed the locative usage of the prepositionless genitive in Homer ... shown in occurrences such as: Il. 17.372 néphos d'ou phaineto pásēs gaíēs oud' orēōn". Her statement is completely untrue, since Homer's plain genitive is indeed discussed in my book (Bortone 2010: 125), and the very same example from Homer that she mentions was there cited. Nevertheless, on her false statement that I had left the said phenomenon "completely unsurveyed", Luraghi builds a very serious and gratuitous accusation against me: she concludes that "Here and in several other similar cases, it is hard to avoid the suspicion that the author has deliberately ignored some relevant evidence because it did not fit into his picture".

It is, by the way, not even explained what "my picture" would be; if Luraghi means a localistic interpretation, that is another distortion. I am by no means a committed localist and, as my book makes clear (Bortone 2010: 302–303), I believe that a localist interpretation is applicable only within limits. Luraghi is certainly more localistic than me: her 1996 book is thoroughly and unquestioningly so.

This takes us to another factually incorrect assertion that Luraghi makes. In an attempt to present the approach of my book as nothing particularly original, she declares that my theoretical premises—that prepositions have an intrinsic meaning, and that this is best accounted for by a localistic theory—are "nowadays widely shared views". Anybody who has worked on semantics, even superficially, knows, of course, that the localistic theory is not the widely shared view.

Luraghi also asserts that "Much could be written on the sometimes hardly comprehensible criterion by which Bortone discriminates between spatial and non-spatial meanings of prepositions". The phrase "much could be written" is here used by Luraghi as a licence not to write any justification for what follows. She simply objects, without explanation, to my classifying the Greek phrase "the report had been read out in front of the king" as describing the spatial location of an act, and to my classifying the phrase "may his ways be made straight before thee [Lord]" (which means "may his behaviour be rectified in accordance to your will") as metaphorical. I continue to regard my classification as self-evidently plausible. In her book (2003: 311) Luraghi declares, on the other hand, that the phrase epi tois krinousi ‘to the discretion of the judges’ has a local meaning, asserting that this is "clear: a human landmark is conceived as the space where intentionality is located". I doubt that all readers will think this self-evident. In her "review" too, Luraghi states that the phrase diá núkta does not code time but is a spatial expression. I see it as a temporal (albeit spatially-derived) expression, and all major sources (e.g. Liddell & Scott 1953: 389a, Schwyzer 1950: 453, etc.) agree with me.

Still on Ancient Greek (in her "review", Luraghi talks mostly about the Ancient Greek section of my book, perhaps because she was not in a position to say
much about the parts on Medieval and Modern Greek), Luraghi also contests my assertion that, after Homer, cases fully retaining their original meaning after prepositions become rarer. My claim, however, is statistically true: the spatial meaning of case forms, in [preposition+case] syntagms, often faded across time, making some combinations of the same prepositions with different cases more similar in meaning. Luraghi, furthermore, attacks my comment that sún was diachronically “stylistically higher and rarer” than metá+genitive. She retorts that sún was not rarer in Homer, Herodotus, the tragedians, or Xenophon. However, Herodotus is Ionic, and Xenophon (and, all the more, Homer, and the tragedians inspired by him) had features from Ionic and other dialects; my comment referred to the whole history of Greek from Classical (Attic) to Modern Greek, and as such it was absolutely correct. Luraghi herself (2003:151) repeated in her book that “In the Attic prose writers, the most notable peculiarity of sún is its decreasing frequency”. Anyway, the fact that sún is more common than metá in Xenophon and in Euripides was something I had clearly indicated in my book (Bortone 2010:122, 152). But on this Luraghi concludes: “It should be clear by now that Bortone’s treatment of Ancient Greek adpositions and cases leaves much to be desired”.

My treatment of Ancient Greek did not aim, of course, at listing all the details of Ancient Greek: for that, there is already a vast body of scholarship spanning several centuries, to which I referred the reader. To devote the whole of my book to Ancient Greek prepositions alone would have been repetitive and unimaginative. Luraghi herself (2003: 2) acknowledged that “Ancient Greek is perhaps only second to English as to the number of studies devoted to it”. For me, what was important, new, and illuminating, was to take a look, for the very first time, at the complete history of the language. This resulted in a book that, although no doubt far from perfect, has been widely commended for having, among other things, a coverage of unprecedented extension, and for being therefore a contribution to several fields. Luraghi’s only comment on this fact is that my book was therefore “too ambitious”. Her book (like her previous one) was only about Ancient Greek, and most of it applied only to Homeric Greek—and indeed, without this having been made clear always to the reader (cf. Luraghi 2003:288–289). My book, in particular, traced the replacement of prepositions by a new generation of improper prepositions, highlighting the importance of this process; Luraghi, striving to downplay my contribution, in her review calls this phenomenon “a well-known renewal process”. If it is so well-known, it is unclear why she herself seems to have barely noticed it; despite publishing two books on Greek prepositions, she failed to see the crucial significance of the “improper prepositions” (cf. Luraghi 2003: 61) both for the history of prepositions in Greek and for a theoretical analysis of them.

My book also provided a very extensive introduction (Part 1) to the topic area. As I see from blogs and comments by people I do not know, it is one of the
things in the book that readers found particularly helpful. Luraghi, in response, proclaims that this part of my book “leaves one wondering about its usefulness”. Another thing that was especially appreciated by reviewers (cf. Pappas 2011: 431, 432, 436; George 2011) was the variety of languages that I was able to consider besides Greek, i.e. not just, say, Germanic and Romance—what for Luraghi (2003: 41) are “the most accessible Indo-European languages”. Luraghi’s only comment on this, in her “review”, is that I should therefore have made also more reference to typological works.

Luraghi also dismisses my discussion of Brøndal’s treatise on prepositions, saying that my reference to him was “anachronistic”, i.e. that Brøndal is too old to matter (his book came out in Danish in 1940, and from 1950 on, in other languages). However, the attention I gave to Brøndal (dissenting from him), needs no defence: Brøndal was widely translated and influential, and recent works on Greek and Indo-European prepositions such as Hewson and Bubenik (2006), among others, also discuss him. What is indefensible is Luraghi’s accusation of anachronism, all the more in the light of the fact that, in her publications, Luraghi too often quotes as important and valid the views of scholars of the early 20th century (cf. Luraghi 1996: 60), and of the 19th century (cf. Luraghi 2003: 191, 193), and she relies unhesitatingly on the papyri data that was available in 1909 (Luraghi 1996: 65).

Luraghi, furthermore, states that in my book I had not provided glosses and transliterations for Hebrew, Arabic, and other scripts (although I had provided both texts and translations). She is again misrepresenting my book to the readers: I did provide transliterations for all those languages when they were needed—e.g. pp. 36, 44–45, 70–71, 77, 92–94, 101, 164, 173, 176–177, as well as throughout the bibliography. In any case, using the classical languages in their proper alphabet is a standard and time-honoured practice, as those familiar with philological literature know. The idea of transliterating everything in my book was briefly discussed with Oxford University Press, but since the Greek data that I covered spanned almost 3,000 years, the shifts in pronunciation would have rendered words unrecognisable across time, even for classicists. And my choice of not glossing everything, as other reviewers realised, was “understandable because there are so many examples ... that the inclusion of I[nter]L[inear]G[losse]s for every one of them would have made for a very clumsy presentation” (Pappas 2011: 437).

Luraghi even criticises me for not mentioning Lakoff and Johnson’s “Companion Metaphor” at the point in my book that she would have preferred it (I did mention it elsewhere in my book). In fact, in her “review”, she presents many aspects of my book as wrong or inappropriate when they weren’t. She starts her piece by pointing out that I did not cite my unpublished doctorate in the bibliography of my book, as if this was some sort of wrongdoing on my part. Her
criticism is paradoxical, given her disinclination to duly cite my doctorate in *her* publications despite reproducing its content; but another point here is that, when Luraghi turned her own thesis into a small book after fifteen years (Luraghi 1996, in Italian) she, too, did not list her own thesis in the bibliography. So her criticism appears to be, once again, baseless and deliberate. The fact that I did not list my doctorate in my bibliography is of course normal—other scholars who wrote a doctorate on somewhat similar topics and then reworked it into a book did not put their PhD theses in the bibliography (e.g. Horrocks 1981).

I could easily deconstruct more of Luraghi’s piece but space limitations prevent me from adding more. I shall only add a comment on what Luraghi does not say. Luraghi did not manage to bring herself to say anything positive in eight pages, which is in itself intriguing, if one compares Luraghi’s “review” with the opinions expressed about my book by all the specialists. Coulter George, who has worked on Ancient Greek prepositions, wrote in George (2011) that my book was “an extremely impressive book … very convincingly argued … an indispensible source of examples … splendid”; Pappas (2011) who knows also Medieval and Modern Greek, hailed it as an “excellent treatise … a detailed examination … a very impressive piece of scholarly work that showcases both philological and linguistic analysis at its best … B[ortone] very thoroughly examines evidence … he covers a plethora of topics … invaluable … a joy to read … truly excellent work”. Blogs on Greek linguistics have extolled my book, with concurring comments from all readers; Vit Bubenik also thought highly of my work, as did Peter Mackridge, Mark Janse, Jim Miller, and many others besides. To be sure, none of my unbiased reviewers found my book utterly flawless, and I myself never believed that it was, nor indeed that a book of that kind could be. But Silvia Luraghi’s “review” is not an equitable and objective assessment that, naturally, would also find minor oversights and points for discussion in a book of over 360 pages. It is a single-minded attack that, as I have demonstrated, has very little scholarly basis, and whose motives one can infer from the events I outlined at the start.

References


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