

Reviews

Kim, Dong-Hyuk. *Early Biblical Hebrew, Late Biblical Hebrew, and Linguistic Variability – A Sociolinguistic Evaluation of the Linguistic Dating of Biblical Texts* (Supplements to Vetus Testamentum, 156). Leiden 2013. XVII + 184pp. Brill.

“The bigger, the better.” This rule seems to be the guiding principle of many doctoral dissertations published recently. Hence, it is so encouraging to see a dissertation which is concise and lucid, applies a clear methodology to deal with a specific problem, uses a wisely delimited body of evidence and arrives at conclusions which contribute to the current discussion on the topic. This is the case with Dong-Hyuk Kim’s book, which originated as a dissertation at Yale University. Its publication in the prestigious series of *Supplements to Vetus Testamentum* is an honour not only to the author but also to the series itself.

The author tackles a thorny issue which has recently become the topic of a vigorous debate: the linguistic dating of biblical texts. He sees correctly that the current discussion requires not a revaluation of the same arguments that go back and forth between the proponents and opponents of linguistic dating but rather a fresh re-examination of the data with a new methodological approach which has not yet been applied to the biblical texts. Therefore, Kim chooses to draw on methodology established during the last fifty years by sociolinguists. He proceeds as follows.

After an introduction which defines the problem and shortly presents the variationist approach, the author offers an essential survey of scholarship. This part reads almost as a novel. It introduces the *dramatis personae*, the scholars with opposing views, and tension between them. The same tension will accompany the reader through the entire book and make its reading even more exciting. The contenders can be divided into two contrasting groups: the followers of Robert Polzin and Avi Hurvitz and their challengers, especially Ian Young, Robert Rezetko and Martin Ehrensverd. In 2008, these three scholars coauthored two volumes in which they denied categorically any possibility of the linguistic dating of biblical texts (Young, I., R. Rezetko and M. Ehrensverd. *Linguistic*

Dating of Biblical Texts. 2 vols. London 2008. Equinox). They concluded that all features understood in diachronic terms as differences between early and late biblical Hebrew are simply due to the stylistic choices of individual authors. The last five years of research witnessed a virtual stalemate between two camps that have been unable to come up with new argumentation which would compel another side to modify its position.

In the next forty pages, in chapter four, the author expounds his methodology. This part is necessarily long because he must introduce the basic concepts of sociolinguistics to the reader, whom he supposes to be rather ill versed in that field. His definitions are essential and present the commonly accepted and authoritative views. Some of his points are crucial for biblical scholars, in particular for those educated in a traditional, prescriptive philology. In keeping with what is now current in linguistics, Kim explains that variation is a natural and essential component of language and that it is a main mechanism of language change. More importantly, variation is not random or chaotic but it has “structured heterogeneity,” that is, it occurs in statistically predictable patterns which can reveal linguistic and social factors behind the speaker’s choices of seemingly free variants. The graduality of language change is the key concept discussed in this chapter. Indeed, changes in a language happen gradually, through stages which witness the coexistence of usually two competing forms which occur in variation. Moreover, the form that eventually becomes obsolete rarely disappears from language completely; it rather survives as an archaic or dialectal variety. Already at this point the reader can anticipate Kim’s main conclusion. The challengers, who argue against the linguistic dating of biblical texts and against the possibility of spotting linguistic changes in biblical texts in general because of the coexistence of two forms assigned to different periods in the same text, cannot be right since they do not take into account the very nature of linguistic change.

Next, as is typical in every project, the author discusses the corpus of texts chosen for investigation and their dating. His corpus is constituted by the Hebrew portion of the Hebrew Bible as witnessed by the Masoretic text. Following three recent English introductions to the Hebrew Bible by Werner Schmidt, John Collins and Michael Coogan and their historical-literary (extra-linguistic) considerations, he divides the biblical texts into five periods: preexilic, late preexilic to early exilic, exilic, postexilic, and disputed or undecided. He also discusses narration and recorded speech, two basic types of texts which constitute one of the independent variables that are to be taken into consideration in his research.

Chapter four is dedicated to the specifics of the historical-linguistic analysis of texts, with particular attention to the problems involved in the analysis of biblical writings. It also introduces two types of linguistic changes according to their sources. The first type is “changes from below,” that is, those which pass unnoticed, are caused by internal, linguistic factors and usually originate from

lower social classes. The second type is “changes from above,” which occur with full public participation, usually by the initiative of higher social classes, and often take the form of the importation of a new prestigious feature foreign to the speech community. Kim assumes that changes “from below” are more prominent in oral communication and thus detectable in biblical recorded speech while changes “from above” are characteristic of a more formal register and thus noticeable in biblical narration. Following this assumption, he checks whether a new variable is more prominent in recorded speech or narration and on this basis he decides which type of change it represents.

Chapter five contains the main part of the author’s research. It consists of an examination of eight morphological, syntactical, phraseological, and lexical variables that appear often in the polemic about the feasibility of the linguistic dating of biblical texts. For each variable Kim discusses its distribution throughout biblical texts assigned to the different epochs and establishes whether there is or not a meaningful correlation between the variable itself and the time period. When required, he introduces into the picture the second independent variable, that is, the type of text, in order to determine if the variation of a particular variable is a case of a change “from below” or “from above.” His conclusions concerning each of the eight examined variables can be summarized as follows:

1. *זֶהָם* vs. *זֶהָם־*: a true variable with regard to time; the shorter form was gradually replaced by the longer; an example of change “from below.”

2. *וְהָיָה/וְהָיָה־* + *כִּ/כְּ* + inf. const. vs. *וְ* + *כִּ/כְּ* + inf. const.: the progressive abandonment of the formula without the introductory verb is an example of change “from below.”

3. *הַמֶּלֶךְ* + king’s name vs. king’s name + *הַמֶּלֶךְ*: if idiolectal, it is without chronological significance; more probably a change “from above;” if so, it requires caution because its use may reflect stylistic choices, not chronology.

4. *בֵּין...וּבֵין* vs. *בֵּין...לְ*: there is a gradual transition from the first to the second variable; variation between them during the exilic/postexilic period should be understood as a change in progress; it is difficult to decide the nature of the change (from “below” or “above”).

5. *בֵּית הַ* vs. *בֵּית הָאֱלֹהִים*: a example of a change from “above;” it cannot be used as a reliable chronological indicator because the first variable, although gradually replaced by another, remains a fully available stylistic option.

6. *מִמְלָכָה* vs. *מְלָכוֹת*: the shift from an almost exclusive use of the first form to a preference for the second one is an authentic linguistic change; this shift was a non-irreversible, conscious change from “above” and thus it cannot be used as a chronological indicator.

7. *עָדָה* vs. *קָהָל*: the first variable fell out of use in the late literature but this does not mean that it gave way completely to the new variable because *קָהָל* had been always available while the use of *עָדָה* was typical of the Deuteronomist. Consequently, this pair is not a reliable marker of a linguistic shift.

8. **זעק** vs. **צעק**: concerning this pair, the Bible documents a change in progress; the second variable is newer and becomes predominant around the late preexilic period; this was a change from “below” and it can generally be considered a chronological indicator of the development of biblical Hebrew.

The last chapter contains the author’s conclusions and a summary of his arguments. Kim points out correctly that the variability in the biblical evidence, which is taken by the challengers as the main argument against the analysis of biblical Hebrew in chronological terms, is actually a proof of an authentic linguistic change from early to late biblical Hebrew because the state of variation is characteristic of every linguistic change. In assessing the evidence, it is, however, necessary not only to ascertain the existence of a change but also to determine its nature. Indeed, a change “from below” can be taken as an indicator of a chronological change, while a change “from above” is conscious and hence may constitute a stylistic choice. Kim also notices that three of the seven authentic changes are “from above.” This ratio comes as a surprise because, according to historical and present-day sociolinguistics, changes “from above” are very rare. The author ascribes this phenomenon to scribal influence on the language of the Bible. I would like to add that the discovery of this unexpected ratio is a further confirmation of the value of his research and the method itself because it is unexpected from the sociolinguistic view but it is comprehensible considering the nature of the biblical text. Moreover, according to Kim the evidence pictures not two distinct bodies of early and late biblical Hebrew but a multidimensional continuum with a great deal of variability. He concludes, therefore, that there is no watershed moment in the history of biblical Hebrew such as the exile (contra Hurvitz) and that, although it is legitimate to describe different chronological layers of biblical Hebrew, it is not practicable to use linguistic data for the dating of a problematic text.

Kim’s argumentation and conclusions are in general solid but several points should be added.

On page 98, Kim discusses shortly a well know variable, the alternation of the two forms of the independent pronoun of the first per. sing., **אֲנִי** and **אני**. The longer form is often held as a marker of early biblical Hebrew while the shorter is considered a characteristic of late biblical idiom. Kim chooses to exclude this variable from his study not because it has no potential chronological significance but because it is virtually unattested in narration and thus it is impossible to discuss the kind of change (from “below” or “above”) which this variable could represent. Now, concerning this variable, it is necessary to refer to the following paper: Revell, E. J., “The Two Forms of First Person Singular Pronoun in Biblical Hebrew: Redundancy or Expressive Contrast?” *Journal of Semitic Studies* 40/2 (1995):199-217. Revell accepts that there is a chronological factor in the distribution of this pair of pronouns in the Bible. Additionally, he

proposes that the choice also had a sociolinguistic motivation: the shorter form was the marked form that signalled that the speaker had a right to expect attention from the addressee, either due to status difference, or due to the importance of the speech for the speaker. Because of its excessive use, the marked form was eventually replaced by the unmarked longer form of the pronoun. Regardless of the correctness of Revell's proposal, his analysis is very instructive in the context of diachronic research on biblical Hebrew. It shows that variation, and consequently a linguistic change, can be due to sociolinguistic factors, not necessarily to the contact with another language or to the inner quality of languages that are subjected to change by their very nature. Naturally, such a possibility renders diachronic research more complicated and, at the same time, more fascinating. In comparison with the challengers of the linguistic dating of biblical texts, Revell's research is exemplary for yet another reason. Unlike the challengers, Revell not only proposes that variation between the two pronouns of the first pers. sing. is stylistic but, mostly importantly, he gives possible reasons for the stylistic choice of the pronouns. The lack of such explanations for variables treated by the challengers as stylistic variants renders their research unconvincing and their use of the concept of "stylistic" variants simply abusive.

In his conclusions, on pages 158-159, Kim makes a very important claim which requires further research and probably a more subtle formulation. Kim states in categorical terms that the exile was not a watershed moment in the history of biblical Hebrew, not a period that decisively separates its varieties, early and late. His argumentation is, however, somewhat tortuous. On one hand he recognizes that in the case of six of the seven authentic changes he examined the use of a new form increased decisively during or after the exile; on the other hand, he states that not all of the changes can be ascribed to this critical period. Moreover, he observes that there changes "from above" were consciously imposed and not-irreversible. In his mind, the non-irreversibility and conscious nature of these changes frustrate their use as evidence in favour of the exile as a decisive period of the history of biblical Hebrew. He admits that the exile provided a favourable context for the initiation of many changes because of contact with other languages (principally with Aramaic) and because of a social upheaval which had to have serious repercussions on the Hebrew speech community, its size and composition. However, he rejects the significance of external factors in favour of the decisive impact of the exile on the history of Hebrew because he thinks that this is common sense thinking that is contradicted by the findings of his research. How should one evaluate Kim's claim about the impact of the exile? On one hand, the picture of organic and progressive changes that emerges from his research is convincing. On the other hand, Kim also admits that a significant number of changes as well as their intensity are to be connected with the exile. Hence, one must logically conclude that it had to impact the history of Hebrew somewhat. Two observations are in order. First, in the wake of Kim's research, it seems plausible that we should stop think-

ing about the exile as influencing the Hebrew language in almost a catastrophic manner. Second, since Kim analysed just eight variables, in order to radically re-evaluate the role of the exile, it is necessary to extend the same kind of analysis to a larger number of features.

On page 155, Kim states again that the linguistic dating of a biblical text is unfeasible. He takes this position not only because the features presented traditionally as characteristic for early or late biblical Hebrew are not reliable. He adds an unassailable linguistic argument. Since the language change is variationist in its nature, the adoption of a single variable in a text is unpredictable and can occur during any moment of the period in which the change took place. Obviously, it is impossible to know if a given text is an early adopter of the change or whether it lags behind and adopts the change only in the final period of the change. Consequently, if a text is an early adopter, its dating using the linguistic variables will result in a too low date; if a text is a late adopter, its dating will be too high. Moreover, as Kim observes, many changes in biblical Hebrew are “from above,” that is conscious. Therefore, these changes can reflect the scribes’ manipulation of the language and not a genuine linguistic change that would constitute a reliable indication of the antiquity of a text. A relatively high number of changes “from above” is yet another argument in favour of abandoning the linguistic dating of a particular text. Without doubt, this is the case with the dating of a short text. However, it is still possible to build a linguistic profile of an entire book, especially if it has a unitary composition and did not undergo too many revisions (for example, *Qoheleth*), and use such a linguistic profile to assign to it a place in the history of biblical Hebrew which, of course, is also related to the time of creation of the book. Kim is, however, not explicit about the relevance of such general characterizations.

Because of his cautious approach and limited scope, Kim also does not comment on a broader significance of his research for the field of biblical studies. What Kim patiently documented is the composite nature of the biblical evidence, with layers and features that must be assigned to different periods. Most logically, the linguistic history of Hebrew detectable in the biblical text indicates that the biblical text itself had a long history of formation and cannot be dated to one period, as it is done by some schools of thought that assign the composition of the Hebrew Bible exclusively to the Persian and Hellenistic periods. Kim’s book is another convincing proof of the falsehood of such a vision of the literary history of the Hebrew Bible.

In conclusion. Kim introduces a new quality to the discussion about the linguistic dating of biblical texts thanks to his proper use of sound sociolinguistic methodology rather than the common sense philological approach that has dominated the controversy till now. Since he examined only selected variables, his research must be continued and expanded to include other relevant data and problems, such as loanwords, semantic changes of particular words or expres-

sions, and scribal alterations. However, already as it stands, Kim's book is a solid contribution to the debate and it must be consulted by anybody who wants to participate in it.

Krzysztof J. Baranowski

Gideon Goldenberg, *Semitic Languages – Features, Structures, Relations, Processes*. Oxford 2013. Oxford University Press. XIX + 363 pp. ISBN 978-0-19-964491-9

The book under review appeared just a couple of months before the death of its author who was a great scholar, a great man and a great friend. Gideon Goldenberg was famous first of all thanks to his numerous articles which combined most remarkable originality, immense erudition, depth of thought as well as methodological precision and clarity. In a quite unusual way he was at the same time conservative (in the best sense of the word!) and open to the newest ideas which he could critically evaluate and develop. It was his wisdom and fascination with the achievements of our predecessors which made him discover forgotten treasures of the past and make them function and shine in modern times – this was quite exceptional in the times when there is a general tendency or even a common practice not to refer to publications which are more than five years old. He had a very profound knowledge of so many languages and he abstained from the use of data from languages he did not know well enough which was a very unusual practice in times when almost everybody feels obliged to use second- or even third-hand data from languages which he practically ignores. This explains why he has almost never used data from the non-Semitic languages of the Hamitosemitic/Afroasiatic macrofamily although he had no doubts about the existence of this macrofamily and supported the research on it. He had courage to give us the signal 'I do not know' without being hypercritical.

The book under review is rather a collection of essays than a complete and systematic comparative grammar of the Semitic languages. It is not a sketch or an 'Introduction' either, although it can be used by both slightly advanced students (there is a wealth of paradigms and examples in transcriptions!) and 'grown-up' linguists. Its composition is rather unorthodox. The book starts with the general 'Introduction' (pp. 1-9) discussing such basic issues as 'Semitic languages in general linguistic research', 'Linguistic approach and a few features found in Semitic languages', 'Data, transcriptions and glossing'; then comes the general presentation of the Semitic languages (pp. 10-20), then there are chapters on 'Distribution of the Semitic Languages' (pp. 21-29), on 'Writing systems and scripts' (pp. 30-43), on 'Genetic classification' (pp. 44-57), and on 'Special achievements of Semitic Linguistic Traditions' (pp. 58-63). Then comes a part

which is both descriptive and comparative-historical: 'Phonology' (pp. 64-80), 'Closed class morphemes' (pp. 81-115), 'Derivational morphology' (pp. 116-129), 'Entity terms' (pp. 130-139), 'Syntactic relations and the case system' (pp. 140-148), 'Predicative Relation' (pp. 149-198), 'Verbal systems' (pp. 199-225), 'Attributive relation' (pp. 226-227), 'Compleitive relation' (pp. 278-299) and finally 'Existence, Belonging and Possession' (pp. 300-311).

Goldenberg was a master expert especially on Ethiosemitic and on Syriac and Neoaramaic languages not to mention Hebrew, both old and modern. His interests concentrated on both synchronic analysis as well as on the study of the history of languages, their evolution and prehistory. His discoveries and opinions have always been original and very important. Here I can mention and shortly comment only a few chosen questions. As far as the genetic classification is concerned, it is remarkable that he has refrained from an ultimate classification of Arabic as 'Central Semitic' although he has placed it between Northwest and South Semitic leaving non-committal as to its specific affiliation (p. 56). Personally I still think that my idea of the old Semitic languages as a dialect continuum makes sense with Arabic having much in common both with the North and with the South. Goldenberg has also not sustained the traditional division of Ethiosemitic into North Ethiopian and South Ethiopian and he has four branches, i.e. 1. Ge'ez, Tigre, Tigrinya, 2. Amharic-Argobba, 3. Gafat-Kəstane West Gurage, 4. Harari – East Gurage going back to Proto-Ethiopian which, in its turn, goes back to 'South Semitic' which has 'Modern South Arabian' (I prefer the name 'Modern Semitic of Southern Arabia' = MSSA) as another branch (pp. 56-57). The problems with the classification of Epigraphic South Arabian (I prefer the name 'Ancient Semitic of Southern Arabia' = ASSA), Eblaite (most probably a dialect of Akkadian), Ugaritic (having many common elements both with Northwest Semitic and with Arabic) as well as of some less known idioms have been left.

On pp. 64-65 we read about the emphatic consonants that "the only plausible reconstruction of those consonants is by defining them as being originally ejective". This is practically a *communis opinio* but I still maintain that glottal, pharyngeal and uvular articulations refer to non-distinctive features of these phonemes as long as there was no phonemic opposition/contrast between these articulations and they were 'the same' from the point of view of the system. I am not even convinced that we have to ascribe the alleged weaker glottalization of 'emphatics' in MSSA and a tendency to realize them there as pharyngalized-uvularized allegedly under the pressure of Arabic (pp. 76-77).

On p. 132 we read that 'The derivational affix *-ān*, though not unequivocally indicating in itself plurality, is combined with an external plural suffix in no few cases in Akkadian, Aramaic or Hebrew to form Pl.M. forms of nouns that have no singular forms in *-ān*. Being thus associated with plural, it has often been regarded as part of nominal plural formation in */-ān- + Pl. suff./*, and will sometimes be found behaving like one compound plural marker" (p. 132). It should be connected

with Ge'ez and Tigre masculine plural forms with the same *-ān* amply exemplified on p. 135. The residual use of *-ān* (elsewhere also used for deriving abstracts which coincides well with the plural meaning) in compound plural ending is an innovation of Akkadian. Plural *-ān* occurs also in Berber and in Cushitic.

Goldenberg correctly rejects attempts at reconstructing ergative in Proto-Semitic as well as the hypothesis about the original lack of case in Proto-Semitic as proposed at first by Owens and now by Hasselbach.

Goldenberg is inclined (p. 49) to explain the substitution of Akkadian and Ethiosemitic **iqattal* by *yaqtul-u* Imperfect as an innovation of 'Central Semitic' (in which he doubts on p. 56), thus supporting the view that this change was motivated by a drift within the system to avoid the clash (alleged clash in my view!) between gemination functioning in the tense system and gemination that is a part of the formation of derived verb system (p. 49). I think that Akkadian *iparrVs(u)* (the final *-u* not occurring before the pause marking the end of sentence) became generalized as Present because when under the influence of Sumerian the syntactic order had changed to SOV, the Preterit *iprus* and the original Imperfect/Present *iprus-u* > *iprus* could no more be distinguished due to the loss of the final *-u*. The Imperfect/Present survived in the position not at the end of the sentence, i.e. in dependent clauses as 'Subordinative' (cf. Goldenberg not accepting this theory on p. 235) as postulated by Kuryłowicz and recently accepted by Kouwenberg. The hypothesis that the Subordinative *iprus-u* was shifted to main clauses in Northwest Semitic and in Arabic is wrong. The domination of *yaqtul-u* and the elimination of the Present with gemination (but with good traces in Classical Arabic!) is rather a case of partial retention than innovation. This is also confirmed by the existence of a cognate of *yaqtul-u* (and of Subjunctive *yaqtul-a*!) in the most archaic Cushitic 'Afar and Saho. Goldenberg has been overcautious (p. 52) when he declared that an interpretation of MSSA Imperfects of the *ykōtāb* type with Akkadian-Ethiopic type having geminated Imperfect/Present as structurally unrelated was not impossible. We know that intensive verbs (and their Present forms) are derived with long *-ā-* after the first stem consonant in Beja as well, which confirms that *qattala* and *qātala* were variants at a prehistoric stage and both were used as multiplicative/continuous forms to renew the category of 'Present' already in Proto-Afroasiatic. Even after Nebes' study suggesting that there could not (really?) be a cognate of **iqattal* in ASSA there is a chance that in ASSA there was **yVqātil(V)* like in MSSA.

Many subjects and problems have not been even mentioned in the book most evidently because when Goldenberg was writing it he was fighting with his deteriorating health. But what we can find in the book is usually very illuminating and well balanced. Every Semitist and every linguist seriously interested in the Semitic languages will have to consult this truly accomplished although not fully achieved book which is a collection of treasures.

Andrzej Zaborski

Rebecca Hasselbach, Case in Semitic – Roles, Relations and Reconstruction. Oxford 2013. Oxford University Press. XIII + 353 pp. = Oxford Studies in Diachronic & Historical Linguistics 3. ISBN 978-0-19-967810-9.

This is the first full-size monograph devoted to the study of case in Semitic. After a summary of ‘Methodological Considerations’ (pp. 1-15) the authoress discusses the basic evidence and traditional reconstruction of the Semitic case system (pp. 16-89), problems of case as a typological problem (pp. 90-124), grammatical roles and the alignment of Semitic (pp. 125-181), head and dependent-marking in Semitic (pp. 182-257), the function of case markers in Semitic (pp. 258-326) and then final ‘Conclusions’ (pp. 327-332) close the book.

As far as the synchronic description of the grammatical category of case in the Semitic languages is concerned, there is nothing basically new and nothing really wrong. It has been explained that Proto-Semitic could not be ergative already before, e.g. by Satzinger (e.g. ‘Absolute state and absolutive case in Afro-Asiatic’, in: XII Incontro Italiano di Linguistica Camito-Semitica (Afroasiatica) – Atti, ed. M. Moriggi, 63-69, Rubettino 2007; A. Zaborski ‘On the alleged ergativity in Hamitosemitic/Afroasiatic languages’, in: Collectanea linguistica in honorem Casimiri Polański, eds M. Brzezina, H. Kurek, 309-317, Kraków 1999). The problem is with the reconstruction of this category in Proto-Semitic to which actually the book is mainly devoted. It cannot be accepted that (p. 328) ‘The GEN in *-i* is most likely a later innovation deriving from the *nisbe* (why ‘*nisbe*’ with dialect *imāla* and not Classical Arabic *nisba*? – AZ) ending.... The secondary nature of the GEN can be confirmed by the fact that it is the only case marker for which we can propose a reasonable etymology” not only because this reasoning is rather circular. The etymological connection of *-i* with *nisba* *ī/-iyy-* is quite conjectural and if possible at all, then a derivation of *nisba* from genitive *-i* is a more reasonable although not a simple hypothesis, actually only a hypothesis. First of all genitive *-i* must be of Afroasiatic origin since it occurs also in Egyptian, Cushitic, and even in Berber (cf. K.-G. Prasse, Manuel de grammaire touaregue t. II – Nom, Copenhagen 1974, pp. 107-108 and 112-113 and L. Garland, Regards sur le berbère, Milano 2010, p. 145 and 150). The big problem is that in Classical Arabic *nisba* suffix *-iyy-* is followed (!) by each of the case endings including genitive! There is also a different problem with compound suffix *-ān-iyy-* which occurs mainly in Arabic dialects but also in Classical Arabic, as well as *-aw-iyy-/āw-iyy-*. The authoress reconstructs three prehistoric stages of the Semitic languages, i.e. 1. “Archaic Proto-Semitic” when Semitic allegedly had no inflectional case marking and nouns generally ended in *-a*, 2. Second Archaic Proto-Semitic stage during which Semitic allegedly developed a case to mark nominal subjects, the nominative in *-u* while the ending *-a* which was

reanalyzed as accusative, was still the basic and citation form of the noun, 3. The last Proto-Semitic (but actually not 'Proto' since even the authoress says that this system is found in classical Semitic languages! – AZ) in which allegedly "the marked-nominative system shifted to nom/acc., with nominative being reanalyzed as the basic form of the noun. At this stage the genitive *-i* developed out of the Nisbe ending, resulting in the triptotic declension in the sing." (pp. 331-332). The reconstruction of the allegedly caseless Archaic Proto-Semitic does not make sense even in the light of the data of the most archaic or conservative Semitic languages. It must be decisively refuted in the light of the data of other Afroasiatic (Hamitosemitic) languages which show beyond any doubt that case must be reconstructed not only for Proto-Semitic but already for Proto-Afroasiatic. The majority of Semitists still have either a very limited knowledge of other branches of the Afroasiatic macrofamily or no knowledge at all. The authoress of the book under review has read only some of the relevant studies on Cushitic, Berber and on the still enigmatic Omotic (perhaps even not Afroasiatic at all or in the best case West Cushitic being among the most innovating branches of Afroasiatic with very little or no value for 'deep' reconstruction of Proto-Afroasiatic) ignoring several basic studies, e.g. by Satzinger (see above as well as 'Some remarks on the Afroasiatic case system', WZKM 94 (2004), 177-183; there is also Satzinger's forthcoming paper 'Proto-Afroasiatic is 'Marked Nominative' or of Nominative-accusative Alignment' available on his homepage since about two years, and there is also V. Blažek, 'Traces of a common case system in Afroasiatic', in: 'Loquentes linguis – Studi linguistici e orientali in onore di Fabrizio A. Pennacchiotti', ed. P. G. Borbone *et al.*, 91-101, Wiesbaden 2006).

Actually even the data and proposals found in the studies which the authoress did read (pp. 72-87) demonstrate that all the cases of Proto-Semitic go back to Afroasiatic! It is really astonishing that she disregarded this evidence saying that "First, we need to avoid evaluating the features found in one branch through the lenses of another" (p. 11) which hypercritically questions the validity of comparative-historical linguistics in general. It is true that 'comparative work on vowels hardly exists' (p. 10) but when we find e.g. *-u* with the same grammatical function in two or more languages and the same happens in the same frame of the case subsystem with *-i* and *-a* then this is just an optimal opportunity to reconstruct the identity of vowels at least in final position! Saying that "because of such difficulties, the present study is limited to evidence from Semitic languages, even for stages for which a comparison with other Afroasiatic branches would be desirable" is an example of sharp hypercriticism. Semitists have to realize and to accept the fact that reconstructing Proto-Semitic cannot be reasonably done without comparing the data from other branches of Afroasiatic and first of all from the most archaic or conservative languages. E.g. the authoress is at loss when she starts the subchapter on Cushitic with a mention of very innovating Gawwada which has no inflectional case marking! This makes as

little sense as if someone had started a discussion of Indoeuropean case mentioning at first Afrikaans which is the most analytic, i.e. inflectionless Indoeuropean language! The most important since very conservative Beja is mentioned only later but the authoress has no idea that it has not two but three cases with genitive *-i* appearing not only in genitive constructions, e.g. *tāk-ī-t kām* 'a man's naga', *ta-tak-āt-ī-t kām* 'a woman's naga' but also in possessive pronouns, e.g. the 2nd sing. masc. is nominative *bar-ū -k*, accusative *bar-ō-k* and 2nd sing masc. independent possessive sing. is nominative *bar-ī-u-k*, accusative *bar-i-ō-k*, as well as before possessive suffixes when a noun is in the genitive case, e.g. *bāb-i-ók ó-sim* 'your fathers name' (Wedekind K., Wedekind Ch., Abuzeinab Musa, A Learner's Grammar of Beja (East Sudan), Köln 2007, p. 40, 60, 66, 70; E.M. Roper, Tu Bedawie, Hertford 1928, p. 14, 24, 26, 32). Semitists should study the archaic Berber, archaic Cushitic (mainly 'Afar-Saho and Beja) and Egyptian as well as archaic Chadic (mainly East Chadic, e.g. Mubi).

We read (p. 332) that "Taking the Modern Arabic dialects as an endpoint, Semitic would have gone from originally caseless and strongly head-marking to case-inflected and split, and back to caseless and strongly head-marking. This means, it would have completed a full cycle. If the reconstruction proposed here is correct, Semitic provides us with a unique example of a language family that went through several different marking types during its history". First of all Modern Arabic dialects cannot be an end-point because some of them have preserved quite archaic features, sometimes even features that have not been retained in the Classical Arabic dialect, and, furthermore, there are living Semitic languages (in the Neo-Ethiosemitic and Neo-Aramaic branches) which are certainly more innovating than Modern Arabic dialects. The Semitic language family is certainly not unique in its evolution through different typological stages and Egyptian evolution from Old Egyptian to Coptic is a better example of a great cycle as emphasized already years ago by C.T. Hodge, The Language Cycle, Language Sciences 13, 1970, 1-7.

In my opinion two declinations, i.e. both triptotic and diptotic must be reconstructed for Proto-Semitic and probably also for Proto-Afroasiatic.

It is my sad duty to conclude that this reconstruction of Proto-Semitic without case is unacceptable and the reconstruction of the syntactic order hardly relevant and conclusive. This is not a success at all.

Andrzej Zaborski

Peter Behnstedt, Manfred Woidich, Wortatlas der arabischen Dialekte, Band II: Materielle Kultur. Leiden 2012. Brill. XIV + 629. ISSN 0169-0423; ISBN 978 90 04 21156 8. = Handbook of Oriental Studies vol. 100/11.

This is the second volume (for a review of the first volume see 'Folia Orientalia' 48 (2011), 292-294) of the great atlas of modern Arabic dialects prepared by two leading dialectologists with contributions of some data and comments from about 37 Arabist experts on different Arab regions. To the first volume there are four *Corrigenda* (p. 497) to maps number 30, 31 and 100, and to p. 316c) and many *Addenda* to all the lemmata (there is a misprint on p. 498 where we find No. 22 instead of 20 for 'widow').

There are altogether 127 lemmata (their numbering continues the numbering of the first volume) with corresponding maps which contain a wealth of data on the usage, etymology and cultural background. Although maps enable us a good overview of the general situation (in spite of the fact that inevitably some small *sigla* are sometimes difficult to locate) it must be emphasized that reading of very detailed and scholarly commentaries is a must in almost every case since some less common lexemes appear just there as well as precise information on the location of some rare words on small territories (e.g. Ghaza).

The choice, i.e. the selection of the lemmata was not an easy task and sometimes it was difficult to prepare good ways of eliciting correct terms and their meanings. For example it had to be difficult to collect different words for 'shoes' since there are so many different kinds of 'modern' and traditional shoes in different regions, some of the names overlapping like *šibšib* which appears as 'shoes' (No. 232b and p. 209– there had to be two lemmata for 'shoes'!) and as 'sandals' (No. 233 and p. 213). I remember the derogatory *'abū šibšib* used in Cairo in 1975. In two or three cases the choice of lemmata has been a bit controversial either because they are too polysemantic (e.g. No. 222 'thing' and 623 'stuff/junk') or too general (e.g. No. 231 'clothes' but we do not have lemmata concerning at least regionally popular traditional clothes not to mention traditional mail head dress and female face covers) or too specialized, e.g. No. 252 'thyme' which has only phonetic variants going back to Classical Arabic *sa'tar* but there is nothing on 'coffee', 'dates', 'salt', 'butter', 'hashish' etc.. Sometimes it is possible that the words quoted refer rather to different objects, e.g. in case of 'clothes' there may be some words pertaining to particular pieces of clothing but not to 'clothes' in general. I want to emphasize that minor shortcomings like that have been rather inevitable and it would be unfair to list too many lacunae in such a huge project.

I find it quite positive that the authors included also lemmata concerning modern things, e.g. different parts of cars, refrigerator, mobile phone, coca-cola and other soft drinks etc.

The atlas is a great source of data not only for linguists but for a study of culture in general. It is interesting to note that the names of some things substantially differ in so many regions, e.g. 'garden', 'well', 'lighter', 'matches', 'cooking pot', 'baking oven', 'window', 'room' etc. It is a riddle why in some regions the differentiation of the names of some objects is quite big, e.g. in Mauritania, cf. 'window', 'furniture', 'matches' 'purse' while a big variety is much less surprising in a country like Yemen with its long history and so many different dialects and also different languages.

On the other hand the names of some things are not so much differentiated, e.g., 'trousers' and 'sesame'. It is interesting that *salīṭ* 'oil' occurs only in Yemen (plus neighbouring Saudi Arabia) and Oman (also in al-Andalus in the past) while elsewhere there is almost exclusively *zayt*. In a few cases the word is the same apart from minor phonetic variation, e.g. 'salad', 'meat', 'raisins', 'sugar'. It is also visible that there are some 'cultural zones' known also from other data. E.g. Iraq has several special 'Iraqisms' (e.g. 'city/town quarter', 'pursley', 'to plough' (partially – cf. p. 382!), 'raisins', 'petrol station' (*banzīnxāna!*), 'paint', of Arabic and of foreign (Turkish, Kurdish, Persian) origin; also Libyan, e.g. 'room', 'baking oven', 'water tape', 'furniture', 'bottle', 'sandals', 'eggs', 'farm', 'cowshed', 'change (small coins)', 'lorry/truck', 'steering wheel' and 'clutch as well as 'gear' (Italian loan-words!) differing in several cases from other countries and even Tunisia not always follows general Maghrebian lexical stock, e.g. Tunisian 'window', 'road/street' (*kayyās*, p. 62 and 55), 'watch', 'socks', 'shirt' (together with Libya!), 'eggs' ('*dām* is unique!), 'petrol station' (unique *kyōsk!*), 'coriander' (also unique *tābil* reappearing in the Gulf and partially in Cairo Arabic), 'spectacles' and 'paint'. It is rather natural that words of Persian (often shared with Iraqi dialects, altogether the Index on pp. 623-624 lists c. 130 Persian *comparanda*) e.g. 'bed', 'carpet' and Urdu (sometimes Hindi, altogether about 20) origin are found in the Gulf States and in Oman.

In short: this is a great, truly monumental publication. No doubt there will be many additions due to the progressing research but a solid basis is already here. Let us hope that a next volume or volumes will appear rather soon.

Andrzej Zaborski

Pierre Larcher, Le système verbal de l'arabe classique. 2^e édition revue et augmentée. Aix-en-Provence 2012. Presses Universitaires de Provence. 186 pp. ISBN 978-2-85399-841-3.

This is the second revised and augmented edition of the book which originally appeared in 2003 and was reviewed e.g. by Wolfdietrich Fischer (ZDMG 160, 2010, pp. 173-176). The main novelty is that the first chapter entitled 'Paradigms' has been considerably enlarged to give students of Classical and of the Modern Literary Arabic a detailed account of verbal paradigms in Arabic script and in transcription. By the way: is there an explanation for students why later in the book pausal forms are transcribed in case of participles and verbal nouns, e.g. *mufā'il*, *fi'āl*, *mufā'ala* etc. (p. 57)? On the other hand sometimes there are non-pausal forms, e.g. *'al-Qāhīrat* on p. 116 and *qāla* instead of *qāl* before the pause marking direct speech on p. 135, example No. 2. I think that the transcription of long vowels in *verba ultimae* *w* and *ultimae* *y* as <uw> and <iy>, e.g. *saru-w* instead of *sarū* (p. 27) is controversial. In the new edition in chapter No. X more attention has been devoted to the *iqṭalla* derived class and there is more on negation (especially *mā fa'ala*) in chapter No. XVI. There are also some modifications and additions elsewhere. There is no doubt that this is a very good synthesis and textbook especially as far as verbal derivation is concerned, although we have also the third volume devoted to the verbal group of the monumental syntax of the modern literary Arabic by Hashem El-Ayoubi, Wolfdietrich Fischer and Michael Langer (Syntax der Arabischen Schriftsprache der Gegenwart, Teil II: Die Verbalgruppe, Wiesbaden 2010. Reichert Verlag) and an important big 'Modern Literary Arabic – a Reference Grammar' by Ron Buckley, Beirut 2004, Librairie du Liban Publishers.

I have some more and some less important remarks. P. 14: from a synchronic point of view it is possible to consider imperative and 'injunctive'/jussive of the 2nd persons as stylistic variants but not from a diachronic point of view! P. 17: variants like *yaqduru/yaqdiru* (with some verbs there are three possible vocalizations!) may show that at a prehistoric stage the vocalization of the preterite/jussive and of imperfect/'inaccompli' or of subjunctive was different. P. 48-49: there is a serious risk that some traditional examples of the 'intensive' meaning of *fa'ala* forms are to some extent artificial, created by grammarians to confirm the initial presumption, e.g. *qattala* 'to massacre' and *kassara* 'to break into many pieces'. To this subchapter I should have given the title 'Multiplicative function' rather than traditional 'Intensive'. There should be a mention of *Aktionsart* variants like in the example *gallaqtu l-abwāba* in which the 'distributive' meaning ('one after another', see also p. 51 and 60) is rather lexically conditioned. P. 50: factitive (which is not the same as causative!) verbs are denominative! P. 57-58: I do not think that a semantic difference between verbal nouns *fi'āl* and *mufā'ala* can be

ascribed simply to their different morphological derivation and shape or even syntactic relations. Derivation is largely irregular and the final semantic outcome depends on many factors, like chronology, morphological and purely semantic resemblance, frequency of use etc. E.g. *jihād* 'holy war' occurs in the Koran but *mujāhada* does not being probably later even in comparison with *mujāhid* which is found in the Koran; *jihād* is both morphologically and semantically connected with *qitāl* being its euphemistic variant. *Mufā'ala* (identical with the feminine passive participle!) is usually considered to be more common than *fi'āl* which is, by default, more expressive. P. 58-59: the author is right saying that the classical example *qātala-hu* 'he was trying, making efforts to kill him' is rather improbable. It is either a rather artificial construction of grammarians or it may illustrate a secondary use of original multiplicative and pluractional *fā'ala* not with plural but with a singular subject. The meaning 'insistence' is one of the secondary features of the main features 'repetition, durativity, continuity etc.' which *fā'ala* (in prehistorical period a variant of *fa'ala*) still preserves. In case of telic verbs repetition, continuation of action, especially with singular object, may automatically mean 'insistence, striving, perseverance' etc. and this was the reason why *fā'ala* has been given the label 'conative' although there are examples to the contrary, e.g. *qātala-humu llāh* 'God confound them!' (Koran 9:30). The French label '*action efficiente/action sur*' applied by Henri Fleisch pertains to a lexically conditioned feature only of some verbs. In case of stative verbs *fā'ala* forms are transitive with a clear causative and factitive tint which is a link with the causative and factitive functions of *fa'ala*. P. 66: '*aqtaltu-hu* can be well translated into English as 'I had him killed'. P. 68-69 class IV verbs with '*la valeur tropative*' are transitive denominal verbs – 'estimative' function being a variant of factitive, see also p. 73. P. 89: '*valeur simulative*' of, e.g. *tamārada* 'to pretend to be ill; to feign illness, to malingering' is not due to a double value of insistence and reflexivity but rather reflexivity and factivity while the feature 'contrary to the actual state of affairs' is also lexically conditioned. P. 94: 'estimative' *istaf'ala* forms are denominative. Double meanings like in case of *istakbara* which with direct object can be transitive and thus factitive/'estimative' (*istakbara-hu* 'he deemed him great/important') but without direct object intransitive/self-estimative (*huwa stakbara* 'he was/became proud, haughty; he displayed arrogance') and this double function suggests that at a prehistoric stage there could be **istaf'ala* and **itsaf'ala* which coalesced due to phonetic reasons. On *istaf'ala* there is also the article by Karel Keller 'Der X. Verbalstamm und seine Funktion im modernen Arabisch', in 'Studies in Near Eastern Languages' and Literatures – Memorial Volume of Karel Petrůček, ed. by Petr Zemánek, Praha 1996, pp. 297-309. P. 119-120 : on *if'alla* class going back not only to Proto-Semitic but also to Proto-Hamitosemitic/Afroasiatic, see now my paper 'Towards a reconstruction of verbal derivation in Afroasiatic/Hamitosemitic: R3/D3 or *iqṭalla* class', in 'Archaisms and Innovations in the Semitic Languages', ed.

by J.P. Monferrer Sala and W.G.E. Watson, Cordoba 2013, 195-203. P. 144: the author correctly says that the future meaning of the Perfect *nādā* 'he will shout' (by the way: this is a good example of *fā'ala* without any 'conative' feature!) is due to the adverb 'on the day of Resurrection' but I do not think that it is necessary to make a difference between a simple human prediction of possibility and a divine prophecy of irrevocable future. P. 147: I do not think that modal Subjunctive *yaf'al-a* should be structurally opposed to *fa'ala*. Modal uses of *fa'ala* as optative are secondary, i.e. depend on the perfective function like in many other languages. P. 157: in Modern Literary Arabic *mā fā'al(a)* and *lam yaf'al* are stylistic variants (like in Classical Arabic of older periods) but *lam yaf'al* has a decisively 'literary', 'bookish' and even 'snobbish' flavor not only because it has disappeared from spoken dialects. There is very little on the use of Energetics in the part on tense, aspect and mode (pp. 133-162) as well as active participles.

I can imagine a third edition of this very useful book with more examples from modern literary texts.

Andrzej Zaborski

Warwick Danks, The Arabic Verb – Form and Meaning in the Vowel-Lengthening Patterns. Amsterdam – Philadelphia 2011. John Benjamins. XVIII + 281 pp. ISBN 978-90-271-1573-4.

This is a revised version of a doctoral thesis and it deals with the subject indicated in the subtitle, i.e. the III or *qātala* and the VI or *taqattala* derived verbs in Arabic, practically in Modern Literary Arabic. The former verbs are usually defined as 'conative' and the latter are usually considered as their 'reciprocal' forms and there is an incomplete synopsis of opinions by different scholars on pp. 66-69 where, e.g. the basic studies by Henri Fleisch and Larcher, not to mention my 2006 paper, are not mentioned. Danks brings another statistical scrutiny of the actual occurrence of the derived verbs on the basis of the 4th edition of the English version of Wehr's dictionary and the results differ very little from the results published by McCarthy and Prince (1990) as well as al-Qahtani (2003 and 2005). I do not think that Bohas's theory of 'Matrices, etymons and radicals' really deserved discussion since it is basically wrong especially in its (pseudo)etymological part.

Danks assumes that the main semantic feature of *qātala* verbs is 'mutuality' and in Table 27. he suggests that out of 465 *qātala* forms 361 verbs have this feature while 104 do not. He himself admits that he has tended towards a liberal interpretation of mutuality, e.g. he has counted the form *'āwana* 'to help, assist

(s.o) in/with (s.th.)' as expressing mutuality! In case of *taqātala* only less than two-thirds of 389 verbs have 'reciprocity' as the dominant meaning (Table 28.) As far as conativity is concerned, Danks says that no conativity occurs without mutuality while mutuality without conativity is encountered frequently and he pretends to have found only one example of allegedly conative meaning without mutuality feature, i.e. *wārā* 'to hide, conceal, try to keep secret (s.th.)' (p. 102). I can hardly agree with Danks' 'characterization of the function of the *ta-* prefix in pattern VI as detransitivising with few exceptions' (p. 129) – how reciprocal action can be intransitive? That many *qātala* verbs express features 'multiplication, frequency, durativity' etc. of action ('conative' feature being their secondary feature in case of telic verbs and especially with singular object, e.g. *qātala-hu* can mean only 'to repeat efforts to kill him') has been well-known since a long time. There are indeed traces of plurality of *qātala* class verbs and there are even *qātala* and *qattala* forms of the same verb having the same 'intensive/pluractional' meaning because at a prehistorical stage, already in Proto-Afroasiatic, *qattala* and *qātala* were variants or allomorphs. But I disagree with Danks when he says that "verbal plurality is a viable explanation of pattern III mutuality and pattern VI reciprocity". I do not think that it makes sense to look for a connection between long *-ā-* and 'activity', 'focus on process', 'internal temporal complexity' etc. in participles, verbal nouns, nouns of instrument and of occupation (pp. 143-154) at least in a synchronic analysis since there is a risk of sound symbolism ('Lautsymbolik') deviation. Diachronically it makes sense to compare III class Imperfect *yu-qātil-u* with the Active Participle *qātil* but this is quite another problem.

In Chapter 8. (pp. 157-187) Danks presents 'An aspectual model for Modern Standard Arabic' in which he takes for granted that *Aktionsart* and grammatical aspect must not be held as distinct and he concentrates on *Aktionsart* dubbed 'lexical aspect'. In Chapter 9. (pp. 189-211) he analyses different 'aspects' (actually *Aktionsarten*!) of *qātala* and *taqātala* like 'activity', 'surprise', 'giving', 'inception', 'semelfactive' and comes to conclusion that "over 93% of actual usage of pattern III and pattern VI verbal forms is attributable to verbs in categories unmarked for telicity". Danks considers only one (sic!) verb as telic, i.e. *ta'āfā* 'to recuperate, recover, regain health' but the consequences drawn from the examples of usage (No. 255a and 25b on p. 210) are not correct since the imperfective meaning in No. 255a is due to the use of auxiliary *lā yazāl* and the Imperfect form *yata'āfā*, and the perfective meaning in 255b is due to the use of *qad* with Perfect *ta'āfā*! I do not think that allegedly 'inceptive' verbs like '*āhada* 'to promise (s.o.) to do (s.th.)' or '*hāsaba* 'to call (s.o.) to account/ hold (to s.o.) responsible' are simply atelic. They are rather neutral as far as telicity is concerned.

The book must be read by everybody interested in problems of the semantics of verbal derivation in Modern Literary Arabic in spite of several controver-

sies. The author has used arabiCorpus but for a thorough study of *Aktionsarten* the use of real texts with real examples of usage (*Belegstellen*) should be much more extensive. There is still much to do.

Andrzej Zaborski

Gunvor Mejdell, Lutz Edzard (eds), High vs. Low and Mixed Varieties – Status, Norms and Functions across Time and Languages. Wiesbaden 2012. Harrassowitz. 210 pp. = Abhandlungen für die Kunde des Morgenlandes Band 77. ISSN 0567-4980. ISBN 978-3-447-06696-9.

The volume under review is based on papers delivered at the ‘Oslo Workshop on High and Low varieties, diglossia, and language contact : linguistic products and social processes’ which was organized at the University of Oslo on June 14th and 15th, 2010.

Gunvor Mejdell in ‘High and Low varieties, diglossia, language contact, and mixing: social processes and linguistic products in a comparative perspective’ introduces the main concepts and summarizes the problems analyzed and results attained in the particular papers. The author is right saying (p. 10-11) that the term ‘diglossia’ should be used in the proper, i.e. a ‘narrow’ sense and not for situations like ‘standard-with-dialects’, bilingualism, high and low variety of the same language etc. Therefore two criteria are crucial: 1. a relatively very close genetic relationship, practically a relationship of a decisively older and a younger stage (or stages) of the evolution of the historically the same language, 2. secondary acquisition of the ‘High’ variety which is nobody’s real native language so that this H variety is not used in spontaneous spoken communication. Already here there should be a mention (there is an outline of the history in the paper by Taine-Cheikh on p. 89) that the term ‘diglossia’ was used already in the 19th century for a classical diglossic situation in Greece where the high and the low varieties were not simply in complementary distribution but the separation of them in public life (till 1976) was enforced by law. William Marçais (see the reference on p. 107) actually took the term and the idea of diglossia from Greeks but it was Ferguson who made it popular in his rather overestimated paper of 1959. In any case both Greek (before 1976) and Arabic (continuing and still openly or tacitly enforced !) diglossias are prototypical! The idea of ‘spectro-glossia’ introduced after el-Sa’īd al-Badawī’s publication of 1973 should have been mentioned since it raised questions how complementary the complementary use of *al-fuṣṣḥa* and of spoken dialects, not to mention ‘intermediate Arabic’

really is. I would like to emphasize that in numerous studies on Arabic diglossia which have appeared since over a quarter of century it has been rather forgotten that actually the language situation in almost every Arab country is different and it is a clear simplification when Arabists speak about diglossia in general, frequently concentrating on the situation in one country, e.g. Egypt. There are many variants of diglossia in Arab countries! E.g. in Egypt, Cairo Arabic has already acquired a status near to the national spoken language, it is a language of literature (poetry, drama and prose) although a hybrid '*fuṣḥammiyya*' (the name introduced by G. M. Rosenbaum) is exploding. E.g. in Tunisia, Algeria and Morocco there is not only diglossia but also exoglossia with French still much used as 'high' language so that some linguists have coined the name triglossia which has been used sometimes also with reference to the use of Arabic, French and Berber. I have not found a mention of diglossia in Germanophone Switzerland (and at least in some regions of Germany and Austria), which in many respects resembles the situation e.g. in Egypt but nevertheless is different, first of all because Standard German (resented even by educated Swiss!) can be and is sometimes spoken in some rather informal situations. When I asked recently a friend who is a German linguist working in Switzerland whether he had problems with Swiss German, he explained that he used his own German dialect when speaking to his university colleagues who preferred his dialect rather than Standard German!

I am not convinced that 'the gradual fall of polite pronominal forms of address in languages that have or had them' (p. 21) is a real and not just temporary symptom of a tendency towards lessening of authority and formality in social interaction. Other means of language politeness are being introduced and will be introduced.

Tore Janson has written about 'Vulgar Latin and Middle Arabic' (pp. 24-31) trying to find both similarities and differences between their sociolinguistic and purely linguistic positions. Both can be seen either as texts or as (norms for) spoken forms, depending on the outlook of the researcher" says Janson (p. 29). The fact is that we do not have a direct and more or less detailed knowledge of spoken Latin and spoken early Romance and the same is the case of our knowledge and/or ignorance of Arabic as really used in many spoken dialects of the past, none of which was really identical with Classical Arabic (although Beduin speech was a model of 'good, correct language' for Sibawayhi and many other Arab normative grammarians!) which, in its turn, is not a source of any particular modern spoken dialect.

Jérôme Lentin has presented 'Reflections on Middle Arabic' (pp. 32-52) in which he emphasizes that "Mixed Arabic varieties have been written for centuries" (p. 33). Moreover "Middle Arabic combines standard and colloquial features with others of a third type, neither standard nor colloquial" (p. 39) and "beside the standard usage of Classical Arabic, ...another standardized (sic! AZ) register (was) available for those who wanted to use it". This has been a very important discovery

made by Lentin in his writings and his paper is the best in the volume. It is usually taken for granted that standardization of a language is equal to its codification in normative grammars and dictionaries by grammarians. But norm is not the same as codification (e.g. see p. 180, note 1). Actually good speakers (in every language group they are a minority!) of even unwritten languages have good linguistic intuitions and they are able to decide whether this form is correct or not. Of course we know that even in languages which have official normative grammars there are dubious cases since every living language is an open system. Nevertheless Middle Arabic could be practically standardized/normalized (classical norm and colloquial norm being poles of reference!) and I think that Lentin is right when he says that it really was. That there were different registers, different styles (not to mention mistakes!) is obvious since this is the case in any language.

Lentin discusses shortly also the problem of the existence of artistic colloquial Arabic (pp. 49) which gave reason to the idea of existence of another diglossia within the spoken dialects, something like 'the people's diglossia'. It is well known that by default the language of poetry must be always different from the daily spoken variant. Poetry using a variety imitating common daily speech is a 20th century European invention. In case of artistic Arabic colloquial(s) poetic and to some extent also prose texts use rare, special words and syntactic forms to be artistic but this is not diglossia yet! It is a normal, universal difference between artistic, poetic language and daily speech – without such a difference (big or small!) no artistic literature is possible.

Johannes den Heijer presented the paper entitled 'On language and religion identity: the case of Middle Arabic, with special reference to the Christian Arab communities in the medieval Middle East' (pp. 53-87). The author asks the question whether it is really possible to speak of distinct 'Christian Arabic' and he summarizes his analysis in the following way: "For the time being it is hard to either confirm or deny the existence, for the Arabic-speaking world as a whole and over the centuries of unambiguous markers of broad sociolinguistic categories that can be identified as specifically Muslim, Jewish or Christian, in terms of Arabic phonetics, orthography, morphology or syntax". (p. 66). Then come analyzes of some sample texts and language varieties and mainly loanwords are found to be typical of Christian Arabic texts but the author finally emphasizes that much more research of much bigger corpora is needed to find a definite answer. I should like to add that in the 1974/75 academic year in Cairo I used to read rather regularly the Coptic weekly 'al-Muwāṭīn' whose language was strikingly different from the language of the 'standard' Arabic press like 'al-Ahrām' and 'al-Aḥbār' not only because it was simpler and much more 'natural' and less formal. I had the same impression when reading also other modern Christian texts (e.g. guidebooks, catechisms, devotional literature etc.) in Egypt, Lebanon, Syria and Iraq.

Catherine Taine-Cheikh's paper is entitled 'Arabe(s) et berbère en Mauritanie: bilinguisme, diglossie et mixité linguistique' (pp. 88-108). The author-

ess emphasizes the prestige of the local al-Hassāniyya Arabic both as spoken language and the language of folk literature but she says that this prestige might have diminished slightly since people living in towns (how many? AZ) have been losing their contact with the Bedouin world (p. 106) and there are symptoms of an emergence of a new intermediate language.

Lutz Edzard has dealt with ‘Elements of diglossia in Biblical Hebrew and Modern Hebrew’ (pp. 109-122). There is some truth in the hypothesis (!) that in Modern Hebrew a diglossia-like situation can be observable (cf. p. 120) since some limited use of older language continuing the Biblical (and Mishnaic!) is still continued and will be continued but in my opinion the question is whether this semi-diglossia is emerging or slowly disappearing? I do not think that we can speak about diglossia in Biblical (excluding Qumranic?) Hebrew as done by Rendsburg and Young – it is obvious that there had to be stylistic and regional as well as social registers and variants but this was not a diglossia like in the Arab world today which is admitted by Edzard.

Bernt Brendemoen has discussed ‘Prestige registers vs. common speech in Ottoman Turkish’ (pp. 123-132). This is a very well written, brilliant story and history of the official, very formal, embellished and full of Arabic and Persian loanwords language of the Ottoman court used as formal language till Atatürk’s reforms which abolished religious instruction as well as Arabic and Persian as subjects in secondary schools in 1927 and introduced Latin alphabet and language purification and modernization in 1928. There is a wonderful story of Atatürk’s official speech of 1934 which contained only two words of Arabic origin and was modernized to such an extent that it was partially incomprehensible so that it was delivered by Atatürk with the stuttering of a school child who has just learnt to read (p. 131)!

Claus Peter Zoller has written about ‘Hindi bilingualism and related matters’ (pp. 132-157) in a very interesting and insightful way. Polyglossia with exoglossia and very dynamic interference of several languages (including dead Sanscrit!) are characteristic of the language situation there.

Kristin Hagemann has asked the question ‘Romance glosses in a Latin text: evidence of diglossia?’ (pp. 158-168) and her answer is that “these glosses, although clearly demonstrating a different (lower) register than the Latin text, as well as untraditional way of spelling, are not evidence of diglossia” (p. 167)

Jan Erik Rekdal has presented ‘Macaronic texts in the early Irish tradition’ (pp. 169-178) which were created for both satirical (in comic poems) and religious (sermons) purposes.

Karen Gammelgaard presented the paper ‘Czech code mixing 1990-2010: from domain specialization toward graded registers’ (pp. 179-196) in which she discusses the on-going trend of register mixing so that users apply neither Literary Czech nor Common Czech in their pure forms. The concept of diglossia cannot be reasonably applied to the situation in the Czech Republic (p. 192) and

I do not think that it could make sense to speak about the present Czech linguistic situation as post-diglossic as one of the Bohemists did.

Ernst Håkon Jahr has discussed the question ‘High and low in Norwegian? Dialect and standard in spoken Norwegian – a historical account of competition and language status planning’ (pp. 197-208). He has emphasized that Norway is very dialect-tolerant and “the reason for this is Parliament’s firm policy of promoting the use of ‘low’ dialects in the schools, and thus blocking the establishment of a ‘high’ variety of Norwegian which could assume dominance in schools and in society at large” (p. 207). This is a very interesting and rather rare if not unique language policy indeed. It is clear that it is possible only due to the specific cultural and political history of Norway and the present stable welfare situation.

The whole volume is a valuable and a very readable collection of papers.

Andrzej Zaborski

Abulḥayr Al-iṣḥbīlī (s. V/XI), *Kitābu ‘umdati ṭ-ṭabīb fī ma‘rifati n-nabāt likulli labīb (Libro base del médico para el conocimiento de la botánica por todo experto)*. Edición, notas y traducción castellana, correcciones e índices de J. Bustamante, F. Corriente y M. Tilmatine, «Fuentes arábico-hispanas» 30, 33, 34, 3 vols. en 4 tomos, Madrid: Consejo Superior de Investigaciones Científicas, 2004, 2007, 2010), 785 + 857 + 1049 (530 + 519) pp. ISBN: 978-84-00-08239-0

Works like this – its four tomes are weighty in all senses – are not published every day, nor even every year. Indeed, one would have to go back considerably further to find anything remotely comparable.

Abū l-Ḥayr’s *‘Umdah* is, undeniably, a major work, in that it provides a veritable mine of information on a whole range of fields (though with evident common grounds in many respects) including botany, the history of medicine and science in general, and also linguistics, particularly lexicography. Despite the partial edition-translation published by Asín, and the somewhat idiosyncratic edition undertaken by al-Ḥaṭṭābī, there was a clear need for a new edition and a new translation of the complete text. There was equally a need for a thorough study of the lexicographical and botanical material, and to a lesser extent material concerning other fields. Abū l-Ḥayr collected a dazzling wealth of plant names, and the resulting compendium was enhanced by its linguistic value, in that it included terms in languages that are now extinct, or have developed almost beyond recognition since the eleventh century, when the *‘Umdah* was compiled.

In terms of the sheer amount and quality of the lexical, botanical and mineralogical information it contains, Abū l-Ḥayr's *ʿUmdah* is clearly a landmark compilation of scientific knowledge. This new edition in four volumes is structured as follows:

1. The first volume contains a preface in Spanish (pp. 1-2) and in Arabic (pp. 9-10), the latter including a note on the system used to transcribe the non-Arabic terms in the text; the edition of the text itself (pp. 10-689), and an index of plant names indicating the etymology of the non-Arabic terms (pp. 692-857).

2. The second volume includes its own preface (p. i), a list of the abbreviations used in the critical apparatus (pp. iii*-vii* [unnumbered]) and a complete annotated translation of the Arabic text (pp. 1-785).

3. The third volume, in two tomes, provides a general index (pp. 7-8), a preface (pp. 9-10), corrections to volumes one (pp. 11-15) and two (pp. 17-27), a full bibliography (pp. 29-35) and a complete set of indices covering: plant species (pp. 38-221), animals (pp. 222-233), minerals (pp. 234-235); an index of transcribed terms: botany (pp. 236[I]-896[II]), zoology (pp. 896-914) and mineralogy (pp. 914-916); an index of non-Arabic terms (pp. 917-965, the *loca* for the language names mentioned appearing on pp. 917-922); an index of proper names: anthroponyms, ethnonyms, demonyms and nisbas, professions and human groups, names of stars, houses and signs of the zodiac (pp. 966-984); an index of toponyms (pp. 984-1006); an index of works cited in the text (pp. 1006-1008); an index of medical and pharmacological terms (pp. 1009-1038); and finally an index of trade, industry and technology (pp. 1038-1049).

As this brief outline of the contents suggests, these volumes offer an immense amount of highly-varied and fascinating information. Hence the need for a new edition fulfilling all the requirements of textual criticism and accompanied, naturally, by a translation of the complete text together with a scholarly study. The authors have provided all this, achieving throughout the two thousand six hundred and ninety-one pages a considerable degree of scientific rigour.

The edition of the Arabic text based on the two surviving manuscripts is excellent; the text has been perfectly structured, and technical terms are numbered and thus easy to locate in the indices. References are shown in bold to distinguish them from the commentary or gloss accompanying each technical term. The Spanish translation is a faithful rendering of the Arabic text, and terms are retained in transcription. The translation, for its part, is enhanced by a detailed critical apparatus, in which the authors provide interesting linguistic and botanical information on the term or species in question. In addition to the edition of the Arabic text and the annotated translation, the two tomes of volume three contain a wealth of detailed analytical and documentary information on the various types of technical terms used in the text.

This is undoubtedly an impressive, admirable and praiseworthy achievement, the fruit of several years' painstaking work. The meticulous edition

and accurate translation (corrections to some passages are included in vol. III,1) are matched by an outstanding critical apparatus which seeks above all to identify terms and species; this is often a challenging task, involving the identification and etymological reconstruction of lexical items and the identification of botanical, zoological and mineralogical terms used centuries ago and in places very remote from each other, as suggested by the countless terms appearing in the text. The superbly rigorous results testify to a prodigious effort on the part of the authors, from the first page to the last.

To express any reservations at all with regard to an undertaking on this vast scale would seem unfair. The following notes are in no way intended to detract from my respect and admiration for the high scientific task done by the three authors, but rather reflect the extent of my interest and at the same time my desire to contribute to the dazzling scholarship evident in their work. The notes may be of some interest, inasmuch as they provide useful etymological information on plant names which may help to chart the linguistic route travelled by a given term before it entered the Arabic language; this in turn may help to track the history of the plant in question. For example:

- *amīrbārīs* (III,1:258) < Syr. *'amabarīs*.
- *anāgāllīs* (III,1:261) < Syr. *'anagallīs* < Gr. *anagallīs*.
- *arz* (III,1:283) < Aram. *'arzā* / Syr. *'arzā*.
- *asārūn* (III,1:284) < Sir. *'asārōn* < Gr. *ásaron*.
- *azāḍarāht* (III,1:300) < Aram. *'azād derakt* < Neop. *āzād deraxt*.
- *baṅḡ* (III,1:340; I:727) < Pers. *baṅ* < Sansk. *baṅgā*.
- *bunduq* (III,1:355) < Aram. *pūnduq* / Syr. *pondiqā* < Gr. [*káryon*] *pontikón*.
- *hilyūn* (III,1:850-851) < Syr. *helyūn* < Gr. *héleion*.
- *idḥir* (III,1:508) < Aram. *'idḥir*.
- *iḡḡāš* (III,1:508; cf. I:694) < Aram. *'aggās* / *'aggāš* (< Akkad. *anḡašu*).
- *ihlīlag* (III,1:509) < Aram. *halīlag* (cf. Syr. *hlīlqā*) < Pers. *halīlag/h* < Sansk. *harītaka*.
- *iklīl al-malik* (III,2:509) < Syr. *kēlīl malkā* < Heb. *kēlīl malkā*.
- *isfānāḥ* (III,1:511) < Neop. *espenāx*, cf. Gr. *spinákion*.
- *māzaryūn* (III,2:580; cf. I:839) < Syr. *māzaryūn* (cf. Aram. *māzaryōn*) < Pers. *māzaryūn*.
- *miškiṭrāmašīr* (III,2:583) < Syr. *mūšk ṭrāmašīr* < Pers. *mosk tarmašīr*.
- *mulūḥiyā* (III,2:590) < Syr. *mulūḥiyā* (cf. Ar. var. *mulūkiyā*) < Gr. *molóché*.
- *nārdīn* (III,2:603) < Syr. *nārdīn* < gr. *nārdinon* < Sank. *naladā*.
- *sūrunḡān* (III,2:717; cf. I:776) < Syr. *sūrāṅgān* < Pers. *sūringān*.
- *uśnah* (III,2:814; cf. I:703) < Pers. *uśnah*.
- *utrūḡḡ* (III,2:815) < Aram. *'utrūḡā* / Syr. *'utrūḡā* < Neop. *toranḡ*.

The botanical term *ās* ('myrtle'; cf. I:699-700), from the Aram. *'āsā* / Syr. *'āsā* < Akkad. *asu(m)* (cf. Old Assyrian *ašu[m]*), is missing from the indices

in vol. III,1, while *yāsamīn* is missing from III,2:832 (cf. I:854). The term *bud hindī* should be replaced by the correct form *burru l-hindī* (II, p. 287). For the romancism *qūrbuh* (III, pp. 904, 911), since diphthongs occur in documents from *al-ṭaġr al-wuṣṭā*, the transcription *quw^erbuh* (< *quwarbuh*) would perhaps be appropriate, although we must say that diphthongization is not attested in the Romandalusi register in the old documents from the Baetica.

The bibliography (III,1:29-35) omits two interesting sources: Maimonides' *Šarḥ asmā' al-'uqqār* (*Šarḥ asmā' al-'uqqār: L'explication des noms de drogues. Un glossaire de matière médicale composé par Maimonide. Texte publié pour la première fois d'après le manuscrit unique avec traduction, commentaires et index par Max Meyerhof, El Cairo: Institut français d'Archéologie orientale au Caire, 1940*) and al-Kindī's *al-Aqrabādīn* (*The Medical Formulary or Aqrabadhin of Al-Kindi. Ed., trans., and glossary by Martin Levey, Madison – Milwaukee – London: University of Wisconsin Press, 1966*). Similarly, I would have welcomed a reference to Immanuel Löw's enormous and fascinating *Die Flora der Juden*. 4 vols., Hildesheim – New York: Georg Olms, 1967 (= 1928), which contains a great deal of botanical information.

These remarks are, of course, purely anecdotal in terms of the huge amount of information so carefully examined in these four volumes. The quality of the research, the accuracy and splendid presentation of the glossary and translation, and the design and execution of the work as a whole, add up to an excellent overall result, and open up a whole new approach to this kind of material, perfectly and seamlessly blending the required textual, linguistic and botanic scholarly criticism.

Many scholars still believe that 'science' (as a discipline, as the fruit of man's cultural development) is somehow separate from other cultural phenomena, simply because it has developed in a linear fashion over the centuries, whereas – according to them – the disciplines grouped under what have become known as the 'Social Sciences' can only address those matters which cannot be verified by any 'scientific procedure'. Evidently, for certain fields of study (e.g. linguistics) this distinction has ceased to have any real meaning in its overall sense, since 'knowledge' has gradually been reconstructed using new basic concepts whose effectiveness within the 'Social Sciences' started to bear its best fruit from the 1970s onwards, and undoubtedly continues to do so today.

The book under review is, in many ways, an exemplary consequence of this proposition. Developments in all the varied fields of linguistics have contributed greatly to studies like this one, enhancing its particular blend of botany and historiography with outstanding results that could only be achieved through an interdisciplinary approach with a single purpose: to provide the scientific community with a source as important as the *'Umdah* in the best possible conditions, so that researchers in other disciplines, such as botany, zoology and mineralogy – as well as those working in the history of science and

medicine – can have ready access both to the source itself and to full information on all its elements.

For reasons that need not be explored here, such a feat has hitherto never been attempted, so this edition of the *‘Umdah* is particularly welcome; not only has it filled a gap, but it has done so in an exemplary manner, marking a qualitative leap forward of a kind not previously attempted in Spain. For all these reasons, we can only congratulate the editors, and offer them our heartfelt thanks for producing such a meticulous scholarly text, which will be of immense value for botanical studies, Arabic studies in particular, and indeed for scientific research in general, to which scholars in the “humanities” also have an essential and important contribution to make.

Juan Pedro Monferrer-Sala

Anna Krasnowolska, *Mythes, croyances populaires et symbolique animale dans la littérature persane*, Paris 2012 (“*Studia Iranica*”, cahier 48, pages 1-244)

The book contains the texts of five lectures given by the author in Paris in November-December 2010 within the scope of *Conférences d’études iraniennes Ehsan et Latifeh Yarshater*. Anna Krasnowolska is the head of the Iranian Section at the Jagiellonian University’s Institute of Oriental Studies in Cracow. She has published more than 130 papers on many aspects of Iranian culture and is a regarded authority in the field of Iranian studies.

The book refers to Iranian mythology, popular beliefs and symbolical animals occurring in Persian literature. The five chapters present five animals: the bull, the scorpion, the nightingale, the boar and the horse. These are by no means the marginal topics one could suspect. In archaic cultures animals were treated with respect: without bulls people could hardly cultivate the land and grow crops. When sophisticated philosophical terms had not yet been developed people used to speak about the most important things of their existence using metaphors derived from nature.

As we read in the English summary of the introduction to the present book its author approaches the problem of “the persistence and continuity as well as the constant process of transformation, of the motifs, images and ideas in Persian literature, within the passage of time and in changing cultural conditions” (p. 29).

For instance, as we learn from Chapter Three (*Le rossignol et le poète*) the famous rose-and-nightingale motif evolved. In Ferdowsi’s (10th c.) *Šāhnâme* the nightingale was a singer of only one tradition: the past glory of Iran and its epic heroes (see p. 113 of the present book). In the course of time the nightingale became an enamoured lover’s symbol (*âšeq*) and the hero of sublime court love

poetry (p. 120). Later on, for Rumi (a mystical poet of 13th c.) the nightingale was a symbol of strong feelings and exceptional spiritual qualities (p. 132).

The banal, as it could be supposed, bull's motif from Chapter One (*Kākil – Gāv-e Gil. Mystérieux héros d'une fête hivernale*) gives the author the possibility to explain the enigmatic popular customs described by al-Biruni (a Persian erudite who wrote his works in Arabic in 10th / 11th c.). Namely, here the matter concerns an ancient festival, held on three consecutive days in the middle of the Iranian month of Dey. On the first day of that *triduum* garlic and other vegetables were eaten with meat. The next day clay or dough was moulded into the shape of human figures, which were placed at the entrances to houses.

According to Anna Krasnowolska the festival was an illustration of the chaos which developed when one king had died (the ending of the year was meant) and another had not yet taken power (the winter solstice). Her interpretation of the name of the festival's third day is convincing: *Kākil / Gāv-e Gil* < *gāv* "bull" + *gil / gel* "clay, soil" (p. 57-59).

Another enigmatic fragment of al-Biruni's *Chronology* (*Kitāb al-āṭār al-bāqiya* ...) is thoroughly elucidated by Anna Krasnowolska in Chapter Two of her book where the old Iranian scorpion chasing formula is analysed (*Une formule magique contre les scorpions*).

The author examines the evolution of the functions performed by various plots present in Persian literature. She analyzes the connections existing between the oral and written transfer of tradition, between popular culture and its high equivalent known from court literature.

In Chapter Four (*Arbre – Animal – Eau. Le songe de Xosrow Nušīnrawān*) an interesting interpretation of the Sassanian king Xosrow's dream is given. The author absorbingly presents, in the same chapter, the parallels existing (as it was brought to light by other scholars) between Vis and Rāmin (the pair of lovers known from Gorgāni's poem *Vis-o Rāmin*, 11th c.) on the one hand and the famous Celtic lovers Tristan and Yseut (p. 145) on the other.

Sometimes Anna Krasnowolska's considerations become polemical and this contributes additionally to progress in Iranian studies. She is right when she criticizes some Iranian scholars who do not know ancient Greek sources, ones important for a better understanding of some problems connected with the culture of ancient Iran (p. 181, 186).

Anna Krasnowolska has noticed some topics which had escaped the attention of Khaleqi-Motlagh and other specialists in Iranian studies who had missed the intruder's motif and symbols taken from nature which are present (as she demonstrates) in three texts: Gorgāni's *Vis-o Rāmin*, Ferdowsi's *Bizhan and Manizhe* (*Šāhnāme*) as well as in *Xosrow's dream* (*Šāhnāme*), cf. p. 153.

The last chapter (*Le sauveur et son cheval*) shows the durability of Iran's mythological tradition. For instance, the motif of the horse which bears a hero to the other world or undergoes sacrifice also appears in modern Persian literature, in

the prose works of Simin Dânešvar, Hušang Golširi and Rezâ Dânešvar. These texts refer to the modern intellectual currents which convulsed Iran during the Islamic revolution of 1978-80 although they utilised old mythological topics.

Each chapter of the book under discussion is accompanied by an English summary. This can help the reader whose French is not perfect.

It is almost impossible to unearth anything erroneous in Anna Krasnowolska's book. If anything is present then it is only of a technical character, e.g., in the bibliography (p. 22) the name Sotude as the author whose book was quoted on page 166 (footnote 113) is lacking.

Anna Krasnowolska's book contains articles of hers published earlier yet these have been enlarged and enriched. "The textual base of its final three chapters has been broadened to lyrical poetry, romantic epos and modern Persian prose" (p. 29, English summary).

The book of Anna Krasnowolska shows her erudition and the capacity to detect parallel phenomena in various domains (literary works, folklore, popular beliefs, calendar mythology). Many of those parallels have not been noticed by other scholars and that is why the book in question really contributes to a better perception of Iranian culture when analysed over its long history and in all its aspects.

Andrzej Pisowicz