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## *Review Essay*



# A rephilologized diachronic analysis of “Post-Classical Greek”

*Pitfalls and principles for progress*

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### Abstract

Picking up on the notion of “rephilologization” promoted by the editors of the 2020 volume *Postclassical Greek* as a recurring theme in the book under consideration here, I offer in this review article an overview and critique of the ways in which philological analysis intersects with grammatical and diachronic analysis in the 12 studies contained in this work. In addition, I discuss various pitfalls and principles for progress in analysing Greek in its Postclassical instantiations.

### Keywords

rephilologization – Postclassical Greek – corpus analysis – future tense – diachrony

Dariya Rafiyenko and Ilja A. Seržant (eds.) *Postclassical Greek: Contemporary approaches to philology and linguistics*. Berlin: de Gruyter Mouton, 2020.

## 1 Introduction

The book *Postclassical Greek: contemporary approaches to philology and linguistics* (2020), edited by Dariya Rafiyenko and Ilja A. Seržant and published by de Gruyter as volume 335 in their Trends in Linguistics series, provides a rich and representative slice of current research into Post-Classical, Byzantine and Medieval Greek. In addition to this characteristically large diachronic scope, the volume presents a great variety of linguistic data (e.g., literary texts, papyri, inscriptions) and theoretical approaches (e.g., (historical) sociolinguistics, language contact, historical linguistics). The selected twelve contributions in the book were originally presented at a 2016 conference in Mainz called *Postclassical Greek: Intersections of Philology and Linguistics*, as mentioned by some contributions.

The introductory chapter provides both a background to Post-Classical Greek and an overview of the changes in different linguistic domains (phonetic, morphological and syntactic) which set apart later periods of Greek from Classical Greek. Importantly, the editors use the term “Post-Classical Greek” to refer to all the later Greek from 323BC up to 1453AD,<sup>1</sup> but unfortunately only discuss the major changes up to the early Byzantine period, an issue to which I return below. The chapters have a rough thematic grouping into “Grammatical Categories” and “Sociolinguistic Aspects and Variation.” This review is structured as follows. First, I discuss the contents of the introductory chapter and the theoretical aims of this volume (Section 2). Next, I review the different contributions (Section 3). Subsequently, I discuss the overall value of the book and try to determine whether it has successfully achieved its aims (Section 4). Finally, I distill various methodological pitfalls and principles for progress from the book for analysing the many different periods after Classical Greek (Section 5).

## 2 *Rephilologization* and the long and varied history of ‘Post-Classical Greek’

Rafiyenko and Seržant provide a succinct overview of the major linguistic characteristics of the different periods of Greek after Classical Greek, detailing the most important changes in different linguistic domains (phonetics, morphology and syntax) and the different dimensions of variation which characterize the different periods (internal (i.e., diatopic, diastratic) vs. external factors (i.e.,

1 When using “Post-classical Greek” (i.e., in double quotation marks) I refer to the same long period.

language contact). Their chapter starts with the vexed question of periodisation of what they call “Post-Classical Greek.” They discuss the diversity in periodisation before adopting the periodisation based on extra-linguistic criteria (i.e., historical periodisation such as Hellenistic and Roman) which they duly call controversial, but for which they see no alternative due to the absence of linguistic criteria for periodisation. They clearly introduce the historical and linguistic background to the formation of the Koiné and its development of registers that was greatly influenced by the Atticist movement. After highlighting the role of diastratic variation, they illustrate diatopic variation across the dialects and the influence of language contact with Coptic and Semitic before concluding that “Post-Classical Greek” was very heterogenous in diastratic, diachronic, and diatopic terms. The main part of their contribution provides a “grammar of Postclassical Greek” for the major changes from Hellenistic to Early Byzantine Greek, meaning that later periods which fall under their term “Post-Classical Greek” are excluded. There they discuss the changes of later Greek familiar to us from our many linguistic philological histories and recent work, such as phonological changes, restructuring of morphological patterns (e.g., inflection, tense, voice), creation of new categories (e.g., periphrases), reorganisations of systems (e.g., cases vs. prepositions) and some other phenomena that they consider separately, such as double negation, the changing grammar of subordination, and word order.

Their chapter concludes with a somewhat sudden endorsement of the *rephilologization* of historical linguistics. While they do not fully introduce this trend, they connect it to their stated conviction “that ‘pure’ linguistic research will not be as fruitful as it should be if significant variational factors ... or the impact of standardization ... are not taken into account” (Rafiyenko & Seržant, p. 12). In other words, they emphasize the importance of the multi-dimensional analysis of language data. With this bundle of contributions they would like to endorse the rephilologization trend, and I try to evaluate the success of this endorsement in Section 4. To be able to do this, we first need to understand this trend better, the notion of philology that it contains, and its relation to multi-dimensional analyses. Pleas for rephilologization are partly a response to the increased availability and use of larger diachronic corpora which come with their own risks, as rephilologization aims to bring “language historians to a close encounter with individual texts on the methodological level and to an engagement, on the theoretical level, with approaches that ground language change in localised acts of social interaction and interpretation” (Adamson & Ayres-Bennet 2011: 204).<sup>2</sup> In other words, the movement aims to put the vari-

2 Cf. Dollinger 2016: 61–62. Renewed research traditions such as historical pragmatics or the

ous contexts of historical language data back into the centre of attention. The added philological component then is conceived broadly as attention to relevant qualitative aspects of the textual data such as the role of the author, text type, genre, and socio-cultural factors (Dollinger 2016: 61), i.e., a particularizing approach. The term “rephilologization” implies an opposition with linguistic approaches that focus on making empirical generalisations over the different texts, but supposedly do not incorporate these factors enough in their analyses (cf. also the term “pure” linguistic research above). Of course, this opposition is somewhat specious, since many quantitative historical linguists actually take this philological background as a prerequisite for fruitful research.<sup>3</sup>

Nevertheless, it pays to be aware of the different stakes that are held by different approaches, since, as suggested by Morpurgo Davies 2011, researchers from either side of the spectrum not only work with the same data (albeit with different aims) but also stand to gain from the insights of their colleagues. She distinguished three scholarly prototypes that are rather familiar to the field of Ancient Greek, which historically has a strong philological background: (1) the literary philologist: studies texts for literary or cultural purposes but is wary of linguistic theory; (2) the linguistic philologist: focuses on the history of the language, works with compilations (grammars, lexica, etc.), which in their turn are based on texts that s/he consults using philological techniques such as textual criticism, palaeography, epigraphy etc.; (3) the linguist: analyses a corpus (oral or written) to reach quantitative generalisations and form predictions from within a linguistic theory. In reality, most scholars move in between these prototypes, but, importantly, the rephilologization of historical linguistics suggests the necessity of a reintegration of linguistic philological methodology. As I argue here, however, we should be wary of the potential drawbacks that we might invite back in when trying to reintegrate linguistic philological methodology (see especially Section 4 and Section 5 below).

The plea by Rafiyenko and Seržant to make multi-dimensional analyses, ones that go beyond diachronic analyses of language data, a methodological axiom is well-made and supported with relevant examples from the long history of “Post-Classical Greek.” Still, reading the coverage of such a long period and their emphasis on variational factors left the uncomfortable thought that we only have a bird’s-eye view of what “Post-Classical Greek” really is. In fact,

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current interest in historical sociolinguistic questions may be connected to this plea, cf. Adamson & Ayres-Bennet 2011: 204.

3 For example, both Markopoulos 2009 and Bentein 2016 pay due attention to the influence of philological factors (e.g., register, textual transmission) in their combined qualitative and quantitative diachronic analyses.

there seems to be an uncomfortable tension here and in various contributions between the emphasis of variational factors and the long diachronic outlook which they seek to combine; that is, the long diachronic outlook with unclear and heterogenous periodisation contributes to imprecise comparisons over long stretches of time, although emphasis on particularized variational factors argues against this. A first example can be found in the domain of word order, for which Rafiyenko and Seržant (p. 11) mention the change from OV to VO in the Koiné (Horrocks 1990). Systematic quantitative evidence for the different periods of later Greek and the different types of clitics which supposedly evidence such changes is still absent. Thus we only have some identified trends much like we find them in our linguistic histories of Greek. Another case in point is the disputed role of future periphrases across different periods of “Post-Classical Greek.” Rafiyenko and Seržant (p. 7) naturally mention the development *t<sup>h</sup>élō*, which became the Modern Greek future marker but was “only sporadically found (primarily in papyri)” in the early Byzantine period (5th–10th c.). They effortlessly contrast it to the modal uses of future periphrases in specific works of very different periods: the Old and New Testament and Malalas (6th c. AD). While I understand that this comparison allows them to bind the contributions by Kölligan and Tronci together, this type of comparison is superficial, since (i) these are just one set of works of a larger period, meaning that they are very unlikely to be representative of the whole period; (ii) the diachronic gaps between these authors is substantial; and (iii) the influence of earlier models cannot be underestimated for these authors (as argued by both Tronci and Kölligan). Stricter periodisation of smaller periods could potentially rid us of such comparisons. In fact, I would go even a step further than Rafiyenko and Seržant and say that linguistic periodisation is (almost) never unproblematic, but, importantly, that does not mean that linguistic periodisation is without purpose. Smaller periods that are commonly used in historical linguistic research will not only help quantify specific changes more precisely but will also help build up much needed linguistic profiles of periods which can be used to answer historical sociolinguistic questions about the relationship between language and life, or on the individual level, between linguistic style and identity (see further Section 5).

### 3 Review of the contributions

The first contribution by **Giuseppina di Bartolo** analyses changes in the grammar of purpose and result clauses in the Greek documentary papyri of the Roman period. She aims to show the most common uses of *ἵνα* and *ὥστε* as pur-

pose and result clauses, their variation in the documentary texts as opposed to literary texts and detail their respective developments. The paper starts with an introduction of the subject, the documentary papyri and the “Greek modal system in flux.” Next she presents the uses of ἵνα and ὥστε as introducers of purpose and result clauses, their overlap and decrease in distinctiveness over time. Among others, she points out some overlap between purpose and result uses, some “unusual” constructions (ἵνα+indicative and ὥστε+subjunctive) and some specific cases in leases where, according to her, ὥστε specifies a condition.

Methodologically, there are several questionable aspects to this paper. First of all, the data on which she bases her analysis is problematic. She says (p. 21) that “The evidence from the papyri has been collected through an examination of edited documentary texts beginning with 40 volumes of the Oxyrhynchus Papyri. Further data comes from a collation using various papyrological databases (e.g., the search mask of Papyri.info)”. Further quantitative information is absent. As a result, in addition to not knowing which specific papyri she used, we have no reference point according to which we can judge the validity of her analyses.<sup>4</sup> Second, she frequently contrasts the uses from the papyri with those found in literary texts to sketch the differences between these registers, but bases these comparisons only on a limited literature selection. Important work on purpose and result clauses such as Kurzová 1968, Wakker 1987, Crespo 1988 and Revuelta Puigdollers 2017b which could have been used to her advantage are left out. For example, the role of intentionality and prospectivity which she mentions as distinguishing purpose from result clauses was already discussed by Crespo 1988. Moreover, register comparisons with the literary texts remain somewhat superficial, since she compares documentary uses with uses found in specific authors from very different periods, such as Polybius for the Ptolemaic period and Theophanes Confessor for the late Post-Classical period.

These issues connect to her large diachronic scope, which leads to two related problems: (1) she tries to describe more than we have analysed quantitative data for yet. For example, in describing the prevalent uses of ἵνα and ὥστε purpose and result clauses, she provides many parallels from earlier, later Greek and even Modern Greek (using linguistic histories or work-specific grammars). However, these periods are not under discussion here and, for lack of a corresponding corpus analysis for these periods, the diachronic distribution of ἵνα and ὥστε is still unclear in those periods. And (2) she argues that the reduction of the modal system with the loss of the optative, subjunctive, and infinitive

4 The exception is di Bartolo (2020: 23, n. 2) which mentions that there are around 600 instances of ἕπως in the Roman period.

led to the reorganization of the purpose and result clause system.<sup>5</sup> However, the data given cannot substantiate such a claim, if it were in fact the case that these changes are interrelated. It would have at least been necessary to show how the functions of the optative or infinitive and the purpose or result clauses are related and how the later replaces the former. The example closest to such evidence is the discussion of the “unusual”  $\text{ἵνα}$ +indicative which she explains as due to phonological confusion of the subjunctive endings but which “causes a morphosyntactic change” (di Bartolo, p. 30), since she interprets it as a foreshadowing of Modern Greek  $\text{να}$ +verb ‘X should VERB’. However, these unusual occurrences do not have this deontic modal meaning, so I fail to see the point of this parallel.<sup>6</sup>

The second contribution by **Chiara Gianollo** deals with the vexed issue of genitive-dative syncretism, in particular the replacement of affected external possessors in the dative case by means of the genitive case. She provides a historical reconstruction of the syntactic factors which caused such prenominal genitives to be reanalysed as an external possession construction, thereby taking over functions from the dative. To make her case she provides a careful step-by-step analysis of the historical data and argues how only in the New Testament were the syntactic factors such that the prenominal genitive (which was available earlier) could be reanalysed as an external possession construction, since only the New Testament had a predominantly postnominal genitive system. The reanalysis itself consists in her view of a movement operation that changes the dependency of the genitive from a noun to the verb, whereas the affectedness of the genitive is derived from the prenominal position.

A very positive point about this paper is how corpus-based it is, as Gianollo uses analysed linguistic data from every period that she discusses. Similarly, when dealing with the data from *Stolk 2015*, she carefully weighs the papyrological and the New Testament evidence. Yet, the contribution ought to have incorporated not only *Stolk 2015* but also *Stolk 2016* and *2017*,<sup>7</sup> since she also deals

5 As di Bartolo (2020: 22) states: “To understand the use and development ... a few general features of the Greek modal system and its development over time need to be explained. In particular, the reduction of the modal system, which involved the gradual disappearance of the optative and subjunctive moods and of the infinitive, led to reorganization in the way purpose and result were expressed.”

6 The independent modal use of a previously dependent  $\text{ἵνα}$  clause is actually the result of insubordination, the conventionalization of main clause use by a subordinate clause, a process by which  $\text{ἵνα}$  obtains many main clause uses such as directive, wish and declarative. For this issue, see la Roi (2020). Also, the combination of  $\text{ἵνα}$ +indicative was already discussed by Bentein (2015: 117).

7 This paper is mentioned in the introductory chapter of the volume.

with the earlier dating of genitive-dative syncretism, provides important comparative data and discusses possible earlier starts of the syncretism. An aspect that was not completely convincing was the supposed process of change, which to me (as a non-generativist) seems too much based on accounting for the surface elements with supposed movements. To explain the distinct role of prenominal genitives, she supposes a link with dependency, as the prenominal genitive exchanged its dependency with a noun for that with a verb. However, it seems improbable that this dependency change can only have been there in New Testament Greek due to being maximally distinct from the other genitives. The same case and function relationship was there before, meaning that its distinct characteristics would have caused reanalysis already.

The third contribution by **Daniel Kölligan** provides a fine-grained analysis of the inventory of future expressions in the earliest Byzantine world chronicle by John Malalas (6th c. AD), written in a language close to the vernacular, and compares the available future expressions from Classical and the later Greek of Malalas. He analyses Malalas' uses of the synthetic future, the *praesens pro* future and the alleged periphrases μέλλω, ὀφείλω, ἔχω, θέλω and βούλομαι. First he demonstrates that the decline of the synthetic future is reflected in its use in limited linguistic contexts, whereas the *praesens pro* future is used only with specific verb types. A varied picture emerges for the alleged periphrases, since some have unexpected future uses (participle of ὀφείλω as future), others are used modally only (θέλω and βούλομαι), and others have future-in-the-past and counterfactual uses (μέλλω and ἔχω).

Kölligan usefully singles out several distinctive aspects of the constructions, such as combined verb types (telicity), syntactic restrictions (morphosyntactic collocations), formal parallels, and evolutions, which he uses to determine the grammatical status of the constructions. Also, he pays due attention to philological factors such as textual differences or the linguistic profile of the author John Malalas. Less convincing is the choice of comparison with the Classical period only. It would have been much more helpful to compare the inventory to uses found in earlier Post-Classical periods. For these periods we are referred to Browning 1983, Markopoulos 2009, Horrocks 2014 and Lee 2010 (for θέλω). But the Post-Classical Greek developments which precede John Malalas are not presented,<sup>8</sup> nor essential studies such as Mayser 1926 or Mandilaras 1973.<sup>9</sup> As a result, very tentative (though I must admit attractive) evolutionary paths are

8 The exception is Kölligan (2020: 76) which discusses the phonological changes in the future and subjunctive domain.

9 Also, when it comes to ὀφείλω, the bibliography misses important recent studies such as Ruiz Yamuza 2008 and Revuelta Puigdollers 2017a.



presented (e.g., for the takeover of subjunctive functions by the future and the formal renewal of a future construction pattern with ὀφείλω, Kölligan pp. 76, 85), but the corpus data for these changes is absent. Also, this approach leads to somewhat paradoxical conclusions such as “The language of early Byzantine authors has to be understood as a language in its own right that deserves further attention” (Kölligan p. 94), and somewhat unsurprising conclusions such as “The language used by Malalas is intermediate between Classical and later Byzantine Greek in that it retains a number of patterns of the former and does not yet show characteristics of the latter” (Kölligan p. 94).

The fourth contribution by **Joanne Stolk** is an interdisciplinary study of the use of prepositions (εἰς, πρὸς, and ἐπι) in the Greek papyri which integrates linguistic, palaeographic and papyrological evidence. Against the backdrop of the generalizations that we possess of how these prepositions were used in documentary papyri from Egypt, she demonstrates that a reconsideration of these three types of evidence combined provide better interpretations for problematic textual passages.

This chapter is the shortest of the volume and regrettably so, because it provides a good example of what a rephilologized analysis could look like. The fact that we understand strong tendencies for the use of these prepositions from quantitative studies (e.g., Stolk 2017) makes the rephilologized analysis of the problematic contexts all the more credible. Thus, the availability of analysed quantitative data strongly benefits rephilologized analyses, as (re)reading texts needs full understanding of the language which could synchronically have been used in it. Furthermore, Stolk rightly mentions the possible pitfalls of such an interdisciplinary approach, for example, that one might wrongly want/start to reinterpret examples which do not fit a current theory. She concludes that editing of papyri is a timely affair, since it typically consists of reading, misreading, correction, and reinterpretation. Importantly, Stolk’s interdisciplinary approach could help speed up this process.

The fifth contribution by **Liana Tronci** investigates the synthetic future and the future periphrases μέλλω and θέλω in the Septuagint (LXX) and the New Testament. After providing a critical introduction to these two works and the distribution of voice markers in the synthetic future in Classical Greek, she analyses the various ways in which the future domain is reorganised: disappearance of Attic futures, replacement of media tantum futures, increase in passive futures and the replacement of the synthetic future by future periphrases (which according to her are not used as future periphrases in the LXX or the New Testament).

Tronci’s introduction contains a useful up-to-date overview of research into these Post-Classical texts and relevant philological background, such as that

the LXX is different from ordinary written Greek at that time (*pace* Horrocks 2010: 106, 146). The analysis is carefully made with due discussion of the different types of diachronic data (cf. the contrastive discussion of the voice system of Classical and Post-Classical Greek, the attention to both papyrological and literary evidence, and the support of her analyses with quantitative data). Still, the analyses are not altogether very surprising due to her earlier findings<sup>10</sup> on this topic. In addition, the important work by Basset 1979, Wakker 2006, and Allan 2017 on the development of μέλλω is left out.

Furthermore, her claim that the LXX and the New Testament only have modal uses of μέλλω and θέλω and not future uses is probably not accurate, given the examples where future is the best contextual interpretation. She suggests that *Exod.* 2.14 is either volitional θέλω or intention, but this example is best interpreted as a future use in this context. Just before, Abraham had hit and killed an Egyptian man because that man had punched one of his fellow countrymen. Subsequently, he hid the body, thereby hoping to escape notice. Then it turns out that this act had not escaped notice, therefore sparking the question from his fellow countrymen whether he is also going to kill them: μή ἀνελεῖν με σὺ θέλεις, ὃν τρόπον ἀνείλες ἐχθές τὸν Αἰγύπτιον; ‘Will you/Do you want/intend to kill me, the way that you killed the Egyptian yesterday?’ Examples for LXX are not discussed. By contrast, for the New Testament, she discusses various examples of θέλω and μέλλω, which in her view cannot be interpreted as a future auxiliary but are primarily used with a modal meaning. While she indeed discusses some convincing modal examples of θέλω, she mentions *John* 5:40 without discussing it, although it is in my view an example of future use. This example comes from a reprimand of Jewish leaders by Jesus, who says that his authority comes from God, and that what Jesus does is fulfil God’s work. Therefore, Jesus confronts them with the paradoxical fact that they read all scripture to obtain life but do not come to Jesus himself to obtain life. Thus, θέλετε here, as I see it, expresses that the Jewish leaders are not going to come to Jesus (cf. the dependent future final clause): καὶ οὐ θέλετε ἐλθεῖν πρὸς με ἵνα ζώῃν ἔχητε ‘And you will not come to me so that you might have life’. Finally, there are convincing examples of future μέλλω in the New Testament (and contemporary literary works<sup>11</sup>) which are not discussed by her. For example, in *Luke* 9.44.3 Jesus predicts his future death to the apostles the second time saying, ὁ γὰρ υἱὸς τοῦ ἀνθρώπου μέλλει παραδίδοσθαι εἰς χεῖρας ἀνθρώπων ‘for the son of man will be delivered into the hands of men’ (my own transla-

10 See Tronci 2017a, 2017b, 2017c and 2019.

11 See Jos. A. J. 1.5, cited by Crellin in his chapter (p. 273).

tion). In this context the immediate future ('is about to') or intention ('intends to') is not available, and μέλλει refers to a future state of affairs which is presented as certain, the way that the future indicative presents future state of affairs.<sup>12</sup>

The sixth contribution by **Brian D. Joseph** takes a closer look at the replacement of the infinitive by the finite ἵνα/να clauses, a change which took place in the period from Post-Classical to Medieval and Modern Greek. He suggests that this replacement offers counterevidence to the unidirectionality hypothesis from (Haspelmathian) grammaticalization, since the replacement comes with a decrease in bondedness (instead of increase as per the hypothesis), and as such qualifies rather as degrammaticalization (a weakening of the structural bonds between the verb and the complement). To support this view, he discusses evidence from the Koiné all the way up to Modern Greek dialects and concludes that the Greek infinitive<sup>13</sup> offers a treasure trove for degrammaticalization research, because the replacement happened repeatedly throughout the history of Greek with different verb types.

The paper has a theoretical focus and illustrates nicely what the evidence from the history of Greek has to offer to debates in general linguistics. While research on grammaticalization in Ancient Greek has gained much attention, degrammaticalization research on Ancient Greek is rather limited.<sup>14</sup> Still, probably due to the theoretical focus, the Ancient Greek linguist is left wanting more systematic backing of the conclusions with diachronic data, since the evidence from very different periods is presented by way of exemplification. To illustrate the long history of "Post-Classical Greek", Joseph discusses the New Testament (1st c. AD, which is often used as a short-cut for the Koiné), the Acts of Pilate (4th c. AD) and the Chronicle of Morea (14th/15th c. AD, another frequently used exemplary text), but does not introduce the much earlier evidence for this replacement from early Post-Classical Greek and much relevant research which has been carried out on the replacement/retreat of the infinitive.<sup>15</sup> Incorporation of this data would have strengthened his recommendation of the infinitive replacement as a rich future research area. Moreover, the absence of this data

12 cf. la Roi (2019: 70–71) on the variable epistemic strength of future indicative clauses.

13 Joseph (pp. 146, 160) rightly points us to the relevance of this analysis for the infinitives in other Balkan languages.

14 For an example, see Allan 2017, who suggests that the non-modal/future meaning of μέλλω as 'delay' is an instance of degrammaticalization.

15 See Bentein 2015, 2017a, and 2018, and the absent grammars Mayser 1926 and Mandilaras 1973. Especially relevant is Bentein 2018 since it deals with the retreat of the infinitive and how this has affected different verb types. For his discussion of the future, it is surprising that Markopoulos 2009 is missing.

makes it all the more attractive to think that we are not just dealing with one long recurring trend of degrammaticalization that starts in the Koiné and continues to affect Modern Greek, but that this replacement is affected by other processes as well. For example, the independent Modern Greek  $\nu\alpha$  clauses cannot easily be compared to the infinitive (see Joseph, p. 155), because historically they are insubordinated clauses, and  $\tilde{\nu}\alpha$  clauses underwent insubordination (i.e., the conventionalization of main clause use by a subordinate clause) from as early as Classical Greek. As discussed by la Roi (2020),  $\tilde{\nu}\alpha$  clauses underwent insubordination in Classical and Post-Classical Greek and, as a result, acquired directive, wish, and declarative uses, uses which we in fact also see in Medieval Greek.<sup>16</sup>

The seventh and longest contribution is a joint one by **Nikolaos Lavidas** and **Dag Trygve Truslew Haug**, who examine backward control, the situation where “the controller is expressed overtly in the *embedded clause* and controls an empty position in the matrix clause” in the PROIEL diachronic corpus of Herodotus, the New Testament and Sphrantzes (15th c. AD). After introducing the extended version of the PROIEL corpus and its potential for diachronic analyses, they illustrate which constructions genuinely are discontinuous (i.e., not backward control constructions), which ones increase from Herodotus to the New Testament, and how Sphrantzes’ use of these constructions is an archaizing mix of older and newer uses (as corroborated by his use of other linguistic features such as gerunds and voice morphology). Importantly, they demonstrate the usefulness of quantitative analyses for answering philological questions with regards to the linguistic profile of an author.

Methodologically, this chapter makes some relevant recommendations: (1) the need for quantitative data analysis for replicable results; (2) the importance of stylistic considerations (e.g., “parallel grammars”) and conspiring changes (e.g., participles changing to gerunds) to evaluate the synchronic language state evidenced in an author; and (3) the use of “checkpoints” for diachronic analyses. Nevertheless, this last point can be questioned, most importantly because of the metaphor of a checkpoint. A checkpoint implies a full status update of all new and old linguistic features, but the three texts that make up the extended PROIEL database do not fully function as such. After all, these texts

16 In addition, Joseph (p. 147) characterises the infinitive *pro imperativo* as a dependent element by assuming a higher controlling verb, but historical evidence for such a higher controlling verb is absent since the infinitive can be used independently not only for directives but also for exclamatives and wishes (see Denizot 2011 and la Roi 2020). For a parallel discussion of (in)dependent infinitives in Slavic, their status and the historical processes leading to them, see Wiemer 2019.

very likely differ from other contemporary texts for a wide range of reasons. Much like example-based comparisons, such a diachronic analysis is somewhat coarse grained (depending on the subject) and could provide generalisations that are based on limited data. Still, their quantitative method will lead to replicable results, which, as they say, is a fundamental principle for progress.

The eighth contribution by **Marina Benedetti** provides an interesting contrastive analysis of the construal of diathesis in the perfect by Theodosius of Alexandria (4th c. AD), the role of textual evidence for his categories, and current views on diathesis and the perfect. First she offers a lucid introduction of Theodosius' *Κανόνες* as an ahistorical rule-book that does not conform to the textual evidence (cf., e.g., the unattested perfect *τέτυφα*). Next she successfully reinterprets his tripartite perfect system as consisting of diathetically indifferent forms ("middle") and non-indifferent forms ("active" and "passive"). When comparing this synchronic system with the historical development of the perfect, Theodosius' division between aspirated or *-χα* perfects and root perfects reflects their diathetic orientation, since the former were introduced as diathetically non-indifferent alternative to the diathetically indifferent root perfects.

Her argument is carefully made and supported by due consideration of philological and historical data. In fact, the reviewer admits to wanting to hear more after reading the analysis, because (much like Stolk's contribution) this contribution is clearly situated at the interface of philology and historical linguistics and could offer more avenues of interdisciplinary research. An aspect that would deserve further consideration is the role of the chosen lexemes for declension patterns in the tradition of ancient scholarship. While Benedetti (p. 207) rightly acknowledges that this method is part of a widely shared ancient didactic practice, it would be interesting to find out how this tradition has evolved. For example, both Apollonius Dyscolus and Aelius Herodian use *τύπτω* as a lexeme, and the latter even discusses the unattested *τέτυφα*.

The ninth contribution by **Carla Bruno** is an in-depth analysis of the variation, both linguistic (co-text and context) and sociolinguistic (addressee relationship), of directive strategies in a small corpus of Ptolemaic papyri. Bruno clearly and concisely illustrates how the illocutionary force of different directive strategies, e.g., the variable force of directive imperatives (as signalled by linguistic cues of modulation) vs. performatives and indirect speech acts, which are more restricted in their force, correlate with sociolinguistic factors such as symmetrical vs. asymmetrical relationships of the speaker and addressee. To illustrate, performative speech acts are typical of asymmetric exchanges as they make the exchange dynamics explicit.

The choice by Bruno to conduct an in-depth investigation of only one period has surely contributed to a great amount of qualitative observations. As a result, this study provides a welcome model for further quantitative studies on larger corpora which would investigate both whether these correlations hold up to scrutiny for more papyri and whether one could find relevant deviations in specific papyri from the Ptolemaic period or later periods. A small comment would be that Bruno analyses all conditionals in close proximity of directive strategies as mitigators, but does not distinguish between the types of conditionals. Not only the modal value of the conditional but also the type of conditional (predicational, propositional, or illocutionary; see Wakker 1994) codetermines possible mitigating effects.

The tenth contribution by **Robert Crellin** deals with the question of how the strategies of (non-)adaptation of Hebrew personal names by authors of different backgrounds relate to the construction of their identity. By analysing the ways that the Septuagint, Ezekiel the tragedian, Philo of Alexandria, Flavius Josephus, and contemporary epigraphic and documentary sources choose to present Hebrew personal names, he reveals, among others, that adaptation is a later trend which is especially adopted in Flavius Josephus and contemporary epigraphic and documentary sources to adapt to Greco-Roman culture where Greek was dominant. By contrast, the former authors with Jewish origins prefer non-adaptation to maintain their Jewish identity. Thus, Crellin convincingly argues that these changing practices have their origin in authors' sociolinguistic considerations, as they want to place themselves among specific groups and audiences.

An important result with wider significance for other fields of "Post-Classical Greek" is that Crellin showed that non-adaptation cannot simply be explained as an example of low-level Greek. Rather, what may look like low-level Greek is something else altogether. Furthermore, Crellin neatly illustrates the potential perks of combining a diachronic analysis with a philological-cultural framework, as the quantitative data itself could have been misinterpreted if the social background of adaptation was left unconsidered. For the future it would be interesting to find out whether these diachronic social considerations extend to other elements which have been viewed as low-level Greek on the part of Jewish writers or simply as Jewish/Hebraistic elements.

The eleventh contribution, by **Sonja Dahlgren** and **Martti Leiwo**, is on the effects of the Coptic system on written L2 Greek in Egypt. Dahlgren and Leiwo provide a persuasive alternative account to viewing nonstandard spellings as "bad Greek" or due to phonological change (from the more phonologically conservative Macedonian Greek) by emphasizing the role of the Coptic system for L2 writers. Among others, they demonstrate that the underdifferentia-

tion of vowels due to the more limited Coptic vowel system and Coptic stress accent (which reduced vowels) leads to nonstandard spellings such as  $\pi\upsilon\rho\upsilon\sigma$  ‘of wheat’ for  $\pi\upsilon\rho\omicron\upsilon$  and  $\pi\acute{\epsilon}\mu\psi\epsilon$  ‘(to) send’ (for imperative  $\pi\acute{\epsilon}\mu\psi\omicron\nu$  and infinitive  $\pi\acute{\epsilon}\mu\psi\alpha\iota$ ). The way that Greek loanwords were integrated into Coptic corroborates these processes.

In a way comparable to Crellin, Dahlgren and Leiwo provide a more nuanced view of what has been called “bad Greek” before. Their argument fully takes into consideration other potential explanations and contains examples where their explanation is more attractive than, for example, one of phonological change. A relevant question for further research would be to ask where the two meet in time. For example, can we observe diachronic changes in spelling due to increase in proficiency? In other words, does tracing L2 Greek speakers through time reveal that they move up the proficiency scale of bilingualism (see Dahlgren & Leiwo p. 285) and accordingly spell items differently?

The twelfth and final contribution by **José Luis García Ramón** studies the intense interaction of supradialectal structures of the Koiné with local dialectal structures from the 3rd to the 1st century BC. As argued by García Ramón, what may look dialectal may actually be a supradialectal structure used before the dialectal attestation, a hybrid structure (a combination of both) or a hyperdialectalism which uses a dialectal feature to make a new form which is extra dialectal. Also, sometimes the evidence is simply too limited to determine the exact heritage of a form, for example due to limited preservation of the dialect or reliance on linguistic reconstruction. Nevertheless, the detailed discussion of every dialectal passage provides us with welcome guidance through the material.

The contribution gives a useful overview of the complexities of dialectal evidence for the Koiné. One aspect which may trouble readers is the treatment of the terms “formula” and “formulaic expressions,” as García Ramón does not provide a definition of these terms but mentions both simple collocations and complex phrases as falling under this umbrella. He could, for example, have defined them with parameters such as frequency and/or a fixed form-function relationship or have made use of recent work on formulaic language in the papyri (Nachtergaele 2015) or Homeric formulaic phrasing (Bozzone 2014). Finally, a work which could have been given as relevant background to the decline of dialectal differentiation is Bubenik 1989.

#### 4 Overall value of the book

Now it is time to discuss the overall value of the book and answer the question whether the contributions have successfully contributed to the aim of the editors to endorse rephilologized or at least multidimensional analyses which go beyond historical linguistic data analysis. A mixed answer should be given to this question in my evaluation. On the one hand, the book is to be commended for its sheer breadth of coverage and because it gathered specialists from important research fields of “Post-Classical Greek,” who not only provide welcome new insights but also present useful overviews of their fields. Also, the book has many contributions which opt for a multi-dimensional analysis to a diachronic problem and thereby support the editors’ view that multi-dimensional analyses are better equipped to solving historical linguistic issues or tracing language change in Ancient Greek than unidimensional analyses. In fact, there are some contributions which, as discussed above, illustrate how illuminating it can be to combine the analysis of quantitative historical data with philological factors, such as Stolk, Benedetti, Crellin, and Tronci, in a rephilologized manner. On the other hand, we have seen several examples where a rephilologized analysis of the quantitative historical data has rather demonstrated the specific challenges of this combined approach. For example, in their attempt to generalize over longer periods, some contributions have relied on linguistic philological methods that do not stand up to scrutiny of corpus linguistic demands, among others because of the use of author shortcuts for linguistic phenomena of a period or impressionistic linguistic histories which actually cover longer periods of time than the contributions in question discuss. Perhaps the occasional absence of important earlier work in several contributions must be attributed to the high demands of a multi-dimensional or rephilologized analysis, although these things could have been flagged by the reviewers and/or editors. Finally, it should be noted that the editing of the volume is sufficient but would surely have benefited from further revision to comply to de Gruyter’s standards (for five specific areas see the Appendix). Despite these shortcomings, the fact that the contributions reveal the pitfalls and principles for progress in analysing “Post-Classical Greek” make the book a useful starting point for future research into the long and varied history of “Post-Classical Greek.” Therefore, I try to distill the most important guidelines that we should take away from this book.



## 5 Pitfalls and principles for progress in analysing “Post-Classical Greek”

This concluding section is divided by the six principles which, according to the reviewer, the editors, and some contributions of the volume, lead to a fuller understanding of the long history of “Post-Classical Greek.” Let us now discuss the benefits of these principles, for researchers with a philological angle, a linguistic angle, and all those in between.

As demonstrated by several contributors, a *rephilologized* analysis of historical language data can lead to meaningful revisions of existing explanations. The particularized philological and multidimensional explanations of distributional changes can put previous (and sometimes limited) quantitative generalisations in a new light. Ideally, we should take the best of both linguistic philological and historical linguistic approaches by incorporating the philological factors which influence historical linguistic developments and integrating these factors into our generalisations over specific periods (as some have done before).<sup>17</sup> At the same time, we should avoid inviting the drawbacks of linguistic philological approaches back in, such as the use of authors as shortcuts/checkpoints or reliance on our beloved linguistic histories of the long diachrony of Greek as accurate supporting evidence to characterise a linguistic issue for a larger period.<sup>18</sup> Rather, reliance on corpus-based analyses of successive smaller periods will be a much stronger basis for generalizations and, importantly, also be more helpful to solving philological issues. Furthermore, to tackle the gigantic amount of data that we have for “Post-Classical Greek”, we can and should make use of the many helpful analyses that have already been made for smaller periods (e.g., the in-depth knowledge of the workings of Classical Greek), provided that comparisons stay pointed to successive periods of “Post-Classical Greek” and not only with the Classical period.

Analyses based on imprecise periodisation such as Hellenistic-Roman for 4th BC–6th AD or 500–1100AD as Early Medieval Greek contribute to fallible generalizations which could have been prevented if smaller and *more precisely periodised data* were used. As mentioned above, periodisation is (almost) never uncontroversial.<sup>19</sup> Still, adopting more precise periodisation of smaller

17 As mentioned above, some already take this to heart in their diachronic analyses, which therefore are already rephilologized.

18 In fact, the idea of the New Testament as exemplification of the Koiné has also slowly been relegated to the past by New testament researchers, for which see O'Donnell 2005 and Pang (2016: 125–135).

19 Cf. for Latin, Adams (2011: 257) on the downsides of the broad and negative term “Late

periods will make it easier to create quantitative investigations which actually lead to falsifiable generalizations.<sup>20</sup> Moreover, such results would benefit philological approaches the most, since, as we have seen in the contributions by Stolk and by Lavidas and Haug, the support of quantitative generalisations can solve textual issues and more accurately determine an author's linguistic profile. At the same time, the use of such better periodised analyses enhances the transparency of the results. Also, it reduces a potentially fallacious off-shoot from philological thinking: teleological analyses, i.e., analyses which assume that earlier language stages follow a linear innovation trail from Ancient all the way to Modern Greek, making innovative structures which resemble Modern Greek ones seem the intended goal of the innovations.<sup>21</sup>

With our inherited view from linguistic philological histories that larger periods are characterised by certain innovations comes the fallacious expectation both that previous authors would lack these innovations and that contemporary authors must have them at the risk of contradicting this fallacious historical characterization. First of all, we know very well that changes rarely run so parallel and more often than not an innovation is anticipated in earlier periods. Second, the nature of linguistic innovation spread would actually suggest the reverse of this expectation, since innovations start small and spread by repetition through a speech community by speakers (see Croft 2000 and for an application Bentein 2012). Moreover, the evident constraint of "skewing effects" (Rafiyenko & Seržant, p. 12) from various variational factors and influence from linguistic-stylistic standards on the speed of innovation spread

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Latin": "Linguistic periodisation of this type is unsatisfactory (cf. the remarks of Frelouille (1996)), partly because the history of a language may be seen as a continuum, with diachronic changes running across any chronological divisions that one cares to set up, and particularly because many of the changes that were eventually to alter the shape of Latin and contribute to its Romance outcomes (such as changes to the system of gender, the spread of prepositions for plain case usage, the loss of deponent verbs) can be traced back to an early date. It is arbitrary to fasten on to any 'late' period, say CE 200–600, with blinkered vision, and may create an expectation that there will be identifiable within that period a host of new usages belonging exclusively to it." See also Adamik 2015 for a critical discussion of different types of periodisation and an attempt to use a combination of historical, sociolinguistic and communicative factors to set up a more satisfactory periodisation of Latin.

20 In fact, the one most suitable to this end is the periodisation suggested by Lee (2007: 113) and applied by Bentein (2016): early Post-Classical 3rd–1st BC, middle Post-Classical 1st–3rd AD, late Post-Classical 4th–6th, and early Byzantine 7th–8th.

21 For the hotly debated role of teleological mechanisms in language change, see Croft (2000: 66–73) and Luraghi (2010: 364–366).

underlines this gradual nature of change.<sup>22</sup> Third, the availability of so much later linguistic data can promote harmful teleological thinking, as certain innovations are often retained in later stages of “Post-Classical Greek” or even Modern Greek. As a result, those later retained structures may retrospectively be construed as the goal and force behind initial changes. However, there are many documented examples which argue against such a linear view of linguistic innovation: (1) in this book: the external possessor construction was attested before the New Testament and it is differently attested in contemporary papyri (Gianollo), earlier future uses of periphrases are not as present in specific authors as implied by previous overviews (Tronci and Kölligan), the participial future use of ὀφείλω from John Malalas appears unpreserved in later Greek (Kölligan); (2) re-evaluation of alleged grammaticalization paths: the earlier evidence for ἐθέλω demonstrates that it follows the path volition > (generic-)habitual > intention > futurity, instead of the common volition > intention > futurity path (la Roi (forthcoming), *pace* Markopoulos 2009: 40–45) and the grammaticalized future auxiliary μέλλω unexpectedly acquired the meaning ‘delay’ through degrammaticalization (Allan 2017); (3) newly acquired functions of prepositions are abandoned later on (cf. the loss of ὑπέρ for comparisons and διά for AGENT, SOURCE, and OPPONENT) (Bortone 2010: 192 and Bentein 2017b).<sup>23</sup>

Thus, the analysis of smaller, successive and more precisely periodised data will lead to *corpus-representative results*, meaning that the generalizations are more likely to hold for the whole corpus of that period. After all, the degree of variation and change in one author generally cannot compete with the amount of variation in a whole period. One might take from the preceding that an author-specific analysis is undesirable in itself, but that is surely not the case. As demonstrated by the successful qualitative investigations of specific authors in this book, such in-depth analyses can explain presence or absence of specific variations or changes, for example, the use of archaizing constructions by John Malalas or the markedly different strategy towards Hebrew name adaptation by Flavius Josephus. In fact, such more nuanced factors are unfortunately not always incorporated by the historical linguists who focus on creating falsifiable generalizations (Dollinger 2016: 67). Therefore, those at both ends of the historical linguistic and philological continuum are bound to learn from each other: a philologist’s discovery of a meaningful variation/change may lead to system-

22 For the gradualness of language change, see Traugott & Trousdale 2010.

23 Naturally, these examples cannot be expected to exhaust this type of evidence, but merely indicate that this issue of non-linear development is not as frequently discussed as is desirable.

atic quantitative and multi-dimensional explanations for a period, which in turn may be used by the philologist for a fuller explanation of author-specific variation.<sup>24</sup>

In addition to corpus representative results, new *generalisable findings* have as a precondition that relevant previous generalisations are falsified first. In the investigation of “Post-Classical Greek,” we therefore have the responsibility to test older generalisations before advancing our new ones. In practice, this means that we should (1) incorporate older research as much as possible (which sadly is not always the case, as discussed above), and (2) seek to fine-tune existing coarse-grained overviews that we possess with regard to the phenomena at hand (e.g., in linguistic philological histories or author-specific grammars). Also, when we have specified the periodised data and how new generalisable findings relate to fallible previous ones, we should detail our precise data collection process in order to make the research replicable. In fact, in methodological discussions of historical linguistics it has been argued that each research should outline this process *before* the analysis so as to avoid confirmation bias, anecdotal quantitative evidence, or imprecise qualifications (e.g. many, often, sometimes, etc.).<sup>25</sup> However, this practice is still not that widespread in Ancient Greek linguistics, in part due to our philological heritage and in part due to the fact that many editors do not ask you to provide a list of investigated instances. Yet, cumbersome as it might be, it would ultimately save future researchers time to elaborate on our previous research, which, interestingly, means that good results would receive their due reconsideration. In short, facilitating replicable research has a double benefit.

Moreover, incorporating these principles will facilitate a richer *data synthesis* in areas that need it. First of all, the road to period-specific grammars of the long history of “Post-Classical Greek” will be shorter. Second, rephilologized reconsiderations of domain specific overviews, as done, for example, for the claims in Markopoulos 2009 by Tronci in this book, can be expected in the near future. Third, a historical sociolinguistic perspective (cf. Cuomo & von Trapp 2017) could easily be presented for a specific period as a rephilologized alternative to domain specific and linguistic philological histories.

To sum up, the trend of rephilologized diachronic data analysis to which this book aims to contribute has much to offer us. The varied nature of the contributions helped us see the many areas which could potentially benefit from the combination of these two perspectives. As I hope to have demonstrated,

24 Cf. Dollinger (2016: 82), who argues that philological competence is a precondition for profound quantitative historical linguistics.

25 See Jensen & McGillivray (2017: 12–15).

the successful combination of these two perspectives will be difficult if we are not wary of the drawbacks that we risk inviting back in when reintegrating linguistic philological methodology to analyse the long and varied history of “Post-Classical Greek.” Nevertheless, the growing body of rewarding research into this long history as found in this book will surely help elucidate the, for now, darker subperiods of the “Post-Classical Greek” language.

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## Appendix

1. **Typos.** There are some awkward typos, for example in a section title (see García Ramón, p. 316 “Koiné and Dialect *n* in Inscriptions Written in Thessalian: The Formulaic Expressions” with an unnecessary *n*) and, surprisingly, in the keywords of contributions “Romand period” for “Roman period” in di Bartolo’s contribution, and “Greek dialec” for “Greek dialect” in Garcia Ramon’s.<sup>26</sup> There is also a consistent misspelling of “Palmer” (the linguist) as “Palme” (the papyrologist) in di Bartolo.
2. **Glosses.** The presence of Leipzig glosses will hopefully help disseminate the results of this book. Still, García Ramón’s contribution unfortunately lacks them and others limit annotations, e.g., not every contribution annotates mood and, for example, Stolk and Joseph do not annotate tense-aspect.<sup>27</sup>
3. **Bibliography.** Reference styles are generally very consistent, except for the introductory paper by Rafiyenko and Seržant<sup>28</sup> and some minor mistakes.<sup>29</sup>
4. **Size.** The size of the contributions varies considerably, with the shortest contribution being just 13 pages long (by Stolk) and the longest 39 pages (by Lavidas and Haug).
5. **Index.** This 339-page book concludes with only one short English index of just 2.5 pages, but an *index locorum* would be useful to future research. Comparing the index with the contributions, the most important terms are represented, although some important terms are missing: (1) *prepositions*, even though Stolk’s contribution is dedicated to it and the introductory summary pays much attention to prepositions; (2) *personal names*, which is only mentioned as “personal names in Classical Greek,” but

26 Perhaps this typo contributed to the lack of dialect as a separate term in the index?

27 Gianollo sometimes lacks tense marking, for which, see p. 45 example 6. Also, Crellin (p. 273) uses glosses except for two examples where the linguistic content is not at issue.

28 They do not put book titles consistently in cursive or include first names (see for both, for example, Schmid and Schmitz) and present other small deviations, e.g., a comma after Luraghi, a missing full stop after Horrocks 2014, and both a comma and a period after the title of the book to which Horrocks 2007 belongs.

29 di Bartolo (p. 38) confuses Palme and Palmer by calling both Palme and writes “Greek: A comprehensive grammar”; Bruno (p. 243) refers to Eleanor Dickey as both Elenor and Eleanor; and Dahlgren & Leiwo (p. 299) did not put the title of Dahlgren’s 2017 book in cursive. Furthermore, the treatment of the name José Luis García Ramón is not consistent with how the names of the other authors are treated, since they are presented as first name last name in the table of contents, the first page of their chapter and its running headers, whereas José Luis García Ramón is presented as García Ramón José Luis.

Crellin's contribution focuses on the treatment of Hebrew names in various stages of Post-Classical Greek; (3) *language change* or *morphosyntactic change*, which is fundamental to the introductory chapter and many contributions. Also, sometimes specific terms are subsumed under rather general terms (see, e.g., "conjunction") or terms of lesser importance are noted such as "Jordan" or "Palestine."<sup>30</sup>

- 30 One wonders, for example, why Herodotus is mentioned as a term for Crellin's contribution but more important authors such as Ezekiel the tragedian, Philo of Alexandria and Flavius Josephus are absent as indexed terms.