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Review Article

Theodore Markopoulos. *The Future in Modern Greek: From ancient to medieval*. Oxford: Oxford University Press, 2009. 288 pages. ISBN 9780199539857

Abstract

The new book by Theodore Markopoulos (2009) on the diachrony of the Greek future tense is subjected here to a thorough evaluation and is found to be a major and important contribution. Still, correctives are offered to some analyses and to claims made about grammaticalization here.

Keywords

Ancient Greek; diachrony; future tense; grammaticalization; Medieval Greek; Modern Greek; periphrasis

1 Overview and General Remarks

Author Theodore Markopoulos (hereafter M), basing himself on his 2006 University of Cambridge Ph.D. dissertation but going beyond the material there, has produced, in *The Future in Greek*, a truly remarkable book. It is both interesting and important and it makes a significant contribution along two key lines of inquiry. These two lines represent two of his main goals in this study: to elucidate aspects of the development of the expression of future tense in Greek from ancient times through medieval Greek, thus covering a time span of more than some 2000 years (roughly 5th century BC to 15th century AD), and to test claims that have been made about grammaticalization, i.e. about generalizations that apply in the emergence of grammatical material in a language.

I should be clear and upfront about two things right at the outset: I have a personal stake in both of these goals, as I have written much about the Greek future (Joseph 1978/1990 and 2003, not in M's bibliography; and Joseph 1983/2009 and 2001, Joseph & Pappas 2002, and Pappas & Joseph 2001, all of which he has taken into consideration in his work) and much as well about grammaticalization, generally in a well-intended but nonetheless quite critical mode (Joseph 2001 and 2004, both included in M's bibliography; and Joseph

2003 and 2006, neither one considered by M, though the latter was not available at the time he did the bulk of his work on this book). Thus in writing this review, I of course make every attempt to assess M's contributions objectively, but at the same time, given my personal history with the subject matter, I cannot, and thus do not, hide my own views. Accordingly, though I try here to be objective, my comments can be read with these potential biases in mind.

The reason careful attention is needed to the development of the future in Greek is that, arguably, of all the elements of the tense categories in the Ancient Greek verbal system,¹ it is the future that has changed the most dramatically. The present tense, the imperfect, and the aorist are all preserved as categories and for the most part, the ancient forms, or at least something recognizably derived from the ancient forms – allowing for differences in stem formations and in some endings, due to sound changes and analogical restructuring – have been preserved.

The future, however, is something different. The modern type with *tha*,² e.g. *tha grapsō* 'I will write', appears at first to be built in some way on the Ancient Greek future, e.g. *grapsō*, but a quick look around at contemporary dialectal Modern Greek and at even early 20th century Greek, with variants like *θena γrapso*, *θela γrapso*, and so on to be found, reveals a rather different picture. And, going farther back in time, through Early Modern Greek and Medieval Greek, one finds a variety of forms with *thelō* 'want' plus infinitives or finite complements of different sorts, thus *thelō grapsei(n)*, *thelō (na) grapsō*, *thelei (na) grapsō*, inter alia, all denoting some future action in some way. And going even further back yields future formations with *ekhō* 'have' and with *mellō* 'be about to', generally followed by infinitives.

Thus there is much to be explained, and even though scholars over the years have not shied away from treating the latter Greek future, there was still more to be done, and M sets out in this study to reassess and extend the evidence and to evaluate various claims that have been made about these forms. As stated on p. 14, M's goal is to examine the 'want', 'have', and 'be about to' future constructions (FCs), and in so doing, to address

¹ I exclude here moods, as the ancient optative has been lost altogether, and the various forms – the infinitives and participles – that made up the nonfinite verbal subsystem, as they have both been drastically reduced (see Joseph 1978/1990, 1983/2009).

² Throughout this review, I present Greek forms in italicized transliteration, except where the pronunciation seems crucial, even when M has them in the Greek alphabet. Moreover, the transliteration mostly follows standards for Ancient Greek, even though for many of the letters the pronunciation was quite different by even as early as the Hellenistic period. Thus I give *the* and *ekh-* here for what was pronounced [θe] and [ex] respectively at the time.

the following questions about them: “(a) What was the exact process of development for each of the constructions? Is there any interconnection between them? And if so, what exactly? (b) Which are the possible causes of the attested development?”. Moreover, since, as noted above, this study is cast in the framework of grammaticalization, he further asks: “(c) What can this investigation tell us about the theory(ies) of language change?” In covering this territory, M also examines, where relevant, some related constructions and/or verbal categories, especially the Modern Greek perfect with ‘have’ and its predecessors, though the treatment is, of necessity, not exhaustive.

This is an ambitious program, ranging over a broad span of time and dealing with a language that is amply documented at all of the relevant periods. M’s presentation goes era by era, starting with Classical Greek (5th - 3rd centuries BC), and continuing through Hellenistic-Roman Greek (H-R, 3rd c. BC - 4th c. AD), Early Medieval Greek (EMG, 5th - 10th c. AD), and Late Medieval Greek (LMG, 11th - 15th c. AD), and within each era, it goes type by type, covering each of the three FCs. The data from each era is laid out carefully, with analysis as to subtype (in terms of function, form, distribution, and related features where relevant, such as complement type) and summative statistics offered as well. Thus he takes both a quantitative and a qualitative approach to the constructions, providing numerous illustrative examples, many of which are discussed in great detail. M thus takes as well a philological approach, assessing the value — and validity — of crucial individual examples taken from a large corpus of texts. He works with a larger amount of material than any comparable study of similar diachronic scope in Greek (far more, for instance, than my studies of developments with the infinitive: Joseph 1978/1990, 1983/2009); it is noteworthy that M’s list of “Abbreviations of texts” alone is 9 pages (234–242) and the bibliography of just his primary sources runs to 29 pages (243–271), with very useful annotations on the less familiar medieval texts as well as carefully drawn subsections for different chronological periods and different genres. In this way, it is clear that M has added to the usual range of literary materials a vast trove of data from nonliterary sources (notary books, agreements, private letters, and the like) and has thus extended the range of data usually considered in more cursory treatments of the history of the Greek future, allowing for a more nuanced and detailed view of the relevant developments.³

³ M has a hefty set of references as well in his coverage of secondary literature; one area where his coverage breaks down is with regard to the Balkan Sprachbund, which he invokes on a few

M is fully cognizant of the issues involved in dealing with these texts. For some, the manuscript tradition needs to be carefully taken into account, and interventions into the text by well-meaning but not always benign editors need to be sifted out; indeed, on several occasions, M goes to the sources and reports on manuscript readings, which often are at odds with editorial whims. Moreover, M takes great pains to place crucial texts in their sociolinguistic and sociostylistic setting to the extent possible, often commenting on aspects of the diction or the function of a text or what is known about the author, for instance, as clues to the register of a given text. Register is a crucial concern, especially with regard to Medieval Greek, due to the very real possibility of influence on the language of the texts from learned language based on earlier stages of Greek. M is thus properly mindful of the potential effects of Post-Classical and Medieval Greek diglossia, as a predecessor to the well-known modern situation; archaizing high-style texts can well show artificial elements of usage and also can throw off any attempt at careful chronologizing of linguistic features.

Still, in this regard, I have to note one quite minor failing: the reader is not given the basis for M's text selection; he *seems* to have included *everything*, but various comments make it clear that he did sift some texts out from his consideration. For instance, he refers on p. 17 to having examined a "near totality of the available textual sources normally considered to be written in a low register" but does not say what was omitted, or why. Similarly, he excluded in a systematic fashion "texts written in higher, i.e. archaizing, registers" for the reasons mentioned above, but includes, where helpful, texts of what he calls a "middle register", without providing a clear definition for what determines "middle" here and why the particular texts so designated were included. He does say explicitly, with regard to the sociolinguistic/sociostylistic factor of register, that he reserves the designation "low-register" for "texts of a more or less informal register of use (e.g. private letters) or texts written by an individual assumed to belong to the lower social / educational ranks or both" (p. 16), and "high-register" material is presumably identifiable from its archaizing language, but an explicit specification of "middle register" would have been helpful. Still, the textual coverage is impressive and lends considerable credibility to M's findings.

With this general overview of the book and M's methods completed, I turn now to some discussion of "meatier" aspects of the book, examining the

occasions (p. 109, 205) but with general reference to Tomić 2006 and Aikhenvald & Dixon 2007. While these are certainly recent sources, the former is riddled with errors and cannot be relied upon as a result and the latter is hardly a comprehensive treatment (better to cite early sources like Sandfeld 1930 or a recent handbook like Asenova 2002).

framework he adopts and some of his findings, but also returning again to methodology, and offering a critique of one area where he feels he has made a particularly major advance, namely his account of the controversial *Qe na* future construction of Late Medieval Greek.

2 The Framework

As noted above, the theoretical framework within which M works is that of grammaticalization. M decides to couch his discussion “in terms of the functional-typological perspective of grammaticalization, which is considered a type of process rather than a *theory sensu stricto*” (p. 2-3), and sidesteps the issue of whether “grammaticalization” constitutes a well-worked out “linguistic theory” in a technical sense. Though M is interested in defending grammaticalization against some of the criticism leveled against it, he is right, in my opinion, to avoid this “battle”, as there is little to be gained by this particular critique of grammaticalization. But I would say that on some of the points on which he does offer a defense, in my view, his defense falls short of the mark. In particular, he takes issue with counter-assessments that challenge “the assumption of grammaticalization as a distinct process” (p. 3), citing my own critiques along those lines (in Joseph 2001, 2004). I have suggested that for all the fact that grammaticalization is talked about as “a process” in the literature (note M’s own characterization quoted above: “a type of process”), most of what is observed and labeled as such under this rubric is best taken as a *result* of independently needed and well-recognized processes of language change, namely (phonetically driven) sound change, (morphologically and conceptually driven) analogy, semantic change (especially metaphor), reanalysis, and borrowing, and not as a separate process in and of itself (see also Newmeyer 1998, 2001, and Janda 2001 for similar criticism). M calls my claim “probably exaggerated”, noting that “the great majority of cases of grammaticalization involve the cooccurrence of different phenomena across grammatical levels (semantic ‘bleaching’, phonological erosion, syntactic irregularities), a fact that was emphasized by Lehmann (1995) and has hitherto remained largely undisputed, although there exist obviously instances where the correlation between the different types of change is not fully observed”. I have to say, partly in my own defense but also as a general point about grammaticalization, that M’s characterization of my criticism is *surely* exaggerated, for he misconstrues my point. I do not deny and have never denied that instances of grammaticalization involve various levels of analysis, but my intent has always been to focus attention on the fact — one which M himself readily admits — that

it is not the case that the different levels are always coordinated; that is, there can be phonological erosion (or sound change more generally) without any effect on grammar, semantic bleaching (and other such changes) without any effect on grammar, shift in syntactic categorization without any phonological erosion (as in the case of *concerning*, originally a participle but used essentially as a preposition now, but, importantly, with no alteration in its phonetic realization), and so on. To my way of thinking, as stated most recently in Joseph 2007, to insist on the special character of “grammaticalization” *because* all levels line up in *some* cases, namely those that are called (prototypical) instances of “grammaticalization”, is to privilege one particular convergence of events of change over numerous other such events where there is no such convergence;⁴ and, to focus on this one particular convergence and build an analytic edifice based on it, as has been done in the literature on grammaticalization, is, in my view, to ignore much of what happens in linguistic diachrony. The emergence of grammatical forms is interesting to be sure, but is far from all there is going on in languages diachronically.

To return to M’s work, while he does adopt some key features of grammaticalization studies that inform his analyses, he does not do so uncritically. In fact he looks to test certain claims made within the general framework of grammaticalization against the interpretations he gives to the Greek data concerning the future tense. Some of these are highlighted below, at the end of this section.

But generally, M embraces most of what grammaticalization studies have to offer. At times even, he slips, unfortunately in my opinion, into a looseness of terminology that is all too characteristic of work in grammaticalization, using the term as a label for all sorts of developments that have little to do with grammar per se, at least in a strict sense. For instance, on p. 37, when he says that “the *ekhō* A[uxiliary]V[erb]C[onstruction] in A[ncient]G[reek] is grammaticalized as a construction expressing ability”, reflecting the fact that the

⁴ At the risk of seeming to be nitpicking with regard to M’s defense of grammaticalization as a unified notion, I note that he offers a compelling argument against a unified notion of “periphrasis”, obviously something key to any study of the future in post-Classical Greek. He writes (p. 12): “The overall basic problem associated with this notion [i.e., periphrasis] is deciding when a construction ... should be considered a ‘periphrasis’. Various criteria have been proposed ... but no agreed upon answer has emerged. ... Unfortunately, these properties do not always co-occur, hence the difficulty of isolating any necessary and sufficient properties of [periphrastic] constructions.” What I find interesting, regarding the question of special status for grammaticalization, is that by substituting into this just-quoted passage the term “grammaticalization” for “periphrasis”/“periphrastic” and “development” for “construction”, one would essentially come up with my critique of grammaticalization.

verb that canonically, and historically, means ‘have’ can also be used with an infinitival complement to mean ‘have the ability’, i.e. ‘can’; what is never made explicit, here or in any discussion of such developments in the grammaticalization literature, is what is “grammatical” about this, and why this should be viewed as anything other than a change in the semantics of a particular combination of words, possibly in a specific form. If every instance of the development of polysemy is considered to be “grammaticalization”, then existing definitions of this notion in terms of movement along a cline of lexical to (less) grammatical to (more) grammatical, or even degree of bondedness (see below), are inadequate; yet, if the notion is broadened to such an extent that it refers to *any* change, then surely it loses considerable utility. And, if modality itself is always considered “grammatical”, then what of transparent constructions like English *be able*? Is it “grammaticalized”? If so, by what other criteria?

Similarly, the use of *thelō* with an infinitive in requests to mean ‘please’, which, as M documents in Chapter 3, pp. 81–82, innovatively emerged in H-R Greek and remained evident in abundance in the EMG papyri, is, we are told, a “particle” use of *thelō* that furthermore shows a “purely grammatical character” (p. 110); but by what sense of “grammar” is a discourse usage like this “purely grammatical”? Are particles (an uncertain category anyway, cf. Zwicky 1985) intrinsically “pure grammar”, or is M here simply signalling a categorial shift away from verbal status (but getting carried away in so doing)? So also on p. 186 M refers to the use of *thelō* with a finite complement (e.g., *thelō na grapsō*) as a future formation, as opposed to its more usual function as a volitional construction, as a late-emerging⁵ “second grammaticalization for *thelō* to express future reference, this time in a different syntactic construction (clausal complementation instead of infinitival)”. Since there are no other changes with this type except the semantic shift from volition to future, it seems fair to ask what developments involving phonetic and morphological

⁵ For M, this means post-14th century; note that Joseph & Pappas 2002 offer a different take on this future formation, suggesting rather that it is a relatively early formation involving the on-going replacement of the infinitive by finite complementation, based on the ubiquitous *thelō* + Infinitive future, and a precursor to an impersonal *thelei na grapsō* type that they see as the starting point for the later reductions such as *the na grapsō* (see section 4 below for more discussion on this formation). M is at some pains on pp. 179–186 (see also p. 170 regarding a unique early example in *Digenes Akritas* that he eventually admits could be a future) to argue that several early-ish instances of especially *thelō na grapsō* constructions are not future (contra Joseph & Pappas) but rather volitional, but I must say that while he may be right about a few cases being ambiguous between the two readings, there are several that, even after his discussion, seem to be fairly solidly future, as M himself acknowledges, and in any case, clear cases are to be found from Cyprus in the 15th century *Chronicle of Makbainas* (see also section 3 below).

reduction and such are, if the future use alone of a particular construction constitutes a “grammaticalization”.⁶

And, in one instance, it seems to me that M’s interest in grammaticalization gets in the way of the most revealing analysis. That is, on p. 69, he confronts the transition in H-R Greek from an ability/possibility meaning (CAN) for the *ekhō* + Infinitive AVC to a futurity meaning (WILL), and attempts an explanation. In keeping with grammaticalization studies’ interest in the conceptual underpinnings of usage, he looks to a semantic/conceptual basis for this change and models it in terms of the “logical square”, as “used by Medieval philosophers ... [and] recently re-introduced ... by Van der Auwera (2001) and Traugott & Dasher (2002) to explain instances of semantic change in the domain of modality”. With the positive and negative values WILL / CANNOT / WILL NOT / CAN at the four corners of the square, a reverse entailment gives the appropriate relationship allowing for an extension from ability to future meaning. As interesting as this may be as an account, I can’t help wondering if it would not be enough merely to invoke the workings of analogy, specifically a proportional (“four-part”) analogy, along the lines of *CANNOT* + ... : *WILL NOT* + ... :: *CAN* + ... : *X* + ..., where my “+ ...” is shorthand for the relevant properties (complementation and the like) associated with the verbal meanings and “solving for X” would extend those properties into positive future use.⁷

One aspect of linguistic development that grammaticalization studies have long been interested in is the degree to which different parts of what were multi-word constructions at one stage of a language come to show a higher level of bondedness and dependency on one another. This interest is reflected in the oft-cited “clines”⁸ of grammaticality and lexicality (see Hopper & Traugott 1993/2003: 7) ranging over affix-clitic-word, and in fact, Haspelmath (2004) has offered a definition of grammaticalization explicitly in terms of

⁶ For the record, this use of “grammaticalization” is actually the one that I would endorse; that is, for me, the deployment of a construction in a new, grammatical use (as with *thelō* + Infinitive in earlier stages being used for future rather than volition) is a suitable use of the term. This is in keeping with my view that many of the other developments that some scholars see as part and parcel of “grammaticalization” (e.g. the phonetic reduction and the like) are better understood as the result of independently motivated changes that are not connected with the “grammaticalization” – see also the discussion above in this section on this point.

⁷ I readily admit here that my interest in analogy may be getting in the way of my fully appreciating M’s analysis of this development. It may well be that the logical square relationship offers a conceptual basis for the formal analogy I would advocate.

⁸ Henning Andersen (p.c., 2004) has observed that as clines imply a continuum, grammaticalization studies might better talk in terms of “scales” in such cases, with distinct resting places along the way from one end to the other. Hopper & Traugott (1993/2003: 105) do acknowledge that “clines should not be thought of as continua strictly speaking”.

degree of bondedness.⁹ Since the parts of the various periphrases M examines in principle could show bondedness to different degrees, it is interesting, and a bit curious in light of the discussion cited above concerning “cooccurrence” and “correlation” of phenomena at different levels of analysis in grammaticalization, to read (p. 44–5) that:

the (*e*)*thelō* AVC can arguably be considered more grammaticalized than the *mellō* one because of its broader semantics and restricted morphology. However, the (*e*)*thelō* AVC had not achieved strict syntactic cohesion, as similarly to the *mellō* AVC, it did not manifest any preference for adjacency of the verb and its infinitival complement; in fact, in only three out of a total of ten instances are the two elements adjacent. Obviously, despite the more grammatical status in comparison to *mellō*, the degree of bondedness between (*e*)*thelō* and the following Infinitive is not yet significantly different than the one *mellō* exhibits.

Presumably, this would be one of the instances noted above where M admits that “the correlation between the different types of change is not fully observed” (p. 3); alternatively, one could say (as I would) that degree of bondedness is not a prerequisite for grammatical use of a construction, and thus that any significant bondedness between the parts (and the phonological “erosion” that often is a telltale sign of bondedness) is independent of grammatical use and results from an entirely separate (set of) process(es), such as sound change, analogy, and reanalysis.¹⁰

⁹ Haspelmath writes (p. 26): “A grammaticalization is a diachronic change by which the parts of a constructional schema come to have stronger internal dependencies”.

¹⁰ Without belaboring the point, there are other cases in M’s material where parameters of grammaticalization fail to line up and do not develop in lockstep fashion. Two particularly telling such cases involve the *mellō* AVC. On pp. 52–53, M gives statistics to show that in Hellenistic-Roman Greek, the degree of adjacency of the parts of this AVC is not significantly greater than in AG, even though there are changes in the direction of a grammatical function for the construction (e.g. emergence of a new meaning – “deontic undertone”, as M puts it on p. 59) and of less freedom to its parts in terms of a shift in the range of aspectual types for its complement (significantly more aorist infinitives than in AG). And, on p. 128, M considers the emergence in Late Medieval Greek of the impersonal use of *mellō* (in the 3rd person singular present form *mellei*) as “the first clear development along the lines of morphological impoverishment” for this verb, and thus as evidence of the “‘decategorialization’ parameter”. He goes on to say, though, that “the ever-growing frequency of the *mellō* AVC occurring in an impersonal syntax can perhaps be regarded as a different kind of decategorialization, as *mellō* was moving away from the domain of futurity into the one of deontic modality”. Having asserted (p. 128) that “it is well known that deontic modality tends to correlate cross-linguistically with uninflected and impersonal syntactic constructions”, he nonetheless is obliged to note that the development of the *mellō* AVC in this direction “never reached a final stage whereby *mellō* would be restricted to an obligation meaning, at least not in most Greek-speaking areas, with the possible exception of Cyprus”. Thus in this period, outside of Cyprus, one has to reckon with a “grammaticalization” where the parameters did not all correlate.

To close out this section, I mention two specific points that M makes by way of offering some refinements to claims advanced in the literature about grammaticalization. First, on p. 139, noting that “the morphological impoverishment of *mellō* [see footnote 10 above] occurred at a period when this AVC was rather rarely used”, he observes that with this kind of reduction, the cause “is not frequency ... but rather [the verb’s] inclusion in the domain of deontic modality, which in Greek is mostly conveyed with impersonal syntax”.¹¹ Second, and relatedly, M draws (pp. 139–140) on his finding that the deontic modality sense of the *mellō* AVC is associated “with specific written registers” and the inference therefore that “its occurrence is mainly due to sociolinguistic factors” to claim that this development “illustrates quite emphatically that the hitherto neglected (for most practitioners of grammaticalization) sociolinguistic properties of AVCs can interfere ... in a grammaticalization process”. To this latter point, I can only shout “Bravo!”, since a single act_r can undo in an instant an accumulation of changes of the sort that most “practitioners of grammaticalization” would look to as evidence of movement in the direction of grammatical status. For instance, a learned borrowing, bringing into current usage a form from an earlier available stage of a language (e.g. Latin, in the case of the Romance languages), or the learning of a full form from a written or higher-style register in place (or alongside) of a reduced form can have such an effect.¹²

3 Methodological Matters Reconsidered

To turn once again to method, I remind readers that, as mentioned at the outset, M’s approach is not only quantitative, but also qualitative. Quantitatively, he marshals forth an impressive array of tables and charts that document the careful counting he has done over what, by any measure, is an exceedingly large and rich corpus. Qualitatively, he subjects many examples to careful

¹¹ My inclination would be to treat this development as an analogical change, with *mellō* morphologically assimilating to the characteristics of other members of its (new) verbal class. M’s observation can be seen as offering a conceptual basis for the formal analogy involved here, and he may well have had analogy in mind; still, it is good to label it so explicitly.

¹² In Joseph 2007, I discuss the learning by children of the full form of the modifiers *sort of* and *kind of* (versus reduced *sorta/kinda*) in these terms, given that the reduced forms are more likely of higher frequency in the input children hear around them. The reintroduction of Latin *mente* in Old Spanish into the construction that became the Western Romance adverbial formation (cf. French *clairement* “clearly”, etymologically “with a clear mind”) may be a case in point involving learned borrowing, since it seems to have changed dramatically the degree of bondedness between the adjectival base and the rest of this construction.

scrutiny and offers extended discussion in numerous instances of the meaning of a particular sentence that he cites from a text. At times, though, with regard to his attention to individual examples, it seems that he “doth protest too much”, in that some examples leave me, at least, with the feeling that they have almost been overanalyzed (see also footnote 5, above). And, while I found a good many of his explications of textual examples to be enlightening and insightful, there were some that were not, for me, particularly convincing.

To illustrate this particular criticism, consider two instances M cites (p. 61) from texts dated to sometime between approximately the 1st century BC and the 1st century AD, thus from the Hellenistic-Roman period. The first is from *Apocalypsis Esdrae* 27,15: *pōs ekhei doxazesthai hē dexia sou* (“how HAS be-glorified the right-side you”, i.e. “how will your right side be glorified?”), and the second is from *Vita Adam et Evae* 31.2-7: *poson khronon ekhō poiēsai meta to apothanein se* (“how-much time HAVE/1SG to-do after the dying you”, i.e. “how long will I live after you die?”). As indicated by the respective translations above, which are M’s, he takes them to be futures and gives them as clear evidence of the relatively early shift in that era from an ability meaning for “have” + Infinitive to a future meaning. But an examination of the full context that M provides shows that the future meaning is not necessarily called for, despite his interpretation.

For the former example, the fuller context reads: “And I will annihilate [AG synthetic future] the race of man, and there will be [AG present subjunctive] world no more. And the prophet said: and how will your right side be glorified?”. Based on this, it seems that a potentiality reading (i.e., “ability” for events not for persons) cannot be ruled out (i.e., “... and [so] how can ... be glorified?”), and even though M feels that the other future expressions enhance the future interpretation of *ekhēi doxazesthai*, one could argue just the reverse: given other expressions for future, why would yet another one be used in so short a span of text? As for the latter, a similar argument can be made. The fuller context for that one reads: “Eve says to Adam: why are you dying while I’m still alive? And how long will I live after you die? ... You will not be long [future-referring present tense] after me, but we’ll both die [future-referring present tense] together.” M here asserts (pp. 61-62) that “Eve does not question her ‘ability’ to survive after Adam’s death but rather wants to know how long she will live alone”, but again a potentiality meaning seems reasonable here (i.e., “How long can I live ...?”), and again the occurrence of alternative means of expressing the future (the two future-referring present tenses) invites the inference that the author used a different form (the *ekhō* AVC) to express a different meaning.

Looking beyond the content of surrounding context, it is further the case that in a few instances, more careful attention to the form of particular

examples points towards a somewhat different analysis from that which M gives. For instance, on p. 71, M cites the following instance of an *ekhō* AVC from a 4th century AD papyrus: *eikhes ... euphranthēs* “you could / would have enjoyed yourself”, and points out (p. 72) that this “constitutes a manifestation of the construction ‘V + V_s’”, and is indeed the earliest such example with *ekhō*. Here, though, a check of the wider formal context in which the example occurs suggests a different interpretation: the word following *euphranthēs* is *sun* ‘with’, and since it begins with *s-* and since the infinitival form expected from AG, ending in *-nthēnai*, underwent apocope to *-thēn* by the 4th century, it is likely that *-thēs* here represents the phonological assimilation of the final *-n* of an apocopated infinitive to the initial *s-* of *sun*. This would then be just an ordinary *ekhō* + Infinitive AVC. Similarly, with another putative case of a V + V_s construction, this time with *thelō* in Late Medieval Greek, but constituting a crucially early example, for that period and with this AVC, M puts forth (p. 166) an example with two concatenated third person plural verbs from the 11th-century text, *Stratigikon*: *ola [ta kastro] theloun apostatēsoun* “all [the castles] will rebel”. In this case, though, the formal context makes this example less certain, since 3PL *theloun* ends in *-n* and the next word, *apostatēsoun*, begins in *a-*, so that the sequence *-n # a-* here could be masking an occurrence of the subordinator *na*; that is, *theloun apo...* could be read as *theloun n'apo...*, at which point it becomes an early example of a *thelō na grapsō* type future. Thus its importance is no less but the formal analysis M gives for the example is open to question.

As a final point of criticism of a methodological nature, I have to draw attention to M's occasional, somewhat offhand, invocation of language contact (see also section 4 on M's account of the *the na* future) without laying much of a foundation for a contact explanation. Thus on p. 58, in discussing changes in H-R Greek with infinitival aspect, he hints that “the language contact situation of the H-R period [with] ... numerous speakers of other languages who learned Greek as a second language” may have been responsible. Similarly, on p. 72, M mentions contact involving Demotic Egyptian as a possible source for the emergence of periphrastic constructions in the first place in Hellenistic Greek. In each case, one has to admit that of course the H-R period was an era of extreme language contact as far as Greek was concerned, so in a sense contact is always an easy suggestion to make for practically any development found in that period; however, simply throwing it into the mix as a possibility without exploring it to any degree would appear to be treating language contact as a last-resort type of explanation. Furthermore, M's statement (pp. 72–73) that “no thorough understanding of the sociolinguistic situation and, especially, its outcome in Hellenistic-Roman Egypt is yet available” overlooks the important work of Bubenik 1989.

4 M's Treatment of the *the na* Future

Language contact plays a role too in M's account of one particularly intransigent and controversial area of development with the *thelō* futures. It must be said first that one of the many virtues of this book is that M uncovers some new and interesting facts, and offers some novel interpretations, about various AVCs in post-Classical Greek. For instance, on pp. 142, 147, he adduces convincing examples from *Digenis Akritas* of *ekhō* followed by a bare finite verb as complement (i.e., an *ekhō grapsō* future type) for Late Medieval Greek,¹³ and similarly notes (p. 128) a somewhat parallel case involving *mellō* from the same period (*Chronicle of Morea*, l. 3702: *emellen poiēsoun*, with an impersonal form of *mellō* and a bare 3rd person plural verb). Such types have not been mentioned before in the literature so M's drawing attention to them is a significant contribution indeed. The same can be said with regard to some of the new accounts M provides for long-standing points of controversy, such as the use, discussed on pp. 149–155, of the *ekhō* + Infinitive AVC embedded under a subordinator (mostly *na*, but at least once under the generalized complementizer *tou*); M insightfully, and innovatively, takes this pattern to correlate with “the morphological demise of the Subjunctive allow[ing] future-referring / modal AVCs to be used in Subjunctive contexts ... [a] possibility ... probably dependent on the optional inability of *na* to mark clearly the modality of the clause it introduced directly and in its own right” (pp. 154–155).

However, with one area where M feels that he gives a novel interpretation, there is less success, in my view (and here he takes on some claims made in Joseph & Pappas 2002 (hereafter J & P), so my bias may well show, I realize). This is the *the na grapsō* future construction, discussed at length on pp. 186–208. As pointed out in footnote 5, J & P, following others before them, take this future formation to be a reduction ultimately of the *thelō na grapsō future* type,¹⁴ via impersonalization (perhaps motivated by the redundancy of the double marking for person on both the future verb and the complement) to *thelei na grapsō* (attested only rather late, admittedly a weak, but hardly fatal,¹⁵ point in the J & P account), and subsequent (and attested) developments

¹³ See above, in section 3, concerning his one putative very early (4th century AD) example of this construction, which I find less than convincing.

¹⁴ That is, *not* the *thelō na grapsō volitional* construction; that is why the existence of *thelō na grapsō* futures, which M reluctantly admits as a distinct possibility for the middle of the Late Medieval period (see footnote 5), is important.

¹⁵ It is possible of course that some instances of 3rd person singular *thelō na grapsō* futures are in fact the type J & P were looking for, but the analysis of such forms is ambiguous between personal and impersonal *thelei*. To be sure, M is right that date of attestation in texts is hardly an

from that, including *thel' na grapsō*, *thela grapsō*, *thala grapsō*, and, of course, *the na grapsō* (and ultimately *θα na / θan / θα γρapsο*). While J & P take the existence of reduced forms with *-l-* (e.g. Cretan *thela*) as significant for suggesting a *thel' na* stage, M largely ignores such forms, mentioning them on p. 200 only to dismiss them as falling outside his scope due to the chronology of their attestation.¹⁶ While a full assessment of M's account is beyond the scope of this review article, inasmuch as a good part of it hinges on details of interpretation for particular examples and on claims about possible sound changes involved and other related developments, the nub of M's explanation for *the* can be examined here. His main claim is that *the* is geographically restricted to Crete and Cyprus and that it arose due to "contact between Greek and Romance speakers, especially Old Venetian and Old French" (p. 198). While it is true that there is Romance influence in both Crete and Cyprus at the time M sees *the* emerging (14th century), he claims that *the* is the result of Romance speakers carrying over their reduction of nontonic vowels into their production of Greek and of Greek speakers using a reduced form *the* (presumably taken over from the Romance nonnative Greek speakers) as a "volitional particle" (p. 201) modeled on a Romance syncretic 2nd/3rd person singular / 3rd person plural volitional form (cf. Old Venetian 3SG/PL *vol* and modern Veneto dialect 2SG/3SG/3PL *vol*, cited on p. 199). That this volitional *the* — and it must be admitted that some instances of *the* are volitional, as J & P indicate (taking that to be evidence of derivation from a reduction of forms of *thelō*), and as M notes as well — then took on future meaning is, for M, "directly related to the emergence of this meaning in the fuller *thelō* AVC₂" (p. 202), at a point where a relation between *the* and *thelō* was not opaque (opacity, in M's estimation (p. 202), setting in only by the end of the 16th century).

insignificant factor to take into account, but at the same time, it should be noted that there are well-known cases of truly archaic forms not being attested until very late in a given literary tradition: Ancient Greek *ēor* "female cousin", for instance, is attested from only the 5th century AD (in a gloss in Hesychius) and yet is the inherited outcome in Greek of Proto-Indo-European *swesor- for "sister" that managed to escape attestation in some 2000 years of documented Greek before that (see Janda & Joseph 2003: 15–16 for discussion and other examples of late attestation of clearly old material in different traditions).

¹⁶ It seems to me to be somewhat unrealistic to expect every form to be attested at a suitable time period; after all, as is implicit in footnote 15, attestation is partly just a matter of chance, and this is even more so with forms that might well be taken to be colloquial or reflective of fast speech, no matter how much one assumes that even low-register texts are close to spoken language. That is, I am willing to assume a greater fluidity at each period with the range of possible forms than M envisions, even though he does reckon with multiple expressions of futurity at all stages.

As with other cases where M invokes language contact (see above, section 3), this account fails to be convincing, largely, I would say, because he provides little social motivation for the entry of *the* into wider use in Greek beyond the nonnative origin he posits (Greek in the mouths of nontonic-vowel-dropping Romance speakers). Moreover, one has to wonder about the posited equating, by Greek speakers, of *the* with Romance syncretic *vol*; were Greeks learning Romance sufficiently well to be able to notice such a possible (and at first glance rather subtle) parallel? If, alternatively, the “volitional particle” use of *the* was the result of Romance speakers creating an innovative use for their quasi-Greek *the*, one is again confronted with the problem of determining suitable social circumstances that would lead to an Italian minority affecting the majority Greek speakers in the relevant areas such that native Greeks would adopt the reduced form *the*.

For some individual authors in Crete at the relevant period who used *the*, such as Marinus Falieros, such an account of his use of *the* might be plausible, since, as M notes, he was an Italian living in Crete. But generalizing from one such possible contact-induced use of *the* to a widespread use throughout the Greek-speaking population seems to involve more wishful thinking than linguistic plausibility. At the very least, some justification or at least suitable parallels in other contact situations should have been given. Some seeming parallels involving English that come readily to mind are not really comparable: the influence of the Romance language French, spoken by a prestigious minority, on English in the Middle English period is to be attributed to the fact that English speakers learned and came to use French, not to their being affected by English as filtered through use by French speakers; and, the case of Indian English, where the English filtered through an indigenous population did win out and affected even some British English speakers in India, is not comparable since the demographics are quite different – the indigenous speakers in India in the formative period for Indian English constituted the majority, while the Romance speakers in Crete and Cyprus were a minority. If contact with Romance speakers is to be a viable explanation for Greek *the na* futures, more attention to the social situation on the ground is called for; just as contact solutions should not be ruled out in favor of language-internal accounts, as M wisely remarks (p. 202),¹⁷ so, too, should contact not be used without a solid basis for understanding how the contact among speakers would have been realized and would have been played out linguistically.

¹⁷ See Joseph 1985: 96 for expression of a similar sentiment, countering an explicit statement to the contrary by a Greek linguist.

Thus there may well be weaknesses in the accounts of *the* that take it as a reduction of some form of *thelō* that emerged wholly internally within Greek, but neither is the contact account that M proposes really very compelling. This area of the development of the future in Greek, it seems, still awaits a definitive solution.

5 Some Specific, and Admittedly Minor, Further Comments

Having subjected much of M's excellent work to careful scrutiny along various thematic lines involving his choice of framework, his methods, and some of his results, I find that I am left with a number of small points of a somewhat scattered nature where I differ with him or where further comment seems warranted. I turn now to some of those.

- i. On p. 57, regarding his example (4), from a 1st century AD papyrus: *ean ... mē melleis erkhesthai* ("If not you-are-about to-come", i.e. "if you are not going to come"), M comments on the present infinitive *erkhesthai*, saying that even though traditionally, based on AG usage, present infinitives are treated as indicating imperfective aspect, here it "clearly conveys a perfective meaning". This point is important to his claim that the AG aspectual values of the infinitives were rearranged in the Hellenistic-Roman period. A perfective sense does indeed work here, but so too, it seems, does imperfective, as the acceptability of the translation "if you will be coming" in this context would suggest. M's point about aspect is well-taken, so perhaps a better example needs to be given here.
- ii. On pp. 74–76 and 104, M discusses, with statistical tables, the variety of complements to volitional *thelō*, and, interestingly, cites a vanishingly small number of examples with an overtly subordinated (e.g. with *hina*) finite complement where the subordinate verb and *thelō* share a coreferent subject; there is just one such example in his count from Hellenistic-Roman Greek literary and papyrological sources,¹⁸ from the 1st century AD, and three from Early Medieval Greek, all from the 6th or 7th century. By far the overwhelming complement-type in such a construction is the infinitive. M's one Hellenistic-Roman example (from Epictetus's *Dissertationes*

¹⁸ M also cites two cases in this period of concatenated V + V with a bare second verb (i.e. no *hina*), which I ignore here but which are very interesting and represent a hitherto undocumented type for this era of Greek, thus constituting a major contribution, in terms of data, that M makes. See also section 4 and footnote 13 above.

2.7.8), however, involves main verb *thelō* and its finite complement (introduced by *hina*) at some distance from one another, separated by a different intervening subordinate clause; thus it may be more like what is seen in English sentences such as *What I want is that you leave at once*, where non-adjacency widens the range of possible complements (what John R. (Haj) Ross in unpublished work from the mid-1970s called “amnesties”). This does not eliminate the very real example but perhaps puts it in a slightly different light. Still, there are other examples from that era, such as the handful (three or four, depending on how some are judged), cited in Joseph 2002, that come from texts M did not include in his survey and which can be added to M’s findings here; admittedly, some of these other examples present some analytic and interpretive problems, but overall, with M’s example and these all taken together, main verb *thelō* with a same-subject *hina*-headed finite complement, must be reckoned as a legitimate complement type for H-R Greek.

- iii. On p. 105, footnote 8, M, following the standard view, cites Trypanis 1960 regarding indirect (metrical) 6th century evidence, in the hymns of Romanos, for an (irregular) accent shift from AG *hina* to Early Medieval *hiná*, as a starting point for the development to later Medieval and Modern *na*. M, however, is hesitant to endorse this view entirely, noting a lack of direct (textual) evidence that would allow the change to be dated definitively. I feel M’s caution is proper, based on the view, to be attributed to Julian Mendez-Dosuna (p.c., september 1999), that *hina*, as a function word, should be viewed as *entirely* unaccented, at least at the phrasal level, so that the aphaeresis of the initial vowel (after the early loss of [h]) is completely regular.
- iv. On p.143, M gives an ostensible example from *Digenis Akritas* (l. 1779) of the *ekhō* AVC with “infinitival complementation ... coordinated with ... finite”, but the infinitives here are *phagein kai piein*, literally “to-eat and to-drink”. M translates their occurrence with *ekhō* as “you can (have enough to) eat and drink”, but Dolger 1953 has shown that the phrase *phagein kai piein* represents a common conjoining of these infinitives that was most probably lexicalized, as early as the H-R period, with the infinitives as nominals;¹⁹ thus, this example most likely does not involve complementation per se but rather is best translated as “you have food and drink”.

¹⁹ Note the Modern Greek noun *to fa(γ)i* ‘the food’, universally recognized as continuing a nominalized articular infinitive, thus from *to phagein* ‘the (act of) eating’.

- v. On p. 216, M argues that instances in LMG of (past tense of *thelō*) with *na* + a finite verb (that is, an *ēthela na grapsō* type) serving as a future-in-the-past (i.e., translatable as “would” or “should”) do not provide support for the view that an “equivalent future-referring *thelō na* + Subjunctive must have developed”. Given the general parallelism, in form at least, between the *thelō* AVC in the present and in its past tense forms (though they did go separate ways, admittedly, in certain respects, as M notes and as Pappas 2001 has amply demonstrated), M seems to me to be overstating things here, especially in the light of seemingly strong cases of a *thelō na grapsō* future discussed earlier (see section 2 and footnotes 4 and 13 for more on this issue).
- vi. Finally, even though the book in general is extremely well produced, with only a few editorial lapses (typographical errors, missing words, and the like) that are mostly self-correcting, I cannot refrain from mentioning two typos that are minor but nonetheless interesting or important, each in a different way. First, the Slavist in me notes that the citation of the early Slavic infinitive of “want”, given on pp. 206/211 as *xotěti*, should instead be *xotěti*, and the 1SG of “have” (p. 172) should have a final jer (ultra-short high vowel) instead of a < b > (it could be written < imamb > with a smaller “b”, if needed). Second, the analogically oriented historical linguist in me finds the editorial lapse of the occurrence of “c.f.” on p. 202 (line 32), versus the correct “cf.” (used properly everywhere else in the book), to be a delightfully wonderful case of orthographic analogy, presumably (see Janda & Joseph 2003:133 on this) based on such double-period abbreviations as *e.g.* or *i.e.*

6 Concluding Remarks

As stated at the outset, this is a remarkable book, and despite the various criticisms I have offered here in this consideration of it, I stand by this initial assessment. In terms of philological care, it is unsurpassed among studies of the grammar of Greek as the language moved from the post-Classical era into Medieval Greek and to the verge of its modern form, and this work sets a standard for subsequent large-scale studies in this area. Further, in terms of sheer interest value, M has put together a study that draws together various analytic threads and stands as testimony to the value of careful synchronic analysis accompanying insightful diachronic interpretation. I therefore consider this book to be essential reading for all Hellenists, regardless of their period of greatest focus, for all diachronicians, regardless of their language

focus, and for those interested in grammatical change, whether grammaticalizationists or not.

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