

continued well into the Middle Ages. Ieraci Bio might perhaps have stated more clearly that Galen developed his ideal in reaction to schools of medicine more hostile to philosophical speculation. Apparently he didn't succeed in silencing the detractors of the broadly educated physician.

Among the articles on the transmission of Galenic texts, Jacques Jouanna's 'Remarques sur la tradition arabe du commentaire de Galien aux traités hippocratiques des Airs, eaux et lieux et Du Serment' is of special relevance to the study of Galen himself. Galen's commentary on 'Airs, waters and places' has been lost in Greek, but references and translated fragments have surfaced at several occasions. A complete translation in Arabic has been preserved in a manuscript at Cairo, but it isn't as yet available. Jouanna adds a new source of fragments from this commentary, contained in an Arabic treatise on medical ethics by al-Ruhawi (a translation appeared TAPhS, 1967), which also testifies to the authenticity of the fragments of Galen's commentary on the Hippocratic Oath preserved in an Arabic translation (published BHM 1956), which until now was regarded as spurious. Arabic scholarship still promises to have something in store for students of Galen.

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M. BARATIN, *La Naissance de la Syntaxe à Rome*. Paris,
Les éditions de Minuit, 1989. 539 p. Pr. F. 195,—.

This book is another manifestation of the renewed interest in the history of ancient linguistics. Its author has various previous publications in this same field to his name and presents us here with an extended version of his 'Thèse d'Etat', defended at Sorbonne University in 1987. His aim in writing this book is "à délimiter les conditions dans lesquelles a pu apparaître et se développer (ou au contraire a été empêchée) une *théorie* syntaxique [as opposed to isolated observations of a syntactic nature, I.S.], c'est-à-dire une conception explicite et organisée de ce type d'analyses" (p. 10). B. looks for traces of a syntactic theory in Roman linguistics and in order to do so he adopts the ancient framework of *oratio/logos*, rather than any modern view on syntax. *Oratio* may be defined as "une combinaison de mots qui constitue une pensée achevée et qui

exprime quelque chose de complet" (p. 9, translating Diomedes, *GL* I 300,18-9). According to B., any analysis performed in this framework may be considered a syntactic type of analysis. Furthermore, B. combats the "conception fixiste" of the history of ancient linguistics which reduces each and every theory to a perversion of original Stoic thinking; instead, B. subscribes to the sound methodological principle of studying his material in its original context (p. 12).

B. goes in search of syntax in three, or rather four movements. All four topics are intelligently chosen for their 'syntactic potential'. Since they all consist of very detailed and intricate analyses, a rapid sketch of the bare outlines must suffice. This will leave more room for discussion of details. As a kind of prologue B. investigates ancient theories on the conjunction, because he expects to find an analysis of relations there. In part I of the main body of the book, he looks for the origin of syntax in dialectics (logic), taking as his point of departure the curious text *De coniunctione temporum* by Diomedes (*GL* I 388,11-397,10). Part II is devoted to the study of linguistic 'correctness', more particularly to the concept of solecism and the question whether any syntactic analysis is at the basis of the judgment that something is a 'fault'. In part III B. offers a thorough and interesting analysis of Priscian's *De Constructione*. However, it appears that each of these four royal highways to syntactic analysis first turns into a muddy footpath and finally comes to a dead end: Traces of syntax may have been there, an original awareness of structure can be detected, but it never grew into a functional system. The *Artes* shattered all comprehensiveness and left us with remarks on the isolated word and its *accidentia*. Syntax was lost, before it could fully develop, and this was due to a conscious decision of the artigraphs. This is the main thesis of B.'s study.

The book covers a wide range of topics and B. shows his familiarity with especially the Latin sources in a great many acute observations of detail. At several places he is able to give convincing reconstructions of rather complicated developments, as when he describes the semantic development of the word *lexis*, which accounts for various difficulties in the study of solecism (p. 292 ff.; already discussed in M. Baratin & F. Desbordes, *La 'troisième partie' de l'ARS GRAMMATICA*, *Hist. Ling.* 13 (1986), 215-40).

Some examples may serve to illustrate these positive points: — P. 35 ff. B. offers a valuable and subtle interpretation of the definition of σύνδεσμος in the

Techne (GG I i 86,3-4) He defends the reading δηλοῦσα instead of πληροῦσα: σύνδεσμός ἐστι λέξις συνδέουσα διάνοιαν μετὰ τάξεως καὶ τὸ τῆς ἐρμηνείας κεχρηγὸς δηλοῦσα. He effectively discards the traditional argument that Τίγρη must have used a form of πληρόω in his definition (cf. Apoll. Dysc. *coni* 247,22-6) by pointing out the incompatibility of Apollonius' quotation (καὶ τὸ κεχρηγὸς τῆς ἐρμηνείας ἔστιν ὅπου παραπληρῶν) with the normal form of a definition where the participle should either refer to the neuter μέρος λόγου or the feminine λέξις. Thus, the quotation must derive from the elaboration of Τίγρη's definition in the form παραλαμβάνεται δὲ (sc. ὁ σύνδεσμος) ἥτοι χάριν .. ἢ .. ἢ ... καὶ τὸ κεχρηγὸς τῆς ἐρμηνείας ἔστιν ὅπου παραπληρῶν (p. 34 f.). The definition in the *Techne* (reading δηλοῦσα) is taken to mean that the conjunction is used when the relationship between certain elements of the expression must be made explicit. "La conjonction 'indique' ce qui est implicite" (p. 38). In fact, B's (attractive) interpretation is anticipated by a Scholast on the *Techne* (GG I iii 436,30 ff.). I fully approve of B's suggestion that the definition in the *Techne* is the result of basically Stoic theory with clear traces of an Aristotelean influence (the word ἐρμηνεία stands out like a flag) (p. 39-40). Exactly the same process is distinguishable in other domains of the ancient theory of the conjunction as well (Cf. my *Ancient Grammar in Context. Contributions to the Study of Ancient Linguistic Thought* (Amsterdam 1990), 153 ff. [henceforth abbreviated as AGC]). — On p. 80 B discusses the difference between the problematic categories *causales* and *rationales*. According to B the *causales* indicate that the propositions they introduce have the (logical) function of *antecedens*, whereas the *rationales* introduce a *δευτερον/consequens*. This is an interesting suggestion, although it is impossible to explain all instances along these lines. B does not even attempt to do so, claiming (p. 88) that when an overall theoretical framework can be posited, it is immaterial whether all individual data can be accommodated. Personally, I am inclined to a somewhat more positivistic attitude, but that may be a matter of taste. — On p. 178 B explains why the Stoics never paid much attention to the ('dependent') subjunctive. He rightly points out that their primary interest in propositions excludes the study of the subjunctive in complex sentences, because such a sentence does not constitute a separate type of proposition. — On p. 411 f. B describes an important difference between Stoic grammatical theory and Augustine's *Dialectica*. The Stoic *lekton ellipsis* is a predicate, which is essential for forming a *lekton autoteles*. The predicate is looked upon as the nucleus of the sentence. In Augustine, on the other hand, an incomplete sentence is a sentence which lacks a predicate. Thus, this is not a *lekton ellipsis*, but an 'énoncé inachevé'. Here we witness a transition from the notion of 'nucleus' to that of 'incompleteness'. — On p. 152 B sums up an admirable analysis of the interaction between rhetorical and grammatical approaches to 'abnormal' usage of language. The grammarians ended up by appropriating a specific element of rhetoric, namely the word-figures, a result of the study of *ornament* ('figures - virtutes'). And due to a symmetrical evolution the rhetoricians adopted a specific element of grammar, the figures resulting from the study of *correctness* ('figures - vitia, ou solécismes excusables'). In Quintilian this integration still shows through: he calls this type of word-figures *genus grammaticum* (Quint. IX 3,2). — On p. 470 f. B has a good discussion of Priscian's theory of the cases, which is thoroughly *semantic* in nature, as B freely acknowledges.

In spite of all these merits (and I have left many other instances unmentioned), this book left me with vague feelings of frustration, caused by a number of reasons. If I will use the remainder of this

review to illustrate some of the drawbacks of B.'s work (and this, too, is a selection), it should be kept in mind that this book is still a worthy testimony to the author's considerable abilities.

1 First of all, one might wonder whether B.'s subject itself is not, in a way, a betrayal of his professed principles. His obstinate search for syntax is at bottom a negative one, in that it ignores what Antiquity does offer in favour of what it does not. This starts with the very definition of *oratio* quoted above, which contains a heavy emphasis on the semantic aspect, as B. acknowledges. The concept of *καταλληλότης*, or *congruitas*, 'coherence', combines what we would call semantic and syntactic aspects, whereas 'syntax' in its original sense is no more than the *combination* of parts into a whole. Why then look for 'syntax' at all costs?

As an illustration, let me discuss p. 416 ff. Here, B. studies the criteria that were used to distinguish conjunctions and adverbs. The most interesting problem is presented by words which can function as both (like *quando*, which is either an adverb of time, or a causal conjunction, irrespective of its 'construction'). B. notices that Priscian distinguishes conjunctive and adverbial use on purely semantic grounds (temporal meaning leading to classification as an adverb, causal meaning to that as a conjunction), and finds this bizarre (p. 417). He then proceeds to the criteria used by Apollonius Dyscolus, dwells extensively on the most 'syntactic' among them, but is forced to reject them as well. He then offers his own solution (but what is the status of that solution?) if the semantic characteristics of the word in question recur in the semantic characteristics of the verb (e.g. time and place ('')), it is an adverb, if not, it is a conjunction. Now, 'place' might perhaps indeed be considered a 'variable du verbe' (p. 420) in Latin grammatical theory (although B. offers no attestations, cf. however AGC 51 n. 46). But I am not aware of any Greek examples (AGC p. 98), and I do not find it a very convincing notion. In fact, the semantic criterion is decisive, and understandably so. Conjunctions and adverbs have quite different theoretical origins. The study of causes originated in a philosophical context and the Stoics laid the basis for later grammatical theory by connecting certain conjunctions with the notion of 'cause'. Construction did not play any role of significance ('I am praised, because I read well' is logically equivalent to 'I read well. Therefore I am praised', or to 'I am praised. For I read well'). Adverbs, on the other hand, played hardly any role in Stoic theory (apart from Antipater's adoption of the *μεσότης* as a part of speech). However, in grammatical theory they are relevant in the theory of interrogatives (which always are either nouns or adverbs) and these, in turn, are connected with the rhetorical establishment of *περιστάσεις*, especially those of time, place and manner. This original theoretical separation may account for the not so bizarre application of the semantic criterion. (See further AGC 130 ff.)

B. is not interested in isolated remarks of a syntactic nature (p. 10)—which are abundant in ancient grammar—, but looks for a real theory—which is not there. In itself, such a negative result is not without its value. But it is the *communis opinio* that there was no syntax in a systematic sense in Antiquity; therefore, this result of

a 500-odd-page book with the title 'The Birth of Syntax' is somewhat disappointing.

2 B.'s method seems irreproachable. who would like to be called "fixiste" these days? However, in a sense B. has a very "fixiste" outlook, if not with respect to the Stoa. His trump ace is called Varro. This is especially apparent in the second half of part I.

I could say *passim*, but cf e.g. p. 56, p. 96 ("nous préférons une interprétation strictement latine de cette classification. Varron a toute chance d'en être l'auteur"), note the subtle preparation on p. 188 where B. casually (and seemingly irrelevantly) remarks that Varro's character structure shows remarkable similarities to that of Probus and—in a different respect—, of Palaemon. And indeed, on p. 199 he is posited as the common source for these two¹, p. 353 etc., sometimes he makes do with Posidonius (e.g. p. 47, 109).

In spite of his familiarity with the dangers of the hermeneutic circle (p. 12; p. 298), B. spends an enormous amount of energy on the reconstruction of lost Varronic texts from later sources, while positing these texts as the basis for later developments (= 'degeneration'). Part I ch. 5 is an extreme example.

These reconstructions are sometimes unnecessarily long (as when B. offers Willmann's solution to the problem of the relationship between Varro and Augustine, p. 226, after a discussion which began on p. 207, where Willmann's was mentioned for the first time), sometimes they contain circular reasonings even within the general framework which is circular as well. On p. 230 f. we are told that in *De lingua Latina* (LL) the "valences syntaxiques" of the conjunction were studied in two different contexts: that of the combination of conjunctions with different moods and that of the combination of different propositions. This hypothetical construct of the way LL dealt with the conjunctions is then styled a *confirmation* of the fact that Varro was the source for the discussion in Diomedes' *De coniunctione temporum* (where the conjunctions were discussed in their combination with various moods), and for contexts where the conjunction figured in an "analyse d'ensembles" (i.e. more dialectical ones). Another example of circularity is p. 247, where B. tries to explain why Varro chose a 'dialectical' model for his LL. If I understand him correctly, he argues that since Augustine's *De dialectica* is concerned with meaning only, therefore LL must have been concerned with meaning only. This means that Varro must have wanted to discuss meaning, and that explains (?) why he chose a dialectical model. Without exception the reconstructions are speculative, as when B. suspects (p. 225) that *De dialectica* must have contained a short analysis of the "valences syntaxiques" of words in the part on the *verba simplicia*, and proceeds to posit an equally hypothetical correspondence with a similar passage in Varro (lost as well). Or when (p. 232) B. solves the problem of Varro's enigmatical word-class system by stating that the 'normal' classification will probably have been found in the (lost) third hexad of LL.

3 The book leaves the general impression that its author suffers from *horror vacui*. Of course, one should be careful of too positivistic an approach to the history of ancient linguistics. It certainly is a good idea to make the pieces fit into an overall scheme. But still the pieces should fit. As I noted already (ad p. 88), B. and I disagree

on this point. Let me illustrate what I mean by sketching B.'s method. What I object to, is that very detailed reconstructions are based on minor and marginal data. As I said, B.'s book consists of a number of case-studies, which have been skillfully welded together. As a point of departure B. takes chapters from the Latin antigraphical tradition (*de constructione temporum*, *de soloecismo et barbarismo*; *de consensu casuum* etc.) The methodical procedure of each part then shows a regular pattern: B. distinguishes two or more layers in the chapter on account of small irregularities or contradictions. One layer is that of the *Arx*. It is characterized by the fact that it takes the *partes orationis* and their *accidentia* as the basic items for any analysis (p. 57). Thus, the analysis never reaches a higher descriptive level, but remains rooted in morphological categories. The other level contains an (often minimal) trace of what B. calls "analyse de structures".

I find this terminology misleading, in that it evokes modern associations with syntax in a late sense of the word, viz. a structural analysis which abstracts from all semantic features. E.g. on p. 336 he uses the expression "analyse de la structure interne" to designate the procedure of Diomedes, *De consensu verborum cum casibus*, GL I 310,30-320,9. This is giving Diomedes too much credit.

B. then proceeds to derive this second level from a source (preferably lost bits of Varro) which was truly syntactic in nature, but has unfortunately been lost. In this way B. creates and erases a theory of syntax with one stroke of his pen—admittedly an unfortunate metaphor for a book of this amplitude. This procedure is, of course, highly speculative.

The tone is set right from the prologue, in which B. tries to show that the original classification of conjunctions was based on an analysis of sentence-type (thus, by implication, a 'syntactic' analysis), only later to be watered down to a single-word analysis. Here, too, the origin of a syntactic theory was suffocated by school practice.

B. thinks Aristotle posited a twofold division of the conjunctions (p. 20, on *Paet* 20, 1456b38-1457ab). Μέν, ἤτοι and δέ signify an opposition or a disjunction, they are "non-conjunctions", while the others do fulfill their primary task of σύνδεσις. One may not agree, but this is innocent enough. But on p. 61 this result undergoes a subtle, but dangerous change. B. now speaks of the Aristotelian bipartition of the conjunctions into disjunctive and copulative ones. This 'elementary opposition' is supposed to be at the basis of all later categories. On p. 67 B. remarks that the discovery of the *parapleromatikoi* changes the whole concept of the conjunction. *Parapleromatikoi* do not connect phrases. B. ascribes their adoption to Aristotelian influence (the "non-conjunctions"), but cf. Apoll. Dysc. on the Stoic Chaeremon (*Com* 248,1 ff.) and Apollonius' own arguments in favour of attributing meaning to *parapleromatikoi* (*Com* 249,21 ff.). On p. 82 ff. B. claims that the Latin classification of the conjunctions into five categories (*copulativae*, *disiunctivae*, *causales*, *rationales* and *expletivae*) is based on the four categories of change, *aductio*, *detractio*,

transmutatio and *immutatio*. This would provide a context of an 'analyse d'ensembles' for the theory of the conjunctions. He more or less successfully links *aductio* with the *copulativae*, and, with growing trouble, *deductio* with the *dyunctivae* and *transmutatio* with the *causales/rationales*. And then he finds himself stuck with the self-created problem of having to match *immutatio* with the *expletivae*, which he solves by assuming that these conjunctions are the 'substitutes' for a tacit assumption, an implicit context (p. 86). He concludes that the expletive "joint cet énoncé à cet autre auquel elle est substituée", a physical impossibility. If he has to return to this correspondence later, he wisely uses the copulative conjunctions only (e.g. p. 315). B's suggestion that the Latin classification of conjunctions is based on the categories of change is highly artificial. But my main objection to it is that I do not see why no grammarian ever mentioned this classificatory principle. We encounter the four change processes in all kinds of linguistic context. They are always quite easy to recognize because of the characteristic terminology (cf. p. 278 ff. dealing with these categories in the context of barbarisms and solecisms). I cannot think of any reason why this terminology would have been suppressed in this one context, unless it is because this is *not* the classification principle used here.

I add some points of detail. — P. 28 D L VII 72 Αἰτιῶδες δέ ἐστιν ἄξιωμα τὸ συντασσόμενον διὰ τοῦ 'διότι', οἷον 'διότι ἡμέρα ἐστὶ, φῶς ἐστίν' οἶονεὶ γὰρ αἰτίον ἐστὶ τὸ πρῶτον τοῦ δευτέρου. B suggests that οἶονεὶ indicates that there is only an apparent relationship of cause between the two propositions. If the conjunction did not make the causal link explicit, nobody would have made the link himself. The difference between complex causal propositions where there is a natural causal relationship between the component parts and those where such a connection is only made by the choice of words is known to the Stoics and—in grammar—to Apoll. Dysc. (*Cont.* 235,8, cf. 217,5, see AGC 146 n. 9). But in the passage from D L, οἶονεὶ is used, because a κατηγορημα, which is incorporeal, can never be an αἰτίον in the strict sense, because an αἰτίον is always a body. Οἶονεὶ simply excuses the catachrestic choice of words (cf. *Mnemos.* 41 (1988), 19, n. 7). — On p. 97 B tries to use the label διατακτικός or διαταγμός to distinguish between various types of *causales* and *rationales*. He claims there are no Greek examples of these words indicating a class of conjunctions. See, however, Apoll. Dysc. *Synt.* 375,3 f., Schol. in Dion. Thr. 284,1-2, Schol. Hom. I 56-8a*, Herodianus *De Fig. (Rhet. Gr.)* III 88,18 Spengel (ὅπως), Theodos. *GG* IV 1 72,12, Choerob. *GG* IV n. 275, 13 f., Epim. Hom. (Anecd. Oxon. Cramer) I 162,26 ff., Mich. Synce § 196 Donnet, *CAG* IV 6 68-10 (and my AGC 129, n. 345). — On p. 107 n. 2 B emphasizes the fact that 'doubt' is never a semantic characteristic of the "conjunctions d'implication", see however Epim. Hom. (Anecd. Oxon. Cramer) I 162,26 ff. on the difference between εἰ and ἐπεὶ, where it is remarked that the former is used ἐν διαταγῷ, the latter διαβεβαιωτικῶς (Incidentally, I, too, claimed that something was 'never' the case in ancient grammar and found my counter example in B (p. 132, cf. AGC 142 on the existence of temporal conjunctions). These things happen.) More seriously, B uses Apoll. Dysc. *Synt.* 375,10-12 (οὐ καλεῖται τὸ 'εἰ περιπατεῖς' διατακτικόν) as an argument for the same point. But Apollonius argues for exactly the reverse. According to him εἰ περιπατεῖς expresses almost as much doubt as ἐὰν περιπατῇς. Still, we do not call the mood of περιπατεῖς 'διατακτικός', but stuck to 'ὀριστικός' instead. Therefore, the claim of those who prefer to call the subjunctive διατακτικός instead of ὑποτακτικός is unjustified. As a consequence, it is not true that Priscian is the first to connect εἰ with a notion of doubt, as B claims.

(p. 111) Incidentally, I think B. tends to give Priscian too much credit, especially when he claims (p. 181 f.) that Priscian was the first to write a true *Syntax*. He ignores the fact that Apollonius probably wrote a *Τεχνη* too, of which the *Syntax* was the crown.

More than once, B. offers quite convincing reconstructions, but overstates his case. This time I shall derive my example from part II, the analysis of linguistic correctness. B. gives a good idea of the development of the concept of *Latinitas*. But I do not think it proves, what it is intended to prove, viz. the existence of a truly syntactic type of analysis in the first century B.C.

On page 352 ff. B. sketches the following development in the study of *Latinitas*. In the first cent. B.C. Latin grammarians underwent the influence of Alexandrian theory. The imposition of Greek grammatical theory on Latin led to the identification of Latin and Greek. Latin came to be regarded as a Greek (Acolic) dialect. Scaurus initiated a reaction and henceforth Latin grammarians concentrated on the differences between Latin and Greek. This stimulated morphological description (where the differences are obvious) but discouraged the development of syntax, because Latin shared its syntactic structure with Greek (?) (p. 317). I am inclined to give a different explanation that takes the wider social and political context into account (cf. M. Dubuisson, *Le latin est-il une langue barbare?*, *Ktema* 9 (1984) 55-68). The Greeks had always divided the world into Greeks and barbarians. Consequently, Romans were called barbarians too and Latin was considered a barbarian language (nor did the Romans take offence cf. Plautus *Mucius portit barbare* (*As* 11)). But when the word *barbaros* acquired pejorative overtones, the Romans began to object to it. This coincided with the growth of Roman influence in the Mediterranean. Several solutions were tried, e.g. that of dividing the human race into three species: Greeks, barbarians and the *tertium genus*, the Romans. More successful was the identification of Romans and Greeks, supported by the mythological story of king Euander, who allegedly travelled from Arcadia in the Peloponnese to Italy some years before the Trojan war. So far, there is nothing to interest a linguist. However, in the first century B.C., this theory all of a sudden finds a linguistic application. A Varro and a Philoxenus connect the languages Latin and Acolic using strictly linguistic (purely morphological) arguments. Now, I do not agree with B. that it is strange to find that *differences* between languages are stressed. I think it is far more surprising if much attention is paid to the *similarities* between two ancient languages. This is strange because ancient grammatical theory is built on the very assumption of such a principal and structural similarity. All languages were supposed to be logically equivalent to one another. What usually interests an ancient scholar is precisely the *divergences* between languages. Therefore, something special is going on here. Now, all linguists who promote the theory of Acolo-Latin are in one way or another related to the Roman politician and general Pompey. Notably, Varro was a kind of minister of propaganda and a lifelong friend to Pompey. When the latter had to deal with the Roman inferiority complex and the Greek arrogance after the Roman conquest of Greece, it seems quite likely that Varro made another scholarly contribution to the solution of Pompey's problems. He combined some of his many capacities (those of the propagandist and the linguist) in order to provide an argument for the equation of Greeks and Romans. It is quite remarkable that the concept of Acolo-Latin disappeared almost as quickly as it emerged—as

did the acuteness of the social problem to which it was related. The linguistic theory leaves no more than some small traces in the corpus of the *Grammatici Latini*. But these cannot be used to explain a shift from syntax to morphology. As far as we know, nothing but morphological similarities were ever remarked upon (e.g. that between the Aeolic *wau* and the Latin *v* (Gr. ἑσθής, Aeol. ῥέσθος, Lat. *vestis*), or the fact that both Latin and Aeolic lack a dualis).

The examples given so far show already that B. is subtle and sophisticated in his argumentation, but more than once comes dangerously close to overinterpretation, as he notices himself (p. 87).

On p. 52 f. B. expatiates on the alleged difference between Priscian's and Heliodorus' view of the *vis* and *ordo* expressed by conjunctions. In fact, there is no fundamental difference between the two grammarians, as B. eventually admits. B.'s difficulties with Priscian's examples are largely due to a misquote (*et pius et fortis Aeneas* instead of *et pius et fortis fuit Aeneas*). When Priscian says *vim, quando simul esse res aliquas significat* (sc. *coniunctio*) (GL III 93,41), *esse* corresponds to Greek 'having ὑπαρξίς', *res aliquas* to πράγματα ('states of affairs'). *Simul* is "en même temps qu'elle conjoint" (so B., rightly p. 53). In general, I think it cannot be doubted that where Heliodorus and Priscian coincide, this points to Apollonius Dyscolus as their common source. No other ancient grammarian had the same authority.

4. Finally, there is the way the material is presented. In fact, this constitutes at the same time a major attraction and a source of irritation. B. has a very attractive style. The book is easy to read, skilfully composed and very persuasive at first sight. It is clearly written on the model of a detective-story. This accounts for exclamations such as "Quel motif a donc pu entraîner le dérèglement de la présentation du solécisme, et finalement cette dégradation? Il faut bien une raison!" (p. 291) and for the *horror vacui* mentioned above. This method of presentation entails that the reader is often willfully left in the dark about where the story will lead to (e.g. p. 184; p. 291), or even about the purpose of a whole chapter (e.g. ch. 1 of part I). And since one clue after another appears to lead up blind alleys, his patience is often stretched beyond reasonable limits.

For example, the attentive reader has been alerted by B. to the possibility of 'grammaticalization' of philosophical concepts. Thus, he will expect a solution in this direction right from the outset on p. 295, where B. deals with the problem of the shifting meaning of λέξις in the context of the *virtutes* and *vitia orationis*. Yet, he has to read through another 18 pages (!) (p. 319, see p. 311) of trial and error, until B. finally comes up with this solution:

It is hardly a reproach to call an author persuasive. B. is very persuasive, but his rhetoric often leads to δόξα rather than to ἐπιστήμη, as appears from many of the examples quoted above.

E.g. on p. 341 B. formulates as a hypothesis that all texts of Diomedes

ultimately derive from a source concerned with the internal structure of case constructions. This means that Diomedes already represents a stage of degeneration to a more idiomatic approach. On p. 342 B. with a subtle shift concludes "cette évolution *montre* [mv italics I S] sur un point précis, comment le courant artigraphique a pu disposer d'une analyse de type syntaxique et l'a dénaturée". And on p. 352 the formulation is stronger still: "Ce que montrent le *De consensu* de Diomède et les textes apparentés, c'est qu'il y a eu des analyses de type syntaxique dans le domaine artigraphique latin, mais qu'elles ont été transformées en listes de tournures idiomatiques." Good rhetoric, bad logic.

The intricate, almost literary, composition of the whole book makes the lack of an index locorum (potiorum) all the more regrettable. It is difficult quickly to find one's way to the many valuable remarks on matters of detail. However, B. makes up for this by providing a very detailed and useful "table analytique de matières" (p. 529 ff.).

All in all, I would like to end on a positive note. The book is extremely rich in information for the patient reader. Moreover, it is refreshingly provocative—the present review testifies to that. B. has without any doubt an admirable grasp of ancient Latin grammatical theory, and it is a pleasure (and an exhausting experience) to let him guide you through the hidden recesses of Latin grammar. But beware of the *dulcia verba* of your guide!

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INKE STUIJER

Virgil, Georgics, edited with a commentary by R. A. B. MYNORS with a preface by R. G. M. NISBET. Oxford, Clarendon Press, 1990. XCI, 345 pp. Pr. £ 45.00.

Ce commentaire sur les *Georgiques*, qui renferme également le texte de l'*O C I*, fut édité par M. Nisbet après la mort de R. A. B. Mynors (M.) en 1989, le livre est à tous points un monument digne du grand latiniste anglais et du poète qui l'a accompagné toute sa vie. On admire l'universalité du commentateur, sa grande érudition dans les domaines techniques de l'agriculture, mais aussi sa sensibilité aux aspects littéraires d'un genre hybride comme le poème didactique. Ainsi, de notre part, guidé par Sir Roger, nous avons compris pour la première fois la description de la charrue (*Georg. I*, 169-175): "an Ascræan poem might be expected to provide something of the sort" (*Comm.* p. 37), l'explication des *zonae* du ciel et de la terre (*I*, 233) est d'une clarté exemplaire. Bien que M. n'utilise jamais (heureusement) le jargon de la critique littéraire