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The Balaam

# THE BALAAM TEXT FROM DEIR 'ALLA RE-EVALUATED

*Proceedings of the International Symposium held at Leiden  
21 - 24 August 1989*

EDITED BY

J. HOFTIJZER and G. VAN DER KOOIJ



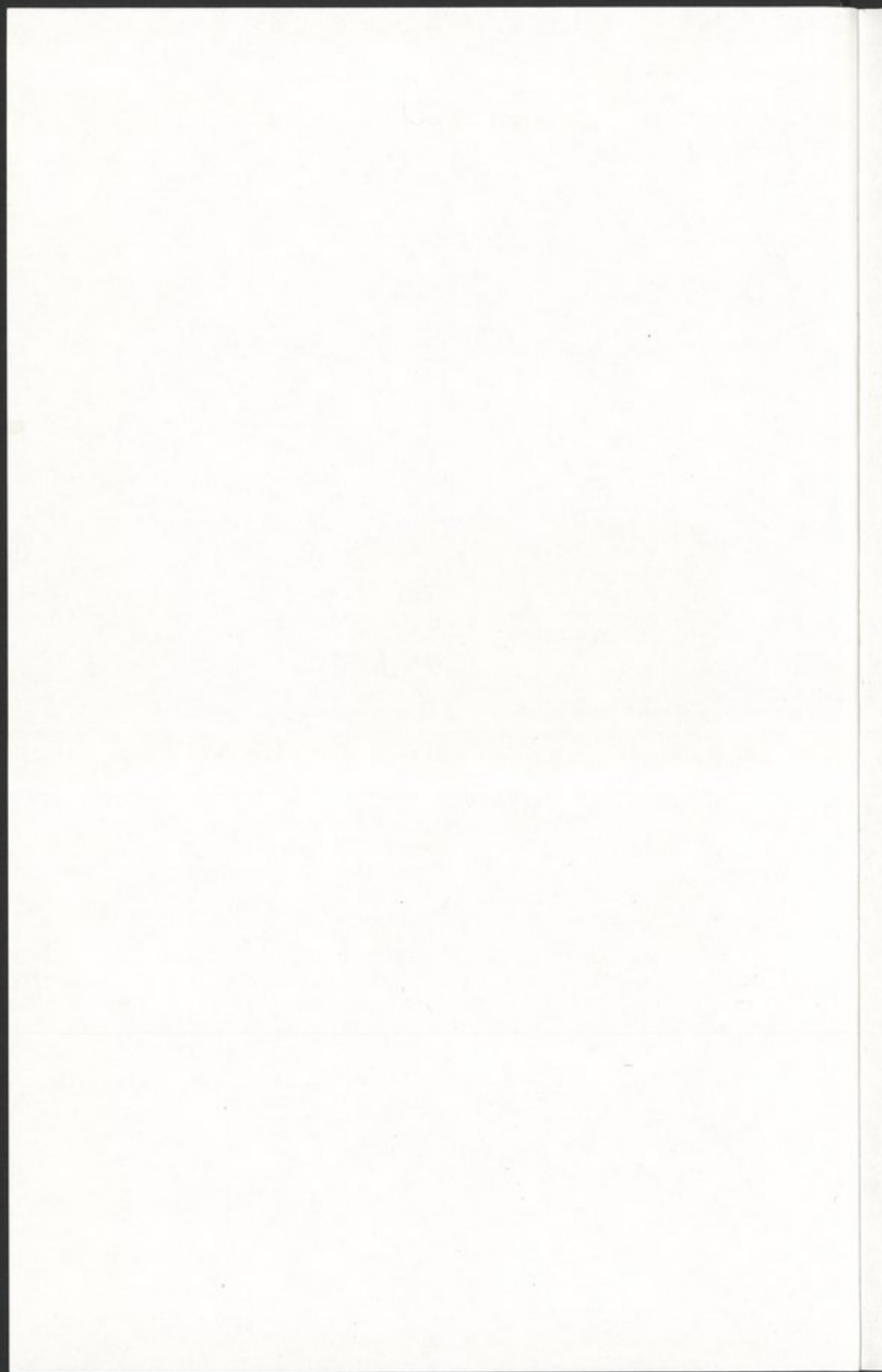


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## CONTENTS

Preface and acknowledgements .....	vii
Bibliographical abbreviations .....	ix
Introduction and archaeology	
H.J. Franken: Deir 'Alla re-visited .....	3
M.M. Ibrahim, G. van der Kooij: The archaeology of Deir 'Alla Phase IX .....	16
General interpretation	
A. Lemaire: Les inscriptions sur plâtre de Deir 'Alla et leur signification historique et culturelle .....	33
B.A. Levine: The plaster inscriptions from Deir 'Alla: general interpretation .....	58
J.A. Hackett: Response to Baruch Levine and André Lemaire	73
Language	
P.K. McCarter: The dialect of the Deir 'Alla texts .....	87
D. Pardee: Response: The linguistic classification of the Deir 'Alla text written on plaster .....	100
Interpretation of details	
J.C. Greenfield: Philological observations on the Deir 'Alla in- scription .....	109
J. Hoftijzer: What did the gods say? Remarks on the first combi- nation of the Deir 'Alla-plaster texts .....	121
G.I. Davies: Response to J. Greenfield and J. Hoftijzer .....	143
Relation with Biblical Studies	
M. Weippert: The Balaam Text from Deir 'Allā and the study of the Old Testament .....	151
H.-P. Müller: Die Funktion divinatorischen Redens und die Tierbezeichnungen der Inschrift von Tell Deir 'Allā .....	185
M. Dijkstra: Response to H.-P. Müller and M. Weippert ...	206
Palaeographical and related aspects	
E. Puech: Approches paléographiques de l'inscription sur plâtre de Deir 'Alla .....	221

G. van der Kooij: Book and script at Deir 'Alla . . . . .	239
M. Dijkstra: Response to E. Puech and G. van der Kooij	263
Three short communications and an additional note based on remarks made during the symposium	
J.-M. Husser: Deux observations à propos des rapports entre le texte de Deir 'Alla (Combination I) et la Bible	273
J. Huehnergard: Remarks on the classification of the Northwest Semitic Languages . . . . .	282
A. Wolters: Aspects of the literary structure of Combina- tion I . . . . .	294
F. Israel: Réflexions méthodologiques sur le classement lin- guistique de DAPT . . . . .	305
List of contributors . . . . .	318
Biblical quotations . . . . .	319

## PREFACE AND ACKNOWLEDGEMENTS

The "Balaam Texts" were discovered by H.J. Franken at Deir 'Alla in the Jordan Valley in 1967. The *editio princeps* of this exceptional inscription, written on wall plaster, appeared in 1976, and many scholars have been studying the text fragments since then, offering new solutions for so many difficulties in their understanding. The number of publications and their impact on so many different fields of study suggested to us the desirability to organise a symposium in order to re-evaluate the "Plaster Texts", twelve and a half years after the *editio princeps*. Both of us were very much interested in organising such a meeting in Leiden, since not only the excavations of the text fragments, but also their conservation and initial study were accomplished by the University of Leiden. At the same time, the renewed excavations at the site of Deir 'Alla, a joint project of the Leiden University with the Yarmouk University in Irbid and the Department of Antiquities in Amman, made it desirable to organise an exhibition about this project in the National Museum of Antiquities in Leiden. This mainly was to cover the settlement phase of the plaster texts. We are most grateful to the directors of the Department of Antiquities (Dr A. Hadidi and Dr Gh. Bisheh) and to the Jordanian Government for their exceptional permission to include the panels with the inscribed plaster fragments in this exhibition, and make them available for study during the symposium.

We have the honour to present here the proceedings of the symposium held in Leiden, August 21-24, 1989. In principle the lectures and written responses are published in the order in which they were presented, under the headings of the subjects of the sessions. This order was partly influenced by circumstances, but it has been retained because sometimes reference is made to lectures presented earlier. There are two exceptions: "archaeology" is placed after the introductory lecture, dealing with archaeology as well, and the short communications are placed at the end. The discussions are represented only by an additional note by F. Israel, added to the short communications.

Looking back, gratefully, at the symposium, we would like to thank many persons and institutions that made this meeting possi-



ble. First of all we wish to thank all those who accepted our invitation to attend the symposium and by their enthusiasm and their dedication to the subject made it unforgettable for us. We especially thank those who lectured and those who responded. The State University of Leiden and the National Museum of Antiquities in Leiden gave us many facilities, such as administrative help and rooms for the meetings. Brill's publishing house gave us a reception. A number of organisations and institutions provided us with the indispensable financial aid, which not only enabled us to realize the symposium, but also to publish its results. In alphabetical order they are the Centre for Non-Western Studies (CNWS) and the Faculty of Arts of the State University of Leiden, the Koninklijke Nederlandse Akademie van Wetenschappen and the Stichting Leids Universiteits-Fonds. We wish to express our sincere thanks for their support.

February 1990

J. Hoftijzer  
G. van der Kooij

## BIBLIOGRAPHICAL ABBREVIATIONS

AASF	Annales Academiae Scientiarum Fennicae
ADAJ	Annual of the Department of Antiquities of Jordan (Amman)
AEPHE	Annuaire École Pratique des Hautes Études (Paris)
AfO	Archiv für Orientforschung. Internationale Zeitschrift für Wissenschaft vom Vordernen Orient (Berlin, Graz)
AHw	W. VON SODEN, <i>Akkadisches Handwörterbuch</i> I-III, Wiesbaden 1958-'81
AION	Annali dell'Istituto Orientale de Napoli (Napoli)
AJSL	American Journal of Semitic Languages and Literatures (Chicago)
AOAT	Alter Orient und Altes Testament. Veröffentlichungen zur Kultur und Geschichte des Alten Orients und des Alten Testaments
ArOr	Archiv Orientalni (Prag)
ARM	Archives Royales de Mari (Paris 1950-)
ARTU	J.C. DE MOOR, <i>An Anthology of Religious Texts from Ugarit</i> , Leiden, 1987
AS	Assyriological Studies (Chicago)
ATTM	K. BEYER, <i>Die aramäischen Texte vom Toten Meer</i> , 1984
AuS	G. DALMAN, <i>Arbeit und Sitte in Palästina</i> I-VII, Gütersloh 1928-'42
AUSS	Andrews University Seminary Studies (Berrien Springs)
AV	Authorized Version
BA	The Biblical Archaeologist (New Haven)
BAH	Bibliothèque archéologique et historique. Institut français d'archéologie de Beyrouth (Paris)
BAR	Biblical Archaeology Review (Washington)
BASOR	Bulletin of the American Schools of Oriental Research (New Haven)
BAT	Biblical Archaeology Today. Proceedings of the International Congress on Biblical Archaeology, Jerusalem, April 1984, Jerusalem 1985
BDB	F. BROWN, S. R. DRIVER, C. A. BRIGGS, <i>A Hebrew and English Lexicon of the Old Testament</i> , Oxford 1906
BeO	Bibbia e Oriente (Milano)
BHHW	<i>Biblisch-Historisches Handwörterbuch</i> I-III, ed. B. REICKE, L. ROST, 1962-1966, Göttingen
Bibl.Or	= BiOr
BiOr	Bibliotheca Orientalis (Leiden)
BiR	Biblical Research (Chicago)
BiTrans	The Bible Translator (London)
BK(AT)	Biblischer Kommentar. Altes Testament (Neukirchen-Vluyn)
BLH	H. BAUER, P. LEANDER, <i>Historische Grammatik der hebräischen Sprache</i> , Halle, 1922
BO	= BiOr
BZ	Biblische Zeitschrift (Paderborn)
BZAW	Beihefte zur Zeitschrift für die Alttestamentliche Wissenschaft (Berlin)
CAD	Chicago Assyrian Dictionary (Chicago 1956-)
CBQ	Catholic Biblical Quarterly (Washington)
CIS	Corpus Inscriptionum Semiticarum (Paris)

- CRAI (BL) Académie des Inscriptions et Belles-Lettres, Comptes rendus des séances (Paris)
- CRAIBL = CRAI
- CTA A. HERDNER, *Corpus des tablettes en cunéiformes alphabétiques découvertes à Ras Shamra—Ugarit de 1929 à 1939*, 1963
- CThM Calwer Theologische Monographien
- DAPT Deir 'Allā Plaster Texts
- DicTalm M. JASTROW, *A Dictionary of the Targumim, the Talmud Babli and Yerushalmi and the midrashic Literature*, 1903
- DISO Ch-F. JEAN, J. HOFTIJZER, *Dictionnaire des Inscriptions Sémitiques de l'Ouest*, Leiden 1965
- ErIs Eretz-Israel, Archaeological and Geographical Studies (Jerusalem)
- Ges<sup>18</sup> W. GESENIUS, *Hebräisches und aramäisches Handwörterbuch über das Alte Testament* 18. ed. – R. MEYER, H. DONNER, 1987 –
- GesB Wilhelm GESENIUS' *Hebräisches und aramäisches Handwörterbuch über das Alte Testament*, ed. F. BUHL, 1915
- GesThes W. GESENIUS' *Thesaurus philologicus criticus linguae Hebraeae et Chaldaee Veteris Testamenti*, Leipzig 1835–'53
- GLECS Groupe linguistique d'études chamito-sémitiques. Comptes rendus des séances (Paris)
- HAL L. KÖHLER, W. BAUMGARTNER, *Hebräisches und aramäisches Lexikon zum Alten Testament*, Leiden
- HEO Hautes Études Orientales (Genève, Paris)
- HSM Harvard Semitic Monographs
- HUCA Hebrew Union College Annual (Cincinnati)
- ICC The International Critical Commentary (Edinburgh)
- IEJ Israel Exploration Journal (Jerusalem)
- JANES Journal of the Ancient Near East Society of Columbia University (New York)
- JAOS Journal of the American Oriental Society (Boston-New Haven)
- JEOL Jaarbericht van het Vooraziatisch-Egyptisch Genootschap Ex Oriente Lux (Leiden)
- JNES Journal of Near Eastern Studies (Chicago)
- JSS Journal of Semitic Studies (Manchester)
- KAI H. DONNER, W. RÖLLIG, *Kanaanäische und aramäische Inschriften*, Wiesbaden, 1962 ff. <sup>3</sup>1971–'76
- KAT Kommentar zum Alten Testament
- KBL = HAL
- KTU M. DIETRICH, O. LORETZ, J. SANMARTÍN, *Die keilalphabetische Texte aus Ugarit*, Neukirchen-Vluyn, 1976
- LexSyr C. BROCKELMANN, *Lexicon Syriacum*, Halle, 1928
- MEE Materiali Epigrafici di Ebla (Napoli)
- MSL B. LANDSBERGER, *Materialien zum sumerischen Lexikon*
- MUSJ Mélanges de l'Université Saint-Joseph (Beyrouth)
- NBG Bijbel; vertaling in opdracht van het Nederlandsch Bijbelgenootschap
- NEB The New English Bible
- NJV New Jewish Version
- OA Oriens Antiquus (Roma)
- OLP Orientalia Lovaniensia Periodica (Leuven)
- Or Orientalia (Roma)
- OrAnt Oriens Antiquus (Roma)
- PEQ Palestine Exploration Quarterly (London)

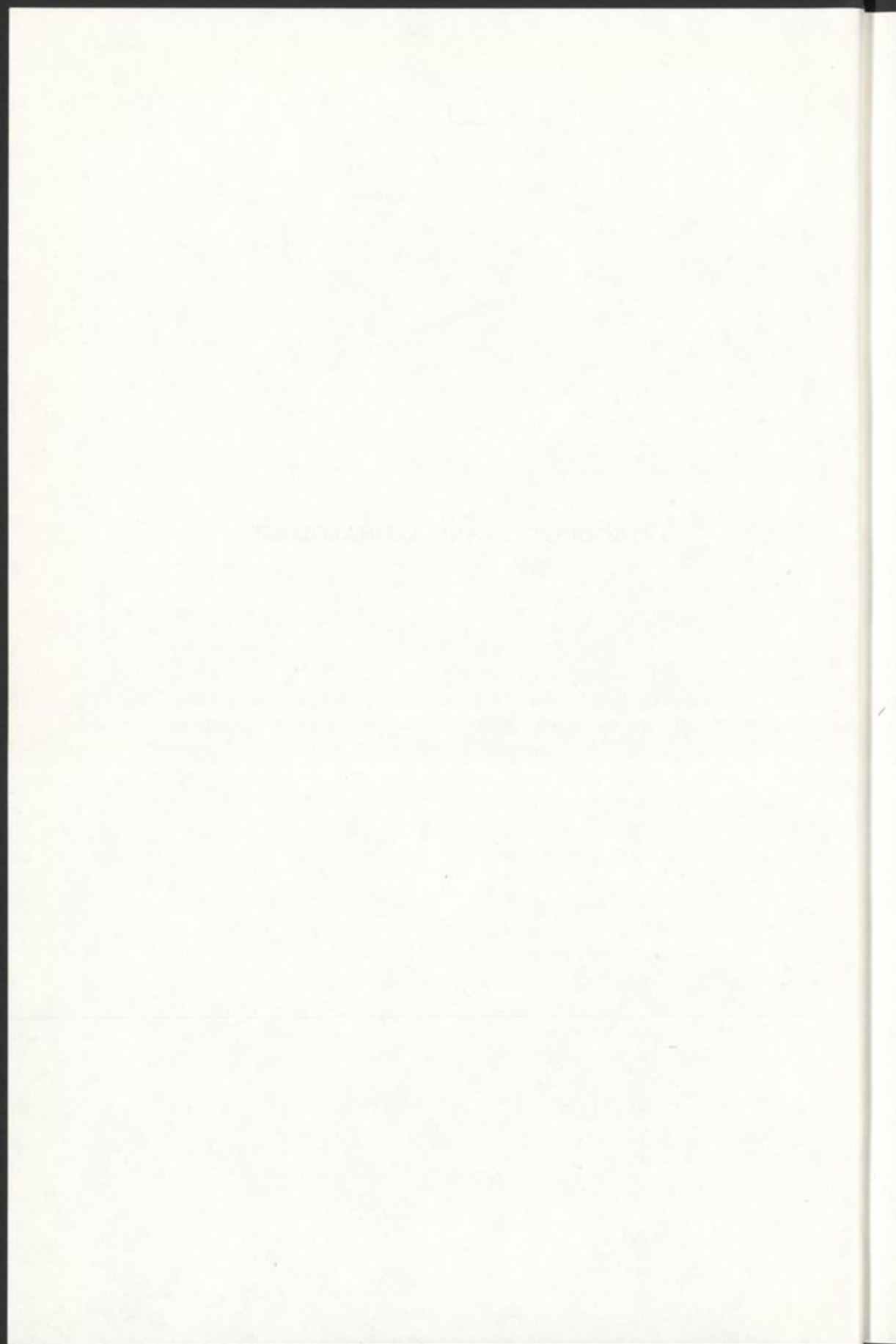


RA	Revue d'assyriologie et d'archéologie orientale (Paris)
RANL	Atti della Accademia Nazionale dei Lincei, Rendiconti classe di scienze morali storiche e filologiche (Roma)
RB	Revue Biblique (Paris)
RHR	Revue de l'Histoire des Religions (Paris)
RLA	Reallexikon der Assyriologie
RSV	Revised Standard Version
SEL	Studi epigrafici e linguistici
Sem	Semitica (Paris)
SVT	Supplements to Vetus Testamentum (Leiden)
Syr	Syria (Paris)
TA	Tel Aviv, Journal of the Tel Aviv University Institute of Archaeology (Tel Aviv)
ThWaT	= TWAT
ThWNT	Theologisches Wörterbuch zum Neuen Testament
TUAT	<i>Texte aus der Umwelt des Alten Testaments</i> , ed. O. KAISER, 1986
TWAT	Theologisches Wörterbuch zum Alten Testament
UF	Ugarit-Forschungen
UT	C.H. GORDON, <i>Ugaritic Textbook</i>
VT	Vetus Testamentum (Leiden)
WdO	Die Welt des Orients (Göttingen)
WMANT	Wissenschaftliche Monographien zum Alten und Neuen Testament
WTM	J. LEVY, <i>Neuhebräisches und Chaldäisches Wörterbuch über die Talmudim und Midraschim</i> , Leipzig 1876-'89
WVDOG	Wissenschaftliche Veröffentlichungen der Deutschen Orient-Gesellschaft
WZKM	Wiener Zeitschrift für die Kunde des Morgenlandes
ZA	Zeitschrift für Assyriologie und Vorderasiatische Archäologie
ZAW	Zeitschrift für die Alttestamentliche Wissenschaft
ZDMG	Zeitschrift der Deutschen Morgenländischen Gesellschaft
ZDPV	Zeitschrift des Deutschen Palästina-Vereins

# THE HISTORY OF THE

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INTRODUCTION AND ARCHAEOLOGY





## DEIR 'ALLA RE-VISITED

H.J. FRANKEN

In memoriam Ali ABDUL RASUL

This time thirty years ago I was busy preparing for the first excavation at tell Deir 'Alla. The first season took place from January to the end of March, 1960. The sixties was a time of enterprise and of great expectations for archaeologists working in the Near East. New insights were gained in how to solve chronological and cultural problems during and after the Jericho excavations in the fifties. New techniques of excavation had been introduced. One had opportunities galore to make a name for oneself as being the most progressive field archaeologist, at least in one's own eyes. It was still the time of the 'Einzelgänger,' who knew their job, the archaeology of the Near East, could read pottery and make typologies of everything found, and knew the dates, the history and the languages and ... the Bible where necessary. They were real leaders of the excavation teams and they discovered great things.

But the time had already passed when excavators published the results of their fieldwork within a few years after the excavations. Specialists began to work with excavations to analyse samples, who handed in their reports on time. But archaeologists had academic obligations. Consequently, no matter how much organisation supported the enterprise in the field, preliminary reports were most likely, and often even remain, the only tangible documents about the finds.

Meanwhile, another process in archaeological research was developing, which proved to be a serious obstacle for plans to publish the complete results of excavations. This process was one of constantly updating methods, rethinking archaeological procedures and introducing new possibilities and techniques to archaeological research. 'If only we had better samples of the soil and artefacts and could do some statistical work, the publications would be much better. Therefore we cannot publish.'

Today, few people seem to be aware of how fast things have changed, especially since the early seventies. It looks almost like two different worlds.

Rethinking Deir 'Alla in the sixties and my hopes for the work, I am embarrassed that the excavations of the Late Bronze Age settlement have not yet been fully published and that several other pieces of research I did have not yet seen the light. Nevertheless, one is indeed fortunate, after so many years, to be able to work on publications oneself in cooperation with competent young people, and together to think about the possible meaning of the excavated materials in the light of modern developments. There is also comfort in the possibility that the interpretation may be more to the point now than it would have been in 1964, when the excavation of the Late Bronze Age levels at Deir 'Alla took place.

The present generation of field archaeologists is probably not so much concerned about the methods of their predecessors as we were thirty years ago. But as modern archaeologists formulate the systematics of their research programs, they encounter unexpected and totally unforeseen hazards.

Accurate recording was the subject of innovation in the sixties and had to be promoted. Then it became common practice, and today every site supervisor on Near Eastern digs is trained to understand, draw and put on record all plans and sections as they appear during the daily work as a matter of routine. Moreover, bureaucracy has also turned up on excavations. Forms have to be filled in by all members of the excavation team. As a result, the excavator needs six months to process the reports produced during a two month excavation season before he or she can even begin to evaluate the season's work.

It is often maintained nowadays that the understanding of the material growth of a site depends entirely on the precision of recording and on the application of other modern methods. Mind you, we can no longer be certain that five times five is twenty-five unless this figure lights up on the screen of a pocket calculator. But the promise of accurate recording has not been fulfilled.

Furthermore, another panacea has crept into archaeology. It began rather innocently attempting to teach archaeologists in all areas to use 'the' right terminology. Good intentions however quickly deteriorated into sheer word-magic at best. Most often however it is nothing more than jargon that communicates nothing but itself. The jargon does not foster clear thinking. Rather it has created muddle-headedness. You belong to the 'in-crowd' if you talk complicated technical language while dealing with absolute trifles. If someone



talks models in historical archaeology, in nine out of ten cases he does not have a clue as to how to deal with his archaeological subject.

The present state of affairs gives no indication that archaeology will explain the archaeological situation itself in an historical and cultural perspective. The present state of archaeology does however indicate the possible future of archaeology. Namely, archaeology will end up promoting itself by showing what great things it is doing to update itself.

Archaeological research seems to be more concerned with better text books about how to excavate and more popular books about how archaeologists work. Furthermore, archaeology produces for itself a continuous stream of more and more complicated locus sheets for almost every expedition.

The situation is comparable to modern university management. Administrators are constantly finding new ways to update administrative techniques. Such techniques however have a point of diminishing returns. The concern with the accuracy of administration and the administration of accuracy absorbs far too much energy and time to be productive. But more important, it prohibits proper scientific research and curtails the inventive and unorthodox mind.

I no longer concern myself with problems of balancing creativity and invention on the one hand and accuracy, or probably rather would-be accuracy, on the other. In defence of the great pioneers in the field of Near Eastern Archaeology, I point to these false hopes of the present day as being a straightforward successor of false hopes that were cherished in the past by the lesser gods. Having played a part in this historic theatre, I shall not try to absolve or excuse myself.

Yet after all these years of development in excavation and processing techniques, Near Eastern Archaeology still lacks fixed and agreed upon rules for the interpretation of ruins. This situation will remain so long as new-fangled ideas are not brought into balance with the humanities, if not with intelligent thinking.

Today, twenty-two years after the texts were discovered I would like on this occasion afforded me to attempt a synthesis of the results of my excavations at the site, trying to separate the essentials from the accidental. Meanwhile my explanation of the ruins of Deir ʿAlla has to be seen in the perspective I have sketched above. I can assure you that I shall not interfere seriously with what the speakers of this conference have to say about their subjects. As an

introduction I would like to say something about how the text was found and what happened afterwards.

A story which has captured the imagination of the public has been recently circulated in announcements of the exhibition which will be opened this week in this museum. It runs something like this. 'On the day that the text was found the great Bileam was sound asleep while his donkey ambled along the King's Highway. All of a sudden the animal stopped and brayed: 'wake up, ya pasha, look what lies there, the aramaic text', the morale being exactly the same as the one in the story in Numbers: a donkey is more clever than the would-be prophet. (As a matter of fact, Prof. Diderik VAN DER WAALS, prehistorian from Groningen, had taken on the responsibilities of the work on the tell, because he was interested in tell stratigraphy and he wanted to get some experience in working in the Near East.)

The background of this anecdote cannot really be guessed from this Sinbad-the-Sailor tale. And it does not do justice to the donkey.

Who was this man who spotted the first bits of plaster text?

When I came for the third time to the Jericho excavations at the end of 1957, the dig had already started. But one project had been designed for me by Kathleen KENYON; I had to dig the north trench down to bedrock in one season. I was given as foreman, Ali ABDUL RASUL, 12 pickmen and 80 workmen to do the job. Miss KENYON took one more measure: she forbade tourist guides to take people to the north trench because what was going on there looked so much like a dig of the twenties, something like an ants nest. Between the two of us Ali and I organised the dig which was a full scale dig by itself. After two months I got assistance from a Dutch student. Ali and I became a very efficient set of managers. We not only shared the general organisation of the work of twelve pickmen but also the overall strategy of where to excavate and the study of the stratigraphy.

Consequently when I started excavating tell Deir 'Alla, Ali ABDUL RASUL was my right hand who could take my place on the tell at any moment. He knew my strategies so well that he would organise work when I had business for the day in Jerusalem or Amman. He was very lucid in his explanations of what was going on and he would tease me by saying the wrong things on purpose.

On the dig in 1967 we were both aware that unexpected develop-



ments could take place. Ali had of course been on many more field expeditions than I had. He had seen many situations which he could compare with Deir 'Alla. I did not have to tell him that the archaeological situation was something out of the ordinary. And in his quiet way Ali kept an eye on what was going on in the trenches. On the 16th of March I had been invited by Paul LAPP to meet some American visitors in Jerusalem. In the afternoon I had phoned him much to his disappointment that I could not leave the dig, since going up to Jerusalem at night meant coming back late the next day.

On the 17th at 8 o'clock in the morning Ali saw the first bit of plaster text being unearthed, stopped the pickman and went down to the camp to call me. We phoned Jerusalem and invited Paul LAPP to come down with Crystal BENNETT and Père Roland DE VAUX, Professor Martin NOTH, and in Amman Dr. Awni DAJANI, the Director of the Department of Antiquities with Gerald LANKESTER HARDING. It is very sad to think that they have all passed away.

The chief administrator of the Netherlands Organisation for the Advancement of Pure Research (ZWO) that financed the excavation was Mr. J.B.H. OTKER. Ben OTKER had taken a special interest in the Deir 'Alla excavation and accompanied the enterprise from the beginning. He had become friends with the people from Deir 'Alla, who remember him and still ask me whenever I visit the village, how he is. I needed infra-red photographic paper for the Linhoff camera to photograph the texts and Ben OTKER in the Hague put three people on the job to get the materials as soon as he received my telegram. No firm seemed to have the right material at that particular moment. Yet he managed to load a refrigerator for cooling films with the required paper, and he added a Leica with an almost complete set of extra lenses and filters and enough infra-red material. The refrigerator was installed on a first class seat of the plane, the only place where it could be connected to electric current. Once in Jordan it could be connected with the battery of the Landrover or run on almost any conceivable fuel.

I had to stop all digging except in the area of the text because I had to use the whole team to make sure that every single bit was not only rescued but also properly treated, and provisionally fixed in paraffin in metal trays which we had to make on the spot.

Then we stored the text in the Palestine Museum in Jerusalem where it was waiting to be packed for transport to Leiden to be expertly treated and restored. I was barely back in Leiden when the

June War broke out and the text was in occupied territory. While everybody was keeping quiet and waiting to see how things would develop politically, it was again Ben OTKER who volunteered to go and take the texts to Holland. Things were so uncertain that our own Ministry of Foreign Affairs did not want to know about this move. Ben went first to Amman to have the export licence confirmed, then to Jerusalem to the head of the Department of Antiquities, Dr. A. BIRAN. Then he went to the Museum where he packed the metal trays and contents in such a way that no damage could be done to them. He arrived at Schiphol airport with I think seventeen teaboxes filled with fragments and packing materials. Thanks to him we could immediately deliver the fragments to the Laboratories of the Rijksmuseum in Amsterdam, where conservationists could experiment with their treatment and conservation. The report of that work is published with the *editio princeps* of the texts.

To conclude the story, let me relate how the text was returned to Amman. Thanks again to the good care of ZWO, Ben OTKER had the texts mounted in the most beautiful wooden cases and flown to Amman in three large boxes. On the 1st of May 1972 the Director of ZWO, Mr. J.H. BANNIER and his co-director, Mr. H.G.A. KORTEWEG, were in Amman to hand over the texts officially to the Director of the Department of Antiquities, Mr. Jacoub OWEIS, with a short ceremony in the Jordan Intercontinental Hotel in Amman. One does not like to think how much money was spent by ZWO from the moment this text was found until it was returned to the Department of Antiquities in Amman, quite apart from the energy that went into the attempts to rescue and consolidate whatever had remained of the original text.

At the risk of being wrong one has to interpret ruins while excavating them, even if only in general terms. One cannot excavate in total ignorance of the archaeological situation. Each season provides fresh information. But information has to be processed to become intelligible. Hence, the results of further research supersede provisional interpretations and preliminary reports. The problem with such procedures is of course that the information that is first published, either by the excavator or by other reporters, has a kind of directness which is remembered more than the fruits of study and reflection which follow, or at least, should follow.

Thus there is the question of the destruction date of so-called phase M of the plaster texts. Some attempts in the past to work out



the evidence from the associated pottery failed. But at present this pottery is being studied by Miss Monique VILDERS.<sup>1</sup> She found, and I agree, that it would be very difficult to date this pottery later than the first half of the eighth century B.C. It could be earlier but not later.

Some problems of a more general nature have often occupied my mind. The problems are related. Why was there such a large sanctuary right through the Late Bronze Age, and what possible relation could it have had with biblical Succoth?

I have refused to accept the identification of the tell with Succoth, but not the identification of the Deir 'Alla district with the emeq or Valley of Succoth. A site like tell Deir 'Alla cannot be identified with any site mentioned in antiquity, unless one knows something about the nature of both. Biblical Succoth has no identity as a place in the Old Testament. Once it is called a town, probably mistakenly. But there is no archaeological town dating from those days that we know of in the area. From the Mount of Olives light structures, or booths, were probably visible in the Valley of Succoth and that is how the Valley of Succoth got its name. And there was a high mount right in the centre which you could clearly see at times, but in Jerusalem one did not talk about what went on there.

I am convinced that superficial identifications bar the way to a proper understanding of history. And this I will attempt to show.

Why was there such a large sanctuary right through the Late Bronze Age? It was not fenced in by a wall. It was not a sanctuary belonging to a city state because there are no traces of settlements of any size dating from the Late Bronze Age anywhere in that valley or its immediate surroundings. Why was the sanctuary with its auxiliary buildings so large? Why was it standing on an artificial platform more than six metres high on the north side where we discovered it? There was what the Arabic name says: a high deir. But why, who had built it, who had kept it up and what purpose did it serve, apart of course, from the most obvious answer in cases of sanctuaries.

At this point I have to include a short technical excursus, important to understanding the site.

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<sup>1</sup> This study will be published as an article called 'The stratigraphy and the pottery of Phase M at Deir 'Alla and the date of the destruction of the plaster texts.'

Out of our Deir 'Alla pottery stems a new approach to some of the archaeologist's problems with pottery. Part of this kind of research is that it enables one to sort potsherds into groups according to the different mineral inclusions added by the potters. When combined with other discrete technical features one can in most cases tell which pottery was locally made and which was not.

In this way regional pottery trade can be traced. For the Late Bronze Age it was previously possible to distinguish local pottery from imports from the Mycenaean world. It was not however possible to distinguish pots made in the Jordan Valley from pots produced, for instance, up in the mountains. Studying the non-plastic inclusions in the Late Bronze Age sherds two years ago, it became obvious to me that Late Bronze pottery travelled to Deir 'Alla from rather long distances, a journey of two or more days.

Local clays used in potting at Deir 'Alla have certain characteristics which may be found in more places, such as river deposits in the ancient Lissan lake. But what certainly was not available near the site and its surroundings is, for instance, basalt sand in combination with pure lime sand. For that one has to travel roughly forty km. to the north to find the nearest deposits.

Pottery tempered with fossiliferous lime sand may have come from the eastern mountains but may also have come from Late Bronze Age sites at the West Bank like Shechem. Pottery with shale comes from a different region. Right through the Late Bronze Age about 20% of the entire pottery repertoire came from elsewhere. Since we are dealing with a sanctuary, one is inclined to think that such pottery was brought in by people who had some business with the sanctuary. Having searched first in the immediate environment for a reason for the existence of the sanctuary, I found that its purpose has to be looked for in Gilead in its entirety, or even beyond.

This fact combined with other indications, not the least of which are the numerous imported objects from Egypt, made me decide that Deir 'Alla is best explained as having been—probably right through its long history—a sanctuary connected with trade. Products from Gilead were traded via Deir 'Alla to the Mediterranean coast and to Egypt. And trade was and always will be sacrosanct. But in need of heavenly protection.

Egypt clearly tried to keep contacts with Deir 'Alla even when it had been politically thrown back on its own borders, as it seems to have been in the days of queen Taousert, and during the early



twelfth century B.C. During the Late Bronze Age the trade may have been controlled by Egypt from Beisan via tell Sa'idiyeh with its rich Late Bronze occupation and via tell Mazar, as the place where functionaries from Deir 'Alla may have been living, beyond the reaches of the terrible eastern Deir 'Alla gales called the sher-qiye. This is also the route chosen by Sheshong I circa 925, who went up the Zerqa, following the trade to its sources. Another place connected with the Gilead trade via Deir 'Alla must have been Shechem.

Deir 'Alla was the place, or one of the places, where after the harvest of various materials the products of the mountain slopes and the table land above were collected, marketed and bought by agents of the big customers and shipped off by caravan. Gilead was a rich production area of all kinds of products. The export of these products must have been channeled in certain fixed ways so as to allow the political powers to control the flow of goods and the markets. Therefore the original layout of the sanctuary on top of its artificial hill may have been constructed under Egyptian supervision after the Hyksos had been expelled from the country.

What were these products and who were the cultivators or the producers? We have MITTMANN's survey of northern Jordan from 1970 and OTTOSSON's study *'Gilead, Tradition and History'* published in 1969. Little fresh information has been published since then.

MITTMANN, whose subject was Siedlungs- und Territorialgeschichte, identified Deir 'Alla with Succoth. According to his finds there is a rather strong increase of sites in the early Iron Age explained by population or tribal incursions from the West Bank and from the north. These newcomers are supposed to have cleared forest areas to make crop raising possible, in addition to herding flocks of sheep and goat. This process is usually seen by scholars in terms of ownership of the land and contrasting interests of the small kingdoms of the Aramaeans, the Ammonites or Israelites. MITTMANN follows this tradition of attempting to attribute the area of Succoth to one of the Israelite tribes that went across the Jordan from the west.

I have never seriously been concerned with this question. But I would like to make some suggestions at this point concerning the interpretation of Deir 'Alla as a trade sanctuary and about its position before and after this supposed influx of people from the west.

The first is that biblical texts mention products from Gilead which

come from trees and wild shrubs. It is difficult to identify some of those mentioned in antiquity with the ones known today. But gums, raisins, certain kinds of balsam and fragrant oils were crops exported by means of caravans. ZOHARI deals with a number of trees and shrubs which produce such aromatic gums and etheric oils, (*Plants of the Bible*, 1982). Thus ZOHARI mentions for instance: Storax tree (*Liquidambar orientalis* Miller), an aromatic gum, 6–10 m high, *Ricinus communis* (wondertree), medicinal oil, 4 m high, Henna (*Lawsonia inermis* L.), for dyeing, 4 m high, or shrubs such as: Ladanum (*Cistus incanus* L.), etheric oil, 0.70 m high, Tragant (*Astragalus gum-mifer* Labill), 0.50 m high.

Trees and shrubs like these were abundant on the western slopes of the Ajlun Mountains. But of course not only in the wadis on the western slopes of the mountains where archaeological surveys have been made, but also between these wadis on the slopes, where we don't look for, or know of, settled life. Modern travellers who have traversed the slopes have often commented on the large amount of bedouin tents they saw there. The more or less natural vegetation goes with sheep herding but is lost when people start clearing the trees from the land. The products of the forests and the maquis were exported, rather than the cereals or other crops raised from areas where forests had been cleared. And the products were collected by shepherds and bushmen, not by farmers.

My second remark concerns the direction of the culture. Was it from the west like one would expect, if Israelite clans settled there? Or was it from the east? And if so, how can one explain this? I am convinced that everything excavated at the site of Deir 'Alla came from the east and not from the west. As far as one can speak of a cultural identity of the site, Deir 'Alla is an Ammonite site in every respect. This does not necessarily contradict the notion that tribes crossed the Jordan in an easterly direction.

And so, the third remark concerns the fact that there is almost as a rule no agreement between literary sources and archaeological finds. This has been beautifully worked out by Hans Jürgen EGGERS, who coined the phrase:

'Archäologische These, literarische Antithese, historische Synthese' in his *Einführung in die Vorgeschichte* (München, 1959).

EGGERS even wrote an 'Archäologische Quellenkritik'. Why have archaeologists working in the Near East not taken note of his book? Because of the title (*Vorgeschichte*) and because they are used to



explain things 'in the light of', or worse, by 'dovetailing' their finds into historically accepted situations. What dovetailing does is combining two sets of information which are of a totally different nature and value.

By its very nature, historical reality must have been far more complicated than can be deduced from archaeological finds. On the other hand, there is no archaeological reason as far as I can see that the site was taken over by Israelite tribes at the beginning of the Iron Age. At least as long as archaeology in the area identifies people from the material culture. If we accept that in the case of defining Israelite culture, then the site never was in the hands of an Israelite tribe.

The fourth remark concerns the search for 'Succoth' or huts. It would be rather difficult to try and locate some of the 'succoth' around the tell. We know for instance that the Iron Age remains at the north and west side of the tell are 4 m below the present surface. To the east in the valley somewhat higher areas made up by pleistocene clay deposits are denuded and lower ones are filled up with eroded materials from the slopes that surrounded the valley on three sides. One may expect that temporary or seasonal buildings are either deeply buried in wash or long since eroded away. During a recent survey a pocket of Late Bronze sherds was found close to and east of Deir 'Alla.

The fifth remark is that Succoth does not have to be a name which was locally used in antiquity. This would mean that the valley was indicated in the Old Testament by the 'huts' as land marks, whereas locally the site may have been named for a deity like Shr'a and known as such by farmers and trade people who came to the site.

The possibility that the name Succoth was not the local name, made me wonder whether something similar might have caused the change of the name Succoth into tr'la as the Talmud states. Deir 'Alla may be taken as a corruption of the Hebrew. The habit of calling the site tr'la may date from a much earlier time and indeed may have been taken from prophetic texts such as Ps. lx:

'Thou didst shake the land, didst cleave it:  
Its breach doth sink down, it doth totter.  
Thou hast let Thy people see hard things:  
Thou hast made us drink wine of staggering'

and: 'Yahweh spake in His Sanctuary:

"I will exult, I will divide Shechem:  
 "And the Valley of Succoth will I mete out.  
 "Gilead is Mine, and Mine is Manasseh:"

(BRIGGS, ICC, 1925)

On the one hand people are horrified by what is described as an earthquake. On the other hand however there is Yahweh's triumphant claim on Shechem and the Valley of Succoth.

Some explanations are possible: the place where people were reeling from a blow, caused by an earthquake, was associated with the non-Israelite sanctuary of Deir 'Alla when it was destroyed by earthquake.

Or, tr<sup>c</sup>la replaced Succoth at an early stage because the valley became known as the place where people became drunk when feasting in the sanctuary after successful dealings.

And thirdly, if after the fall of Jerusalem in 70 A.D., or after the second revolt, a Jewish community lived in the area as exiles, they may have coined the name from Ps. LX, applying its prophecy to their own situation. But the tell was certainly not inhabited in those days.

Where do I get this from? When I occasionally look up something which is connected with Hebrew texts and Hebrew grammar, I look it up in the 'Jubelaufage' of GESENIUS *Hebräischer Grammatik*, published in Halle, Oktober 1889 by E. KAUTZSCH. There I found this explanation of the word tr<sup>c</sup>la.

Tell Deir 'Alla was uninhabited since the 5th or 4th century B.C. but there were large farmsteads since Roman times at various places in the valley. From then on there was a continuous habitation in the area until the 16th century A.D.

My suggestion is that both names were not the names which were used by the locals. Tr<sup>c</sup>la would have indicated the valley and not the tell unless the name was already used while the site was still inhabited. But regardless of when it happened, the change of the name may indeed also have been inspired by a living memory of the rituals that accompanied the trade in and around the sanctuary in the days when the influence of the Jordanian prophet, or seeër Balaam, was manifest. Because Balaam was never forgotten.

It is clear that the Valley of Succoth was the scenery of inter-regional trade, which in the Late Bronze Age and the beginning of the Iron Age was largely controlled by Egypt. And if the products that were traded there did not change, the people who produced the



goods, the bushmen, and the ones who transported them, the Midianites or the Ismaelites, did not change either, no matter which Israelite tribe came across the Jordan from the West Bank. That is why in the 8th century B.C. we find religious concepts in agreement with the international character of the trade. Research nowadays should not in the first place ask which tribe owned the site but address such questions like who controlled the trade and supervised the trade-routes at various periods. The site itself represented the religious centre of a complex of activities that needed lots of space in the valley like every market site. This probably lasted for more than a thousand years.

## THE ARCHAEOLOGY OF DEIR 'ALLA PHASE IX\*

Moawiyah M. IBRAHIM, Gerrit VAN DER KOOIJ

Archaeology has a considerable impact on the understanding of the Deir 'Alla plaster texts. Apart from the immediate archaeological situation of the inscribed plaster fragments (see p. 239, below) several other archaeological subjects have been dealt with or touched upon in connection with these texts:

1. The character of the settlement of phase IX (M) in general, and the question whether a cultic place is connected with the texts;
2. The identity of the culture of Phase IX in relation to neighbouring regional cultures, both nearby and further afield (Ammon, Israel, Judah, Aram-Damascus and the Phoenician coast);
3. The dating of the settlement/culture of Phase IX, and its destruction. Several of these subjects, for example, were touched upon during the International Congress on Biblical Archaeology in Jerusalem in 1984, Epigraphic Session (BIRAN, ed., 1985), especially by B. LEVINE, but also by A. LEMAIRE (cf. too his 1985), and in the discussion, in particular by J. BALENSI (BIRAN ed., 1985, p. 368).

The subjects have partly been dealt with by H.J. FRANKEN, 1976 (*editio princeps*) as well as by M. MARTIN (1976), based on the excavations of "Phase M" in 1967 (c. 300 m<sup>2</sup> was exposed then, namely squares B/C-E 2-6, excluding B/C6) and on preliminary studies of the materials. A third study based on the 1967 dig, but including analyses of some of the pottery concerned, is to be published soon by M. VILDERS.

The excavations at Deir 'Alla were resumed in 1976 by a joint expedition of the Department of Antiquities in Amman, Leiden University, as well as Yarmouk University in Irbid, since 1980. Main preliminary reports have been published in *ADAJ* (FRANKEN, IBRAHIM, 1978, IBRAHIM, VAN DER KOOIJ, 1979, 1983, 1986). The stratum called Phase M has been labelled Phase IX in these reports.

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\* A major part of this paper had been prepared, but not read, at the symposium. However, most of the issues dealt with in it played a role in the discussions at the archaeological session, so it was thought necessary to include them in the proceedings. On the other hand, the part that was presented at the symposium is communicated here in a short version.

The continuation of the excavations of Phase IX occurred in a minor way in 1976 (B/C5, the plaster text area), 1979 (B/C6) and 1982 (B/C8), but the main work was in 1984 and 1987.

Quite a bit of the archaeological information from Phase IX was included in the temporary exhibition about the Deir 'Alla project in the National Museum of Antiquities in Leiden and accordingly published in the accompanying book (VAN DER KOOIJ, IBRAHIM, ed. 1989).

The study of most of the material remains is still in its initial stages. This means that the subjects referred to can only be dealt with provisionally here.

### *1. The character of the settlement*

It is possible to study the character of the settlement rather well. Although only a small part of the remains has been uncovered (c. 800 m<sup>2</sup>, probably about 1/3 of what is left of the settlement) the quality of the remains is relatively good, for two reasons:

- The settlement had been destroyed suddenly, accompanied by fire at many places. This was apparently caused by an earthquake, as was concluded in 1967 from long cracks found immediately below the debris (FRANKEN, 1976, pp. 7f.)

- The debris, had been relatively little affected by erosion and pit digging of later inhabitants except for the part in the E. squares. There erosion and egalisation for Phase VI has almost completely removed the remains of Phase IX.

### *The stratigraphy*

The total process of building up, use, modification and destruction of the phase is rather complicated. At many places the walls have been rebuilt and in several rooms walls have been added and removed, doorways closed and roofs fallen in. Also the final destruction went in stages. At first the roofs came down and parts of walls. Then the other parts of walls collapsed by a second earthshock, probably, and by man levelling the ruins. This second stage occurred after some time, because the new surface had been used a bit. The first destruction apparently was sudden enough for the inhabitants to leave probably all their chattels behind, but remains of victims caught by the collapse have not been found.



*The architecture*

The excavated architectural complex shows a series of small rooms (Fig. 1). Most of them had been roofed, some by a reed mat only, but a few courtyards (some having one or more bread ovens) were open or only partially covered. The yard floors were often originally cobbled, but mud had washed over them and this was covered with reed layers, apparently during the rainy seasons. All the walls were made of mud-brick (size  $46 \times 32 \times 11$  cm). No stone foundation was used, but only reed layers to build the walls on. Most of the walls were as wide as one brick's length, some of one brick's width, and a few both length and width wide. It is difficult to combine rooms into larger units, because very often doorways are not clear: many of the walls do not have indications of expected door openings. Apparently the thresholds were high and constructed of mud bricks. In one case (the room in square B/B4, mainly used for storage) all the walls had been preserved 1 m high, but there were no doorways. Clearly the room had to be entered via the mud brick steps found at both sides of the W wall.

*Use of space*

The excavated architectural complex consists of about 40 rooms, including the unroofed ones. The contents of the rooms indicate storage and work facilities. In fact altogether 15 groups of loom weights have been found in them, each comprising more than 15 pieces, probably representing one vertical loom. One, or perhaps two of these groups were found in an arrangement that reflects their use. Apart from that, also about 15 large groups of varied pottery were found. They generally include storage jars (often filled with wheat or barley), small jars and jugs, craters, and sometimes also one or more cooking pots, dishes and sieve-spouted jugs. Rarely a lamp and a "sieve bowl" on three legs are found with them. These two groups of finds may indicate about 15 separate households, each with food storing and food serving facilities as well as weaving equipment.

About 10 upper grinding stones were found in a functional context in different rooms, but no corresponding large lower quern. This may mean that one or more of these lower stones were placed at a central location. A central baking or cooking area was used too,



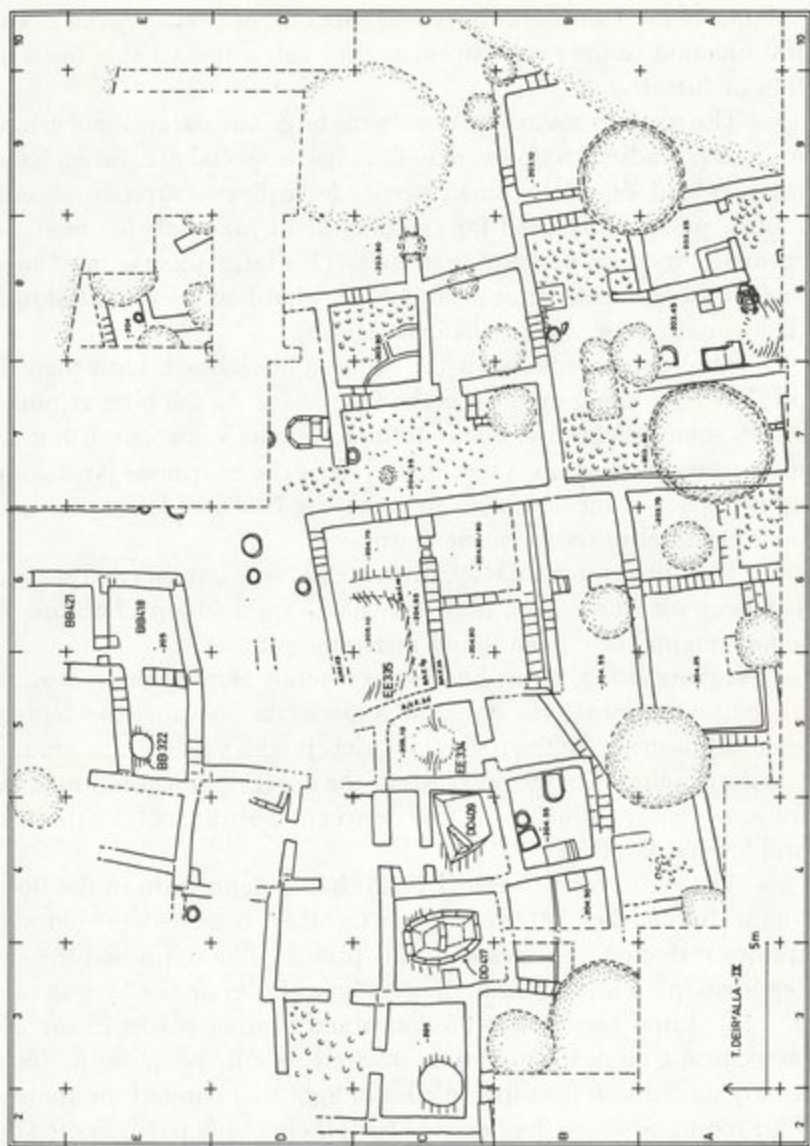


Fig. 1. Top plan of the recovered architectural remains of the last stage of Phase IX (Area B). Dotted lines refer to later disturbances (pits, erosion, egalisation).

where the people went with their dough and cooking pot as well as their fuel (dung and threshing remains), which was found stored in many of the rooms.

Some of the rooms may have had a special, or perhaps even a central function in the community, as they had a remarkable installation or furnishing.

- The room in square B/A8 with the large and exceptional grinding and pounding complex, may have had a special use. Small hand mortars and especially small pestles have been found in several rooms, presumably used for grinding plant materials for food, or cosmetic, medical or paint materials. This large mortar may have had a different use, but it is not known what had been ground in it (it was not in use during the destruction).<sup>1</sup>

- The room in squares B/C3-4 has a brick lined, bath shaped, pit (DD417) taking up most of the floor space. In the pit a grinding stone, some pestles and about 10 loomweights were found, but no further data to indicate a special use. Only the enormous jar, taking up the space of the complete alcove to the NW in a lying position, is exceptional in the unburnt room.

- The burnt room DD409, further east, with a trapezium shaped 1 m deep pit with a step, may have had a special loom, because 30 loom weights have been found inside the pit.

- In square B/C5, room EE334 has a gently sloping shallow pit in its centre, but it may not have had a special use, because the depression had its origin in the underlying debris with wall stumps around standing higher. On the other hand, the depression was maintained for some reason—during the first destruction burnt roof debris filled and leveled the floor.

- The room further east (EE335) had a depression in the floor too, with "benches" at two sides. In fact these benches were old wall stumps reshaped at places with clay plaster. The room had the exceptional ink written Balaam text on lime plaster on the W wall (see p. 241). Lime plaster was used at about 5 other places in the architectural complex but with no obvious specific purpose; perhaps it only served to reflect the little bit of light that entered the rooms. This room, however, had enough light (being only partly roofed by reed matting). The religious contents of the illustrated text written on the plastered wall obviously gave a religious meaning or function to the room, but nothing of a definitely cultic character has been found inside the room or in the vicinity.<sup>2</sup> One may only point at the

<sup>1</sup> The room has been extensively described in the writers, 1989, pp. 82-86.

<sup>2</sup> A "hand-pipe" (made of serpentinite) was found in the room to the south.



fact, that some sanctuary rooms have benches along the walls to put objects on, but only the southern bench would be suitable for this and no objects were found. Thin layers of plant matter had accumulated on the floor of the room itself and some trodden sherds were found there, as well as a lamp near the NW corner. So the room was "empty"<sup>3</sup>; perhaps it included a place to sleep, to obtain a vision! Unfortunately the W and NW part of the architecture are not completely clear, so it is not certain in which way the room was connected with those parts. A doorway originally existed to the SE, but it was blocked in a secondary phase, before the final destruction. The space to the NE, with a special mud brick built structure, as well as a small oven, is not yet completely excavated and understood.

The kind of religious space we are dealing with here cannot yet be compared with rooms found elsewhere, including those at Kuntillet Ajrud (MESHEL, 1978).

— Two other rooms have been considered as having a special use, namely rooms BB421 and BB418 in square B/E5–6 (see *ed. pr.* p. 15). The southern one had a jar with a short inscription, the one to the north had a stone with a short text. Both texts have the word *šr'*, preceded by respectively *zy* and *'bn* ("of *š.*" and "stone of *š.*"). The word has to be taken as a name (HOFTIJZER, *ed. pr.*, p. 274); probably a geographical name or perhaps a personal one. A deity's name has been considered because of a religious interpretation of the stone, suggested by a shiny surface. An exceptional type of goblet (fig. 2d) found in these rooms and a large pierced conical weight interpreted as an outsize loomweight (see *ed. pr.* Pl. 16b) may be further evidence. For that reason FRANKEN thinks of a cult connected with weaving. This would mean a house or workshop cult. On the other hand, the shape of the conical weight is unlike that of the loomweights used at so many places in the settlement. The stone, which is a bit shiny almost all over the surface, may be interpreted as a weight (the specific mention of "stone" may indicate this). Taking this line the jar could be taken as a measure too. With this interpre-

(ADA/ XXII, 1977–78, Pl. 28, and the writers, 1989 object no. 98) These objects have been associated with incense and ointments, but in fact their use is uncertain.

<sup>3</sup> It is unlikely that the inhabitants removed objects from the room during the destruction, since this clearance did not happen at any other place. Moreover, no objects can have been removed after the destruction, because the debris on top of the floor had not been disturbed in antiquity (see the section drawing in ADA/ XXII, 1977–78, p. 66, deposit B/C5.57).

tation the rooms would rather have to be connected with trade, as well as with household activities, judging from the pottery repertoire (though the goblet remains exceptional) and the loomweights found.

*General character of the culture*

The cultural character of the settlement may be concisely and preliminarily described as follows.

The population exploited the immediate surroundings. The yellow clay was taken from the nearby Lisan banded clay beds and used for most of the bricks, wall and roof plaster, as well as loom weights. Stones were presumably taken from the wadi Zerqa and used unworked for floors (mainly courtyards). Pebbles were used as tools, for example, for whetting and polishing (plaster surfaces).

Animal bones, especially those of sheep and goat, as well as antlers of different types of deer (*cervus*, but mainly *dama mesopotamica*), were used for different kinds of tools (cf. the writers, 1989, nos. 79-93, and CLASON, BUITENHUIS, 1989). Sheep and goats were kept and herded. They were used not only for meat and wool, but also for milk; some of the pottery vessels found probably have to be connected with the processing of milk.

The agricultural soil around was irrigated, at least partly. The use of Zerqa water does not demand a very complicated canal system to create an oasis in the steppe lands (A very rich variety of plant remains was found; see VAN ZEIST, HEERES, 1973, and especially NEEF, 1989).

It can be inferred from comparative data, that the agricultural settlement in the steppe region was of significance for herding nomads living there seasonally. The usual exchange of goods can be presupposed; and it is possible that woven products played a specific role in this.

With the data available it is not necessary to interpret the large number of looms (see above) as an indication for a craft centre at Deir 'Alla with a more than local significance. The fact that at a random moment (the time of destruction) only one or two of the looms were in use<sup>4</sup>, rather suggests that weaving was practised oc-

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<sup>4</sup> The loomweights in the NE room of square B/A6 and probably also those in B/A7 were lying in such an arrangement that it can be interpreted that they had fallen from a burning warp-weighted loom. It is interesting to mention here, that



casionally only, perhaps limited to a household use. On the other hand it is possible that weaving was seasonally conditioned and practised much more at one moment than another.

The building complex was rather flimsily constructed, but the furnishing of the rooms was often rather rich. Sometimes with lime plaster on one wall (or rather part of a wall), often a lot of ceramics were available, including imported ware (with contents, see below). Some bone inlay panels were found in the room of square B/A8, belonging to wooden furniture or a box; well shaped pestles were in use and a small decorative basalt tripod mortar, etc. (see for illustrations the preliminary reports and these writers, 1989). The evidence from the room of the plaster text does not indicate a cultic centre of the settlement, but allows for the reconstruction of another kind of religious centre, not yet archaeologically known.

## *2. Relations to other sites and regions*

A full description of the identity of the culture of Phase IX in diachronic and synchronic relation to other ones is not yet possible. Many of the comparative and interpretative studies still have to be accomplished. A full understanding of the kind of settlement, architecture, economy, as well as the use and the artifactual background of the different groups of smaller artifacts will only be possible at a later stage of research.

In this section we will

- a. refer to the regional situation, and
- b. compare typologically some of the artifacts with those from surrounding regions in order to understand artifactual relations.

### *a. Settlements in the region*

Ecologically the lower middle part of the Jordan Valley is a steppe, with some natural oases caused by brooks like the Nahr ez-Zerqa, Wadi Rajeb, Wadi Kufrinji and a few springs. The cultural history as known from surveys, a few excavations and ethnohistory indicates an alternating use of the area stressing either agriculture or herding, something which is reflected in the character or use of settlement

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small carbonised pieces of cloth were preserved here and that an analysis of the thread shows that it was made of hemp (not wool or linen).

sites. The East Jordan Valley Survey, conducted in 1975 and 1976 by M.M. IBRAHIM, J. SAUER and Kh. YASSINE,<sup>5</sup> makes it clear that the Zerqa river is the southern most border of the inhabitable part of the Jordan Valley, except for the eastern parts of the wadi areas further south (from Shune onwards). The survey also suggests a quantitative fluctuation in site use, including a resettlement of the Wadi Kufrinji—Zerqa region, in the later Iron I period, with a more or less continuing occupation, mainly of the same sites, through the Iron II period, but only a few inhabited sites in the Persian period. Three of the conspicuous sites in the region have been more or less extensively excavated, e.g. (from N-S) Tell es-Saidiyeh, Tell el-Mazar and Tell Deir 'Alla, making it possible to compare the cultural assemblages more closely.

However, at Mazar no settlement contemporary with Deir 'Alla Phase IX has been touched upon yet, judging from the cultural material published (YASSINE, 1983). Mazar Phase V seems rather be related to Deir 'Alla Phase VI. On the other hand the excavations at Tell es-Saidiyeh revealed settlements with pottery assemblages comparable to that of Deir 'Alla Phase IX. The publications of this material suggest Deir 'Alla IX connections with Stratum VII, but it seems that comparable material is also found in Stratum VI and IX (PRITCHARD, 1985 and TUBB, 1988). A much closer comparative study, qualitatively as well as quantitatively is needed to be more precise.

#### *b. Other regions*

Looking beyond the local region it is clear, that some of the pottery traditions, represented at Deir 'Alla Phase IX (locally made FRANKEN, 1976, p. 11), are also found elsewhere to the east of the Jordan (e.g. at Pella, for the older types), as well as to the west. The storage jar types of Phase IX for example are frequently found there, especially in the N (e.g. Tell el-Fara, Stratum VIIId; Hazor, Stratum VI, but also Strata VII and VIII; Samaria Strata III and IV) as well as at the short lived site of Kuntillet 'Ajrud.<sup>6</sup> This is also

<sup>5</sup> The survey is published preliminarily: Part I in *BASOR* 222, 1976, pp. 41-66 and parts I & II in YASSINE, 1988, pp. 159-207.

<sup>6</sup> An archaeometric study of the pottery from Kuntillet 'Ajrud (using neutron activation analyses: J. GUNNEWEG, I. PERLMAN, Z. MESHEL, 1985, pp. 278-280) indicates a provenience of the storage jars from the "southern coastal region", more specifically Ashdod.



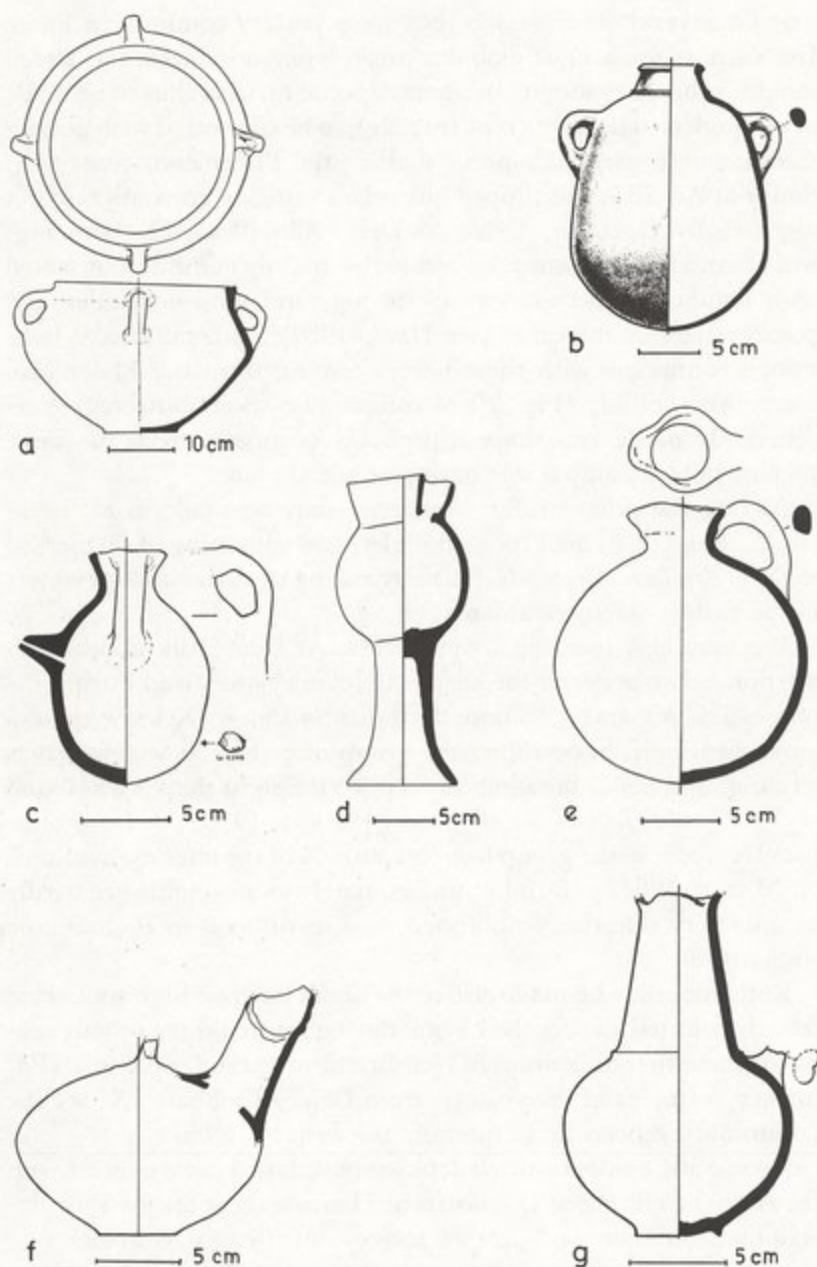


Fig. 2. Some characteristic and exceptional pottery shapes from Phase IX (not to the same scale). a. crater, reg. no. 3011; b. storage jar, reg. no. 2844; c. spouted juglet, containing shells, reg. no. 3088; d. goblet, reg. no. 1990; e. jug, reg. no. 3186; f. "jug" with large spout, reg. no. 3087; g. jug, red slipped and burnished, containing cummin, reg. no. 2975. (drawings H. de Reede and A.J. Cool, no.d.).



true for several other vessels, but some pottery common in Phase IX, such as the almost globular juglet with short neck and trefoil mouth, is hardly spotted elsewhere. Special mention has to be made of two juglets of the same type (Fig. 2g), to be connected with pottery traditions represented especially along the Phoenician coast (e.g. tombs at Achzib), red slipped burnished ware, so conveniently put together by CULIGAN, 1982. At Deir 'Alla Phase IX both jugs were found filled with carbonised herbs, mainly cummin but mixed with fenugreek and grapes in one jug, and with coriander and pomegranate in the other (see NEEF, 1989); both may have been traded containers with these herbs, coming from the Phoenician coast. Another jug (Fig. 2f) of comparable ware, but completely differently made and shaped, probably originates from the same region, but no comparable examples are known.

Among the other artifacts reference may be made to a "necklace", found in a small spouted juglet, and consisting of 60 pierced shells of *Arcularius Gibbosulus* (L.) originating from the shallow waters of the eastern Mediterranean.<sup>7</sup>

We may also mention loomweights. At Deir 'Alla a clear distinction exists between the shapes of loomweights used during the phases IX, VI and V. Those used during Phase IX show quite a variety in itself, also within one group of c. 15–30 weights. It is striking, that about the same variety of 5 different shapes was found at Tell Qasile (B. MAISLER/MAZAR, 1950/51, Pl. 39, 3, from Stratum IX; see also the group from Stratum X of the later excavations, A. MAZAR, 1985, p. 80), but unfortunately loomweights are hardly or only very selectively published, so it is difficult to evaluate the similarities.

Reference may be made also to the basalt bowl on high connected feet, distributed all over the Levant during the first three or four centuries of the first millenium BC (cf. BUCHHOLZ, 1963, pp. 59f.). (For further examples of the artifacts from Deir 'Alla Phase IX, see the preliminary reports and especially the writers, 1989).

No specific evidence is available to postulate a close contact with the Aramaic culture at Damascus or Hama, except for the short inscriptions on stone and jar (see above) classified as Aramaic.

It may be useful to add here that the pottery culture of the later

<sup>7</sup> See fig. 2c and the writers, 1989, no. 47 for the juglet and no. 93 for the shells.

Deir <sup>6</sup>Alla Phases VI and V has close relations to the so-called Ammonite assemblages from e.g. the tombs in Amman, Sahab and Meqabelein (Deir <sup>6</sup>Alla phases V and IV), as well as to those of the Iron Age settlements of Sahab, Area B (IBRAHIM, 1975, pp. 70-74) and Tell Safut (12 km NW of Amman; see WIMMER, 1987, especially pp. 166-172 for the Iron IIc and Persian material).

### 3. Dating Phase IX

Dating the remains of Deir <sup>6</sup>Alla Phase IX is preliminarily being accomplished by two methods:

- a. cultural stratigraphy and comparison;
- b. <sup>14</sup>C analysis.

a. *Comparative studies* of cultural assemblages from different sites cannot yet offer a very precise date, for two reasons. Well established artifact types (e.g. of pottery) usually appear not only in one, but rather in two or more successive strata/phases of a site (supposing a correct archaeological stratigraphy is established), so for a precise comparison frequency studies have to be included and relevant factors, such as the possibilities of cultural contact, have to be evaluated. This information for other sites is hardly available. The second reason is the margin for absolute dates of different strata. For many Iron Age strata absolute dates have been proposed, often based on textual information only, but often alternative dates are possible.

This means for our subject, that the cultural relations of Deir <sup>6</sup>Alla IX with other sites in the region and further afield, as discussed briefly above, indicate the 9th and 8th century BC. In any case before any Assyrian cultural influence is visible. This influence, however, may have started decades before the actual military-political incorporation of the region into the Assyrian empire (from c. 730 BC), but at some places it may also have become visible only decades after the Assyrian conquest.

On the other hand a *terminus ante quem* is given by the cultural identity of Deir <sup>6</sup>Alla Phase VI, which has a close connection with Ammonite sites referred to above, to be dated in the 7th century or perhaps the end of the 8th century BC.

b. Some *Carbon-14* analyses have been done with carbonized plant remains (grain and leaves) from the final destruction of Phase IX.

All three point to a time between 770 and 880 BC, with a high



probability of the date being at the end of the 9th century BC. (see Mook, 1989).

A carbon-14 analysis of a sample from an earlier collapse of Phase IX gives a century older result; one from the preceding phase, two centuries older. Two carbon-14 dates from Phase VI point to the second half of the 8th century BC.<sup>8</sup>

### Conclusion

A date for the destruction of Phase IX has to be looked for in the 9th and 8th centuries BC, but the statistic probability lies around 800 BC, and the last quarter of the 8th century is not really possible. Comparative cultural stratigraphy cannot yet add much to this. The date of c.760 BC for the destruction, suggested by an identification (LEMAIRE, 1985, p. 272) of the destructive earthquake with the one mentioned in the Old Testament for that time (e.g. Amos 1:1) is quite possible, but another earthquake may as well have been responsible for the destruction in this earthquake-rich region.

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<sup>8</sup> Phases VIII and VII are only locally preserved and existed for a short period only (certainly Phase VIII).

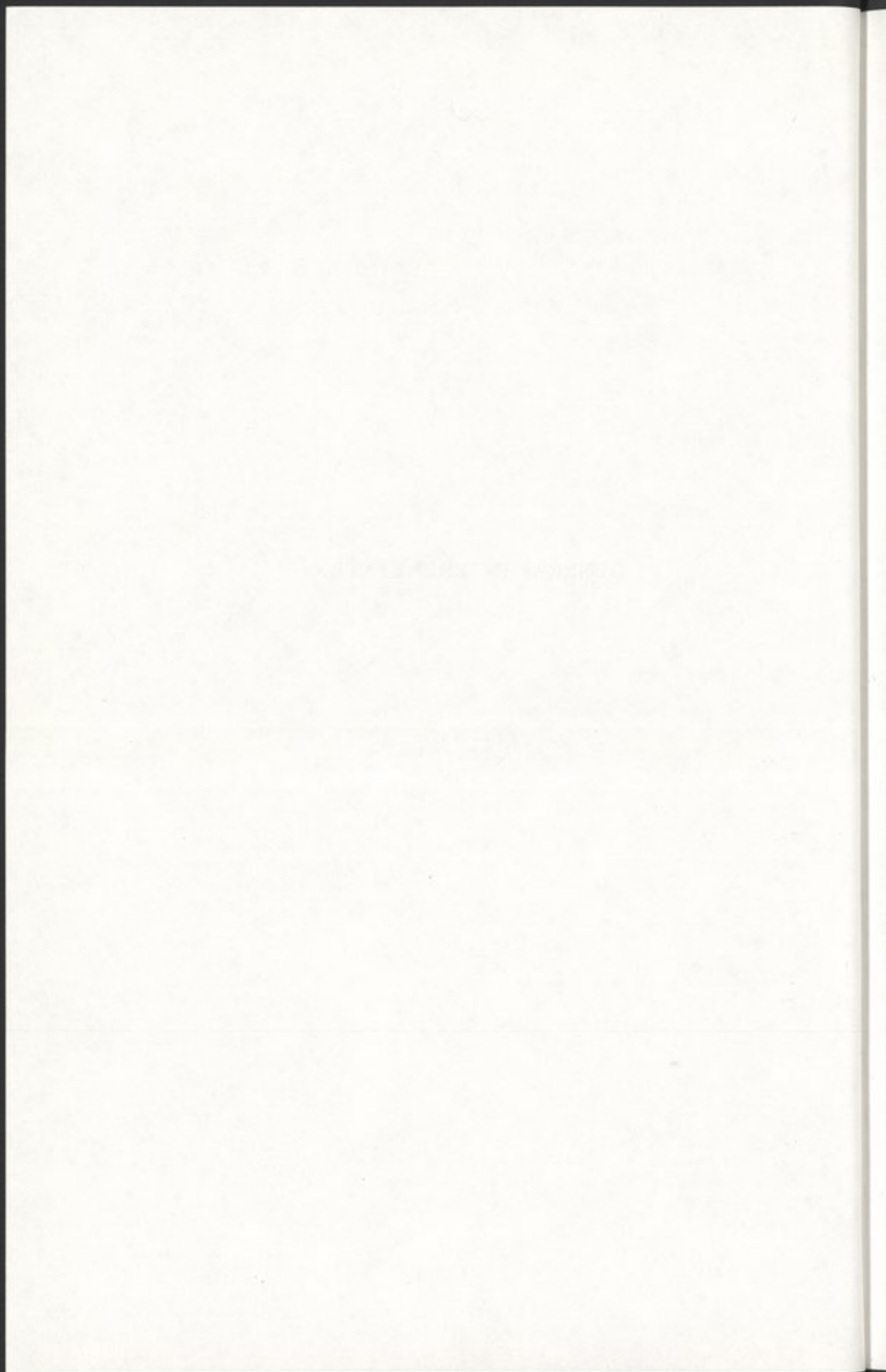


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## GENERAL INTERPRETATION





## LES INSCRIPTIONS SUR PLÂTRE DE DEIR 'ALLA ET LEUR SIGNIFICATION HISTORIQUE ET CULTURELLE

André LEMAIRE

La bibliographie des études sur les inscriptions sur plâtre de Deir 'Alla ne cesse de s'allonger<sup>1</sup> sans que l'on ne voie apparaître un début de consensus quant à la date, à l'écriture, à la langue et à la lecture de nombreux mots de ces inscriptions, même s'il y a eu quelques améliorations de lecture et de placement après l'*editio princeps*<sup>2</sup>. Bien plus, la relation entre le groupement I et le groupement II reste très incertaine. Enfin, alors qu'il semble possible de restituer quelques lignes complètes au début du groupement I, ce n'est malheureusement toujours pas le cas pour le groupement II malgré la longueur conservée de plusieurs débuts de ligne.

A ces difficultés, divergences et incertitudes concernant les in-

<sup>1</sup> Cf. en annexe, des indications bibliographiques complémentaires à W.E. AUFRECHT, *A Bibliography of the Deir 'Alla Plaster Texts*, *Newsletter for Targumic and Cognate Studies*, Lethbridge, September 1986, 8 p.

<sup>2</sup> Cf. surtout les propositions de: A. CAQUOT - A. LEMAIRE, "Les textes araméens de Deir 'Alla", *Syria* 54, 1977, pp. 189-208, spéc. p. 193; P.K. McCARTER, "The Balaam Texts from Deir 'Allā: The First Combination", *BA-SOR* 239, 1980, pp. 49-60, spéc. p. 51; H. et M. WEIPPERT, "Die 'Bileam'-Inscript von Tell Deir 'Allā", *ZDPV* 98, 1982, pp. 77-103, spéc. pp. 81-82; J.A. HACKETT, *The Balaam Text from Deir 'Allā*, HSM 31, Chico, 1984; A. LEMAIRE, "Les inscriptions de Deir 'Alla et la littérature araméenne antique", *CRAI* 1985, pp. 270-285, spéc. pp. 277-279; *id.*, "L'inscription de Balaam trouvée à Deir 'Alla: épigraphie", dans J. AMITAI, éd., *Biblical Archaeology Today, Proceedings of the International Congress on Biblical Archaeology, 1-10 April 1984 (BAT)*, Jérusalem, 1985, pp. 313-325, spéc. pp. 315-319; E. PUECH, "L'inscription sur plâtre de Deir 'Alla", *ibidem*, pp. 354-365, spéc. pp. 358-360; *id.*, "Le texte 'ammonite' de Deir 'Alla: les admonitions de Balaam (première partie)", dans *La Vie de la Parole, De l'Ancien au Nouveau Testament, Etudes ... offertes à P. Grelot*, Paris, 1987, pp. 13-30, spéc. pp. 15-17. Il va sans dire que ces propositions restent parfois très incertaines, voire contradictoires. Ainsi, après vérification de l'original, avons-nous abandonné, en *CRAI* 1985, pp. 278-279, la proposition de placer le fragment IIIa, écrit à l'encre rouge, au début de la ligne 1 (*BAT*, pp. 317-319) car on y lit clairement un *ʔ* et non un *r*. De même, nous accepterions volontiers aujourd'hui, à cause du parallélisme probable, de restituer *šd/yn*... au lieu de *šg/yn lhyh?* (*CRAI* 1985, pp. 278-280) vers le milieu de la ligne 5 (cf. A. WOLTERS *infra*). Par contre, après nouvel examen des originaux à Leiden, il nous semble toujours que, paléographiquement, la trace de la troisième lettre du début de la ligne 2 convient mieux à un *l* qu'à un *ʔ* et qu'il vaut mieux lire *nšš* et *šrh* à la ligne 8 (au lieu de *nšš* et *šrh*).

scriptions proprement dites, s'ajoute le fait que leur contexte archéologique, la phase M/IX de Deir 'Alla n'a fait l'objet que de la publication de rapports préliminaires, dont certains, il est vrai, sont assez développés. Or il est possible que la publication définitive de cette phase M/IX éclaire le problème de la datation et celui de l'identification régionale éventuelle de la culture matérielle: araméenne, ammonite, israélite ou "galaadite"?

Nombre de ces points s'éclaireront peut-être lors de ce symposium cependant il peut paraître, pour le moins, prématuré de le commencer en proposant une interprétation générale des inscriptions alors qu'on ne peut, apparemment, s'appuyer sur aucun consensus minimal. C'est cependant ce que nous essaierons de faire, non seulement en tenant compte des recherches publiées et de nos propres recherches antérieures, mais aussi et surtout en utilisant plusieurs approches différentes qui pourront dégager certaines convergences.

#### I - DATATION DE LA PHASE M/IX DE DEIR 'ALLA

Les datations au Carbone 14 de la phase M/IX de Deir 'Alla publiées jusqu'ici semblent indiquer une date vers 800 av. J.-C.<sup>3</sup>, corroborée par les datations au Carbone 14 des niveaux inférieurs et supérieurs<sup>4</sup>, et il semble que les archéologues tendent maintenant à dater cette phase M/IX soit dans le courant du VIII<sup>e</sup> s. av. J.-C.<sup>5</sup>, soit, plutôt, vers 800 av. J.-C.<sup>6</sup>. Cette datation archéologique reste encore, bien sûr, assez approximative, cependant il apparaît bien qu'on puisse retenir, au moins provisoirement, une datation vers 800 ou dans la première moitié du VIII<sup>e</sup> s. av. J.-C.

Cette datation archéologique, essentiellement basée sur l'analyse

<sup>3</sup> Cf. J. HOFIJZER - G. VAN DER KOOIJ, *Aramaic Texts from Deir 'Alla (ATDA)*, Leiden, 1976, p. 16.

<sup>4</sup> Cf. M.M. IBRAHIM - G. VAN DER KOOIJ, "Excavations at Deir 'Alla, Season 1984", *ADAJ* 30, 1986, pp. 131-143, spéc. p. 142: "770-880 B.C."

<sup>5</sup> *Id.*, "Excavations at Tell Deir 'Alla, Season 1979", *ADAJ* 23, 1979, pp. 41-50, spéc. p. 50.

<sup>6</sup> Cf. G. VAN DER KOOIJ, "The Identity of Trans-Jordanian Alphabetic Writing in the Iron Age", dans A. HADIDI éd., *Studies in the History and Archaeology of Jordan III*, Amman, 1987, pp. 107-121, spéc. p. 109; *id.*, "Tell Deir 'Alla (East Jordan Valley) During the Achaemenid Period, Some Aspects of the Culture", dans H. SANCISI-WEERDENBURG, *Achaemenid History I, Sources, Structures and Synthesis*, Leiden, 1987, pp. 97-102, spéc. pp. 97-98.



au Carbone 14, peut être rapprochée de la datation paléographique proposée, indépendamment, dès 1967, par un spécialiste de la paléographie araméenne ancienne, le professeur J. NAVEH: "We suggest, therefore, that this wall inscription from Deir Allah be dated to the middle of the 8th century or even earlier (by one or two decades). Its script represents an early stage in the development of the Aramaic cursive"<sup>7</sup>.

Une troisième approche pourrait corroborer une datation de la destruction de la phase M/IX dans la première moitié du VIII<sup>e</sup> s. av. J.-C.: d'après tous les rapports préliminaires des fouilleurs, cette destruction semble clairement attribuable à un tremblement de terre.<sup>8</sup> Le rattachement d'une destruction à un tremblement de terre, à Deir <sup>c</sup>Alla, semble généralement une sérieuse possibilité à envisager car il s'agit d'un phénomène naturel plusieurs fois attesté aux époques historiques dans cette région.<sup>9</sup> Cependant un tremblement de terre qui aboutit à une destruction systématique d'habitations, à la destruction de tout un niveau archéologique, reste assez exceptionnel, même dans cette région. Dès lors, comme nous l'avons déjà proposé<sup>10</sup>, on doit se demander s'il ne faut pas rapprocher cette destruction du "fameux" tremblement de terre mentionné dans le livre du prophète Amos (1,1; cf. aussi 4,11; 6,8-11; 8,8; 9,1; Zacharie 14,5)<sup>11</sup>, probablement un peu avant la moitié du VIII<sup>e</sup> s. av. J.-C., peut-être plus précisément dans le deuxième quart de ce siècle car, selon notre chronologie<sup>12</sup>, le roi Ouzzyahu/Ozias a commencé à régner seul vers 776 et Jéroboam II, roi d'Israël est mort vers 750.

Ce rattachement vraisemblable invite à comparer la phase M/IX de Deir <sup>c</sup>Alla à divers niveaux d'autres sites, surtout de Cisjordanie, possiblement aussi détruits par ce tremblement de terre: en

<sup>7</sup> J. NAVEH, "The Date of the Deir <sup>c</sup>Allā Inscription in Aramaic Script", *IEJ* 17, 1967, pp. 256-258.

<sup>8</sup> Cf. dernièrement M.M. IBRAHIM - G. VAN DER KOOIJ, *ADAJ* 27, 1983, p. 583; *id.*, *ADAJ* 30, 1986, p. 137.

<sup>9</sup> Cf., par ex., D.H. KALLNER-AMIRAN, "A Revised Earthquake Catalogue of Palestine", *IEJ* 1, 1950/51, pp. 223-246.

<sup>10</sup> *CRAI* 1985, p. 272.

<sup>11</sup> Cf. J.A. SOGGIN, "Das Erdbeben von Amos 1,1 und die Chronologie der Könige Ussia und Jotham von Juda", *ZAW* 82, 1970, pp. 117-121.

<sup>12</sup> Cf. A. LEMAIRE, *Histoire du peuple hébreu*, Que sais-je? 1898, Paris, <sup>2</sup>1985, pp. 46-47.

particulier à Hazor, niveau VI<sup>13</sup>, à Samarie, niveau IV<sup>14</sup> ou niveau V<sup>15</sup>, à Sichem, niveau VIII<sup>16</sup> et à Lakish, niveau IV<sup>17</sup>. Il reste aux archéologues, et spécialement aux céramistes, à comparer le matériel de ces divers niveaux pour infirmer ou confirmer cette éventuelle contemporanéité, si cela est possible.

## II - GÉOGRAPHIE HISTORIQUE ET PHASE M/IX DE DEIR 'ALLA

Comme beaucoup de commentateurs semblent l'avoir pensé, et comme l'a écrit récemment B. HALPERN: "Location is the most obvious starting-point for classifying the DAPT dialect ... At the same time, Deir Alla lay within Israelite territory"<sup>18</sup>. Cette position *a priori*, implicite ou explicite, semble largement répandue, cependant une étude historique plus approfondie<sup>19</sup> semble révéler que le rattachement politique de Deir 'Alla au royaume d'Israël à la fin du IXe s. ou dans la première moitié du VIIIe s. av. J.-C. est très incertain et finalement peu vraisemblable.

Comme il n'existait pas, au début du VIIIe s. av. J.-C., d'entité politique indépendante, de "royaume" de Galaad ou de la moyenne vallée du Jourdain, un premier coup d'oeil sur la situation géographique de Deir 'Alla révèle que ce site peut, *a priori* et pour cette époque, se rattacher soit au royaume de Samarie (Israël), soit au royaume de Damas (Aram), soit au royaume ammonite.

Il semble que, au moins depuis l'époque davidique jusqu'au coup d'état de Jéhu en 841, la moyenne vallée du Jourdain et au moins une partie du territoire de Galaad aient été rattachées au royaume israé-

<sup>13</sup> Cf. Y. YADIN, *Hazor II*, Jérusalem, 1960, pp. 24, 26, 37; *id.*, *Hazor*, The Schweich Lectures 1970, Londres, 1970, pp. 113, 181, 185, 198, 200.

<sup>14</sup> Cf. Y. YADIN, "Ancient Judaean Weights and the Date of the Samaria Ostraca", *Scripta Hierosolymitana* 8, Jérusalem, 1961, pp. 9-25, spéc. p. 24, n. 72.

<sup>15</sup> Cf. J.W. CROWFOOT et alii, *The Objects from Samaria*, Londres, 1957, p. 470.

<sup>16</sup> Cf. E.F. CAMPBELL, "The Excavation of Shechem and the Biblical Tradition", *BA* 26, 1963, pp. 2-26, spéc. p. 20.

<sup>17</sup> Cf. D. USSISHKIN, "The Destruction of Lachish by Sennacherib and the Dating of the Royal Judean Storage Jar", *Tel Aviv* 4, 1977, pp. 28-60, spéc. p. 52.

<sup>18</sup> B. HALPERN, "Dialect Distribution in Canaan and the Deir Alla Inscriptions", dans D.M. GOLOMB éd., "Working With No Data", *Semitic and Egyptian Studies Presented to Th. O. Lambdin*, Winona Lake, 1987, pp. 119-139, spéc. p. 121.

<sup>19</sup> Cf. notre communication: "Les territoires d'Ammon, Moab et Edom dans la deuxième moitié du IXe s. av. n. è.", à paraître dans les actes du 4ème Congrès d'Histoire et d'Archéologie Jordaniennes, Lyon, 1989.



lite. En 841, le coup d'état de Jéhu semble lié à une tentative de Joram d'Israël, appuyé par le roi judéen Achazyahu, de reprendre la ville de Ramot-Galaad prise ou menacée par les Araméens de Hazaël<sup>20</sup>, tentative qui échoua<sup>21</sup> et révèle la gravité de la menace araméenne sur les possessions israélites du Nord de la Transjordanie à la fin de la dynastie des Omrides.

L'histoire de la dynastie de Jéhu fut profondément marquée par cet affrontement entre Israël et le royaume araméen de Damas. Dès le règne de Jéhu (c. 841-814), Hazaël priva le royaume israélite de tout son territoire transjordanien: "Hazaël les mit en déroute dans tout le territoire d'Israël à l'est du Jourdain, tout le territoire de Galaad, de Gad, de Ruben et de Manassé depuis Aroër sur l'Arnon, et le Galaad et le Bashân" (2 Rois, 10,32-33).

Une analyse du contexte historique international situe probablement cette perte après la dernière campagne assyrienne de Salmanazar III dans la région, en 838 (-837?)<sup>22</sup>. Bien que le texte biblique ne le précise pas explicitement, on peut déduire de certains indices du texte de la stèle de Mésha et de diverses allusions dans les oracles prophétiques d'Amos (surtout 1,3 et 13) que cette guerre fut une guerre de conquête et d'annexion systématique du territoire avec massacre (*hrm*) de populations<sup>23</sup>, les Ammonites et les Moabites y étant les alliés, et éventuellement vassaux, des Araméens de Damas.

Sous le successeur de Jéhu, le roi Joachaz de Samarie (c. 819-814-803), Hazaël puis son successeur Barhadad contrôlèrent presque totalement le royaume de Samarie (2 Rois 13,3) qui dut accepter l'établissement de comptoirs araméens dans sa capitale (cf. 1 Rois 20,34), la réduction de ses forces armées à "cinquante cavaliers, dix chars et dix mille fantassins" (2 Rois 13,7), ainsi que

<sup>20</sup> Cf. 2 Rois 8,28-29; cf. aussi 1 Rois 22 où les rois d'Israël et de Juda sont devenus postérieurement Achab et Josaphat.

<sup>21</sup> Cf. le coup d'état de Jéhu: 2 Rois 9-10,1-28.

<sup>22</sup> Sur le problème d'une éventuelle campagne assyrienne en 837, cf. J.E. READE, "Assyrian Campaigns, 840-811 B.C., and the Babylonian Frontier", *ZA* 68, 1978, pp. 251-260, spéc. p. 254; W.T. PITARD, *Ancient Damascus*, Winona Lake, 1987, p. 149.

<sup>23</sup> Cf. J.A. SOGGIN, "Amos VI,13-14 und I,3 auf dem Hintergrund der Beziehungen zwischen Israel und Damaskus im 9. und 8. Jahrhundert", dans H. GOEDICKE éd., *Near Eastern Studies in Honor of W.F. Albright*, Baltimore/Londres, 1971, pp. 433-441, spéc. p. 434: "... "die Aramäer in Gilead eine bewusst geplante Politik der Ausrottung bzw. der Vertreibung der israelitischen Lokalbevölkerung verfolgten."



le passage des armées araméennes sur son territoire cisjordanien, spécialement pour aller assiéger Gat et recevoir la soumission de Joas, roi de Juda (cf. 2 Rois 12,18-19). A la fin de son règne, Hazaël avait porté la puissance araméenne à son zénith<sup>24</sup>. Une inscription araméenne le mentionnant, découverte récemment à Samos, vient même de révéler qu'il exerçait probablement un certain contrôle politique, une sorte de suzeraineté, sur le royaume d'Umq, dans la basse vallée de l'Oronte, et qu'il franchit l'Euphrate<sup>25</sup>.

Au début du règne de Barhadad, successeur de Hazaël, l'armée araméenne assiégea même Samarie (cf. 1 Rois 20,1-21; 2 Rois 6,24-33), siège qui ne semble avoir été levé que sur la rumeur de l'arrivée d'une grosse armée étrangère (2 Rois 7, 6-7), peut-être une armée assyrienne car Adadnirari III reprit les campagnes vers l'Ouest à partir de 805. On doit souligner que lors de cette campagne araméenne contre Samarie, Barhadad semble avoir été à la tête d'une coalition de plusieurs rois et qu'il avait établi son camp, sa base d'opération militaire, à Soukkôt<sup>26</sup>. Quelle que soit la localisation exacte de Soukkôt: à Tell Deir 'Alla ou à Tell Aḥṣas<sup>27</sup>, cette indication semble confirmer, pour cette époque, le contrôle araméen sur la Transjordanie du Nord et, plus spécialement, sur la partie transjordanienne de la moyenne vallée du Jourdain (*mq skwt*: cf. Psaume 60,8; 108,8).

Les rapports politiques araméo-israélites ne se rééquilibrèrent que sous le roi Joas d'Israël (c. 805-803-790; cf. 2 Rois 13,22-25) qui, en particulier, repoussa une attaque araméenne en plaine, à Apheq (1 Rois 20,26-30; 2 Rois 13,17), probablement dans la plaine de Yizréel<sup>28</sup>. A la suite de cette dernière défaite, Barhadad

<sup>24</sup> Cf. W.T. PITARD, *Ancient Damascus*, 1987, pp. 151-159.

<sup>25</sup> Cf. H. KYRIELEIS - W. RÖLLIG, "Ein altorientalischer Pferdeschmuck aus dem Heraion von Samos", *Mitteilungen des Deutschen Archäologischen Instituts - Athenische Abteilung* 103, 1988, pp. 37-75, où on lit *zy ntn hdd lmr'n hz'l mn 'mq bšnt 'dh mr'n nhr*, "Ce qu'a donné Hadad à notre maître Hazaël, depuis Umq, dans l'année où notre maître a traversé le fleuve": cf. F. BRON - A. LEMAIRE, "Les inscriptions araméennes de Hazaël", *RA* 83, 1989, pp. 35-44; cf. aussi I. EPH'AL - J. NAVEH, "Hazaël's Booty Inscriptions", *IEJ* 39, 1989, pp. 192-200.

<sup>26</sup> D'après la Septante, cf. Y. YADIN, "Some Aspects of the Strategy of Ahab and David (I Kings 20; 2 Sam. 11)", *Biblica* 36, 1955, pp. 333-341, spéc. p. 337; W.T. PITARD, 1987, p. 168.

<sup>27</sup> Cf. A. LEMAIRE, "Galaad et Makir", *VT* 31, 1981, pp. 39-61, spéc. pp. 50-53.

<sup>28</sup> Cf. 1 Samuel 29,1; E. KLOSTERMANN, *Eusebius, Das Onomastikon der biblischen*

reconnut l'indépendance du royaume d'Israël<sup>29</sup> avec réciprocité des droits commerciaux et restitution par Barhadad des villes que son père Hazaël avait prises au père de Joas, c'est à dire Joachaz (cf. 1 Rois 20,34)<sup>30</sup>. Il s'agissait apparemment d'un retour au *statu quo ante*, non pas à celui du début du règne de Jéhu, mais seulement à celui du début du règne de Joachaz, comme le confirme 2 Rois 13,25:

"Joas fils de Joachaz reprit à Ben-Hadad fils de Hazaël les villes enlevées par les armes à son père Joachaz".

Ainsi, par ce traité (*b'rit*: 1 Rois 20,34), le royaume d'Israël retrouvait sa totale indépendance mais Joas reconnaissait probablement le Jourdain comme sa frontière orientale avec le royaume araméen. On notera d'ailleurs qu'en 2 Rois 7,15, les éclaireurs israélites arrêtent au Jourdain leur poursuite de l'armée araméenne.

Sous le règne de Jéroboam II (c. 790-750), le royaume de Samarie paraît avoir retrouvé une certaine prospérité et exercé un certain protectorat sur le royaume de Juda (cf. 2 Rois 14,25). Les livres des Rois ne mentionnent explicitement aucune reconquête du Nord de la Transjordanie par Israël, cependant la plupart des commentateurs pensent qu'un certain nombre de textes bibliques y font allusion dans le cadre de l'extension de la suzeraineté israélite depuis Lebo-Hamat jusqu'à la Mer de la Aravah (2 Rois 14,25; cf. Amos 6,13)<sup>31</sup>. C'est ainsi que Amos 6,13 est généralement interprété comme une allusion à une victoire israélite à Lo-Debar<sup>32</sup> et à Qarnayim (probablement Cheikh-Sa<sup>c</sup>ad)<sup>33</sup>. Le contrôle israélite sur

*Ortsnamen*, Leipzig, 1904, p.34, lignes 11-12; M. HARAN, "The Rise and Decline of the Empire of Jeroboam ben Joash", *VT* 17, 1967, pp. 266-297, spéc. p. 270; J.M. MILLER - J.H. HAYES, *A History of Ancient Israel and Judah*, Londres, 1986, p. 301.

<sup>29</sup> Cf. 1 Rois 20,32 où l'appellation "frère" manifeste l'égalité retrouvée entre les rois de Damas et de Samarie.

<sup>30</sup> Pour cette interprétation, cf. déjà M. HARAN, *VT* 17, 1967, pp. 270-271.

<sup>31</sup> Cf. J. GRAY, *I and II Kings*, Londres, 1970, pp. 615-617; J. BRIGHT, *A History of Israel*, Londres, 1972, p. 254; J.A. SOGGIN, *A History of Israel*, Philadelphia, 1984, p. 217; J.M. MILLER - J.H. HAYES, *A History of Israel*, Londres, 1986, pp. 307-310.

<sup>32</sup> L'identification de Lo-Debar reste incertaine, cf. A. LEMAIRE, *VT* 31, 1981, p. 49.

<sup>33</sup> Cf. F.M. ABEL, *Géographie de la Palestine II*, Paris, 1967, pp. 413-414; D. KELLERMANN, "Aštārôt - Aštārôt-Qarnayim - Qarnayim", *ZDPV* 97, 1981, pp. 45-61; B. MAZAR, *The Early Biblical Period, Historical Studies*, Jérusalem, 1986, p. 161. On y a trouvé une "stèle égyptienne de Ramsès II" (R. DUSSAUD, *Topo-*



Galaad dans les dernières années du royaume israélite semble indiqué par:

- les références à Galaad en Osée 6,8; 12,12;
- le concours de Galaadites lors de la prise du pouvoir par Pégah (2 Rois 15,25);
- la mention de Galaad dans la liste des territoires israélites conquis, avec déportation de la population, par Tiglath-phalazar III en 2 Rois 15,29<sup>34</sup>;
- la probable mention de Galaad comme limite du territoire araméen conquis par Tiglath-phalazar III<sup>35</sup>;
- la mention d'un recensement de cette région vers la fin du règne de Jéroboam II (1 Chroniques 5,11-17).

Il est plus difficile de fixer la date de cette reconquête qu'Amos a critiquée comme éphémère et inutile dans le contexte de la menace assyrienne (Amos 6,13-14). Avec M. HARAN<sup>36</sup>, il semble possible de distinguer, dans l'activité prophétique d'Amos, qui a probablement commencé "deux ans avant le tremblement de terre" (Amos 1,1) une période durant laquelle Israël/Jacob est encore "petit" (Amos 7,2.5) et Galaad opprimé par les Araméens (Amos 1,3) et leurs alliés ammonites (Amos 1,14). Les oracles contre les nations voisines d'Amos 1,2-2,6 se situent apparemment dans un contexte où Israël se sent encore inférieur et opprimé par les royaumes voisins alors que le *turtanu* assyrien Shamshi-ilu est encore tout-puissant (cf. Amos 1,5)<sup>37</sup>; or ce véritable "Assyrian king of the West" resta en poste au moins jusqu'en 752<sup>38</sup>. Une autre partie de l'activité d'Amos semble se situer tout à fait à la fin du règne de Jéroboam II dont le prophète annonce la mort ainsi que celle de sa "maison" (Amos 7,9.11) liée à la perspective de la chute du royaume et de

graphie historique de la Syrie antique et médiévale, BAH 4, Paris, 1927, pp. 344-345) et une sculpture de lion dans le style "néo-hittite" (cf. G. CONTENAU, *Syria* 5, 1924, pp. 207-210, pl. LI).

<sup>34</sup> Cependant on notera l'absence, dans cette liste, de Mégiddo et de Dor. Cf. aussi 1 Chroniques 5,26.

<sup>35</sup> Cf. H. TADMOR, "The Southern Border of Aram", *IEJ* 12, 1962, pp. 114-122.

<sup>36</sup> *VT* 17, 1967, pp. 266-297; *IEJ* 18, 1968, pp. 201-212.

<sup>37</sup> Cf. A. MALAMAT, "Amos 1:5 in the Light of the Til Barsip Inscriptions", *BASOR* 129, 1953, pp. 25-26; A. LEMAIRE - J.M. DURAND, *Les inscriptions araméennes de Sfiré et l'Assyrie de Shamshi-ilu*, HEO 20, Genève/Paris, 1984, p. 44.

<sup>38</sup> Cf. J.D. HAWKINS, "The Neo-Hittite States in Syria and Anatolia", dans *The Cambridge Ancient History III,1*, 1982, pp. 404-405.



l'exil des Israélites (Amos 7, 11. 17; 8, 13). C'est dire que, comme l'a déjà bien vu M. HARAN<sup>39</sup>, la reconquête de Galaad par Jéroboam II se situe probablement à la fin de son règne et est probablement contemporaine du règne d'Assur-nirari V (754-745), c'est dire qu'on peut la situer vers 750 av. J.-C.

Comme l'avait pressenti Amos, cette région ne fut probablement replacée sous contrôle israélite qu'une vingtaine d'années tout au plus puisqu'elle fut transformée en province assyrienne par Tiglath-phalazar III c. 733.<sup>40</sup>

Ainsi, selon toute vraisemblance, le pays de Galaad et la partie transjordanienne de la moyenne vallée du Jourdain comprenant le site de Deir 'Alla ont été contrôlés par les Araméens de Damas de c. 835 à c. 750, puis par les Israélites de Samarie, avant d'être annexés par l'Assyrie c. 733 av. J.-C.

Au terme de cette recherche de géographie historique et dans l'attente d'une analyse plus poussée du matériel archéologique de la phase M/IX de Deir 'Alla, on notera simplement, sans tenir compte, pour l'instant, du problème linguistique posé par les inscriptions sur plâtre, que le rattachement de cette phase à la culture araméenne ou, tout au moins, à un certain contrôle politique araméen, semble confirmé par les autres petites inscriptions trouvées dans ce niveau, inscriptions fonctionnelles (<sup>2</sup>bn šr<sup>c</sup> et zy šr<sup>c</sup>)<sup>41</sup> dont le caractère araméen ne semble pas avoir été discuté.

### III - LA DISPOSITION PRIMITIVE DES INSCRIPTIONS SUR PLÂTRE

A la suite d'une étude détaillée et, en particulier, d'un examen personnel au Musée d'Amman en 1984<sup>41bis</sup>, il nous semble que l'hypothèse suivant laquelle ces inscriptions étaient primitivement écrites sur une stèle n'a pas de fondement solide. Selon toute vraisemblance, comme d'ailleurs certaines inscriptions à peu près

<sup>39</sup> VT 17, 1967, pp. 278-284.

<sup>40</sup> Pour cette transformation, cf. H. TADMOR, *IEJ* 12, 1962, p. 121; B. ODED, "Observations on Methods of Assyrian Rule in Transjordan after the Palestinian Campaign of Tiglath-Pileser III", *JNES* 29, 1970, pp. 177-186; *id.*, dans A. MALAMAT éd., *World History of the Jewish People, First Series, Volume IV, 1, The Age of the Monarchies: Political History*, Jérusalem, 1979, pp. 270 et 362, n. 111.

<sup>41</sup> Cf. *ATDA*, pp. 15, 167, 267; A. LEMAIRE, *CRAI* 1985, p. 273.

<sup>41bis</sup> Cf. surtout "La disposition originelle des inscriptions sur plâtre de Deir 'Alla", *SEL* 3, 1986, pp. 79-93.

contemporaines de Kuntillet 'Ajrud<sup>42</sup>, il s'agit d'inscriptions écrites à l'encre sur la paroi plâtrée ou, plutôt, chaulée d'un mur. De façon plus précise, l'inscription était probablement écrite sur la paroi orientale du "Mur 36" commençant vraisemblablement à l'angle du mur 42 et du mur 36.

Il est plus difficile de préciser si tous les fragments conservés actuellement proviennent d'une même colonne<sup>43</sup>:

1 – On notera tout d'abord que, si l'on tient compte des fragments anépigraphes XIV et XIIIa, il apparaît que l'encadrement à l'encre rouge était prévu pour inscrire une autre colonne à gauche de celle du groupement I et donc que l'hypothèse d'une présentation éventuelle en plusieurs colonnes doit être sérieusement envisagée même si la colonne prévue à gauche du groupement I n'a, apparemment, pas été inscrite.

2 – L'hypothèse suivant laquelle tous les fragments inscrits recueillis auraient été primitivement écrits dans une seule colonne, adoptée par E. PUECH<sup>44</sup>, P.K. McCARTER<sup>45</sup> et G. GARBINI<sup>46</sup>, ne semble pas, au moins pour l'instant, avoir abouti à un résultat positif quant à la continuation des lignes du groupement II après le groupement I.

3 – Le lieu de trouvaille, différent pour les groupements I et II, et le contenu, lui aussi, semble-t-il, assez différent, en particulier du fait de l'absence de la mention de "Balaam" dans le groupement II, semblent plutôt favoriser l'hypothèse de deux colonnes différents.

4 – La hauteur conservée du plâtre lié au groupement II (84 cm) et spécialement celle du début des lignes inscrites (63 cm) paraissent à peu près suffire pour qu'un scribe puisse écrire verticalement sans trop de difficulté.

<sup>42</sup> Cf. surtout Z. MESHEL, *Kuntillet 'Ajrud, A Religious Centre from the Time of the Judaean Monarchy on the Border of Sinai*, The Israel Museum Cat. n° 175, Jérusalem, 1978; M. WEINFELD, "Kuntillet 'Ajrud Inscriptions and Their Significance", *SEL* 1, 1984, pp. 121–130; A. LEMAIRE, "Date et origine des inscriptions hébraïques et phéniciennes de Kuntillet 'Ajrud", *ibidem*, pp. 131–143; *id.*, "Manuscrit, mur et rocher en épigraphie nord-ouest sémitique", dans R. LAUFER éd., *Le texte et son inscription*, Paris, 1989, pp. 35–42.

<sup>43</sup> Cf. A. LEMAIRE, "Fragments from the Book of Balaam Found at Deir Alla", *BASOR* 115, sept. 1985, pp. 26–39, spéc. p. 31; *id.*, *SEL* 3, 1986, pp. 85–89; cf. aussi E. PUECH, "Remarques sur la disposition du texte", *Le Monde de la Bible* 46, 1986, p. 38.

<sup>44</sup> Cf. *RB* 85, 1978, p. 116 et surtout "Le texte 'ammonite' . . .", 1987, p. 14.

<sup>45</sup> P.K. McCARTER, *BASOR* 239, 1980, p. 49.

<sup>46</sup> G. GARBINI, "L'iscrizione di Balaam bar Beor", *Henoch* 1, 1979, pp. 166–168.



Bien qu'il s'agisse là plutôt d'indices que d'arguments vraiment décisifs, il nous semble actuellement plus probable que le groupement I et le groupement II n'appartenaient pas primitivement à la même colonne. Concrètement le groupement II occupait probablement le bas et le milieu de la colonne la plus à droite, près de l'angle du mur 42 et du mur 36 (première colonne), tandis que le groupement II occupait le haut de la colonne située à sa gauche (deuxième colonne) et que les colonnes 3 et 4 n'ont pas été inscrites.

Cette disposition en colonnes accentue encore la ressemblance de cette inscription avec l'aspect général de colonnes d'un manuscrit<sup>47</sup>, ressemblance déjà soulignée par:

- 1 - l'emploi de l'encre pour écrire sur une surface à peu près blanche;
- 2 - la délimitation d'un encadrement au gros trait rouge, horizontal pour indiquer la limite supérieure de la colonne d'écriture et vertical pour marquer la fin des lignes;
- 3 - l'emploi de l'encre rouge pour les "rubriques": titres et passages importants;
- 4 - l'écriture cursive régulière dénotant un scribe professionnel;
- 5 - la longueur même des lignes: environ 31,5 cm<sup>48</sup>, qui n'est pas sans évoquer la longueur des lignes des manuscrits araméens anciens: par exemple environ 32 cm pour le manuscrit d'Ahiqar trouvé à Eléphantine<sup>49</sup> et environ 33,3 cm (avec une marge d'environ 2,5 cm) pour le manuscrit araméen de l'inscription de Béhistoun<sup>50</sup>, tandis que la longueur moyenne des lignes des lettres officielles d'Arsham est à peu près la même: environ 30-35 cm<sup>51</sup>.

Comme l'a bien souligné A.R. MILLARD: "This inscription from Deir Alla probably represents a column of a scroll"<sup>52</sup>, "it shows

<sup>47</sup> Cf. déjà A. LEMAIRE, "Manuscrit, mur et rocher...", 1989, pp. 37-38.

<sup>48</sup> Cf. A. LEMAIRE, *SEL* 3, 1986, p. 86.

<sup>49</sup> Cf. Ed. SACHAU, *Aramäische Papyrus und Ostraka aus einer jüdischen Militärkolonie zu Elephantine*, Leipzig, 1911.

<sup>50</sup> Cf. J.C. GREENFIELD - B. PORTEN, *The Bisitun Inscription of Darius the Great, Aramaic Version*, CII, I/V, Londres, 1982, p. 2.

<sup>51</sup> Cf. G.R. DRIVER, *Aramaic Documents of the Fifth Century B.C.*, Oxford, 1954, d'après les planches; cf. aussi B. PORTEN - A. YARDENI, *Textbook of Aramaic Documents from Ancient Egypt I, Letters*, Jérusalem, 1986, pp. 102-129 et COWLEY 30/31 (pp. 67-73), COWLEY 17 (p. 95), COWLEY 25 (p. 97) ... etc.

<sup>52</sup> A.R. MILLARD, "In Praise of Ancient Scribes", *BA* 45, 1982, pp. 143-153, spéc. p. 149.



how a column of Aramaic writing would have appeared on a papyrus or leather scroll"<sup>53</sup> au VIII<sup>e</sup> s. av. J.-C.

#### IV – GENRE LITTÉRAIRE

L'épigraphie nord-ouest sémitique comporte de nombreux types classiques d'inscriptions: monumentales, votives, commémoratives, dédicaces, marques de propriété, graffiti, messages, listes, textes économiques... Il est clair que la présentation extérieure des inscriptions sur plâtre de Deir 'Alla n'évoque aucun de ces types classiques. Comment expliquer cette présentation matérielle spéciale? Il semble que la manière la plus simple soit de comprendre que l'inscription sur plâtre de Deir 'Alla a été copiée directement à partir d'un rouleau manuscrit, plus spécialement d'un rouleau de manuscrit littéraire.

Cette interprétation semble confirmée par le contenu des inscriptions. En effet, bien qu'il y ait de nombreuses divergences dans la lecture et l'interprétation du groupement I, presque tous les commentateurs semblent s'accorder aujourd'hui sur le fait que, selon le titre écrit à l'encre rouge au début de la ligne 1, il s'agit de la copie d'un extrait<sup>54</sup> du *špr* [b]l *ḥm*. *br b*<sup>c</sup> *ḥr*. *š. ḥzh ḥn*, "texte/livre de Balaam fils de Beor, l'homme qui voyait les dieux". Balaam fils de Beor y reçoit une visite divine durant la nuit avec transmission d'une parole divine, apparemment l'annonce d'un châtement destructeur. "Et Balaam se leva le lendemain" et se mit à jeûner et à pleurer pendant plusieurs jours. Son "peuple" le visite alors et lui demande d'expliquer sa conduite: "Pourquoi jeûnes-tu? Pourquoi pleures-tu?". Balaam leur demande alors de s'asseoir (ou de se convertir? comme le proposent A. WOLTERS et M. DIJKSTRA: *infra*) et il leur révélera sa vision divine avec l'annonce d'un châtement destructeur. Suit une description de l'obscurité et de la terreur, ainsi que, probablement, un envahissement du ciel par toutes sortes d'oiseaux et de la terre par divers animaux sauvages... Le texte semble ensuite défier toute interprétation suivie.

<sup>53</sup> *Id.*, "Epigraphic Notes, Aramaic and Hebrew", *PEQ* 110, 1978, pp. 23-26, spéc. pp. 24-25: "Scribal Practices at Tel Deir 'Alla".

<sup>54</sup> Malgré E. PUECH, "Le texte 'ammonite'...", 1987, p. 15, la restitution d'un mot (*ysry?*) avant *špr* ne nous semble pas s'imposer. En tout cas, elle ne peut s'appuyer sur le parallèle du début de l'inscription de Siloé où on ne lit que *ḥnqbh* et non *ḥn ḥnqbh* (malgré E. PUECH, "L'inscription du tunnel de Siloé", *RB* 81, 1974, pp. 196-214, spéc. p. 199 qui a lu deux fois *ḥn*).

Quelles que soient les incertitudes, surtout celles de la fin du texte, il s'agit là visiblement d'un texte que l'on peut rapprocher de la tradition biblique de Nombres 22-24 ainsi que d'autres traditions bibliques de visions ou de prophéties concernant Abraham<sup>55</sup>, Jacob (Genèse 28,11-18), Joseph (Genèse 37, cf. 40-41), Josué (Josué 7,6-16), Samuel (1 Samuel 3; 15,10-23), Natan (2 Samuel 7,4-17) . . . La mention de l'assemblée divine se retrouve en 1 Rois 22 et Isaïe 6, la réaction des auditeurs à l'annonce d'une menace de destruction en Juges 20,26; 21,2; 2 Rois 22,11-20; Joël 2,15-27, tandis que l'obscurité du ciel est souvent liée au jour du Seigneur dans les oracles prophétiques (cf. Amos 5,20; Joël 2,2.10; Sophonie 1,15; Ezéchiel 32,7-8)<sup>56</sup>. Ce titre et les rapprochements littéraires confirment le classement du texte du groupement I comme un texte littéraire de genre prophétique.

Il est beaucoup plus difficile de préciser le genre littéraire du groupement II dont aucune ligne n'a pu être restituée complètement. Si l'on admet comme plus vraisemblable qu'il puisse s'agir d'un texte écrit dans une colonne différente, dont rien n'indique qu'il s'agisse aussi d'un extrait du "livre de Balaam" même s'il a pu être copié d'un même manuscrit original<sup>57</sup>, sa présentation extérieure est la même et il s'agit apparemment aussi d'un texte littéraire. Son contenu reste très incertain: plusieurs expressions pourraient laisser croire qu'il s'agit là de conseils à un jeune dauphin, à un futur roi, qui pourraient se rattacher à un genre littéraire plus ou moins sapiential. Malheureusement tout cela reste extrêmement incertain.

De façon plus générale, avec A.R. MILLARD, on peut reconnaître que "The plaster inscription from Tell Deir 'Alla is important because it presents us with our oldest available specimen of a lengthy literary text in the West Semitic Alphabet"<sup>58</sup>. Il nous semble que, à quelques nuances près, la plupart des commentateurs pourraient s'accorder sur ce point.

<sup>55</sup> Cf. J.D. SAFREN, "Balaam and Abraham", *VT* 38, 1988, pp. 105-113.

<sup>56</sup> Cf., par exemple, M. WEINFELD, "The Balaam Oracle in the Deir 'Alla Inscription", *Shnaton* 5-6, 1981-82, pp. 141-147 et LXVII.

<sup>57</sup> Dans l'antiquité, un même manuscrit pouvait contenir plusieurs textes littéraires différents écrits l'un à la suite de l'autre.

<sup>58</sup> A.R. MILLARD, *PEQ* 110, 1978, p. 25.



## V - LANGUE DES INSCRIPTIONS SUR PLÂTRE

Ce point reste l'un des plus controversés et nous n'avons pas l'intention de reprendre ici une analyse linguistique détaillée que nous avons conduite et publiée ailleurs<sup>59</sup>. Nous nous contenterons de remarques générales.

Sauf J.W. WESSELIUS<sup>60</sup>, personne ne semble avoir soutenu qu'il s'agissait d'une inscription hébraïque. Même si E. PUECH a qualifié récemment ce texte d'"ammonite"<sup>61</sup>, l'emploi des guillemets semble révéler que le terme "ammonite" qu'il emploie ne doit pas être compris au sens strict mais en tenant compte d'une certaine confusion entre les appellations "ammonite" et "transjordanien"<sup>62</sup>. En fait, dans la première moitié du VIII<sup>e</sup> s. av. J.-C., il faut distinguer, en Transjordanie, quatre régions politiques et culturelles: Aram, Ammon, Moab et Edom, et ce que nous savons déjà de la langue ammonite exclut tout à fait le rattachement linguistique de ces inscriptions à l'ammonite<sup>63</sup>.

Si on laisse de côté des propositions de rattachement au madianite<sup>64</sup> ou au nord-arabe<sup>65</sup>, deux interprétations restent en concurrence:

- le rattachement à l'araméen, proposé par l'*editio princeps*, en notant toutefois qu'il s'agirait alors d'un dialecte araméen différent de l'araméen classique d'époque achéménide;
- le rattachement à un dialecte cananéen inconnu jusqu'ici, proche de l'hébreu, de l'ammonite et du moabite, et parfois qualifié de galaadite.

Dans un article récent, B. HALPERN, tenant de la seconde inter-

<sup>59</sup> Cf. notre étude: "La langue de l'inscription sur plâtre de Deir 'Alla", *Comptes rendus du GLECS* 24-28, 1979-1984 (1986, paru en 1987), pp. 317-340.

<sup>60</sup> J.W. WESSELIUS, "Thoughts about Balaam: The Historical Background of the Deir 'Alla inscription on Plaster", *Bibliotheca Orientalis* 44, 1987, col. 589-599, spéc. col. 591.

<sup>61</sup> E. PUECH, "Le texte 'ammonite' . . .", 1987, pp. 13-30.

<sup>62</sup> Cette même qualification d'"ammonite" au lieu de "transjordanien" se trouve déjà dans E. PUECH, "Deux nouveaux sceaux ammonites", *RB* 83, 1976, pp. 59-62, où l'un des sceaux est sûrement paléo-hébreu (cf. P. BORDREUIL - A. LEMAIRE, *Semitica* 26, 1976, p. 63). On notera d'ailleurs que, à la fin de son article "Le texte 'ammonite' . . .", E. PUECH concluait à "une écriture et une langue ammonites aramaisantes" et on peut se demander s'il ne vaudrait pas mieux comprendre "une écriture et une langue transjordanienne aramaisantes".

<sup>63</sup> Cf. A. LEMAIRE, *Comptes rendus du GLECS* 24-28, 1979-1984 (1986), p. 334.

<sup>64</sup> Cf. A. ROFÉ, *The Book of Balaam*, Jérusalem, 1979, pp. 59-70, spéc. p. 69.

<sup>65</sup> Cf. G. GARBINI, *Henoch* 1, 1979, pp. 169-170.



prétation, reconnaissait que la plupart des experts "have pronounced the language Aramaic albeit with qualification"<sup>66</sup> mais il est clair qu'un tel problème ne se règle pas en le soumettant aux voix. On notera simplement ici que les conclusions de notre étude sur la date, la géographie historique, la disposition et le genre littéraire des inscriptions sur plâtre de Deir <sup>c</sup>Alla semblent mieux se situer dans le cadre d'un rattachement à l'araméen que dans celui à un dialecte cananéen.

1) En effet, le premier argument avancé en faveur d'un dialecte cananéen ou "sud-canéen" est celui de la position géographique de Deir <sup>c</sup>Alla qui est dit "within Israelite territory"<sup>67</sup>. Or l'étude de géographie historique nous a révélé que la phase M/IX de Deir <sup>c</sup>Alla se situait probablement à l'intérieur du territoire du royaume araméen de Damas à la fin du IX<sup>e</sup> et dans la première moitié du VIII<sup>e</sup> s. av. J.-C. On notera, de plus, que l'existence d'un dialecte cananéen galaadite, et spécialement d'un dialecte galaadite littéraire, différent de l'hébreu (ou de l'araméen, ou de l'ammonite) reste, à ce jour, une pure conjecture qui ne peut s'appuyer sur aucune entité politique galaadite indépendante au début du premier millénaire avant notre ère.

2) Le deuxième argument avancé en faveur d'un dialecte araméen est celui de l'écriture que certains qualifient d'"ammonite"<sup>68</sup>. En fait, il s'agit là d'un classement *a priori*, basé au point de départ sur une datation basse des inscriptions au début du VII<sup>e</sup> s. av. J.-C.<sup>69</sup>, datation que l'on peut difficilement soutenir aujourd'hui. Cette appellation "ammonite" de l'écriture paraît d'autant plus *a priori* que nous n'avons aucun exemple sûr de cursive ammonite à l'encre avant la seconde moitié du VII<sup>e</sup> s.<sup>70</sup>

3) Un troisième argument, évoqué par J.A. HACKETT<sup>71</sup> et B. HALPERN<sup>72</sup>, est assez surprenant. Ils reconnaissent que "the gra-

<sup>66</sup> B. HALPERN, "Dialect Distribution . . .", 1987, p. 120.

<sup>67</sup> *Ibidem*, p. 121.

<sup>68</sup> *Ibidem*.

<sup>69</sup> Cf. F.M. CROSS, "Notes on the Ammonite Inscription from Tell Siran", *BASOR* 212, 1973, pp. 12-15; *id.*, "Ammonite Ostraca from Heshbon: Heshbon Ostraca IV-VIII", *AUSS* 13, 1975, pp. 1-20, spéc. pp. 10-17.

<sup>70</sup> Cf. E. PUECH, "L'inscription de la statue d'Amman et la paléographie ammonite", *RB* 92, 1985, pp. 5-24, spéc. pp. 12-13 où il date l'ostracon de Nimrud (peut-être, en fait, en écriture araméenne?) de 650-625 et l'ostracon IV de Heshbon de la "fin du VII<sup>e</sup> s. - début du VI<sup>e</sup> s."

<sup>71</sup> J.A. HACKETT, *The Balaam Text from Deir <sup>c</sup>Allā*, *HSM* 31, Chico, 1980, pp. 111-113.

<sup>72</sup> B. HALPERN, "Dialect Distribution . . .", 1987, p. 122.

phic tradition follows that of Old Aramaic"<sup>73</sup> mais soulignent qu'il ne s'agit là que de graphèmes et que nous ne savons pas comment les phonèmes étaient réalisés; pour eux ce témoignage est donc "ambiguous"<sup>74</sup>. Franchement cet argument nous semble spécieux et irrecevable puisqu'il est bien clair qu'il nous est impossible de connaître la langue de l'inscription de Deir 'Alla autrement que par la manière dont elle a été mise par écrit; il en est d'ailleurs de même pour l'hébreu ancien en général ou pour toute autre langue "morte". Il faut donc reconnaître, de façon positive, que la représentation du \**d* par un *q*, et non par un *ṣ*, dans les inscriptions sur plâtre de Deir 'Alla rattache nettement ces inscriptions à l'araméen ancien puisqu'il s'agit là de la seule différence de tradition graphique, en écriture consonnantique, entre la branche araméenne et la branche cananéenne ancienne du nord-ouest sémitique.

4) De façon positive aussi, le rattachement à l'araméen semble tout à fait cohérent avec la découverte de deux inscriptions fonctionnelles araméennes dans la même phase M/IX et à quelques mètres des inscriptions sur plâtre de Deir 'Alla.

5) Si ces inscriptions sont la copie d'un ou de plusieurs textes littéraires, l'état de langue qu'ils représentent n'est pas nécessairement celui de la langue araméenne du deuxième quart du VIII<sup>e</sup> s. av. J.-C. En effet, ce ou ces textes littéraires, pour la date et le lieu précis de rédaction desquels on est réduit à des conjectures, ont nécessairement été rédigés *avant* leur copie sur le plâtre du mur de Deir 'Alla: ils représentent donc vraisemblablement un état de langue araméenne plus ancien que celui des deux inscriptions fonctionnelles de la phase M/IX, état que l'on peut qualifier d'*archaïque*<sup>75</sup> ou de *proto-araméen*<sup>76</sup> et qui pourrait représenter celui du IX<sup>e</sup> ou du Xe s. av. J.-C., par exemple. Il est probable que certaines particularités de l'état de langue de ces inscriptions peuvent s'expliquer dans ce contexte: peu<sup>77</sup> ou pas d'attestation de l'état

<sup>73</sup> *Ibidem*.

<sup>74</sup> J. HACKETT, *The Balaam Text* ..., 1980, p. 112.

<sup>75</sup> Cf. déjà P. McCARTER, *BASOR* 239, 1980, p. 50: "Certain features of the language are characteristic of a literary or at least an archaistic tradition"; A. WOLTERS, "The Balaamites of Deir 'Alla as Aramean Deportees", *HUCA* 59, 1988 (1989), pp. 101-113, spéc. p. 111: "an archaic form of Aramaic".

<sup>76</sup> Cf. E.A. KNAUF, *ZDPV* 101, 1985, p. 190: "Proto-Aramaic".

<sup>77</sup> Sur les attestations vraisemblables de l'état emphatique, cf. A. LEMAIRE, *Comptes rendus du GLECS* 24-28, 1979-1984 (1986), pp. 324-326, 333.



emphatique -<sup>3</sup>, éventuellement *niphal*<sup>78</sup>, lexique parfois proche de dialectes cananéens (*r<sup>3</sup>h*, *p<sup>c</sup>lt*).

La découverte récente des deux inscriptions araméennes de Hazaël à Erétrie et à Samos, provenant probablement du royaume d'<sup>c</sup>Umq et où l'état emphatique n'apparaît pas, spécialement là on l'attendrait, à la fin du mot *nhr*, "fleuve"<sup>79</sup>, semble confirmer que nous avons encore beaucoup à apprendre de cet araméen archaïque et des divers dialectes araméens du IX<sup>e</sup>, voire du X<sup>e</sup> s. av. J.-C.

## VI - SIGNIFICATION HISTORIQUE DES INSCRIPTIONS SUR PLÂTRE DE DEIR <sup>c</sup>ALLA

Si les inscriptions sur plâtre représentent un texte littéraire vraisemblablement copié d'un manuscrit plus ancien, il faut évidemment être très prudent quant à leur interprétation historique. Ainsi:

1) Il est clair que rien ne permet d'affirmer que Balaam ait été contemporain de ces inscriptions et qu'il faille dater ce personnage célèbre de la première moitié du VIII<sup>e</sup> s. av. J.-C. A plus forte raison ne peut-on s'appuyer sur ces inscriptions pour affirmer que la tradition biblique sur Balaam doit être nécessairement postérieure au milieu du VIII<sup>e</sup> s.<sup>80</sup> La datation des inscriptions sur plâtre de Deir <sup>c</sup>Alla dans la première moitié du VIII<sup>e</sup> s. ne fournit qu'un terminus *ante quem* pour une datation de l'éventuelle existence historique du personnage à la source de cette tradition littéraire. En fait, pour dater ce personnage historique éventuel, il faudrait d'abord dater la première rédaction du *spr bl<sup>c</sup>m* et ensuite essayer de situer la tradition historique qui en est la source, si tradition historique il y a.

2) Plus généralement, le *spr bl<sup>c</sup>m* représentant une tradition littéraire, la découverte des inscriptions sur le site de Deir <sup>c</sup>Alla ne

<sup>78</sup> L'absence d'état emphatique et l'emploi éventuel du *niphal* semblent se retrouver dans le dialecte araméen archaïque du royaume de Sam'al: cf. surtout P.E. DION, *La langue de Ya'udi*, Ottawa, 1974, spéc. pp. 135-138, 208-209. 341.

<sup>79</sup> Cf. F. BRON - A. LEMAIRE, "Les inscriptions araméennes de Hazaël", *RA* 83, 1989, pp. 35-44.

<sup>80</sup> Malgré G.W. AHLSTRÖM, "Another Moses Tradition", *JNES* 39, 1980, pp. 65-69, spéc. p. 69, n. 29; M. DELCOR, "Deir <sup>c</sup>Alla et les oracles bibliques de Bala<sup>c</sup>am", dans J.A. EMERTON éd., *Congress Volume, Vienna 1980*, SVT 32, Leiden, 1981, pp. 52-73, spéc. p. 73.



permet pas d'affirmer un lien direct, historique, entre le personnage de "Balaam fils de Beor" et ce site. Selon toute vraisemblance, le lien entre Deir 'Alla et Balaam fils de Beor n'est qu'indirect: la découverte d'extraits du "livre de Balaam" à Deir 'Alla permet seulement d'affirmer que la tradition littéraire au sujet de ce personnage y était connue dans la première moitié du VIII<sup>e</sup> s. av. J.-C.

3) Enfin, en corollaire, si les inscriptions sur plâtre de Deir 'Alla représentent un état archaïque de la langue araméenne, il ne s'agit pas nécessairement d'un état de langue parlé et écrit à Deir 'Alla même au IX<sup>e</sup> ou Xe s. av. J.-C., voire plus tôt. C'est même peu probable puisque nous avons vu que, jusque vers 835 av. J.-C. Deir 'Alla faisait probablement partie du royaume d'Israël et qu'on y parlait et écrivait probablement l'hébreu ancien (du Nord).

#### VII - SIGNIFICATION CULTURELLE DES INSCRIPTIONS SUR PLÂTRE DE DEIR 'ALLA

En fait, puisqu'il s'agit de la connaissance d'une tradition littéraire, le lien entre Balaam et Deir 'Alla, à la phase M/IX, est essentiellement culturel et c'est dans ce domaine de la culture littéraire ouest-sémitique que l'apport des inscriptions de Deir 'Alla est primordial.

1) Si, comme nous l'avons vu plus haut, la phase M/IX de Deir 'Alla faisait partie du territoire du royaume araméen de Damas et si les inscriptions du mur 36 sont la copie d'un ou de plusieurs textes littéraires araméens archaïques, cela signifie probablement que ce ou ces textes faisaient partie du *patrimoine littéraire du royaume araméen de Damas dans la première moitié du VIII<sup>e</sup> s. av. J.-C.*

2) L'existence d'un tel patrimoine littéraire est, de soi, vraisemblable puisque le royaume de Damas paraît avoir été une puissance politique comparable à celle du royaume d'Israël à la fin du Xe et dans la première moitié du IX<sup>e</sup> s. av. J.-C., et même supérieure dans la seconde moitié du IX<sup>e</sup> s. On peut d'ailleurs se demander si la mise en forme de ce patrimoine littéraire et, surtout, sa diffusion n'ont pas été particulièrement liées au règne de Hazaël qui représente l'apogée politique du royaume araméen de Damas<sup>81</sup>. Nous possédons déjà quatre inscriptions araméennes mentionnant Hazaël avec le titre *mr'n*, "notre maître"<sup>82</sup>, et il ne serait pas

<sup>81</sup> Cf. W.T. PITARD, *Ancient Damascus*, 1987, pp. 132-160.

<sup>82</sup> *Supra* n. 79.

étonnant que cette apogée politique ait coïncidé avec une apogée de la culture araméenne du royaume de Damas, spécialement du point de vue littéraire.

3) Dans ce contexte historique, les inscriptions sur plâtre de Deir ʿAlla ne peuvent nous informer directement ni sur la religion ni sur la culture populaires israélites, qu'elles soient yahvistes ou non-yahvistes<sup>83</sup>. D'ailleurs, avec A. WOLTERS, il faut reconnaître que "in general, it is striking that the inscription appears to be *non-israelite* in religion as well as in language and script"<sup>84</sup>.

4) Par contre, ces textes reflètent la culture littéraire et religieuse araméenne archaïque. On notera, en particulier, que le voyant (*hzh*) semble y jouer un rôle politique et religieux très important, déjà attesté par la mention des "voyants" (*hzyn*) dans l'inscription araméenne de Zakkur<sup>85</sup> et que la religion araméenne semble généralement polythéiste (cf. *ʾlhn* et *šdyn* au pluriel) avec coexistence de plusieurs grands dieux<sup>86</sup>. Malheureusement le caractère fragmentaire des inscriptions rend les lectures des théonymes "Sh(amash?)" et "El" incertaines et discutées. Si elles étaient vérifiées, on pourrait rapprocher ces mentions du "panthéon" araméen apparaissant dans les proverbes d'Ahiqar avec le présence de *ʾlʾlhn* et de *šmš*<sup>87</sup>.

5) Cette interprétation des inscriptions sur plâtre de Deir ʿAlla et, plus spécialement, du "livre de Balaam" dans le contexte de la culture araméenne archaïque s'accorde parfaitement avec les éléments essentiels de la tradition biblique concernant Balaam, spécialement celle de Nombres 22-24. En effet, il est clair que Balaam y apparaît comme un personnage célèbre et un voyant réputé (cf. Nombres 24,4.16) mais *non-israélite*. En fait, comme le reconnaît récemment B. HALPERN: "Balaam in the biblical account is associated with *Aram*"<sup>88</sup>. Cela est explicite en Nombres 23,7 et semble implicite en Nombres 22,5, Deutéronome 23,5.

<sup>83</sup> Nous différons donc de J.A. HACKETT, "Religious Traditions in Israelite Transjordan", dans P.D. MILLER *et alii*, *Ancient Israelite Religion, Essays in Honor of F.M. Cross*, Philadelphia, 1987, pp. 125-136.

<sup>84</sup> A. WOLTERS, *HUCA* 59, 1988 (1989), p. 102.

<sup>85</sup> Cf. KAI 202 A 12; J.C.L. GIBSON, *Textbook of Syrian Semitic Inscriptions II, Aramaic Inscriptions*, Oxford, 1975, p. 8, n° 5.

<sup>86</sup> Dans ce panthéon, la première place est souvent occupée par Hadad: cf. récemment J.C. GREENFIELD, "Aspects of Aramean Religion", dans P.D. MILLER *et alii*, *Essays ... F.M. Cross*, 1987, pp. 67-68.

<sup>87</sup> Cf. J.M. LINDENBERGER, "The Gods of Ahiqar", *UF* 14, 1982, pp. 105-118; *id.* *The Aramaic Proverbs of Ahiqar*, Baltimore, 1983, p. 20.

<sup>88</sup> B. HALPERN, "Dialect Distribution ...", 1987, p. 133.



6) Sans entrer ici dans le détail de la tradition biblique sur Balaam, nous voudrions attirer l'attention sur un passage qui nous semble largement méconnu. Il s'agit de Genèse 36,32, repris dans 1 Chroniques 1,43: "Et Bela<sup>c</sup> fils de Be<sup>c</sup>ôr régna en Edom et le nom de sa ville était Dinhabâh".

Apparemment ce verset mentionne le premier des "rois qui régèrent dans le pays d'Edom avant que ne règne un roi sur les Israélites" (Genèse 36,31). En fait, il s'agit vraisemblablement d'une tradition ancienne sur les premiers "rois" araméens, et la capitale de Bala<sup>c</sup>am/Bela<sup>c</sup> est vraisemblablement à situer dans le Haurân<sup>89</sup>. Si, un peu comme Samuel, Balaam a été à la fois un "voyant" et le premier "chef" (juge/roi) araméen, on comprendrait facilement que la tradition littéraire araméenne ultérieure en ait fait le héros d'un livre.

7) Ainsi les références de la tradition biblique au personnage araméen de Balaam fils de Beor s'éclairent quelque peu à la lumière des inscriptions sur plâtre de Deir 'Alla. En fait, c'est parce que ce personnage était célèbre dans la tradition littéraire araméenne ancienne ou archaïque, probablement connue dans une part importante de la Syrie-Palestine, au moins à l'époque de Hazaël et de Barhadad (deuxième moitié du IX<sup>e</sup> s. – début du VIII<sup>e</sup> s.), que les scribes israélites ont cru utile d'annexer, en quelque sorte, ce personnage célèbre régionalement de telle façon qu'il devienne favorable à Israël. Il y a probablement là un phénomène littéraire comparable à la mention d'Ahiqar dans le livre de Tobit<sup>90</sup> et, d'une autre manière, au récit de la désignation de Hazaël comme roi d'Aram par Elisée (2 Rois 8,7 – 15; cf. 1 Rois 19,15).

En nous révélant un fragment de la littérature araméenne antique, les inscriptions sur plâtre de Deir 'Alla nous révèlent, du même coup, l'influence que cette littérature araméenne a pu exercer sur la littérature hébraïque antique et sur la Bible.

### VIII – FONCTION DES INSCRIPTIONS SUR PLÂTRE

En conclusion de ces diverses approches pour une meilleure com-

<sup>89</sup> Cf. A. LEMAIRE, "Bala<sup>c</sup>am/Bela<sup>c</sup> fils de Be<sup>c</sup>ôr", ZAW 102, 1990, pp. 180–187.

<sup>90</sup> Cf. A. LEMAIRE, "Aramaic Literature and Hebrew Literature: Contacts and Influences in the First Millennium B.C.E.", dans M. BAR-ASHER éd., *Proceedings of the Ninth World Congress of Jewish Studies, Panel Sessions: Hebrew and Aramaic Languages*, Jérusalem, 1988, pp. 9–24, spéc. pp. 15–17.



préhension générale des inscriptions sur plâtre de Deir 'Alla, nous voudrions revenir au lieu de leur découverte en nous demandant ce que pouvait bien faire la copie d'extraits d'un manuscrit littéraire sur le mur chaulé n° 36 de Deir 'Alla: quelle pouvait bien être la fonction exacte de cette inscription?

La fonction de ces inscriptions pourrait s'éclairer à la lumière des découvertes archéologiques faites dans le locus B/C 5.57/58 (= EE 335). Apparemment il s'agissait d'une pièce pratiquement vide, recouverte par une épaisse natte faite d'au moins cinq épaisseurs de feuilles de roseau entrelacées<sup>91</sup>; les restes d'une structure, probablement une sorte de banquettes (B/C 5.69), occupaient, en partie, la portion méridionale de la pièce. Bien que plusieurs commentateurs aient fait référence à un lieu de culte, apparemment rien de ce qui a été trouvé dans cette pièce ne permet de penser à une utilisation proprement cultuelle.

A défaut d'indications provenant du matériel trouvé dans le locus B/C 5.57/58, nous en sommes réduits à comparer les inscriptions sur plâtre de Deir 'Alla à des inscriptions similaires. Elles ne sont pas nombreuses! Il s'agit essentiellement des inscriptions sur plâtre de Kuntillet 'Ajrud, à la limite du Négev et du Sinaï, malheureusement seulement en partie publiées; elles datent probablement, comme celles de Deir 'Alla, du deuxième quart du VIII<sup>e</sup> s. av. J.-C.<sup>92</sup> On peut aussi, mais le rapprochement est moins direct, comparer ces inscriptions aux inscriptions araméennes du Ve s. av. J.-C. trouvées dans une tombe à Cheikh Fadl en Moyenne Egypte, actuellement en cours de publication<sup>93</sup>.

En fait, c'est surtout le rapprochement avec les inscriptions de Kuntillet 'Ajrud qui mérite d'être souligné à cause de la similitude du matériau sur lequel a été inscrit l'inscription, de la date et du fait que ces inscriptions étaient accompagnées de dessins<sup>94</sup> et d'abécé-

<sup>91</sup> Cf. *ADAJ* 22, 1977-78, pp. 65 et 68; cf. aussi *ADAJ* 23, 1979, p. 48: B/C 6, qui semble la continuation de B/C 5 (= 57/58) était aussi recouvert d'un toit.

<sup>92</sup> Cf. *supra* n. 42.

<sup>93</sup> Cf. N. GIRON, "Note sur une tombe découverte près de Cheikh Fadl par M.F. Petrie et contenant des inscriptions araméennes", *Ancient Egypt* 1923, pp. 38-43; A. LEMAIRE, "Manuscrit, mur et rocher...", 1989, pp. 39-42.

<sup>94</sup> Cf. P. BECK, "The Drawings from Horvat Teiman (Kuntillet 'Ajrud)", *Tel Aviv* 9, 1982, pp. 3-68; cf. aussi J.M. HADLEY, "Some Drawings and Inscriptions on Two Pithoi from Kuntillet 'Ajrud", *VT* 37, 1987, pp. 180-213.

daires ou de fragments d'abécédaires. Cependant, si le rapprochement des deux groupes d'inscriptions paraît évident, leur interprétation risque d'être aussi difficile et discutée pour l'un et l'autre site.

En ce qui concerne Deir 'Alla, il semble que la copie de textes littéraires sur le mur 36 soit l'oeuvre d'un *maître-scribe*. Dans quel but a-t-il recopié aussi soigneusement ces extraits de textes littéraires sur un mur? C'était probablement la meilleure manière de les faire connaître, c'est à dire d'enseigner ces textes littéraires servant de référence à la culture nationale et religieuse araméenne. A défaut de pouvoir distribuer un manuscrit à chacun, le maître recopiait des extraits de celui-ci sur le mur chaulé afin que les auditeurs/"élèves" puissent en pendre connaissance, éventuellement en les répétant et en les apprenant par coeur.

Un tel *but didactique* à la copie d'inscription sur un mur, plus précisément sur un mur enduit de chaux, semble plusieurs fois évoqué dans la Bible: "Tu répéteras (ces paroles) à tes fils... et tu les écriras sur les montants de porte de ta maison et dans tes portes de villes..." (Deutéronome 6,9). "Tu dresseras pour toi de grosses pierres et tu les enduiras de chaux et tu y écriras toutes les paroles de cette inscription (*hattōrah*)" (Deutéronome 27,2-3; cf. aussi 27,4.8: "très distinctement": *ba'ēr hēyṭeb*). "Et il écrivit là, sur les pierres, le double/la copie de l'instruction de Moïse (*mišneh tōrat mošeh*)" (Josué 8,32).

La présence des inscriptions sur plâtre sur le mur 36 de Deir 'Alla semble donc révéler que la pièce B/C 5.57/58 de Deir 'Alla, probablement couverte et comportant vraisemblablement une sorte de banquettes, a pu servir de lieu d'enseignement<sup>95</sup>, c'est à dire de sorte d'"école". L'emploi de ce dernier mot a parfois suscité des critiques de la part de divers commentateurs<sup>96</sup> qui ne se rendent peut-être pas suffisamment compte que le mot "école" ne doit pas être pris ici avec comme archétype une école moderne, mais plutôt l'école traditionnelle dont un des exemples, encore actuel, est l'école

<sup>95</sup> Cf. déjà A. LEMAIRE, *Les écoles et la formation de la Bible dans l'ancien Israël*, OBO 39, Fribourg/Göttingen, 1981, p. 92, n. 67; *id.*, CRAI 1985, p. 283; *id.*, BAT, 1985, p. 322; *id.*, "Manuscrit, mur et rocher...", 1989, pp. 37-38.

<sup>96</sup> Cf. récemment F.W. GOLKA, "Die israelitische Weisheitschule oder 'des Kaisers neue Kleider'", VT 33, 1983, pp. 257-270; E. PUECH, BAT, 1985, p. 363; M. HARAN, "On the Diffusion of Literacy and Schools in Ancient Israel", dans J.A. EMERTON éd., *Congress Volume Jerusalem 1986*, SVT 40, Leiden, 1988, pp. 81-95.



coranique traditionnelle. D'ailleurs les historiens de la Mésopotamie ou de l'Égypte n'hésitent pas à parler d'écoles dès le III<sup>e</sup> millénaire av. J.-C. et il n'y a aucune raison de ne pas désigner de ce nom un lieu où est donné un enseignement de la culture scribale, littéraire et religieuse<sup>97</sup> tel que semble avoir été la pièce B/C 5.57/58 où ont été trouvées les inscriptions sur plâtre de Deir 'Alla.

## ANNEXE

## INDICATIONS BIBLIOGRAPHIQUES COMPLÉMENTAIRES

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<sup>97</sup> Cf. récemment A. LEMAIRE, *Les écoles . . .*, 1981; *id.*, *VT* 34, 1984, pp. 270-281; N. SHUPAK, "The 'Sitz im Leben' of the Book of Proverbs in the Light of a Comparison of Biblical and Egyptian Wisdom Literature", *RB* 94, 1987, pp. 98-119; E. PUECH, "Les écoles dans l'Israël préexilique, données épigraphiques", dans *Congress Volume Jerusalem 1986*, 1988, pp. 189-203; A. LEMAIRE, "The Sage in School and Temple", à paraître dans J. GAMMIE éd., *The Sage in Israel and the Ancient Near East*, Winona Lake, 1990.



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## THE PLASTER INSCRIPTIONS FROM DEIR 'ALLA: GENERAL INTERPRETATION

Baruch A. LEVINE

This study is the third effort on my part to investigate the inscriptions from Deir 'Alla. In the brief span of time since their publication, these inscriptions have stimulated a considerable literature from which we have all learned a great deal.

My first study was a commentary of sorts, whereas the second was an attempt to establish *Sitz-im-Leben*.<sup>1</sup> In the first study I proposed relating the themes of the Deir 'Alla inscriptions to Syro-Mesopotamian myths and omens, particularly sources pertaining to the Ishtar-Venus astral synthesis. In my view, the goddess addressed in Combination I is Shagar-we-Ishtar, a name written out fully in line 14 of Combination I. In the second Combination, I saw traces of the netherworld descriptions known from such compositions as "The Descent of Ishtar." All of this is in addition to the plentiful affinities to biblical literature.

In my second piece I proposed that the Deir 'Alla inscriptions belonged to an El *repertoire*, a body of literary creativity originally composed at various centers of El worship on both sides of the Jordan; in biblical Israel, as well as in Gilead of Transjordan. Excellent examples of such works are preserved in the Hebrew Bible, including the Balaam orations of the book of Numbers, where El has been synthesized with Yahweh, the God of Israel. I went so far as to suggest that these inscriptions might speak for Israelites in Transjordan who were El worshippers, and as such, similar to those who were the targets of Hosea's denunciations. It seems quite possible to me that some of the El literature preserved in the Hebrew Bible, especially in the Balaam orations, had actually originated in Transjordan. We can all probably agree that the Deir 'Alla inscriptions mandate a re-evaluation of the cultural climate in Transjordan during the tenth-to-eighth centuries B.C.E. The style and diction of the Deir

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<sup>1</sup> B.A. LEVINE, "The Deir 'Alla Plaster Inscriptions," *JAOS* 101, 1981, 195-205. *Idem.*, "The Balaam Inscription: Historical Aspects," *Biblical Archaeology Today*, Jerusalem: Israel Exploration Society, 1985, 326-339.



ʿAlla texts indicate a high level of literary creativity, by any contemporary standards.

Since these earlier studies appeared, basic questions relevant to the plaster inscriptions from Deir ʿAlla have been focused more sharply. There is, first of all, the question of provenance: Are these compositions regional in origin, having been composed in Gilead, or in nearby areas; or are they foreign in origin, having been imported into central Transjordan from Syria, or elsewhere? In other words, do the plaster inscriptions represent native culture, or not? In a larger sense, this is the significance of the language question, although language and cultural provenance do not precisely overlap.

Questions of dating have also occupied considerable attention, with scholarly opinion, generally supported by archaeological data, now opting for a time earlier in the eighth century B.C.E. than was initially thought. The political situation in Gilead, and in Transjordan generally, has come in for considerable attention, as well as the relative valence of the ruling, Aramean administration versus the regional population in determining the cultural climate of the area.

The present address is my response to the invitation to offer some further thoughts in the area of General Interpretation. I cannot guarantee that it will be possible to eschew details of the texts, to avoid reference to language, or to maintain my distance from all biblical associations, subjects to be discussed by others. And yet, it should be possible to revise some of my earlier impressions on the general, interpretational level.<sup>2</sup>

Permit me to propose two methodological caveats: Some of the readings suggested by different investigators should be regarded as open options; they often result from the process of elimination. Such options at times lack real significance, and need not be debated heatedly, and at any rate, cannot be verified palaeographically. We must guard against basing too much on uncertain readings, while

<sup>2</sup> See primarily the contributions of André LEMAIER, "L'Inscription de Balaam trouvée à Deir ʿAlla: Épigraphe" In *Biblical Archaeology Today*, 313-325, and that of Emile PUECH, "Response - l'Inscription sur plâtre de Tell Deir ʿAlla," *ibid.* 354-365. Also see by PUECH "Le texte 'ammonite' de Deir ʿAlla; Les admonitions de Balaam," *La Vie de la Parole, Mélanges Grelot*, Paris: 1986, 12-30. Further see S.A. KAUFMAN, "The Classification of the North-West Semitic Dialects of the Biblical Period," etc. *Proceedings, Ninth World Congress of Jewish Studies*, Jerusalem, 1988, *Panel Sessions: Hebrew and Aramaic*, 41-57.

at the same time using plausible suggestions prudently. The second point to be made pertains to the poetic, or "parallelistic" quality of these texts. I have, of course, corrected the format I initially presented wherever new information relevant to the positioning and spacing of the inscribed fragments has so indicated. And yet, I find the parallelistic alignment highly suggestive and I recommend it to others.

I intend to discuss Combinations I and II in tandem, and then deal with the relationship between them. I am intrigued by Émile PUECH's suggestion that where we again encounter red ink, in line 17 of Combination II, we may have the beginning of a new unit, possibly setting forth the functions of the diviner, in the form of a manual.

As I see it, Combination I is comprised of four identifiable sections:

1) *Lines 1-5*: the introduction of Balaam; the report of his visitation by gods who reveal to him a vision uttered by El. There follows a rhetorical dialogue between Balaam and his associates in which he announces what has been disclosed to him.

2) *Lines 5 (end)-7*: What Balaam saw and heard from El's messengers. Some gods and Shadday-beings convened a council (*mw<sup>c</sup>d = mo<sup>c</sup>ed*) and issued a decree against a goddess, who, by my interpretation, is Shagar-we-Ishtar, whose name is written out fully in line 14. She is ordered to produce celestial darkness by covering the heavens with dense cloud. She is told never to raise her voice again.

3) *Lines 7 (near end)-10*: A depiction of desolation and wilderness, with birds shrieking and wild animals feeding freely. The implication is that where domestic animals had formerly been tended, wild animals now reign.

4) *Lines 10 (near end)-16*: Beginning with the words: *šm<sup>c</sup>w mwsr* "Heed admonition!", this poorly preserved section almost defies interpretation. We will defer any attempt to identify its meaning until first engaging the better preserved, three sections which precede it in Combination I.

I lack a new contribution to each and every section of Combination I. As a matter of interpretation, I now intend to agree with those who find in Section 3 an uninterrupted list of birds and animals, and reluctantly surrender the tempting but less likely reading *nḥš wšrh* "distress and trouble" for the more likely *bny nḥš wšdh* "young falcons and the owl."



Following is my proposed rendition of Section 2, about which I will have the most to say in the context of Combination I:

tp̄ry skry šmyn b<sup>c</sup>b  
 ky šm ḥšk w'l ngh  
<sup>c</sup>tm w'l sm̄r  
 ky thby ḥt/t b<sup>c</sup>jb ḥšk  
 w<sup>3</sup>l thgy <sup>c</sup>d <sup>c</sup>tm

"Sew up, block up the heavens with dense cloud,  
 So that darkness be there, not brilliance;  
 Darkness and not bristling (?):  
 That you may instill dread, in the density of darkness,  
 And may you never raise your voice again!"

May I call your attention to the recent publication by E. REINER and D. PINGREE of parts of the Babylonian omen series, *Enuma Anu Enlil* (EAE).<sup>3</sup> When we examine the protases and apodoses, and the terms of reference recurring in these omen texts, representative of an Old Babylonian tradition, but undoubtedly the work of early, first millennium scribes in their preserved form, we gain insight into the ominous diction of this section of Combination I.

First, a word about the visibility, or shining of stars, in general: Contrast the following entries:

a) (EAE, 50-51: III:15-16):

MUL.MEŠ nam-ru ana IM.ZI.GA  
 MUL.MEŠ SAR.MEŠ-ḥu ana ZI.IM

"Bright stars are for the rising of wind;  
 Scintillating stars (*naphū*) are for the rising of wind."

b) (EAE, 50-51: III:18):

MUL.MEŠ DUL.LA ana IM.ŠUB.BA

"Veiled stars (*katmū*) are for abated wind."

Rising wind (*tību*) signals rain, which is a good forecast. (cf., EAE, 50-51: IV:10-11,13). The Akkadian verb *katāmu* is suggestive, because it variously refers to veiling, or covering by means of a garment, as well as to covering the sky with dust, smoke, or fog (CAD

<sup>3</sup> E. REINER, D. PINGREE, *The Venus Tablet of Amisaduqa, Babylonian Planetary Omens*, 1, (Bibliotheca Mesopotamica, Volume Two), Vol. One, and *Idem.*, *Babylonian Planetary Omens*, 2, *Enuma Anu Enlil* (EAE), Tablets 50-51, Malibu, CA: Undena, 1975 and 1981.



K, s.v.). This wide range of meanings for Akkadian *kalāmu* may clarify usage of the verb *t-p-r* "to sew" in the Deir 'Alla text. After all, we read in Hebrew poetry of the heavens depicted as a tent-flap, and as thin cloth. Thus, Isa. 40:22:

*hannōṭeh kaddôq šāmāyim*  
*wayyimtāhēm kā'ôhel lāsāhet*

"Who spread out the heavens like gauze,  
Stretched them out like a tent for dwelling."

Or Pss. 104:2:

*ôteh 'ôr kaššalmāh*  
*nōteh šāmāyim kayyērī'āh*

"You wrapped yourself in light like a garment,  
Spread out the heavens like a tent-flap."

Yet another Akkadian verb of interest is *arāmu* "to stretch, or place a membrane, skin, or layer of metal over an object." The following protases are instructive:

a) *Ištar* 9:4, and duplicates:

[šumma] *Ištar ina pan šatti ši-ši-tam ár-mat-*

"If in the spring of the year, Ishtar is covered by a 'membrane' -"

b) *Adad* 112:14:7:

*šumma erpetu šalimtu elāt šamē i-rim-*

"If a black cloud covers the upper sky -"<sup>4</sup>

A third Akkadian verb that is suggestive for the interpretation of our text is *adāru* "to obscure," usually occurring in the stative, in the omen texts. Cf., *EAE* 50-51: II:7c:

*DIL.BAT ina ITI.APIN a-dir*

"Venus in month VIII is obscured."

It is significant that in Akkadian, derived forms of the verb *adāru*, such as *adirtu*, for instance, mean both "darkness" and "misfortune, calamity."

The point to be made is that Mesopotamian omen literature uses comparable diction to that of the Deir 'Alla inscriptions, and to

<sup>4</sup> *Apud CAD* A II 229, s.v. *arāmu*, d.

that of the Hebrew Bible, in describing celestial phenomena. We are warranted in concluding, in literary terms, that the diction of omen literature of various sorts resonates in the Deir ʿAlla inscriptions, and helps us to establish their meaning.

At some risk, I would like to comment on the still uncertain reading *smr* in line 7, which by virtue of its parallel position should, I think, constitute an antonym to *ʿtm*, and consequently, should connote "light", in some sense. The reading *ʿtm* is pretty well accepted, though the interpretation of this vocable is still being debated. A relationship to Akkadian *eṭū* (adj.) "dark" (and related forms) is surely logical, but it is less certain how to explain the final *Mem* of *ʿtm*. Others derive *ʿtm* from other roots, while agreeing on the meaning "darkness."

Now, if the reading *smr* is viable, then this vocable may be related to Hebrew-Aramaic *s-m-r* (cf. *s-m-r*), "to bristle, stand up like hairs, nails, etc." (Pss. 119:20, Job. 4:15, and in Late Hebrew). It may be relevant that Sumerian MUL.MUL "stars" at times refers to the Pleiades, and has the Akkadian value *zappu* "the Bristle", in that context.<sup>5</sup> The point is that visible features of heavenly bodies can be referred to as "bristling."

While I am discussing the diction of celestial omens, I would like to call attention to a series of consecutive entries in EAE 50 = 51, IV:65 regarding the astral "profile" we might say, of Ishtar-Venus. I do not fully understand these entries but I sense their relevance:

MUL.ŪŠ A. KE<sub>x</sub> ana NAM.BAD.MÈ.ŠUB.BA  
DIL.BAT ina dUTU.ŠŪ.A IGI-ma zik-rat  
MUL.SAL.A. KE<sub>x</sub> ana NAM.SAL.TUK ana ŪŠ.MEŠ ul-lu-di  
DIL.BAT ina dUTU.È IGI-ma šin-ni-šat

"The Star of Men is for pestilence.

Venus is seen in the West - she is male.

The Star of Women is for taking a wife [...] for giving birth to males.

Venus is seen in the East - she is female."

These statements express the Ishtar-Venus astral synthesis whereby the aspect of fertility associated with Ishtar is fused with the aspect of celestial brilliance. Ishtar-Venus is hermaphroditic, and her female aspect is that of fertility, whereas the male aspect is negative, "anti-life," so to speak.

<sup>5</sup> See CAD Z, s.v. *zappu*.



I see no dichotomy, therefore, between Ishtar as depicted in "The Descent of Ishtar," whose incarceration in the netherworld, and absence bring all human and animal fertility to a halt on the one hand, and the astral phenomenology, on the other. The fusion expressed in the omens clarifies the composite, divine name *šgr w<sup>c</sup>štr*, as it was originally explained by Prof. HOFTIJZER, as expressing the hypothesis of fertility conveyed by the verb *š-g-r* "to issue, give birth."

Usage of the noun *hī[t]* (or: *hī* = Hebrew *hāt*) "dread" in the Deir 'Alla texts correlates well with the biblical diction, as we read in Jer. 10:2:

*ūme<sup>c</sup>ōtōt haššāmāyim ʿal tēhātū*  
*kīy yēhattu haggōyīm mēhēmāh*

"And do not be in dread of the celestial omens,  
 Let the nations be in dread of them!"

The above analysis of Section 2 raises the question of the professional roles attributed to Balaam in Combination I. In Syro-Mesopotamian magical literature, such roles are more clearly designated by official titles and classifications, although inevitable overlapping of functions, and the common utilization of practices are also evident there.

Whereas the vision of celestial darkness, as it is expressed, recalls the functions of the biblical *mē<sup>c</sup>ōnēn* "cloud-observer," a term often occurring together with *qōsēm* "diviner," (actually used with reference to Balaam in Jos. 13:22; cf., Deut. 18:10, 14), Balaam's relationship, both to the gods and to his listeners makes him a veritable *hōzeh*, a function also mingled with that of the *qōsēm* in biblical literature, as we read in Micah 3:6-7:

*lākēn lāylāh lākem mēhāzōn*  
*wāhḥāšēkāh lākem miqgesem*  
*ūbā<sup>c</sup>āh hāššemeš ʿal hannēh<sup>c</sup>īm*  
*wēqādar ʿālēyhem hayyōm*

"It shall be night for you without visions  
 And darkness for you without divination.  
 The sun shall set over the prophets,  
 And daytime shall be darkened for them."<sup>6</sup>

What I find poignant here is the suggestion that false prophets rely

<sup>6</sup> I have taken liberty with the Masoretic pointing, to render the reading smoother.



on a starlit sky and celestial omens, and that they will be sorely disappointed!

Reference to the verb *h-z-h* brings me back to line 1 of Combination I, in section 1 of my outline. I continue to insist that the title *hzh* (= *hōzeh*) occurs there, and find difficulty with assuming a relative clause  $\text{ʔš } hzh \text{ ʔlhn}$  for independent reasons. Taking *hʔ* as an exclamation before a verb in the consecutive tense, *wyʔtw*, strikes me as jarring, stylistically. The problem of where the red ink ends has, I have been informed, been solved by measuring the lengths of those lines wherein it appears. The red ink, it seems, reaches to precisely one-half of the length of the line, and resumes right below, extending over the second half of the second line. It may have no syntactic implications at all. Although we have in Exod. 24:11b: *wayyehēzū ʔet hāʔēlōhīm* "They beheld God" (cf., *ibid.*, vs. 10), the diction of Deir ʿAlla suggests that the object of *h-z-h* is *mhzh* "vision," as we read in line 1: *[w]yhz mhzh* "He beheld a vision" (cf., Num. 24:3,16). I prefer, therefore, to sustain the titulature here, and I have already documented the emphatic syntax required to generate:  $\text{ʔš } hzh \text{ ʔlhn } hʔ$  "He is a divine seer" (cf., Lev. 13:44, I Kings 13:26, Zech. 13:5). I should also mention that *mšʔ* (= Hebrew *maššāʔ* "forensic vision") serves as the direct object of the verb *h-z-h* in biblical diction (Hab 1:1, Lament. 2:14).

To continue the discussion of roles, it is clear that Balaam is principally a *hōzeh* in the Deir ʿAlla text by virtue of the fact (a fact obtaining whether or not we can agree on the syntax of line 1), that he beheld and heard divine visions. These visions, in addition to informing him of the "actions" (*pʿlt*) of the divine *mwʿd*, also included a depiction of celestial darkness strongly reminiscent of Syro-Mesopotamian celestial omens, some specifically relevant to Ishtar-Venus herself. Whereas we might say that omens appear as automatic, impersonal and objective, the spirit of Balaam's visions expresses divine will and authority. This raises a question endemic to the relation between astrology and religion, namely, the role of the gods (or of God), in determining the position of the stars and the other heavenly bodies. In biblical literature this question was finally answered in Isa. chapter 40, a product of exilic times. But the power of gods, individually and collectively, to assign the heavenly bodies to various positions, and to darken them by eclipse and by means of clouds, was hardly an exclusively monotheistic notion!

Second 3 in my outline of Combination I appears to me to be part

of Balaam's transmission, and in the context of omen literature, functions as an apodosis of sorts. Celestial darkness, as a punishment for some acts by the goddess Shagar-we-Ishtar, means that there will be desolation in the land. This situation is predicted, or projected, somewhat symbolically, somewhat realistically. At the very beginning of the effort to interpret Deir 'Alla, Prof. HOFI-JZER had already cited the extensive biblical sources on the role of birds and wild animals in descriptions of disaster and desolation. I merely follow his lead in this matter.

My understanding of lines 1-10 (near end) of Combination I, what I have charted as Sections 1-3, may be summarized as follows: The <sup>2</sup>lhn who appear to Balaam were sent by El to warn Balaam's people of impending disaster. The prediction is expressed as an edict pronounced by a divine council (*mw<sup>c</sup>d*) over the goddess, Shagar-we-Ishtar who has acted against some of the gods and who is being punished. Her punishment, projected in terms similar to the protases and apodoses of Syro-Mesopotamian celestial omens, some pertaining to Ishtar, herself, equates darkness with desolation. The goddess is not permitted to shine. On earth this condition is dramatized by reference to the frenzied movements and shrieking of birds and the abandonment of grazing land to wild animals.

Before attempting to relate Section 4 to these first, three sections of Combination I, permit me to comment that it should not surprise us when we encounter reflections, or versions of Syro-Mesopotamian genres in West Semitic languages such as Aramaic, Hebrew and regional dialects of various sorts. Long ago, W.F. ALBRIGHT noted a passage from the Neo-Assyrian *utukkê limnûti* magical series, translated almost literally in an inscription from Arslan-Tash, composed in a West Semitic dialect.<sup>7</sup> In Ugaritic we have West Semitic renditions of *šumma izbu* omens.<sup>8</sup> There should be no problem, historically or culturally, in concluding that magical sources from the classical omen literature of Babylonia would be known in some form to eighth century B.C.E. writers on either side of the Jordan.

Now, let us turn to the last section of my outline, which I see as beginning in line 10 with the words: *šm<sup>c</sup>w mwsr gry š[gr w<sup>c</sup>štr]*

<sup>7</sup> See W.F. ALBRIGHT, *BASOR* 76, 1939, 5-11.

<sup>8</sup> See the contributions of A. HERDNER, "Nouveaux Textes alphabétiques de Ras Shamra" in *Ugaritica* VII, Paris; 1978, "Présages" pp. 44-63.



(pace, PUECH and others, who divide the text in different ways). How shall we read this statement? To me, there is a symmetry between this statement, and the one in line 13, below: *wšm<sup>c</sup>w ḥršn mn rḥq*, which I translate: "Hear incantations from afar!" I therefore translate the former, opening statement "Heed admonition!" At this point, a real difference in possible interpretations arises, one which could affect our overall understanding of how Section 4 relates to the first three sections of Combination I, as I have outlined them.

a) "Heed the admonition of the adversaries of Shagar-we-Ishtar."

b) "Heed admonition, oh adversaries of Shagar-we-Ishtar!"

Option (a) bids the listeners obey the admonitions of the enemies of the goddess, whom I take to be the gods and Shadday-beings of the inimical *mw<sup>c</sup>d*. The adversaries would be the admonishers! Option (b) makes someone else the admonisher, and bids the enemies of the goddess obey the admonition.

Who is the speaker? To me, it makes better sense to regard Balaam as the speaker, and to posit that his oration simply continued. Balaam would be doing what he is best known for in biblical tradition—he would be pronouncing execrations, if my reading of line 13 is deemed preferable to a reference to the deaf (*ḥēršn*). This model suggests that Balaam is attacking the adversaries of the goddess. After all, he is severely distressed to hear that an edict has been issued against her. He is depicted in Section 1 as empathizing strongly with "his people" (*mh*) and eager to warn them of impending disaster. It would be in character for him to attempt to defend his people by rescuing their goddess.

I wish I could be more certain of the contents of Section 4. In line 14 we read clearly *wkl ḥzw qqn* which likely means: "And all beheld acts of oppression." But I question the syntactic analysis which makes the goddess, whose name appears fully, the object of a possessive construction: "And all beheld the oppression of Shagar-we-Ishtar." The author of the Deir ʿAlla texts knew how to express the masculine plural construct in normal ways. More likely, the name *šgr w<sup>c</sup>štr* begins a new clause in line 14, relating something about the goddess, herself.

All I can offer is the observation that cultic and magical activities are being carried on in Section 4. In line 11, we have two professional titles: *rqḥt mr* "perfumer(s) of myrrh," and *knhh* "priestess." Nobody disputes *knhh*, and I prefer a title, rather than an active participle for *rqḥt* (cf., Hebrew *hāraqqāḥ* in Neh. 3:8, and feminine plural

*raqqāhōt* in I Sam. 8:13). Much less obvious are such possible terms as *ḥkmn* "skilled practitioners," *nyh* "oracle" (rather than "poor woman") and *ḥšb* "craftsman," whether *nš* <sup>2</sup>*zr qm* means "bearer of an offering in a horn," or: "bearer of a horned belt," is, of course, uncertain, as is the sense of *mšn* <sup>2</sup>*zm* nearer the end of the section.

Some have argued that in ancient Near Eastern myth and magic diviners could not take on the gods, or act against them, and that Balaam would not be given an heroic role of this kind in the Deir 'Alla inscriptions. What we have here is something more complex: El, the supreme god of the Deir 'Alla inscriptions, acts to warn the people, through Balaam, of impending disaster. That disaster was decreed by a *mw<sup>c</sup>d* or other gods. There is, therefore, conflict among the gods, themselves, and in championing the cause of the goddess Shagar-we-Ishtar, Balaam is aligning himself with El and his messengers against the inimical gods of the *mw<sup>c</sup>d*.

Let us now turn to Combination II.

I would like to pursue the suggestion I first made, that *nqr* in the second Combination means "corpse," based on a comparison with Isa. 14:19: *kēnēšer niṭ'āb* "like abhorrent carrion," parallel in sequence to *kēpēqer mūhās* "like a trampled corpse." I once mentioned a cognate Aramaic-Syriac vocable *nēslā*<sup>2</sup>. JoAnn HACKETT questioned my interpretation by noting that in Aramaic, the postulated phoneme *Ḍod*, required to produce the Deir 'Alla term *nqr*, would not be represented by *Ṣadē* in the Aramaic dialects. Upon further examination, I discovered that lexicographers had, indeed, confused the situation, and that Aramaic *nēslā*<sup>2</sup> was actually a variant of another verb *n-z-l* "to flow, run," and was irrelevant to my discussion. It turns out that all of the relevant forms I have considered are Hebrew, where postulated *Ḍod* is often realized as *Ṣadē*, after all.

A note in the New Jewish Version of Isa. 14:19 refers the reader to post-exilic *nēšēl* "putrefying flesh, or blood," as justification for not rendering *nēšer* as "offshoot," by extension "scion, offspring" (cf., Isa. 11:1). Context alone would recommend positing two vocables:

- 1) *nēšer* I "offshoot," cognate to the Arabic verb *naḍara* "to be verdant, to shine, grow."
- 2) *nēšer* II "carrion, dead flesh," a phonetic variant of post-biblical Hebrew *nešēl*, and cognate to the Arabic verb *naḍala* "to pull back, tear



off," as is said of "drawing a sword or selecting an arrow from the quiver"; "to extract."<sup>9</sup>

Let us examine the Late Hebrew form, *nēšel*, also written *nāšāl*. In Mishnah, *Nazir* 7:2 (also *ʿAhiot* 2:1). This word appears in a clear context:

"Over which sorts of impurity is the Nazirite required to shave (Num. 6:8f.)? Over a corpse, over the equivalent of an olive from a corpse, over the equivalent of an olive of *nēšel* and over a large, ladle-full of bloodied soil."

The law of Numbers 6 provides that if a Nazirite accidentally comes into contact with a corpse during the term of his vow, he must begin all over again, shave and bathe, etc. What constitutes a sufficient substance to interrupt his votive term? Not only a corpse or a part of one, we are told, but also *nēšel* and bloodied soil. The Talmud of Jerusalem, *ad loc.*, *Nazir* 9:2 explains *nēšel* as follows: "What is a *nēšel*? Flesh from a corpse which has become detached (Hebrew *šenut-taq*) and [bloody] liquid that has congealed."

There are, in fact additional Late Hebrew forms derived from the root *n-š-l*. There is a feminine form *nešūlāh* "refuse, what is cast off" (cf., MAIMONIDES, *Code, Terumot* 11:13: "The waste-product (*nešōlet*) of rotten parts of priestly gifts.")

But, we need not venture so far because I identify another biblical form akin to *nēšel* of Isa. 14:19, and refer you to Isa. 49:6:

*wayyōʿmer: nāqēl mihyōtēkā liy ʿebed lēhāqīm ʾet šibtēy Yisrāʾēl*  
*ūneširēy* (Qere: *ūnešūrēy*) *Yisrāʾēl lēhāšib*

"Is it of so little import that you act as my servant, to reconstitute the tribes of Israel, and to bring back the *cast-offs* of Israel?"

Usually, *neširēy/nešūrēy* has been derived from the verb *n-š-r* "to guard," and this verse has been interpreted with reference to prisoners and captives. Some have suggested, and I agree, that we actually have a vocable deriving from the same root as *nēšel* "carion, corpse." The unifying factor is that of "detachment," said of

<sup>9</sup> See IBN MANZUR, *Lisān al-ʿarab* Beirut 1956 v. 2 p. 663, s.v. *Naḍala*. Stem V, *tanaḍḍala* means: "[to remove] a sword from its scabbard," and the same meaning is attested for the dialectal variant *tanaṣṣala*. Stem V also has the extended connotation "to get out of something," as to get out of a sin or evil deed. I am indebted to my colleague at New York University, Prof. Michael CARTER, for directing me to this reference.

dead, putrefying flesh, and of abandoned, or exiled human beings. The context of the servant passage certainly suggests this because in the continuation, Israel is characterized as follows: *libzôh-nepeš limēlô<sup>c</sup>āb gôy*—"to the despised person, to the abhorred nation." Here, the cast-off is *mēlô<sup>c</sup>āb*, whereas in Isa. 14:19, carrion is *nif<sup>c</sup>āb*!

I prefer this interpretation to concluding that *nqr* means "descendant," and/or that Combination II depicts child sacrifice. The affinities to Isaiah, chapter 14, are in my opinion, compelling, as is the similarity of diction between our text and "The Descent of Ishtar." This persuades me that in Combination II we have a corpse languishing in Sheol; more precisely in a necropolis, or netherworld (*byt<sup>c</sup> lmn*) built by El, himself.

The question now poses itself as to whether Combination II follows topically upon Combination I, or to put it another way: Are both Combinations speaking of Balaam, or of the same prediction?

I am grateful to André LEMAIRE and to Émile PUECH for their painstaking attempts to resolve this question by reconstructing the physical position of the fallen plaster fragments within the structure at Deir 'Alla where they were discovered.

According to LEMAIRE, Combinations I and II were written on plastered surfaces lateral to each other, and for this reason, as well as for others based on content, we need not conclude that the two Combinations are topically related to each other. According to PUECH and others, the two Combinations stood above and below each other, more precisely—Combination II was below Combination I, in the same column, and was sequential to Combination I.<sup>10</sup>

These discussions have re-opened the overall question of the relationship between the two Combinations.

We would do well to re-examine the readable content of Combination II. I find only two clues to an oracular function, and the pronouncement of execrations that might suggest that the corpse of Combination II (or the scion, for that matter) is, indeed, Balaam. At the present time, the name of Balaam cannot be read with any assurance in Combination II, and there is no readable clause or group of words where this name is required, or where its absence can be assumed. The two clues I find are as follows:

<sup>10</sup> A. LEMAIRE, "La disposition originelle des Inscriptions sur Plâtre de Deir 'Alla", *Studi Epigrafici e Linguistici* (SEL), 3, 1986, 79-93. Also see E. PUECH, "Admonitions de Balaam", etc. *Le Monde de la Bible*, 46, 1986, 36-38.



1) in line 9 I read:

*hlʿsh bk lytʿs*  
*ʿw lmlkh lytmlk*

I originally translated as follows, and I see no problems with this translation now:

“If it is for counsel, no one will consult you!  
 Or for his advice, no one will take counsel!”

2) The second clue comes in line 17:

*ldʿt spr dbr lʿmh*  
*ʿl lʿn lk n/mʿpʿt*  
*wmlqb ʿmr*

“—To know how to transmit an oracle to his people,  
 You have been judged for your speech,  
 And [banned] from pronouncing words of execration.”

The sense may be that someone has been deprived of the gifts of the diviner, thus reinforcing the suggested meaning of line 13, as pertaining to one such as Balaam. If, however, PUECH is correct that line 17 begins a new unit, this passage would lose its relevance for defining the relationship between Combinations I and II. We would then be left solely with the statement in line 13, which could just as well be understood as part of the generally moribund description of the netherworld, where the dead never sense emotion or perform any useful function!

I now seriously doubt that Combination II is topically sequential to Combination I, which is to say that it should be understood as recounting the assignment of Balaam to Sheol as punishment for his actions performed in Combination I; or that the contents of Combination II relate to the goddess punished by the *mwʿd*, or to celestial darkness and desolation. I agree with the analysis of PUECH, and with my own original hunch, that the introductory statements of Combination I functioned as the general title of all that followed, in our two Combinations, as well as in what might have been intended for other sections. What we have in the plaster inscriptions is a collection of Balaam's orations, the *spr* of Balaam, son of Beor, who was a divine seer (*hzh ʿlhn*). It is not entirely clear how much of what Combination I says about Balaam is part of the overall introduction to the *spr*. A conservative view would be that only the initial statement served as the title: (*zh/ysry*) *spr blʿm br bʿr ʿs hzh ʿlhn*

*h*<sup>2</sup> "This is/ the admonition of the *recorded collection* of Balaam son of Beor; he is the divine seer!" As is true of biblical collections of prophecies attributed to a single prophet, separate orations may cover a range of subjects. In our case, Combination I preserves one prophecy, and Combination II another, in which the netherworld is depicted in language reminiscent of Isaiah, ch. 14.

There is, however, indication of what may turn out to be a further relationship between the two combinations, and I have already alluded to it: The real link is expressed by the presence of El, and by descriptions of his acts, as found in both Combinations. In Combination I, it is El's *maššā*<sup>2</sup> "forensic vision" that is revealed to Balaam, and in Combination II, it is El who, after satiating himself with lovemaking, builds a netherworld which is then depicted so dramatically. The two combinations (and possibly additional inscriptions) belonged near each other in the structure at Deir 'Alla, whatever its precise function was, because they were part of the El *repertoire* of Deir 'Alla. El is a deity who shows concern for human beings; the preparation of a proper Sheol is also an act of concern! For me, this proposed literary provenance represents the most suggestive aspect of General Interpretation to have emerged.



## RESPONSE TO BARUCH LEVINE AND ANDRÉ LEMAIRE

Jo Ann HACKETT

Let me say first that it is a pleasure to be here and be asked to respond to the papers by Prof. LEMAIRE and Prof. LEVINE on the general interpretation of the plaster text(s). We have had two very different general interpretations offered to us today and I will respond to Prof. LEVINE's first.

### LEVINE

I have greatly appreciated what Baruch LEVINE wrote in his two previous articles about Deir 'Allā: his explication of underworld language in his *JAOS* article of 1981, and his use of the "Descent of Ishtar" there; and his attempt to align Deir 'Allā with pre- or post-Assyrian North Israel, and his suggestions about the 'El cult in his article in the book *Biblical Archaeology Today*. He is today, as he says, "discussing the diction of heavenly omens," and has again given us a great deal of useful information for comparison with the "diction" of the Deir 'Allā text.

I was particularly pleased with LEVINE's several approaches to the problem of "sewing up the heavens" in line 6 of the first combination. I found the comparison to biblical passages that portray the heavens as a veil or a tent, some kind of cloth or garment, more helpful than the comparison with the Akkadian *katāmu*; I have translated *tpr* "sew up" without ever really understanding it, and now LEVINE has provided a context that makes sense of the Deir 'Allā command. My foray into the Chicago Assyrian Dictionary, however, produced less satisfactory results than LEVINE's paper led me to expect. He says, "The Akkadian verb *katāmu* is suggestive, because it variously refers to veiling, or covering by means of a garment, as well as to covering the sky with dust, smoke, or fog . . . This wide range of meanings lends clarity to [the] usage of the verb *tpr* 'to sew' in the Deir 'Allā text." *katāmu* is, of course, defined as LEVINE says: first of all, "to cover with garments"; and besides that, "to cover with dust, sand, [to cover] (the sky) with smoke, etc."<sup>1</sup> There

<sup>1</sup> CAD K 298.

is a perfect opportunity here for passages that suggest combining those two definitions and covering the sky as with a garment, leaving us a garment in the heavens that might be equated with the sky or that might be sewn up. I think, in fact, we need to find that *combination* for *katāmu* to be useful to us. Otherwise, all we have is a verb that means "to cover," sometimes with a garment and sometimes covering the sky, but with no intrinsic connection implied between the "garment" part of covering with a garment, and the "sky" part of covering the sky. But in fact, none of the passages that the CAD quotes does precisely make that connection.<sup>2</sup> I did not look beyond CAD and AHW, but in neither place were there any passages quoted with garments precisely in the heavens, or garments that were taken to be the sky. The biblical passages were, however, as I said earlier, quite nice: the heavens stretched out like a tent (Isa 40:22) and like a tent-flap (Ps 104:2), and I do not think anyone has pointed them out before.

Where LEVINE discusses the words *ṣm* and *smr* I am less interested because I do not read either of those words where he does, but I would make a minor point. MUL.MUL is not "the bristle"; it just means "stars," and was the Sumerian name for what we call the Pleiades. In Akkadian, *zappu* does mean "the bristle" and it is used of the Pleiades, but when HARRA-ḫubullu equates the two, it does not equate MUL.MUL with *all* the meanings of *zappu*. It only equates them when *zappu* means the Pleiades. When *zappu* means cone or bristle, there are completely different Sumerian equivalencies for *zappu*.<sup>3</sup>

Let me move on to LEVINE's suggestions about the section immediately following the direct address to a goddess, and continuing until the end of what we have of Combination I. LEVINE suggests that the command to the goddess and the "birds" section serve as a sort of protasis and apodosis, placing Combination I in the context

<sup>2</sup> A sampling of the passages cited in CAD K 300 include: "to cover (the sky) with smoke, etc.": the wide extent of the sky was covered (by the dust) as by a heavy fog OIP 2 44 v 59 (Senn.); a red cloud arose and covered a red cloud CT 23 37:65 (inc.); [if the sky] is covered and the south wind blows ACh Šamaš 2:2; I covered this province like heavy evening clouds TCL 3 253 (Sar.); I covered this city like a cloud Lie Sar. 211; you Šamaš cover (everything) like fog Lambert BWL 128:39; When you (Šamaš) rise the stars of the sky are outshone [katmū] for the entire day KAR 105:5; His light covers all the cities AfK 1 24 r.i 11; Šamaš your torch covers the lands KAR 32:33.

<sup>3</sup> CAD Z 49-50.



of the diction of omen literature. If I understand correctly, LEVINE's goddess, Shagar wa-Ishtar, is commanded to punish herself for some outburst by sewing up the heavens, covering them with a cloud, and instilling dread in her people, the folks of Deir 'Allā. This is the "protasis" of LEVINE's interpretation, the "if" clause. "If the heavens are sewn up, and the skies covered with cloud, . . ." There follows the apodosis, the "then" clause: LEVINE says, "[this section] appears to me to be part of Balaam's transmission, and in the context of omen literature, functions as an apodosis of sorts." This apodosis begins: "Because" (the word is *kī*, which seems an odd way for an apodosis to begin)—"because" animals and birds will do all sorts of odd things. He describes this section, as I have also, following McCARTER, as a sort of "reversals" passage, so that some sort of ideal order is being violated. This is not to say that the violations are unheard of. I once published the opinion that a small bird's chasing a large bird would be "unnatural," a reversal of the natural order, but since that time I have moved away from the city to the countryside, just outside Boston, and I have seen more birds than I saw in many years of living in cities. I can testify that there is nothing at all "unnatural" in small birds' chasing large ones, especially in the nesting season, but such a sight might still have seemed a reversal of the most common order. After all, even living in cities, one sees the sun covered by clouds; there is nothing unnatural about that, but it could certainly be seen as "ominous," as LEVINE has documented, and as a reversal of the way things should be. I think perhaps our use of our own language on this interpretive point (certainly in my case, anyway) has made difficult a fairly simple line of thought.

At any rate, I find LEVINE's protasis/apodosis language less illuminating of this portion of Combination I. Much rides on exactly what the *kī* toward the end of I,7, means: the *kī* just before the "birds" section is a crux of interpretation, and Prof. HOFTIJZER will address this issue also. Saying that I find the protasis/apodosis language unsatisfying is just another way of restating that I think the *kī* of line seven gives the *reason* for the covering of the sky and not the *consequences* of that covering. The most obvious reading of *kī* suggests that the following clause is causal, of course,<sup>4</sup> and after all, it is not

<sup>4</sup> As do the verb forms. I read the verbs in the section following the *kī* as a series of prefix-conjugations and suffix-conjugations, although all but two of my suffix-

any more authentic to have a scene of chaos represent the *desolation* after the punishment than it is to have chaos and the crossing of perceived natural boundaries represent the *reason* for divine anger. The early stories in Genesis are proof enough of that.

For me, one of the least satisfying of LEVINE's translations is his "Hear incantations from afar!" in line 13. Several of us have translated "the deaf hear from afar" or some such. The deaf hearing from afar is such a lovely and sensible phrase, in context, and why *incantations* should come from afar eludes me. But the deaf hearing does not fit into LEVINE's interpretation at this point in Combination I, because LEVINE (and others<sup>5</sup>) breaks off the "chaos" section in line 10 and sees the rest of Combination I, as far as we can make any sense of it, as a warning addressed to the Shadday-gods, who are in LEVINE's reading the enemies of his goddess. Let me address several issues that come together here in this section.

I have said before, in response to a different paper at a different conference, that I find the parallelism of lines 5/6 of Combination I compelling: "The ʾIlāhīn gathered together, the Šaddayyīn took their places as the assembly." That has always looked to me as though the ʾIlāhīn and the Šaddayyīn are the same group, especially in light of the most likely reading further back in line 5, again parallelistic: "Sit down! I will show you what the Šadda[yyin] (almost certainly) [have done, or some such]; Come, see the works of the ʾIlāhīn!" I would simply caution against seeing two separate and warring groups of gods here. And as long as we are on the subject of gods, I think it is also risky to interpret the words šgr.w<sup>c</sup>štr in line 14 as a divine name. The context is not merely broken: it is

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conjugations could also be read as participles. If one reads the lines following the direct address to the goddess as one section, there is at least one suffix-conjugation in it, *hqrqt* in line 15, and many of us read several more. One alternative reading would be to fit the suffix-conjugation(s) into a sort of "prophetic perfect," i.e., "things that will most certainly have happened," in order to make the *ki* passage represent what will go on after LEVINE's goddess is punished and as a consequence of that punishment. LEVINE's interpretation divides these lines into two sections, with the result that *hqrqt* is not part of his "apodosis." He reads the possible suffix-conjugation verb forms in his "apodosis" as participles or as nouns. I find it easier to take this piece as one longer section, and to translate such a series of verbs as habituais and perfects, or perhaps even present tense for the ones that might be participles, that is, things that are happening or have been happening.

<sup>5</sup> E.g., J. HOFIJZER in J. HOFIJZER and G. VAN DER KOOIJ, *Aramaic Texts from Deir ʿAlla* (Leiden: Brill, 1976) 179, 209; H. and M. WEIPPERT, "Die 'Bileam'-Inschrift von Tell Dēr ʿAllā," *ZDPV* 98 (1982) 98, 103.



broken and utterly confusing at the same time. I grant that the name of the goddess who is addressed in this section begins with  $\dot{s}$  or  $\dot{s}$ , but there is no room for the entire double name in line 6. One might suggest filling in just the first half,  $\dot{s}[gr]$ , as some have, but this is a goddess we know virtually nothing about, and so appealing to her here has never been very satisfying. Perhaps she will turn out to be the West Semitic seamstress-god, and will fit in to our text perfectly.

Several scholars' readings,<sup>6</sup> as well as LEVINE's, propose in one or two places in lines 7 and following, that we should read a list of nouns rather than a mixture of nouns and verbs. For instance, LEVINE reads in lines 7-9: "For the swift and crane will shriek insult to the eagle, and the voice of vultures will resound." So far we have real sentences. Then we have: "young falcons and the owl, the chicks of the heron and the sparrow, a cluster of eagles, pigeons and birds of . . ." (broken). To use LEVINE's own words in another context, I find this absolutely "jarring": in the middle of a perfectly reasonable narrative with subjects and verbs, we have a long list of birds and no possibility of an explanation or much of an introduction. There is a lacuna between the sentences and the list, but it could only contain about 5 letters. What precisely is this list doing here? Why opt for a list anyway, when we can easily make verbs out of two of those birds and have real sentences instead of an unconnected list? I would make the same complaint about all those professional titles further down in Combination I (lines 11-12). Because it is possible to read several of the combinations of letters as magical titles, LEVINE<sup>7</sup> has presented us with another list that continues even through at least one and perhaps two rather large lacunae. Again, why prefer a list, no matter how conveniently magical, when you can just as easily translate sentences that make sense, especially in tandem with the earlier "chaos" passage of birds and animals? My own preference is for sentences.

Baruch LEVINE and I have gone round and round about *nqr* in Combination II. I believe 1989 is the 10th anniversary of our first conversation about it. And I am afraid I will have a few remarks to make on this occasion also.

<sup>6</sup> E.g., J. HOFTIJZER, *op. cit.*, 179-80; E. PUECH, "Le texte 'ammonite' de Deir 'Alla: Les admonitions de Balaam (première partie)," *La Vie de la parole de l'Ancien au Nouveau Testament: Etudes d'exégèse et d'herméneutique bibliques offertes à Pierre Grelot* (Paris: Desclée, 1987) 359; H. and M. WEIPPERT, *op. cit.*, 103.

<sup>7</sup> See also J. HOFTIJZER, *op. cit.*, 180.

LEVINE begins by pointing us to the New Jewish Version English translation of the Bible at Isa 14:19. They translate: "While you were left lying unburied, like loathsome carrion, like a trampled corpse . . ." The Hebrew word that is here translated "carrion" is *nēšer*. The editorial note at "carrion" says: "So several ancient versions; cf. postbiblical *nēšel*, 'putrefying flesh or blood.'" LEVINE wants to use this verse in Isaiah, where it is possible that *nēšer* should be translated as a word parallel to corpse, to justify his translation "corpse" for *nqr* in Deir 'Allā Combination II.

First of all, while the NJV translators are correct to point to the ancient versions for justification of their rendering "carrion" (some Greek versions have *νεκρος*), they are on shakier ground when they point to Mishnaic *nēšel*. There are a few instances of *r/l* interchanges in Hebrew and Aramaic, but they are rare and hardly a solid foundation for a new translation. The ancients who translated *nēšer* "carrion" did so, I would think, because they knew a word *nēšer* that meant carrion; or because they, like the NJV translators, felt the context required it; or because they read, or corrected to, *nēšel*. Other versions read, or corrected to, *nēpel*—"abortion." There is some possibility that this proposed "carrion" *nēšer* later evolved into Mishnaic *nēšel*, but that is a linguistically shaky suggestion.

On the idea of etymologizing Mishnaic *nēšel* from the root \**n-š-l*, *našala* in Arabic, I would simply say that that Arabic root looks suspicious to me. I assume Prof. LEVINE found it in one or more dictionaries, and I found it in one also, listed as confused with *našala*, just as LEVINE reports. But a mix-up in Arabic does not provide an etymology for a Northwest Semitic word, in the first place, unless it bespeaks a mix-up in Proto-Semitic. Secondly, Arabic dictionaries have been known to lie—that is, to get information from a badly pointed manuscript, with the result that new roots appear, or new meanings for roots, that never really existed. So one would want to consult an expert in Arabic manuscripts to find out whether *našala* probably ever meant much the same as *našala* (*šād* and *šād*, of course, look the same in an unpointed manuscript), or whether that was a mistake in the dictionaries.<sup>8</sup>

Furthermore, I still would argue it is not prudent to re-etymol-

<sup>8</sup> Regardless of the results of such a search, the first point still holds, that a confusion in Arabic is not a confusion in Proto-Semitic and need have no impact on a given Northwest Semitic dialect.



ogize Mishnaic *nēšel*. It has a perfectly good etymology in the traditional one that takes it from \**n-š-l*, and I do not think the re-etymologizing is necessary to LEVINE's argument. In other words, there is no reason not to suggest that Deir 'Allā *nqr* meant "corpse," based on the passage in Isa 14:19 where a word *nēšer* seems to mean something like "corpse" (and perhaps supplemented by Isa 49:6). I would say that translating *nqr* as "corpse," in line with all the rest of the underworld language in Combination II, is perfectly reasonable. I am only quibbling over the details. The trouble is that we do not know whether the *š* in this proposed Hebrew *nēšer* (= corpse) would come from original dotted *edh* and so would be eligible as a cognate for Deir 'Allā *nqr*. Any attempt to etymologize this *nēšer* based on Mishnaic *nēšel*, however, is probably ill-advised for LEVINE's argument, and for two reasons: 1) the *r/l* interchange is rare in the first place; and 2) Mishnaic *nēšel* is most easily explained as from a root with original *š*, not original dotted *edh*. Original *š* would, of course, not yield *q* at Deir 'Allā.

So that is the 1989 version of the LEVINE-HACKETT *nqr* saga.

In a response of this kind, we seem always to concentrate on where we disagree and not on where we agree, so let me conclude my discussion of LEVINE's paper by reiterating what I said at the beginning: that he has given us new information for understanding some of the more veiled passages in our inscription, and even more, he has made suggestions for interpretive contexts that help us to make sense of larger sections of the inscription, even when we do not fully understand their constituent parts.

#### LEMAIRE

As I turn to Prof. LEMAIRE's paper, I should point out that my remarks are based on a three-page summary that was provided to me before the conference.

Prof. LEMAIRE was, of course, one of the first commentators on the plaster text(s) from Deir 'Allā, publishing along with André CAQUOT an article in *Syria* shortly after the appearance of the *editio princeps*, and he has been one of the most prolific commentators so his contributions to our understanding of Deir 'Allā have been legion. We have all learned a great deal from his work, not only on the general interpretation of the inscription, his topic today, but on the details of placement of the plaster, on the grammar, and on the

possible function of the plaster inscription. I will comment briefly on just a few of the topics he has brought before us today, and, inevitably, I will concentrate on those topics where we disagree.

My first observation is that I believe Prof. LEMAIRE has stretched the evidence too far in his attempt to see the Deir 'Allā text emanating ultimately from the Aramean Kingdom of Damascus. The weakest link in this entire presentation is, to my mind, his theory, which he has presented more fully in other contexts, that the Deir 'Allā text was necessarily copied word-for-word from a previously-existing red-bordered manuscript that was written in the dialect of Aram-Damascus of the 9th century or even earlier. LEMAIRE finds support for this speculation in the fact that the Deir 'Allā text has a red border (among other characteristics), but I must say that this particular theory finds little to recommend it, and I am afraid I think it owes far too much to an attempt to find some story-line that would account for the odd dialect that the text is written in. That is to say, I think the agenda behind this particular speculation is not an attempt to explain the red border so much as it is an attempt to explain the odd dialect. Most are agreed that the writing of the inscription dates from the 8th century. Since, however, the dialect does not look quite like anything else we have in the 8th century, LEMAIRE's answer is to propose that the dialect is, in fact, something else and, conveniently, he has chosen a something else that no one has ever seen—that is, a dialect of Aramaic earlier than any Aramaic we have yet found. Hence the necessity to assert that only a scribe with a red-bordered manuscript on his lap could have dreamed up the idea of putting a red border around a wall inscription. (A.R. MILLARD<sup>9</sup> made what is to my mind the reasonable form of this suggestion by saying simply that the red-bordered inscription shows us “how a column of Aramaic writing would have appeared on a papyrus or leather scroll” in the same period.) Suggesting the text looks like a papyrus manuscript or some such seems perfectly reasonable to me. Where I think LEMAIRE has gone too far is his suggestion that the plaster text is, therefore, a copy of a particular manuscript, and what is more, a very old manuscript.<sup>10</sup> LEMAIRE's version of this explanation for the appearance

<sup>9</sup> “Epigraphic Notes, Aramaic and Hebrew,” *PEQ* 110 (1978) 25; see also “In Praise of Ancient Scribes,” *BA* 45 (1982) 143–53, esp. 149.

<sup>10</sup> In response to this statement at the conference, Jonas GREENFIELD remarked



of the inscription, that we are in truth dealing with an Aramaic dialect in a manuscript either very old, or copied and recopied in modern script but preserving the ancient spelling, has the advantage that it is materially impossible to disprove, but it has the disadvantage that it is on that account virtually impossible to set a boundary to such speculation beyond which we cannot reasonably go as scholars. There is no rigor, that is, no control. It is an argument of last resort, to be called upon only after every other avenue has been exhausted, and even then it is not really an *explanation*, but only a speculation. Even as an argument of last resort, it is vulnerable to an onslaught of new information: someone might find some Damascus Aramaic that dates from the 11th century and is already clearly

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that the scribe *would* have had in hand a "particular manuscript" that was copied onto the wall at Deir 'Allā. I would, of course, agree and would restate my objection in the following way in light of GREENFIELD's comment:

I assume, and I think most other commentators make the same unspoken assumption, that the scribe did not compose the Deir 'Allā text while standing at the plaster wall, but rather copied the text onto the wall from a manuscript that was prepared ahead of time. It is in that sense, then, inaccurate to maintain as I did that the Deir 'Allā inscription is not a copy of a "particular manuscript." But I assume further that that particular manuscript was a practice manuscript, a copy of the story that was sketched out to fill the wall in question (or some combination of sketching to fit the wall and plastering to certain measurements so that the wall was the appropriate size for the text in question), and that care was taken ahead of time to insure that this mock-up manuscript was a grammatically perfect and elegant version of the story that was to be told.

Furthermore, the physical appearance of the Deir 'Allā plaster inscription leads one to speculate that it might have been drawn to resemble the physical appearance of a papyrus manuscript from the same period, as MILLARD suggested (see n. 9).

We have two reasonable assumptions, then: 1) that there was a mock-up manuscript from which the scribe worked while standing at the wall; and 2) that the physical appearance of the Deir 'Allā inscription might suggest the physical appearance of a formal (papyrus) manuscript of the same time period. There is no evidence to suggest, however, that we should combine these two reasonable assumptions and maintain that the Deir 'Allā inscription's appearance indicates that the manuscript the scribe must have copied from what was a particular formal (not to mention ancient) manuscript with red borders and a sphinx. It is, in other words, an extra leap to combine the "formal-manuscript-appearance" assumption with the "practice mock-up manuscript" assumption to arrive at a theory that the Deir 'Allā inscription was something on the order of a true copy of a formal (and old) manuscript that corresponded to the wall inscription in detail. It is possible to make this final leap in one's mind, of course, but it is not a step that is necessary in order merely to explain the scroll-like appearance of the inscription; there is nothing about our evidence here that compels it. (Perhaps one should add that even if the inscription were the copy of a particular formal manuscript, the age of that proposed manuscript would be impossible to guess.)

the Aramaic we know and love. What then? Does Deir 'Allā have to be pushed back to the 13th century or the 15th? It is undoubtedly a conservative dialect: it has no definite article, for instance. (I think most scholars have stopped using the *alep*'s in the first two lines of the inscription as the definite article. One can get the impression that the only things defined at Deir 'Allā were the lacunae.) Still, even to explain a conservative dialect, LEMAIRE's suggestion feels too much like an admission of frustration and defeat, and I would prefer to look to more *linguistically*-oriented explanations of the dialect, a number of which I am certain we will hear before this conference is over.

I would also argue that Prof. LEMAIRE has too quickly assigned the Deir 'Allā narrative to the Damascus-Aramean cultural circle. In the summary that I have, he writes that "a detailed historical study seems to reveal that, from the end of the 9th century till 732 B.C., Deir 'Allā probably was essentially under the control of the Aramean kingdom of Damascus." Baruch LEVINE's "detailed historical study" in his article in the book *Biblical Archaeology Today* found enough evidence to assert that Aramean control was lost at points during the early 8th century, especially during the reign of Jeroboam II of Israel, and the hedging in LEMAIRE's sentence is a tacit recognition of the same evidence. And we might suggest anyway that it does not stretch the imagination too much to believe that even during periods of sure Aramean hegemony, hegemony does not necessarily mean a wholesale giving up of a native culture in favor of one imported from Damascus so that every text found on the tell dating during this time period must be said to reflect what Lemaire calls "the Aram-Damascus literary heritage." LEVINE and LEMAIRE, like others, both point to the Aramaic inscriptions on a stone and a potsherd that are from the same level on the tell as implying a connection between the plaster text and an Aramean culture. But LEMAIRE goes further and maintains, in fact, that the "book of Balaam" from Deir 'Allā was part of what he calls the Aram-Damascus culture of Phase IX at Deir 'Allā.

But surely there is a problem here. If the presence of bits of recognizable Aramaic serves as the evidence for administrative control from Damascus, as both LEMAIRE and LEVINE argue, then we presumably know what a document that is part of that cultural sphere would look like. It would look like the Aramaic being used



as evidence. The very fact that the plaster text *does not* look like that Aramaic, that an inscribed stone and potsherd have to be called upon to provide the evidence of real, regular Arameans at Deir ʿAllā, should logically push us in the direction of looking for some culture *other* than that of Aram-Damascus for the source of the ideas and mythology included in the text. But then LEMAIRE would point us, I think, to the suggestion that the plaster text we have really was a very ancient story, written many years earlier in the dialect of Aram-Damascus at that time, with a red border, that was simply copied on the wall at Deir ʿAllā in the 8th century, in modern Transjordanian script, so the people could read it, and with the red border dutifully reproduced. I understand that this narrative is probably the only way one can argue both that those sherds are the real Aramaic of Aram-Damascus of the 8th century *and* that the plaster text or the ideas in it come to Deir ʿAllā from Aram-Damascus. But I have already said that I do not think this particular speculation should be allowed to stand. We really are basing a great deal on that red border, and the strain from carrying such a load is beginning to show.

Finally, LEMAIRE's reiterating the biblical evidence for Balaam as an Aramean gives, of course, a large part of the picture, but only a part. Those of us who have struggled with who various biblical authors believed Balaam to be have, of course, noted the evidence for his Aramean origin, but we have also duly noted that there is conflicting evidence.

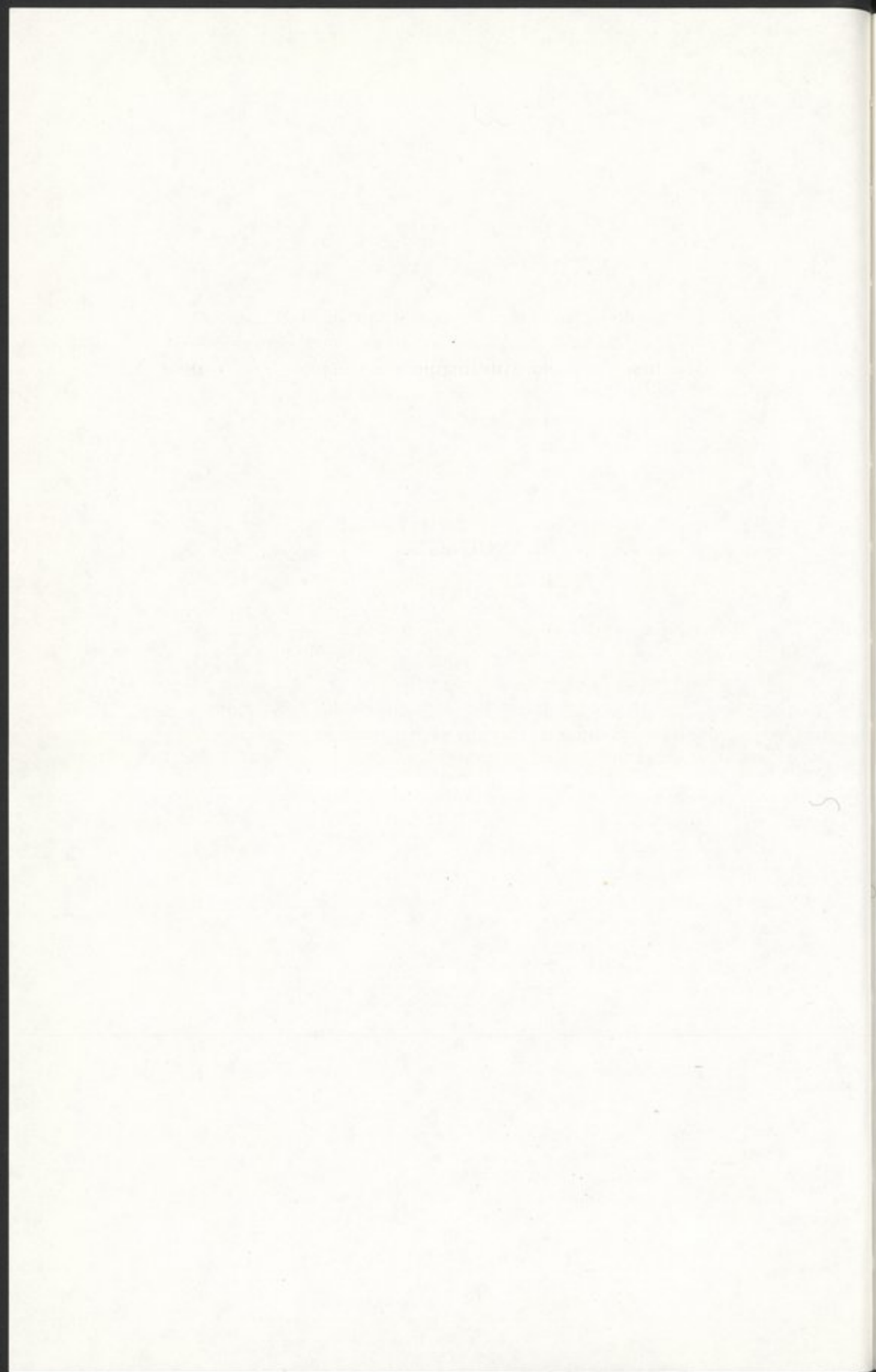
LEMAIRE points to the common Aramean use of "seers" (the root *ḥ-z-w*), but let me close by saying that the occurrence of this word in relation to Balaam at Deir ʿAllā set my thoughts in another direction. I am thinking here of Robert WILSON's discussion of Ephraimite vs. southern intermediaries.<sup>11</sup> Two of the hallmarks of WILSON's southern intermediary, or rather, of the way southerners described their intermediaries, are the reference to the intermediary as *ḥōzeh* and the description of the oracle as *maššaʾ*. Most of us see the word *ḥzh* in the first line of Combination I as referring to Balaam's title, and many of us have suggested reading *maššaʾ* in the second line of Combination I, as a word that defines the vision Balaam received. There is a very little bit of evidence, then, that like

<sup>11</sup> R. WILSON, *Prophecy and Society in Ancient Israel* (Philadelphia: Fortress, 1980) *passim*.

the passages usually assigned to the J writer in Numbers 22–24, the Deir ʿAllā inscription offers a picture of Balaam as a “southern intermediary,” that is to say, it paints Balaam in the same terms as the southern sources in the Hebrew Bible used to talk about intermediaries, according to WILSON’s hypothesis. I am not arguing that Balaam was a Judahite; he clearly is not. WILSON’s hypothesis simply outlines the way Judahites talked about prophecy, the terms they used, and I am suggesting that the Deir ʿAllā plaster text uses some of these same terms. Why this should be true I do not know, and I do not think anyone has worked with WILSON’s suggestions enough to say what it could mean about mutual cultural influence between some group at Deir ʿAllā and some others in the kingdom of Judah, and any others who might be involved, but I would suggest that this is a more fruitful area of future research than setting us all on the road to Damascus.



LANGUAGE





## THE DIALECT OF THE DEIR 'ALLA TEXTS

P. Kyle McCARTER

From point of view of language and dialect, the study of the plaster texts from Deir 'Alla has led to two useful questions for scholarly debate. The first has to do with linguistic taxonomy. How is the dialect of the Deir 'Alla texts to be classified with regard to the other Northwest Semitic languages? As we shall see, this has proved a difficult question to answer, because the language of the plaster texts displays features that resist easy classification into our customary categories. It is this situation that has led to the second question. How are the linguistic peculiarities found in the plaster texts to be explained?

Let us turn first to the question of language classification. Where does the dialect of the Deir 'Alla plaster texts stand within the customary division of the Northwest Semitic group into Aramaic and Canaanite? Although HOFFIJZER categorized the dialect as Aramaic in the *editio princeps* (1976), he identified many features that are customarily thought of as distinctively Canaanite, and subsequent interpreters have pointed to the presence of both Aramaic and Canaanite traits in the text.

Features that have been cited as Aramaic include phonological, morphological, and lexical phenomena. The phonemic inventory of our texts and its alphabetic representation are reminiscent of the Old Aramaic inscriptions:

The characteristic consonantal mergers of later Aramaic (\**d* > *d*, \**q* > *q*, \**z* > *z*, and \**l* > *l*) have not taken place; thus *d* is represented by *zayin*, *q* by *qop*, *z* by *šade*, and *l* by *šin*, exactly as in Old Aramaic inscriptions but in contrast to the situation in contemporary Canaanite (Phoenician, Hebrew, Ammonite, Moabite), where \**d* > *z* is represented by *zayin*, \**z* > *š* by *šade*, and \**l* > *š* by *šin*, but \**q* > *š* by *šade*. (McCARTER 1980: 50)

Similarly, the situation with regard to diphthong contraction reminds us of Old Aramaic:

Diphthongs remain uncontracted in all positions ... whereas they have contracted consistently or sporadically in contemporary Canaanite languages with the single exception of Judahite Hebrew. (McCARTER 1980: 50).

In discussing the morphological characteristics of our text that have been compared to Aramaic, I prefer to omit consideration of articulation with final  $^{-}^{\text{c}}$ . Various interpreters, including myself, have identified examples in our texts, but none is certain or widely agreed upon. It is clear, moreover, that if articulation was used at all in the language of the plaster texts, it was used only rarely and exceptionally. (We shall return to this point later.)

It is better in this context, therefore, to confine our list to the important Aramaic-like features of our dialect that are beyond dispute. In this category is the use of final  $-n$ , rather than final  $-m$ , to mark the absolute form of masculine plural nouns ( $^{\text{c}}lhn$ , "gods" I 1 bis, I 5 bis;  $^{\text{c}}rnb$ , "hares" I 9;  $qb^{\text{c}}n$ , "hyenas" I 10;  $hkmn$ , "wise men" I 11;  $hršn$ , "deaf men";  $mlkn$ , "kings" II 13). The Deir 'Alla dialect shared this feature not only with Aramaic but also with the Arslan Tash dialect and, notably, nearby Moabite, which otherwise stood close to Judahite Hebrew.

Another distinctive feature aligning our dialect with Aramaic is the use of the  $-wh$  third masculine singular suffix on the preposition  $^{\text{c}}l$  ( $^{\text{c}}lwh$ , "to him" I 1,4), on dual nouns ( $kpw^{\text{c}}h$ , "his palms" IXa; cf. ATDA 300 n. 31), and, presumably, on plural nouns. This suffix ( $-awhu/i$ ) is identical to that of Old Aramaic ( $-awhi$ ; cf. Sefire III 8  $^{\text{c}}lwh$ , "to him"), and it probably developed in the same way ( $< *ayhu$ ; cf. GARR 1985: 107, 109). As NAVEH (1979: 136), GREENFIELD (1980: 250), and others have pointed out, the  $-h$  ( $= -ôhu/i$ ) suffix of Moabite seems to have arisen in a similar way ( $-ôhu/i < *awhu < *ayhu$ ). Moreover, the  $-w$  suffix of Judahite Hebrew is best derived from the same original form ( $-aw < *aww < *awwu < *awhu < *ayhu$ ). Thus we have a common development shared by Aramaic, Hebrew, Moabite and the Deir 'Alla dialect in contrast to the development within standard Phoenician ( $-y = -êyu/i < *ayhu$ ); cf. GARR 1985: 108).

The use of  $-at$  as the third person feminine singular ending of the perfect verb ( $hrpt$ , "has reproached," I 7/8;  $nšrt$ , "has belittled" (?), I 8;  $hqrqt$ , "has chased," I 15) is another feature that aligns the dialect of the plaster texts with Aramaic, against Phoenician and Hebrew, which used  $-ā$ .

The Deir 'Alla texts employ an  $^{\text{c}}tp^{\text{c}}l$  conjugation, like Aramaic  $^{\text{c}}Itp^{\text{c}}al$  and  $^{\text{c}}Itpa^{\text{c}}al$ , rather than a  $hitp^{\text{c}}l$ , like Hebrew  $Hitpa^{\text{c}}el$ .



That is, the suffix-inflected or "perfect" form of the tG and tD conjugation is prefixed by <sup>2</sup>- rather than *h*- (<sup>2</sup>*tyhdu*, "they have gathered," in I 5<sup>1</sup> and possibly <sup>2</sup>*tntq*, "he has torn away," in Vc 4). This is somewhat surprising, since our dialect employs an *h*-prefix causative (Hap<sup>c</sup>el or Hip<sup>c</sup>il), as in *hqrqt*, "it has chased," in I 14,<sup>2</sup> and we expect the prefix on verbs of the infixed-*t* conjugations to follow the prefix on C.<sup>3</sup> But this is not what we find at Deir <sup>c</sup>Alla, where the forms are evidently mixed. Such a mixture is attested elsewhere in Northwest Semitic.<sup>4</sup>

A number of the lexical items in the plaster texts remind us of Aramaic. In line 5 of combination I, we find the verb <sup>2</sup>*hwkm* (<sup>2</sup>*ahawikkum*), "I shall inform you," a common Aramaic word occurring only in later Biblical Hebrew. In line 7, the verb *yhb* is used in the indicative mode (*thby*, "you will place" I 7). Though very common in Aramaic (also Old South Arabic, Ethiopic, and Arabic *wahaba*), this verb is used only in the G imperative in Biblical Hebrew and not at all in Phoenician. The Deir <sup>c</sup>Alla word for "son" is *br*, not *bn*, and this, of course, makes us think of Aramaic, despite the fact that Kilamuwa's patronymic uses *br* in his Phoenician inscription (cf. HACKETT 1984b: 64). Furthermore, we have in our texts *hd*, not <sup>2</sup>*hd*, for the number "one" (II 10), in alignment with the predominant Aramaic form but in contrast to Hebrew, Punic and Ugaritic.

Features of our dialect that have been cited as Canaanite include morphological, syntactical and lexical phenomena. In the first category is the use of the N-conjugation, elsewhere unknown in Aramaic,<sup>5</sup> which is found in line 6 of the first combination (*wnšbw*,

<sup>1</sup> I still prefer to think of this form as tG; cf. McCARTER (1980: 53), following HOFTIJZER (1976: 192). On the basis of a technical usage in Rabbinic Hebrew and Aramaic, HACKETT (1984a: 40, 119) assigns it to tD.

<sup>2</sup> The verb is <sup>2</sup>*drq*, later Aramaic <sup>c</sup>*rq*; cf. HOFTIJZER 1976: 219.

<sup>3</sup> Assuming that the Proto-Semitic form *t*-form, <sup>2</sup>*t*(<sup>2</sup>)*qabbara*, became *hitqabbar* or <sup>2</sup>*itqabbar* in a given dialect by anaptyxis and analogy with the causative prefix used in the dialect.

<sup>4</sup> HACKETT (1984a: 119; 1984b: 63) notes the apparent mixture of Hap<sup>c</sup>el and <sup>2</sup>Apel causative at Sefire, citing FITZMYER 1967: 157. HALPERN (1987: 128-29) points to alternation between <sup>2</sup>- and *h*- preformatives in Biblical Hebrew.

<sup>5</sup> The form *nšbt* in COWLEY 15:10 seems to have the form of an N-participle, but it is obscure. If it is derived ultimately from an N-form, it is probably a technical term of commerce that originated in a Canaanite speaking environment. Thus LIDZBARSKI (3.80) attributed it to the influence of Phoenician merchants (so SEGERT

"and they [viz., the Shaddayin] have taken their places")<sup>6</sup> and twice in line 12 of the second (*n<sup>2</sup>nh*, "he has sighed").

A second morphological feature that seems to link our dialect to Canaanite is the formation in line 17 of combination II of a I-*w* G-infinitive with suffixed *-t* (*ld<sup>c</sup>t*, "in order to know"). No I-*w* infinitive is yet attested for Old Aramaic, but the form is produced in later Aramaic with prefixed *m-*, not suffixed *-t*, which is the expected form in Canaanite for the G-infinitive for verbs I-*w* or I-*y*.

The verbal syntax of our dialect is distinguished by the use of *waw*-consecutive verbs, which we ordinarily think of as characteristic of the South Canaanite (Hebrew and Moabite) type of simple past narrative.<sup>7</sup> Among the inscriptions generally classified as Old Aramaic, the *waw*-consecutive sequences are found only in the inscription of Zakkur of Hamath (*KAI* 202).

Finally, there are a number of lexical items that associate our dialect with (South) Canaanite rather than Aramaic (see HACKETT 1984b: 64). These include: (1) the use of the verb *dbr*, "speak" (*dbr*, "he spoke," II 17), probably in the D-conjugation, as in Hebrew and Phoenician but not Aramaic; the occurrence of *lk* as the singular imperative of *hlk* (*lk*, "come!" II 17) and *lkw* as the plural imperative (*wlkw*, "now come!" I 5); the use of the verb *p<sup>c</sup>l* for "do, make" (*yp<sup>c</sup>l*, "let [someone] make" I 2; cf. *p<sup>c</sup>lt*, "deeds" I 5), as in Phoenician and secondarily in Hebrew, but not in Aramaic, where *bd* is the expected verb; and the use of the verb *r<sup>2</sup>h* for common seeing (*r<sup>2</sup>w*, "see!" I 5) alongside *hzh* used for prophetic seeing (*hzh*, "a seer" I 1).

As this compendium of features shows, it is not easy to describe the dialect of the plaster texts as either Aramaic or Canaanite, and it is impossible to do so without some kind of qualification. This difficulty has been recognized from the beginning of the scholarly discussion of the texts. The binary division of Northwest Semitic

1975: 257), and, recently, HALPERN has suggested that "it reflects a terminology in jobbing probably rooted in the Jewish culture underlying the text" (1987: 129 n. 46).

<sup>6</sup> In light of the reasonable certainty of the occurrence of an N-form verb in II 8, this is the most likely interpretation of *wnšbw*. LEVINE (1981: 196) analyzes it as a D-form.

<sup>7</sup> The consecutive tenses are not widely used in Ugaritic and Phoenician (cf. SEGERT 1976: 194).



into Aramaic and Canaanite is based on assumptions about language classification that cannot easily be applied to the Deir 'Alla dialect. Thus, in the *editio princeps* of the plaster texts, when HOFTIJZER proclaimed the language to be Aramaic, he did so with some reluctance,<sup>8</sup> explaining that "... the received distinction between Canaanite and Aramaic languages can—in my opinion—only have a relative value" (1976: 301). Subsequent interpreters who accepted HOFTIJZER's Aramaic classification of the language (CAQUOT and LEMAIRE 1977; FITZMYER 1978; McCARTER 1980; KAUFMAN 1980; LEVINE 1981; etc.) also expressed, in various ways, agreement about the difficulties inherent in the customary binary classification. As LEVINE put it (1981: 185) "Whether one can call this language Aramaic is really a question of definitions." It is not surprising, therefore, that there have been other interpreters who, impressed especially by the features the Deir 'Alla dialect shares with Hebrew, have preferred to describe the language as Canaanite or South Canaanite (NAVEH 1979: 133–36; GREENFIELD 1980: 248–52; and, in most detail, HACKETT 1984a: 109–24; 1984b: 57–65).

It may be appropriate to observe at this point that students of the Northwest Semitic languages seem to be becoming increasingly dissatisfied with the usefulness of the Canaanite-Aramaic distinction for categorizing features found in texts from the Persian Period and earlier. A careful reevaluation of the binary organization of the Northwest Semitic family seems now to be underway. The study of the Deir 'Alla texts is one of the principal things prompting this reevaluation, and this may be counted as one of the very positive results of our work on these texts.

This brings us to the second question asked at the beginning of our discussion. Given the mixed inventory of linguistic features we have described, how are the peculiarities of the Deir 'Alla dialect to be explained?

To answer this question, we should begin with the observation that many of the features that have been identified as distinctively Aramaic or Canaanite in these texts are linguistic retentions. That is, they are features that must be reconstructed for Proto-Northwest

<sup>8</sup> "Provided one wants to maintain the distinction between Canaanite and Aramaic languages . . . , there can be no doubt that here we have to do with an Aramaic one" (1976: 300).

Semitic. To put it another way, the language of Deir ʿAlla shares relatively few innovations with other Northwest Semitic languages.

This is true, in the first place, of the phonology of our texts, as described above. The uncontracted Deir ʿAlla diphthongs correspond to the primitive situation in Northwest Semitic, a situation that was preserved in some dialects (Aramaic, Judahite Hebrew) and lost in others (Phoenician). The phonemic inventory is similarly conservative. As we have seen, the alphabetic representation of consonantal phonemes in the Deir ʿAlla texts is identical to that of the Old Aramaic inscriptions. In the languages of the Old Aramaic inscriptions, however, the consonant mergers that would distinguish later Aramaic from other Northwest Semitic languages (\**d* merging with *d*, \**q* with *q*, \**z* with *z*, and \**t* with *t*) had not yet taken place, so that the orthography reflects the preservation of the older Proto-Northwest Semitic distinctions. After these mergers occurred, the alphabetic representation of consonants in Aramaic texts became distinctively Aramaic (*dalet* being used for *d* < \**d* or *d* < \**q*, *ayin* for *c* < \**c* or *c* < \**q*, *let* for *t* < \**t* or *t* < \**z*, and *taw* for *t* < \**t* or *t* < \**t*). By contrast, the older system employed in the Old Aramaic texts and at Deir ʿAlla reveals nothing diagnostically Aramaic about the consonantal phonology of the languages in which these texts were written. Instead, the system suggests a primitive situation, in which *d*, *q*, *z* and *t* remained distinct phonemes. Presumably, the scribes chose the letters *zayin*, *qoph*, *šade* and *šin* to represent these phonemes because they seemed the closest equivalents available in a repertoire of symbols that had been limited by the phonetic characteristics of the dialects in which alphabet writing had been transmitted. That these choices were somewhat arbitrary is shown by the use of *samek* rather than *šin* to represent *t* in the Fekheriye inscription.<sup>9</sup>

Some of the most striking morphological peculiarities in our dialect must also be characterized as linguistic retentions. The use of the N-conjugation, although it may remind us of Phoenician and Hebrew, is, of course, a survival from Proto-Semitic rather than an innovation shared by the languages that employ it.<sup>10</sup> Similarly, the

<sup>9</sup> See further HALPERN 1987: 122–26.

<sup>10</sup> On this point, HACKETT (1984b: 62) states her case against an Aramaic affiliation for Deir ʿAlla too strongly: "It is possible that Aramaic had an N conjugation at some point in its history. Certainly, Proto-Northwest Semitic included an



formation of the I-w-G-infinitive with suffixed *-t*, rather than prefixed *-m* as in later Aramaic, is a conservative trait, preserving the Proto-Northwest Semitic form. Again, the *-at* form of the third-person feminine singular ending of the perfect verb, which the dialect of the plaster texts share with Aramaic against Phoenician and Hebrew (*-ā*), is a retention of the Proto-Semitic form.

As for syntax, the use of the so-called *waw*-consecutive narrative sequences is regarded by many analysts as a survival from Proto-Northwest Semitic. GARR (1985: 186) describes the situation as follows: "Most dialects—Old Aramaic (Zkr), the Deir Alla dialect, Moabite, and Hebrew—used the old consecutive imperfect; this distribution suggests that the consecutive imperfect was a common NWS verb form. In the other dialects, the consecutive imperfect was lost." The alternative is to regard the *waw*-consecutive narrative sequences as an innovation shared by those dialects that employ it. If this is the case, however, the evidence of the Zakkur inscription is crucial, because it shows that the breakdown is not along Aramaic-Canaanite lines. Instead, the Deir 'Alla dialect sides with Hebrew, Moabite, and the language spoken by Zakkur (the dialect of Hamath or neighboring Lu'ath) against Phoenician and the majority of Old Aramaic dialects.

When we turn to features of the Deir 'Alla dialect that seem more likely to be linguistic innovations shared with other dialects, we again find that the distribution does not correspond closely to the Canaanite-Aramaic bifurcation. This is true, for example, of the correspondences of the original (stressed) *\*-āt* ending on absolute feminine singular nouns, a phonological feature not mentioned above. In the Deir 'Alla dialect, *\*-āt* is written *-h*, representing *-ā* (*ʔnph*, "heron" I 8; *knhh*, "priestess" I 11). This change is shared with Old Aramaic, but also Hebrew, whereas the original *-t* was preserved in Phoenician (as *-ōt*), Ammonite (as *-at?*) and Moabite (as *-at?*).

The morphological innovations in our dialect also resist easy clas-

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N conjugation, since it is present in every other NWS dialect except the Aramaic ones. At present, however, we have no evidence of an N form within a distinguishably Aramaic inscription. If the N conjugation ever existed in Aramaic, it fell out of use very early, certainly earlier than 700 B.C.E. Hence, we would expect a genuine N verb in an Aramaic inscription in 700 B.C.E. only if the dialect of the inscription were extremely conservative, and had retained the N centuries longer than any other known Aramaic. This is, of course, not likely."

sification as Canaanite or Aramaic, as in the case of the use of final *-n*, rather than final *-m*, to mark the absolute form of masculine plural nouns. Both *-n* and *-m* plural markers might be reconstructed for Proto-Northwest Semitic, but it is also possible to regard both as secondary developments (so GARR 1985: 91). In any case, the distribution of their use does not coincide with the customary Aramaic-Canaanite boundaries. Instead, as we have seen, Deir 'Alla shared the use of *-n* with the nearby dialect of Moab, with the dialect of Arslan Tash, and with standard Old Aramaic, in contrast to the *-m* of nearby Ammonite and Hebrew and of Phoenician.

Similarly, with regard to the perfect of the tD conjugation, it is possible that both prefixed *h-* and prefixed  $\text{ʔ-}$  should be reconstructed for Proto-Northwest Semitic, but it seems more likely that both are secondary developments within differing language groups.<sup>11</sup> It is also quite reasonable to argue, as HALPERN does (1987: 128–29), that the larger development in later Aramaic, where *h-* is generally replaced by  $\text{ʔ-}$  in these forms, shows the  $\text{ʔ-}$  prefix to be a secondary, phonological development. In any case, the use of the  $\text{ʔtp}^{\text{c}}\text{l}$  in the Deir 'Alla dialect is shared with Aramaic, but also with Biblical Hebrew.<sup>12</sup>

Another of the distinctive features of the Deir 'Alla dialect described above is the *-wh* third masculine singular suffix on dual and (probably) plural nouns and on prepositions that share the form of plural nouns. As we noted, this suffix is a linguistic innovation that our dialect shared with Aramaic. We also noted, however, that the same suffix is probably to be reconstructed in the development of the Moabite suffix *-ôhu/i* (< *\*-awhu* < *\*-ayhu*) and the Judahite Hebrew suffix *-aw* (< *\*-aww* < *\*-awwu* < *\*-awhu* < *\*-ayhu*). By contrast, the equivalent Phoenician suffix *-y* (= *-êyu/i*) was derived directly from *\*-ayhu*, without the intermediate form *-awhu*. Here again we have a situation in which the dialect differentiation does not follow the customary Canaanite-Aramaic division.

Let me summarize what has been said so far by making two generalizations. First, the Deir 'Alla dialect is extremely conservative in comparison to the Northwest Semitic languages in general.

<sup>11</sup> As explained in n. 3 above.

<sup>12</sup> Cf. HALPERN 1987: 128–29, as cited above in n. 4. It would be difficult to explain all the instances of  $\text{ʔtp}^{\text{c}}\text{l}$  forms in Biblical Hebrew by reference to late Aramaic influence.



Second, in the few instances where the language of Deir ʿAlla shares linguistic innovations with other Northwest Semitic dialects, the pattern of innovation does not suggest a firm or consistent alliance with either the Canaanite or Aramaic group, as these languages have been customarily divided. Let me comment further on both of these generalizations.

We have described our dialect as conservative because of its primitive phonological inventory and the striking linguistic retentions in its morphology and syntax. To these we may add the absence of the *nota accusativi*, the absence or very sparse use of the relative pronoun, and the absence or very sparse use of the article. (As we noted at the outset of this discussion, a number of interpreters, the present reader included, have thought that we found instances of the use of the *zy* relative pronoun<sup>13</sup> or the final <sup>2</sup>*alep* article<sup>14</sup> in the plaster texts. But even if all our proposed examples were sound, the fact would remain that the Deir ʿAlla dialect uses relative pronouns and articular determination with remarkable infrequency.) Taken altogether, these features suggest that the dialect of the plaster inscriptions is archaic. This might be because the text itself is very old, much older than the particular copy of the text that was made at Deir ʿAlla, as LEMAIRE has suggested (1985a: 38; 1985b). But archaism is characteristic of literary language, especially poetry, and the literary quality of our texts may be enough to explain their archaic linguistic features.

HOFTIJZER recognized this aspect of their character and articulated it tentatively in the *editio princeps* (1976: 301), where he proposed that:

<sup>13</sup> Cf. McCARTER 1980: 59 n. 3, where I proposed to read *zy* in II 8 as the relative or determinative pronoun. Subsequent collations of the text by HACKETT (1984a: 63) and myself have failed to confirm the necessary word divider before the *zayin*. I am now satisfied that the correct reading is *mn. p̄zy. bny. ʔš*, "from the upstarts (?) of the sons of men," whatever it might mean. KAUFMAN's reading *mn. m(!)hzy. bny. ʔš*, "from the sight of human beings," is attractive, but the sign in question seems to be a clear *pe*, not a *mem*.

<sup>14</sup> All the proposals are problematic. The phrase *wbškm̄ʔ*, "and in these mountainous regions (?)," proposed by HOFTIJZER (1976: 188-89), disappears with rearrangement of the text (cf. HACKETT 1984b: 59). The reading *kml/yʔ. ʔl*, "according to these words," proposed by CAQUOT and LEMAIRE (1977: 194-95; cf. McCARTER 1980: 52), requires reconstruction of the text at the beginning of I 2, so that, whatever its merits, it can hardly bear the sole burden of evidence for the existence of the -ʔ article. The same is true of the problematic <sup>2</sup>*hrʔh*, "hereafter (?)" (cf. McCARTER 1980: 52), later in the same line.

We have to do with two types of language in our texts, the more "common" language and a "poetic" language, used for e.g. the curses, proverbs and the prophecy itself . . . . That the curses are poetry is also probable because of the *parallelismus membrorum* which occurs there, e.g. II 6,8,9, see also II 35,36. If this hypothesis turns out to be true, we would have in our texts the first clear examples of Aramaic poetry of the first millenium B.C.

In 1980, I commented on these archaic and literary features in terms of general agreement with HOFTIJZER's judgment (McCARTER 1980: 50-51):

If "poetic" is too precise a term to use at this point in our study of the texts, at least we can speak of a "literary" dialect. We have found her the hallmark of the *prose* literary tradition known from Biblical Hebrew, viz., the use of the "waw-consecutive" narrative sequences . . . . Certain features of the language, such as the avoidance of relative pronouns and of articular determination of nouns, are characteristic of a literary or at least an archaistic tradition . . . . With regional modification this language was the common literary vehicle for Israel, Judah [and] Moab, as the elegant narrative sequences of the Mesha stele (KAI 181) demonstrate . . .

I might now add that occasional departures in our texts from the consecutive imperfect syntax<sup>15</sup> point strongly in the direction of poetry. Consider the parallelistic couplet in I 5-6:

ʔl/h/n. ʔtyhḏw  
wnṣbw. ṣdyn. mwḏ  
The g[o]ds have gathered  
and the Shaddayin have taken their places  
in the assembly.

The literary character of the Deir ʿAlla texts is further indicated by the use of literary formulae, idioms and phraseology that are familiar from Biblical Hebrew. In line 5 of the first combination we find the literary formula *wlkw rʔw pʿlt ʔlhn*, "Now come, see the deeds of the gods!" which also survives in Biblical Hebrew (Ps 66:5): *lēkū ūrēʔū mipʿālōt ʔēlōhīm*, "Come and see the deeds of God!" (McCARTER 1980: 53). The account of Balaam's audience with his people in combination I displays narrative conventions or stock language that the Deir ʿAlla texts share with the biblical

<sup>15</sup> As noted, for example, by GARR (1985: 190-91), who cites instances of clauses in which the verb is not initial.



Balaam narrative. As I commented in 1980, "Often the language is so close as to suggest stereotyped patterns in the telling of Balaam stories" (McCARTER 1980: 57).

Archaic as the Deir <sup>c</sup>Alla dialect is, however, it is not Proto-Northwest Semitic. That is, it is not entirely devoid of linguistic innovations, as we have noted. Let us now return to these briefly. They are very important, because it is linguistic innovation that is diagnostic for dialect classification. That is, dialects that share innovations with respect to a common parent language may be said to be related.

The innovations we have noted present an interesting pattern. The use of the "imperfect consecutive" narrative sequences, if it is not a Proto-Northwest Semitic survival, links Deir <sup>c</sup>Alla with Hebrew to the west, Moabite to the south, and the Aramaic dialect of Zakkur of Hamath to the north. Morphological innovations associate the Deir <sup>c</sup>Alla dialect with Aramaic (final *-n* on absolute masculine plural noun, *-wh* third masculine singular suffix on plural noun forms, and the <sup>2</sup>*tp<sup>c</sup>l* conjugation), Hebrew (*-wh* third masculine singular suffix on plural noun forms [reconstructed for the development of Hebrew], and, occasionally, the <sup>2</sup>*tp<sup>c</sup>l* conjugation), Moabite (final *-n* on absolute masculine plural noun and *-wh* third masculine singular suffix on plural noun forms [reconstructed for the development of Moabite]), and, in one instance, the dialect of Arslan Tash (final *-n* on absolute masculine plural noun). Some of the distinctive lexical items noted above can also be classed as innovations, so that they serve to associate the Deir <sup>c</sup>Alla dialect with Aramaic (*ḥd*, "one") and Hebrew (*ḏbr* [D], "speak"; *lk/w* imperative of *hllk*, "go").

These details show that while the Deir <sup>c</sup>Alla dialect resists classification as Aramaic or Canaanite in categorical terms, it fits well into its geographical context. It is remote from Phoenician. It is strongly linked to Hebrew and Moabite. It is also strongly linked to Aramaic. These associations are easy to understand in terms of dialect geography. Hebrew and Moabite were spoken and written in nearby or contiguous communities. The Aramaic region lay farther away to the north, but we know that Damascus exercised a substantial political and cultural influence on northern and central Jordan until the fall of Damascus in 732 B.C. The adoption of the Aramaic alphabet for writing Ammonite is powerful testimony to this influence.

We conclude, therefore, that the language of the Deir 'Alla plaster texts is a local dialect (cf. KAUFMAN 1980: 133; HALPERN 1987: 133) that can be described as both archaic and literary. As KAUFMAN stressed already in 1980, it displays affinities with neighboring dialects to the north, west and south, so that it fits comfortably into its geographical setting in Jordan. There is no reason to assume a Syrian origin for the Deir 'Alla community in order to explain the Aramaic connections of the dialect (cf. LEMAIRE 1985b; WOLTERS 1987), especially since such a hypothesis would leave the Hebrew and Moabite connections unexplained.

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## THE LINGUISTIC CLASSIFICATION OF THE DEIR 'ALLA TEXT WRITTEN ON PLASTER<sup>1</sup>

Dennis PARDEE

### I. GENERALITIES

Order of importance of isogloss categories: phonology, morphology, syntax, lexicon, literary features.<sup>2</sup>

The most important form of isogloss is that constituted by innovations within a linguistic group. Parallel innovations can occur, of course, and the likelihood of that occurring must be assessed in each case.

"Zero"-features are weighted as zero: the absence of the *nota accusativi* and of a relative pronoun in the Deir 'Alla text cannot be used as evidence in favor of either the Canaanite or the Aramaic hypothesis, because both of those language groups have both features. The argument could only be used in favor of a language group which lacked the two features in question.

Because the phonology is unknown in an oral form, we must work from the indications provided by the writing system.

The experience provided by attempts to classify Ugaritic should provide sufficient warning against ascribing improper attention to lexicon and literary features: those who weighted those aspects too heavily classified Ugaritic with Hebrew, while those who observed the less easily borrowable features classified it as a more archaic language.

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<sup>1</sup> Because I did not receive P.K. McCARTER's paper to which it was my assignment to respond until the day it was delivered, these remarks represent (1) comments prepared before the conference and independent, therefore, of McCARTER's paper (some of these notions were already expressed in my review to appear in *JNES* of J.A. HACKETT, *The Balaam Text from Deir 'Alla*); (2) reactions to all of the papers, responses, and discussions that took place at the conference. These remarks are not, therefore, organized along the lines of any one paper but according to isogloss categories.

<sup>2</sup> See already the review cited in note 1. One can consult, in the specific area of linguistic borrowing, I. LEHISTE, *Lectures on Languages in Contact* (Cambridge, MA, 1988), esp. p. 22.



## II

A proto-Semitic retention hitherto not attested in any Aramaic dialect<sup>3</sup> is the N-stem. To avoid the appearance of trying at all costs to see in this text a form of Aramaic, I have accepted the presence of the N-stem in this text. It must be reiterated, however, that this is not certain: of the two apparently clearest cases, *nʾnh* (II 12) could be 1 c. pl. of the G-stem, while *nšbw* (I 8) could be G-stem.

## III

A proto-Northwest Semitic retention attested in both Canaanite and Aramaic is the *w + yaqtul* preterite. Since this feature is well known in Hebrew and Moabite, the point of debate is its occurrence in Aramaic. But twist as one might, it remains indisputable that this fea-

<sup>3</sup> W.R. GARR cites three Hebrew/Phoenician isoglosses in this text: (1) the syncope of causative *h-* in the imperfect; (2) the ending of I-weak infinitives construct in *-t*; (3) the imperative of *hlk* without *h-* (*Dialect Geography of Syria-Palestine, 1000-586 B.C.E.* [Philadelphia, 1985] 229). The first feature occurs only once (*yybl*, "he will lead/bring," in I 11) and cannot be described as certain (so GARR, p. 56). Given that the G-stem is attested in Samalian and the D-stem in Syriac, it hardly appears necessary to analyze the Deir <sup>c</sup>Alla form as Haphel. Even if it were a Haphel, the fact that the *-h-* is elided in the imperfect in Samalian and sporadically in later dialects (DION, *La langue de Ya'udi* [Ontario, 1974] 121-22, 201-2, 332) means that it is not implausible to characterize a dialect evincing this feature as Aramaic. It is probably to be classified as a morphologically linked change (i.e., characteristic of the Haphel/<sup>o</sup>Aphel stem) and not as a general phonetic shift (i.e., it is not a question of the general elision of inter-vocalic *h*).

As for the second feature, there is little reason to doubt that *ld<sup>c</sup>t* in II 17 consists of the preposition *l* plus the noun *d<sup>c</sup>t*, derived from the root *yd<sup>c</sup>*. What is to be doubted is that the form is to be analysed morpho-syntactically as an infinitive construct. The text before this phrase has disappeared (end of line 16), but the following text is well preserved and it consists of a new sentence (*w ...*). Since no direct object is present, we may simply be dealing with the common noun "knowledge," as the editor assumed. Compare biblical Hebrew, where the *qalt*-base noun functions as both common noun and infinitive construct. It must be observed that it is the morpho-syntactic category of infinitive that is important as an isogloss, for the *qalt* form appears as a common noun in later Aramaic and was already present in Ugaritic. Until that morpho-syntactic category can be proven for Deir <sup>c</sup>Alla *qalt* forms, the word *d<sup>c</sup>t* cannot serve to prove the presence of the Canaanite isogloss in this dialect.

The third feature is certainly present and is previously unattested in Aramaic. The question here is how far back the form went. It is already attested in Ugaritic and is thus perhaps to be classified as a proto-Northwest Semitic retention in this dialect. In any case, it is lexically limited and is not, therefore, to be placed high on the prioritized list of isoglosses.

ture is present in one Old Aramaic inscription, the Zakkur inscription (KAI 202), and this fact makes the appearance of the feature in another dialect of Aramaic plausible.

#### IV. PROTO-NORTHWEST SEMITIC FEATURES RETAINED ONLY IN ARAMAIC

1) *-vT* (< *-at*) 3 f.s. pf. of the strong verb. Citing the retention of the old feminine ending in weak roots in Hebrew is irrelevant as proof of the presence of this feature in Canaanite, for the *-at* ending was proto-West Semitic and the important point is the pattern of retention. In Aramaic the old ending was retained in the strong root, i.e., where other phonetic changes have not triggered the retention (as in III-*y/w* roots in Hebrew<sup>4</sup>).

2) The non-assimilation of the *-n* of the preposition *mn* (rare in Canaanite: before the definite article only in Hebrew). Judging from the several occurrences, each followed by a different consonant (*m* in I 5, *r* in I 15, *g*, *p*, and *š* in II 8), the *-n* was everywhere retained in this dialect.

3) The preservation of the old III-weak ending of the word *ssh* "horse" (*ssw* in Ugaritic, *ssh* in Imperial Aramaic, *ss* in Hebrew). This argument would be stronger if the analysis were certain; unfortunately, the context is broken (II 15) and the form could be either feminine or masculine + 3 m.s. pronominal suffix.

#### V. ARAMAIC INNOVATIONS

1) One phonetic ( $\cong$  graphemic, as noted above) feature: {q} for /q/ is a feature limited to Old Aramaic.<sup>5</sup>

2) One morpho-phonetic feature: *-wh* as the form of the 3 m.s. pronominal suffix on a masculine plural noun is a feature characteristic, in this form and in various developments, of the Aramaic dialects. Unfortunately, the phonetics and the historical derivation

<sup>4</sup> \**banayat*  $\rightarrow$  \**banāt*  $\rightarrow$  \**bānātā*<sup>h</sup> (*-āh* by analogy to the strong root). In the final form, the *-t* of the old *-at* ending is, of course, no longer word final and is thus retained. The realization of *hyt* in the Siloam tunnel inscription may represent either of the final two stages, though one would expect the *mater lectionis* if the third stage were already in use.

<sup>5</sup> See the review mentioned in footnote 1 for the necessary reservations to the use of this argument.



of the form are unknown<sup>6</sup>, but the writing *-wh* is certainly an Aramaic isogloss.

3) Two lexico-phonetic features: *br* (< *bn*-),<sup>7</sup> "son," and *ḥd* (< *ʾahḥad*-), "one."

## VI

The lexicon is mixed, with some elements common West Semitic, some primarily Aramaic (e.g., *ʾth* "come," *yhb*, "give," *ḥwh*, "announce," perhaps *ʿll*, "enter"), others primarily Canaanite (e.g., *dbr*, "speak," *rʾh*, "see"). Two points must be made here: (1) I know of no first-millennium Canaanite lexical innovation present in this text. For example, some consider *dbr*, "speak," to be a denominative from *dbr*, "word."<sup>8</sup> However that may be, the meaning "speak" is already attested in Ugaritic<sup>9</sup> and one can argue that that meaning is early Northwest Semitic. On the other

<sup>6</sup> J. HUEHNERGARD pointed out in the course of discussion that the Aramaic and Deir <sup>c</sup>Alla forms could have different derivations and different phonetic realizations. This is, of course, true. But it is equally true that they could have the same derivation and a same or similar phonetic realization—the hypothesis is certainly not implausible.

<sup>7</sup> D. TESTEN, "The Significance of Aramaic *r* < *\*n*," *JNES* 44 (1985) 143–46 (according to TESTEN, the base form *bn*- would be proto-Semitic; the question is the origin of the shift of *n* to *r* in Aramaic and South Arabic: common origin, or parallel development?).

The word *br* cannot be totally ignored as some attempted to do in the course of the conference. It is neither a proper name, nor part of a proper name. It is a common noun serving to indicate the patronymic. The presence of *br* in the otherwise Phoenician Kilamuwa inscription (*KAI* 24:1 *klmw br hy*) proves that the Aramaic word can be used in a Phoenician text to indicate the patronymic but this occurrence is a *unicum* among the thousands of patronymics attested in the various West Semitic languages. One can cite as a counter-argument the fact that in the Hebrew version of the Balaam story the Canaanite form *bn* was used; if the Deir <sup>c</sup>Alla text were in a Canaanite dialect one can only ask why that author did not do the same as did the biblical author. At the conference the following answer was given: the biblical version is an adaptation and the lexical element "son" underwent linguistic adaptation. But why did the same not happen in the Deir <sup>c</sup>Alla text? Is that version the primeval version, never before recounted, with *br* used for the sole purpose of indicating that Balaam was Aramaean? There is no reason to believe such to be the case. We simply know nothing about the antecedents of the story. And the dialect, whatever it was, must have had a gentilic ending to indicate ethnic origin.

<sup>8</sup> E.g., T.O. LAMBDIN, *Introduction to Biblical Hebrew* (New York, 1971) 194.

<sup>9</sup> RS 34.124: 18 (numbering as per new edition to appear in the forthcoming edition of all the texts from the 34<sup>th</sup> campaign at Ras Shamra).

hand, *hwh*, "announce," may be a semantic innovation, for it does not occur in Ugaritic, and that word is characteristically Aramaic. (2) When making comparisons with other Northwest Semitic dialects, we are talking about characteristic distributions, not about presence or absence per se. All of these dialects were so closely related that most lexical items appear in more than one dialect. And Ugaritic, even as poorly attested as it is, has shown that many poorly attested words have a long history to them. The lexicon of the Deir 'Alla text gives an impression of archaicity similar to that of Ugaritic, but may contain the one Aramaic semantic innovation cited.

## VII

The literary aspects find most of their points of comparison in the Hebrew Bible. Here the problem is the absence of a comparable Aramaic literature. Given the similarities and differences between the Hebrew and Ugaritic literatures (and taking into consideration the generalities of literary comparisons), one could hazard a guess that regional and chronological factors were present: though a second-millennium literature of northern Syria would have certain points of contact with a first-millennium literature of southern Canaan, two first-millennium literatures from the southern Canaanite area would be expected, all other things being equal, to have more in common. One will not be in a position to say whether this bit of literature is "more Canaanite" or "more Aramaic" until a significant Aramaic literature from the period is at hand.

## VIII. SUMMARY

§IV and §V indicate Aramaic; §II and §VII indicate Canaanite; all but §V indicate archaic. §II is an isogloss high on the list of priorities (morphology) but it is alone and can be seen as a retention. §IV has one important morphological element and two lexically limited elements. §V is the most important, for it contains innovations of various levels on the prioritized list given in §I. To these may be added the possible Aramaic semantic innovation in the word *hwh*, "announce" (discussed in §VI).

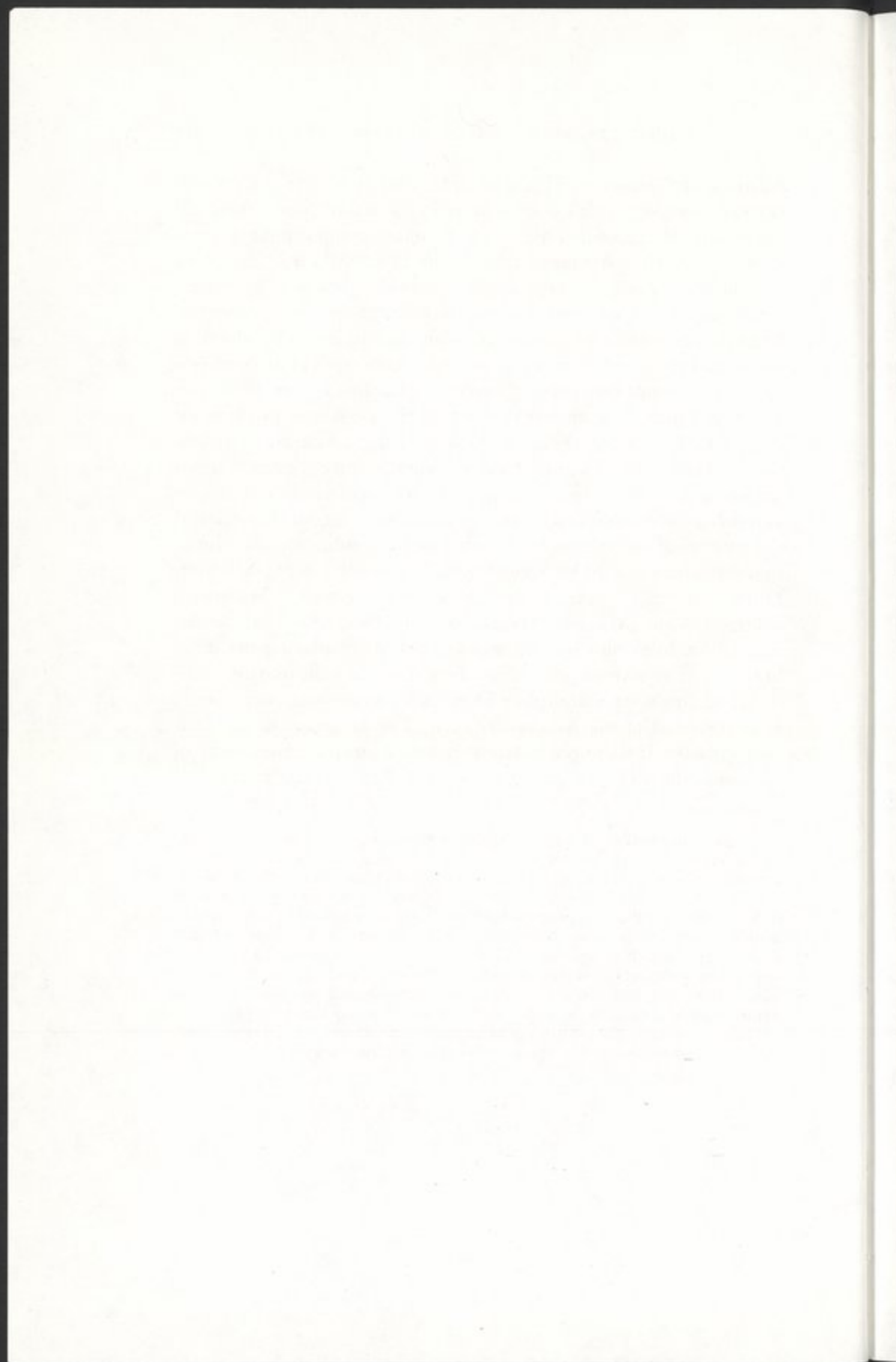
Several speakers at the conference attempted to avoid the trap of a binary system of classification, viz., that this text must be either



Aramaic or Canaanite. This is laudable and may be in the end the correct solution. With the data presently at hand, however, it appears to me that an ascription to the Aramaic group is unavoidable. It is true that the damaged state of the Deir ʿAlla text has as an unavoidable result that arguments based on so few data cannot be conclusive. Moreover, every one of the features discussed above can be explained otherwise than by an Aramaic affiliation. Finally, it is also true that a collection of weak arguments (poorly attested isoglosses) does not constitute a strong argument (a clear set of isoglosses). But it does appear to me that the isoglosses favoring an Aramaic affiliation outnumber those favoring a Canaanite affiliation and that their prioritized value is significantly greater. Listening to the arguments against them in the course of public discussions and private conversations during the conference, I could understand the validity of the argument in each case but could only ask why so many features should be argued away. I cannot accept, therefore, HUEHNERGARD's attempt at a triadic system (three branches from a common node, Aramaic, Canaanite, and Deir ʿAlla) but would only differ from him by millimeters: instead of placing the three branches at a common point of juncture, I would maintain the traditional binary system and place Deir ʿAlla, along with Samalian<sup>10</sup>, near the head of the Aramaic branch. The language of the Deir ʿAlla plaster inscription is typologically a very archaic form of Aramaic, the archaism probably being due to regional isolation.<sup>11</sup>

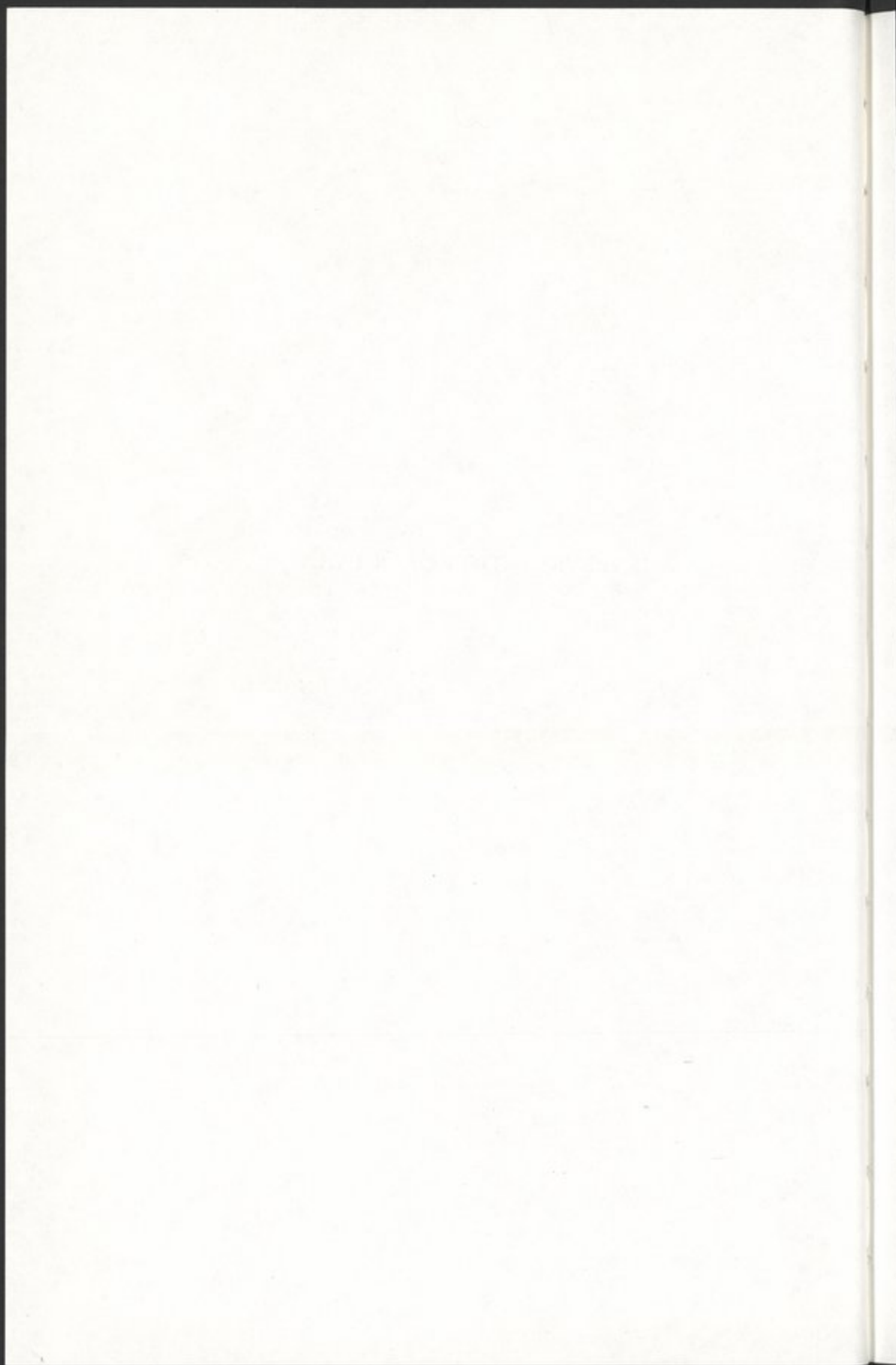
<sup>10</sup> According to the *-wh* isogloss, Samalian should be placed above Deir ʿAlla on the Aramaic branch, for that writing occurs only once, with the noun "father," probably /ʔab + ū + hu/, whereas the plural noun plus 3 m.s. suffix is *-yh*.

<sup>11</sup> S.A. KAUFMAN, *BASOR* 239 (1980) 73. In terms of the most basic formulation, I cannot see that any progress has been made over KAUFMAN's description, neither in publication during the decade since it was made, nor orally in the course of this conference. It is impossible to know whether the typological archaism corresponds to a chronological one (i.e., whether the text was already an ancient one when written down ca. 800 B.C.) or/and has a geographical component (i.e. was imported from Damascus or further away). Suffice it to say that such hypotheses are neither necessary to explain the presence of the text nor, on the other hand, ruled out by any historical or literary data of which I am aware.





## INTERPRETATION OF DETAILS





## PHILOLOGICAL OBSERVATIONS ON THE DEIR 'ALLA INSCRIPTION

Jonas C. GREENFIELD

In preparation for delivering this paper on the Deir 'Alla Plaster Text (= DAPT), I reread a good part of the literature that has been published since the *editio princeps* appeared.<sup>1</sup> I admit to having felt a good deal of alienation. My feeling was that beside the editor of the text, there were only two others, among those who had written, who could lay claim to being an Aramaist. For an Aramaist is not one who teaches a course in Biblical Aramaic every few years and piddles with some Aramaic inscriptions; he is rather a scholar for whom Aramaic is one of the main focuses of his attention. After years of dealing with Aramaic in all of its dialects I thought that I would recognize an Aramaic text when it is set before me. It was dismaying to learn that establishing an Aramaic text was for some scholars simply a balancing act. A matter of reckoning the supposed Aramaic features and setting them against the supposed count of Canaanite features.<sup>2</sup> This sufficed for them. The same effect could be achieved by listing and counting those scholars in one group against those in the other.<sup>3</sup> Simple bookkeeping rather than serious deliberation.

What then are the arguments in favor of DAPT being Aramaic? Let us first examine the morphological details listed in favor of this identification.<sup>4</sup>

1) The plural ending *-n* (i.e. *-īn*) for the masc. noun. Beside Aramaic this is found in Moabite, more or less contemporary with DAPT. It is also the standard plural in Mishnaic Hebrew.<sup>5</sup>

<sup>1</sup> The bibliography presented in Jo Ann HACKETT, *The Balaam Text from Deir Allā*, Chico, 1984, is assumed in this study. I will relate to later articles not listed by her at the appropriate place.

<sup>2</sup> So S.A. KAUFMAN, "The Classification of the North West Semitic Dialects of the Biblical Period and Some Implications Thereof" in the *Proceedings of the Ninth World Congress of Jewish Studies Jerusalem, 1985*; Hebrew and Aramaic Panel Sessions, edited by M. BAR-ASHER (Jerusalem, 1988), pp. 41-57, esp. p. 51.

<sup>3</sup> So A. WOLTERS, "The Balaamites of Deir 'Alla as Aramean Deportees", *HUCA* 58 (1987), 101-113.

<sup>4</sup> See also HACKETT, pp. 109-124.

<sup>5</sup> The plural *-n* is not due to Aramaic influence, since Biblical Hebrew was in

2) Determination in the noun. The examples of determination noted are not from DAPT but from probably later Aramaic material found at Deir 'Alla.<sup>6</sup>

3) Preformative *aleph* in the  $\text{ʔtp}^{\text{cl}}$ . Is this known elsewhere in early Aramaic? Bar-Rakib (*KAI* 216, 14) used a form of the  $\text{htp}^{\text{cl}}$ . At an earlier period we find  $\text{ʔpt}^{\text{cl}}$  clearly used in Ugaritic, while Phoenician, Moabite, and Hebrew have  $\text{htp}^{\text{cl}}$  and  $\text{hpt}^{\text{cl}}$ . This form must clearly be seen as an innovation in DAPT.

4. Infinitive with *mem*-preformative. No example exists!<sup>7</sup> It is worth emphasizing that the *peal* infinitive with *mem*-preformative is a feature of early Aramaic, occurring in the Tell Fekherye inscription and would have been a clear hallmark of Aramaic.

5. The *t* as a sign of the third person fem. sg. perf. as in *hqrqt* (I, 15).<sup>8</sup> This is the usual form in Ugaritic, and is preserved as a rare feature in Hebrew. With suffixes it is the normal form in Hebrew and Phoenician. There is good reason to assume that the regular form for final *w/y* roots in Hebrew, i.e. *hyth*, *rʔth*, *glth* preserves a final *t*, adjusted to the prevalent third fem. sg. perf. form. Note that in Mishnaic Hebrew forms such as *hyt*, *rʔt* etc. are standard in the better manuscripts.

6. The *-yw* in *štyw* (I, 10).<sup>9</sup> If the text read  $\text{ʔštyw}$  (with prothetic *aleph*) I would be the first to raise the Aramaic banner. However, forms with *-yw* are well known in Hebrew. For the perfect note *hsyw* (Deut. 32,37); *ntyw* (Ps. 73,2) *dlyw*, (Prov. 26,7) and the nif'al *nittāyū* (Num. 24,6 Bileam!).<sup>10</sup>

7. The possessive suffix with plural nouns and pronouns *-wh*. This has been compared with Early Aramaic *-wh* (= *awhi*). Note however the presence of *-h* in Moabite *ymh* "his days" (1.8).<sup>11</sup>

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continuous liturgical use. It undoubtedly originated in a colloquial dialect (northern?) which replaced 'Biblical' Hebrew.

<sup>6</sup> The reading *kms*<sup>2</sup>, rather than *mly*<sup>2</sup>, has been argued for by HACKETT, p. 33, and by E. PUECH, most recently in "Le texte 'ammonite' de Deir 'Alla: Les admonitions de Balaam (première partie)" in *La vie de la Parole, Etudes ... offertes à P. Grelot* (Paris, 1987), pp. 13-30.

<sup>7</sup> It is listed by KAUFMAN, p. 51 without any reference. He is the only scholar dealing with DAPT to make this claim.

<sup>8</sup> I have chosen this sure verb rather than *hrpt* since some have taken it as the name of a fowl.

<sup>9</sup> KAUFMAN, p. 51, no. 7.

<sup>10</sup> See GESENIUS-KAUTZSCH-COWLEY, p. 212, #75 *u* for further examples in the perfect and imperfect.

<sup>11</sup> We have no idea what the situation was like in Israelite Hebrew. Note that



8. The jussive forms in the third person masc. sg. imperfect of final *w/y* roots in *-y*. This is an innovation shared with Early Aramaic. There is no trace of this in later Aramaic dialects. I do not believe that there is in the morphology of the DAPT real evidence of the text being in an Aramaic dialect, or one particularly close to Aramaic.

If syntax is properly the next subject to be dealt with, it is quite clear that there are no syntactic features in the DAPT that can be distinctly labelled Aramaic. The rubric in line 1. must be interpreted in a way to include the word *hʿ*. This descriptive phrase is then syntactically proper ʿš *hzh ʿlhn hʿ* 'he is a man who 'sees' the gods'. The attempt to take *hʿ* as the Aramaic exclamation *hā* rather than the copula *hū* would produce a syntactic structure for which there is no known parallel.<sup>12</sup> The nominal sentence beginning with the word ʿš can normally be followed by a phrase beginning with a *waw* consecutive. The Aramaist feels the lack in this text of two items that are frequent in Aramaic texts: a) the use of *l* as the nota accusativi and b) the relative pronoun *zy*.<sup>13</sup>

The following are the morphological features that are without doubt Canaanite, that is they are known from the various languages and dialects that are usually subsumed under the unsatisfactory heading 'Canaanite'.

1) *Nif'al*. There is no trace of the *nif'al* in any Aramaic dialect. This includes Samallian where its presence has been established by restoration. A dubious procedure at best.<sup>14</sup>

2) The *waw* consecutive. The past tense is clearly stated in DAPT by the use of *waw* with an imperfect verb: *wyhz* (I, 1); *wyʿtw* (I, 1); *wyʿmrw* (I, 2), etc. some of which are clearly identifiable as short forms. These are used in narrative prose, rather than in a poetic,

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Samallian Aramaic differs in this point from other dialects of early Aramaic, and Byblian is also different from 'standard' Phoenician.

<sup>12</sup> Can a comparable use of *hā*, followed by past action, be cited? As Gordon HAMILTON, quoted by HACKETT, p. 30, n. 1, noted, the use of red ink should not be taken as a syntactic unit marker. In the first line it finishes with ʿlhn, and is taken up in the second line at the point that it leaves off in l.1. This was confirmed by close examination of the original, on display, in Leiden.

<sup>13</sup> This discussion of the 'Aramaic' features, as well as that of the 'Canaanite' features has profited from the study by B. HALPERN, 'Dialect Distribution in Canaan and the Deir Alla Inscriptions', in *Working with no Data, Semitic and Egyptian Studies Presented to Thomas O. Lambdin* (Winona Lake, 1987), pp. 119-138.

<sup>14</sup> WOLTERS, p. 111 has not bothered to check the evidence.

context as in the Zakkur inscription, the only other Aramaic inscription in which a *waw* consecutive may be discerned.<sup>15</sup> The poetic form of the Zakkur inscription—the Danklied—the use of chiasmus and other features indicate Canaanite influence.<sup>16</sup>

3) Peal infinitives without *mem*-preformative. As noted above this is lacking in DAPT. Even if one argued that this feature was not common to all dialects of Early Aramaic, the presence of *ldt* (II, 17), the typical Canaanite infinitive of a prima *waw* verb demonstrates that the infinitives in DAPT are 'Canaanite' in type. On the other hand the infinitive absolute cannot be used as a 'proof' since it is also found in Early Aramaic.

4) The apocopated form of final weak verbs with *waw* consecutive.

5) Forms of the indicative imperf. pl. without *-n*.

6) The use of the imperative *lkw* "go", undoubtedly Canaanite. Whereas it was possible to show that almost all of the supposed Aramaisms were easily disposed of, the 'Canaanitisms' in the morphology of DAPT are not refutable.

The truly egregious problem may be placed under the heading of phonology, or more correctly, what passes for phonology but is actually a matter of orthography or graphemics. The problem simply stated is that at first blush the representation of the consonantal inventory of this dialect, and I am purposefully avoiding the term phoneme, is the same as that of Early Aramaic. That is, the feature that distinguishes the orthography of DAPT from that of the Canaanite dialects is the use of a /q/ rather than /š/ for etymological \*q. <sup>17</sup> Although I believe that a plausible etymology and interpretation can be found for all the proposed examples of supposed \*q > /q/, it shall be assumed for the sake of argument that this derivation is correct.<sup>18</sup> The one example that I would exclude from this group

<sup>15</sup> E.Y. KUTSCHER denied that this was a *waw* consecutive and interpreted the use of the imperfect as a 'narrative mode'.

<sup>16</sup> See J.C. GREENFIELD "The Zakir Inscription and the Danklied" in *Proceedings of the Fifth Congress of Jewish Studies* Jerusalem, 1969 (published 1971), pp. 332–339.

<sup>17</sup> I would note that in Early Aramaic there are some exceptions too, \*qr "enemy" is written /qr/ in KAI 214, 1.30; and \*hqr is hqr in KAI 222, 1.28.

<sup>18</sup> By \*q > /q/ is meant the graphic representation of an etymological \*q by the sign for the qof. The pronunciation remains unknown to us. In contemporary inscriptions of Tiglath-Pileser III, the ruler of Damascus whose name is written consonantly in the Hebrew Bible as RšYN is found as both Ra-ḥi-a-nu and Ra-qi-a-nu in cuneiform transcriptions.



is *hqrqt* (I,15). If it is a verb, rather than the name of still another bird, there can be no doubt that it is the causative of *qrq* known from both Old Aramaic and Reichsaramäisch. It is however unsound to assume that behind the root *qrq* there is a root *ḏrq*. The reasons for this are: 1) this root does not appear in any known language; 2) in terms of Semitic consonantal patterns it is doubtful that it could exist since the presence of two emphatics in a root was highly unusual; 3) a neater explanation for *qrq* is known; the original root was <sup>c</sup>*rq*, which has both Hebrew and Arabic etymons.<sup>19</sup> The Old Aramaic form came into being by assimilation <sup>c</sup>*rq* > *qrq*.

However, if one does accept the generally shared view that in DAPT the phenomenon \*ḏ > /q/ does occur, what does it signify? Does Ugaritic become a form of Aramaic because most, but not all, etymological ḏ appear as ḏ? Indeed, the phonology of Ugaritic provides an object lesson. In Ugaritic both \*ḏ and \*ṣ have coalesced, as in the later Canaanite dialects, and also in Akkadian, leaving only ṣ. In the Ugaritic repertoire we find that etymological \*ṣ is represented by both /z/ and /ḡ/. Is this phonetic, graphemic or perhaps something else? As is well known there are two tablets (CTA 75,77) that use for some of the phonemes a different set of signs. The Tell Fekherye inscription has been cited by some writers when discussing DAPT. In that inscription there is the anomalous situation that ṣ is written with a /s/ as if we were dealing with Ethiopic or one of the modern Arabic dialects. It is clear that this is only a local phenomenon.

To return to the problem of the \*ḏ, how does one explain the co-existence in Hebrew of the frequent *mḥṣ* together with the unique *mḥq*, both presumably from \**mḥḏ*, or *rbṣ* and *rbḥ* both from \**rbḏ*. To these still other examples may be added. In the Bisitun inscription in Aramaic from Elephantine both *l<sup>c</sup>rqh* and *l<sup>c</sup>ḥ* are found for "toward him". Other words with etymological \*ḏ are also found written with both /q/ and /ḥ/ in the Elephantine papyri and other documents of that period. Will any one claim that the use of <sup>c</sup>*rq* "land, earth" in these texts as well as in Jer. 10,11 is anything more than a historical spelling? One should also note that in Mandaic the word for "land, earth" was written *arqa* rather than *ara*, as expected.

<sup>19</sup> The Arabic etymon <sup>c</sup>*araga* means 'to penetrate into a country' and is a good example of opposite meanings in roots, while <sup>c</sup>*rq* 'to flee' is surely the correct root of *hā-ḥ-ṣ-ḡ-rqīm* "those who flee" in Job 30,3.

Here too orthography can play no role in determining the position of this dialect; this is even truer for dialects that are poorly documented. The graphic realization of a phoneme that is problematic in all the Semitic languages cannot play an important role in determining the dialectic status of a dialect or language.<sup>20</sup>

*Vocabulary.* Are there any lexical elements in the DAPT that may be labeled as particularly Aramaic? From my point of view the only item that can be surely marked in this manner is *hd* (II,10). This would be an innovation that the dialect of DAPT shared with Aramaic. The word *br* that is part of the name *bl<sup>c</sup>m br b<sup>c</sup>r* is used to indicate the ethnic background of *bl<sup>c</sup>m* but nothing else about the language of the inscription can be learned from it. The Kilamuwa inscription (KAI 24) which is in Phoenician indicates Kilamuwa's ethnic background in a like manner. Three verbs that may at first blush seem typically Aramaic—*ʔth*, *hzh* and *hwh*—also are found in Hebrew, *ʔth* and *hwh* admittedly in a specific context, and *ʔth* and *hzh* are known from Ugaritic. Many of the 'Aramaic' vocabulary items listed by some who have discussed this inscription prove under scrutiny to be *fata morgana* when examined closely, and some taken to be Aramaic are not really that. Is *tpr* "to sew" (I,6) Aramaic?<sup>21</sup> The usual Aramaic root is *hyt*. Is *hrpt* "revile" (I, 7–8) Aramaic? Recourse to DALMAN or even JASTROW is not sufficient, rather a painstaking investigation is needed for each vocable. Such an investigation would show in the case of *hrpt* that this occurs only in the Targumim, and there overwhelmingly in the Pseudo-Jonathan targum, a dubious lexical witness. The noun *hrpt*<sup>2</sup> is usually a translation of Heb. *herpā*, while the other noun *hrwpyn* betrays by its form its Hebrew origin.

We are frequently told that in establishing the linguistic affiliation of a language or dialect, the vocabulary is not of prime importance. There are, however, circumstances when this is not so, and the case at hand is surely one of them. In DAPT there is a large number of phrases and expressions whose Canaanite connection is clear and not accidental.

1. *wyhz. mhz. kms<sup>2</sup>. ʔl* (I,1–2). DAPT presents here a complex

<sup>20</sup> It should be noted that the pronunciation of *ḏ* was a problematic subject in classical Arabic and remains so in the modern dialects.

<sup>21</sup> Assuming that this is the correct explanation. Others would see here a form of *pr* "to break, scatter". Would this be Aramaic?



phrase. In Hebrew, in the Bileam pericope the phrase *maḥāze šadday yehēze* is found (Num. 24, 4.16) while in Isa. 13,1 and Hab. 1,1 the phrasing is *maššā<sup>2</sup> ʾāšer ḥāzā*. DAPT combines both phrases.

2. *kh.yp<sup>c</sup>l*. (I,2) In Biblical Hebrew *koh ya<sup>c</sup>āse* is part of an oath clause, and is different in function than *kh yp<sup>c</sup>l* in DAPT, but they both refer to divine action.

3. *wy<sup>2</sup>mr. lhm. šbw. ʾḥwkm. mh. š . . .* (I,5). The use of *yšb* followed by a verb indicating narration is not found in Biblical Hebrew but other anticipatory verbs are used in this manner. Thus I Sam. 15,16: *heref wē ʾaggidāh lēkā ʾet ʾāšer dibber Y ʾelay ha-laylā*; Gen. 49,1: *he<sup>2</sup>āsēfū wēʾaggidāh lākem*. The verb *ḥwh* is found three times in similar circumstances in Job: 1) (32,10) *šim<sup>c</sup>u lī ʾāḥawwe dē<sup>c</sup>ī*; 2) (36,2) *katter lī zē<sup>c</sup>īr wēʾāḥawwekā*; and with a skillful shifting of the verbs 3) (15,17) *ʾāḥawkā šema-lī wēze ḥāzītī wēʾāsappērāh*.

4. *wkw.r<sup>2</sup>w.p<sup>c</sup>lt. ʾlhn* (I,5). This is essentially a continuation of the previous phrase with the imperatives *šbw* and *lkw* connected by the *waw* of *wkw*. Two Biblical verses are parallel to this phrase: 1) *lēkū ḥāzū mif<sup>c</sup>ālōt Y* (Ps. 46,9) and 2) *lēkū urē<sup>2</sup>ū mif<sup>c</sup>ālōt ʾēlōhīm* (Ps. 66,5) with both *r<sup>2</sup>h* and *ḥzh* used in the Biblical text.<sup>22</sup> In both passages divine deeds are related.

5. *ʾl[h]n. ʾtyḥdw.wnšbw.šdyn.mw<sup>c</sup>d* (I,5-6). It should be noted that the root *yhd* is virtually non-existent in Aramaic. The only other occurrence known to date in early Aramaic is in the Zakkur inscription, itself colored with Canaanitisms. The few occurrences in later Jewish Aramaic are clearly based on Mishnaic Hebrew usages. The verb *nšbw* is surely to be construed as a *nif<sup>c</sup>al*, and the usage is similar to that of Ps. 82,1 a verse referred to by various scholars. I do not believe that sufficient attention has been given to the occurrence of *mw<sup>c</sup>d* here. The word *mw<sup>c</sup>d* for the place of assembly, divine or human, is known from Ugaritic, Canaanite and Hebrew sources, but not from Aramaic material. The scene of the gods assembling is reminiscent of the divine assembly known from Ugaritic sources.

6. *šm.ḥšk.w<sup>2</sup>l.ngh* (I,6-7). Although *šm* could be taken as "there" the use of *šyt/šym ḥšk* for bringing on darkness is well known in Biblical Hebrew (Isa. 5,20; Ps. 104,20) and makes the verb

<sup>22</sup> One is tempted to see the *mem* of *mif<sup>c</sup>ālōt* as being a misconstrued enclitic *mem* which belongs properly with the previous verb. This would make the Biblical occurrences and the DAPT even closer in form.

preferable here, and *ngh/hšk* constitute a highly usable word-pair (Isa. 9,1; 50,10; Amos 5,20; 2 Sam. 22,29/Ps. 18,28). The next three words must be of similar import since *w<sup>l</sup>* functions in a similar manner.

7. *šm<sup>c</sup>w.mwšr* (I,10). It is quite possible that the previous word *qb<sup>c</sup>n* or the following *gry* should be read together with *šm<sup>c</sup>w.mwšr*, both have been suggested. I do not, however, think that these two words should be separated. The noun *mwšr* is known particularly from Hebrew, and not from Aramaic where *mardūtā* is the usual term for 'discipline' or 'chastisement'. The combination of *šm<sup>c</sup>* with *mwšr* is found in a number of passages in Proverbs, and in expanded form in Jeremiah and Zephaniah.

The second combination presents greater difficulties in interpretation and translation but here too there are usages that have clear parallels in Biblical texts:

8. *rwšy.ddn* (II,4). The parallel with Prov. 7,18 and perhaps with 5,19 has been noticed.

9. *wrmh.mn.gdš* (II,8). Despite various attempts to understand *byt.<sup>c</sup>lmn* in the previous line in a different manner, it must clearly mean 'cemetery', and *gdš* is then a 'tomb' as in Job 21,32. This word does not have a cognate in Aramaic, although it does have one in Arabic.

10. *hl<sup>c</sup>šh.bk.lyt<sup>c</sup>š.<sup>2</sup>w.lmlkh.lytmlk* (II,9). As HACKETT and others have noted this is an extended question, introduced by the interrogative *h...* and continued by *<sup>2</sup>w...* There is one example of this in Biblical Hebrew in 2 Kings 6,27: *hāmin ha-goren <sup>2</sup>ō min ha-yeqeb*. The king of Israel declares that he cannot supply food for the famine-stricken population of Samaria and asks "shall it come from the silo or from the press?" The question in DAPT follows the form of rhetorical questions known from Ugaritic and Hebrew.<sup>23</sup> In Ugaritic the rhetorical question takes the form *Ø... hm...* while in Biblical Hebrew the form is *h... <sup>2</sup>m...* In DAPT the form is *h... <sup>2</sup>w...* The interchange of *<sup>2</sup>w* and *<sup>2</sup>m* is well known (see Ex. 21,31 vs. 21,32, etc. *<sup>2</sup>w...<sup>2</sup>w...* for *<sup>2</sup>m...<sup>2</sup>w...*). There is no known example of the rhetorical question from Aramaic texts. A word is in order about the two roots used here. It is clear that *y<sup>c</sup>t*, the standard Aramaic equivalent of Hebrew *y<sup>c</sup>t*, occurs only in

<sup>23</sup> See M. HELD, "Rhetorical Questions in Ugaritic and Biblical Hebrew", *Eretz Israel* 9 (1969), pp. 71-79.



Standard Literary Aramaic (Ahiqar Framework Story, Daniel, Ezra). It is unknown in the other Aramaic dialects and the examples that may be adduced from 'Jewish Aramaic' are chimerical.<sup>24</sup> On the other hand the root *mlk* as "to counsel" is found in Akkadian, as well as in Hebrew and Aramaic, and is not a typically Aramaic root. I would translate this line as "will he surely not take counsel with you, will he surely not ask advice (of you)?" The *l* preceding the nouns having an emphatic function, those preceding the verbs a negative function. As is typical of both Ugaritic and Hebrew poetic texts the preposition *b-* has a double duty function.

These ten items, to which some others may be added, are not mere vocabulary but are part of the inner structure of the literary dialect whose sole remains are to be found in DAPT.

Various terms have been used in discussing the language of the DAPT and it might be best to say something about them at this point. Thus some scholars have spoken of a 'linguistic continuum'. The language of the DAPT is considered a stage in the linguistic continuum from the Aramaic north to the Canaanite south. But do such linguistic continuums exist when there are real physical barriers such as mountains, rivers, deserts, etc. Political boundaries are often just as real and linguistic boundaries can be hard and fast. It has recently been suggested that we have in the Deir ʿAlla inscription a sort of pidgin Aramaic, with Amarna Akkadian used for comparison.<sup>25</sup> This comparison is not real, for the Amarna correspondence with Canaan was written in Akkadian, but an Akkadian shaped by Canaanite morphology, and using Canaanite words, the latter on the whole clearly demarcated by the use of the *Glossenkeil*. The language of the DAPT is clearly not a pidgin language.

It is important to note at this point that the current tendency is to see a common 'Northwest Semitic' as the dominant language of the second millennium B.C.E. This idea was first propounded by Johannes FRIEDRICH and then variously expounded by Giovanni GARBINI and Sabatino MOSCATI. Even though there is little to recommend this view it has become rather fashionable recently.<sup>26</sup> It

<sup>24</sup> As has been noted by some scholars *ylʿš* may be from the root *ʿwš* which is rather rare in Hebrew; the forms quoted in Jewish Aramaic of *ʿwš* also seem secondary.

<sup>25</sup> See A. RAINEY, *BASOR* 273 (1989), p. 95.

<sup>26</sup> See most recently G. GARBINI, "Semitico nordoccidentale e amorreo", in his

seems to me, however, that the bifurcation of the Northwest Semitic languages into two basic groupings—Canaanite and Aramaic—took place in the early part of the second millennium. Over thirty years ago EDZARD showed that there were no traces of Aramaic in Amurrite.<sup>27</sup> Indeed there are no such traces in Ugaritic, and if one is to speak of a 'linguistic continuum' then it is between Amurrite, Ugaritic and Canaanite, a continuum in time rather than space. The linguistic phenomena registered for Ugaritic are on the whole Canaanite, and when a subject such as the taxonomy of the Semitic languages is dealt with, the linguistic phenomena known from Ugaritic can be used for the identification of a Canaanite feature, and in turn for that of retained features in the Canaanite dialects of the first millennium.<sup>28</sup>

What language or dialect are we dealing with then when we discuss the DAPT? Although Deir 'Alla is not that distant from Biblical Ammon proper, there is now hesitation to use the term 'Ammonite' for it. Ammonite, as known from the limited corpus of material available, displays a different set of linguistic features. The term 'Southern Canaanite' is best preserved for the Canaanitisms in the Amarna correspondence and related texts from the second millennium. Although the comparisons that have been made by this writer and by others are perforce with Biblical Hebrew, the language of the DAPT is clearly not Hebrew, as known from the major literary works preserved in the Hebrew Bible.<sup>29</sup> Taking geographic factors into consideration it may best be called Gileadite. It is a local dialect, close to the Canaanite of its time, sharing with it essential morphological, syntactic, phonological and lexical features.<sup>30</sup>

Some of the studies of the DAPT have dealt with its relationship

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*Le lingue semitiche*<sup>2</sup> (Napoli, 1984), pp. 113–144, with an excellent bibliography in the footnotes.

<sup>27</sup> D.-O. EDZARD, "Mari und Aramäer", *ZA* 56 (1964), pp. 142–149.

<sup>28</sup> I have not been convinced by S. SEGERT, "Ugaritisch und Aramäisch" in *Studia Semitica J. Bakoš dicata* (Bratislava, 1965), pp. 215–26 and his later articles on this subject.

<sup>29</sup> Although there are interesting ideas in J.W. WESSELIUS, "Thoughts about Balaam: The Historical Background of the Deir Alla Inscription on Plaster", *BO* XLIV (1987), 589–99, I do not accept his thesis that DAPT is written in Hebrew.

<sup>30</sup> This does not mean that contact with Aramaic and shared isoglosses are entirely excluded. There is undoubtedly Aramaic influence on the Ammonite script, and this could have extended to certain morphological, lexical and orthographic features. If we had a better idea of Israelite Hebrew in its varied aspects, it might be possible to assess the role of this dialect in the transmission of Aramaisms.



with the Bileam pericope in Numbers. From the Biblical text it is clear that Bileam was considered an Aramean, but this has no significance as to the language of the DAPT. He was a *hōze*, who saw visions by day and by night, asleep and awake. If *pēlōrā* (Num. 22,5) is not a geographic direction, as it is usually interpreted, but rather an occupational designation, we would learn that he was also an interpreter of dreams, that is he engaged in oneiromancy, which was widespread in the ancient world.<sup>31</sup> In the Biblical narrative he sacrifices seven oxen and seven rams before each pronouncement, as has been suggested, he may have functioned as a *barū* engaging in extispicy. In the so-called Cuthean Legend of Naram-Sin a *barū* sacrificed seven and then another seven sheep and then prophesied, his predictions being dire.<sup>32</sup> In the DAPT there is no inkling of sacrifices being made. Instead there are possible indications of the use of incubation rituals such as fasting and weeping (I,4). I understand the passage not as a means of expressing grief, or a post-factum expiatory act, but the means used to induce a vision. There may also be in the enigmatic list of birds a reference to augury or ornithomancy, another form of foretelling the future in which Bileam bar Beor may have been engaged. And if a bit further indulgence of fancy is allowed it may be suggested that the equally enigmatic *ʿnyl rḡht mr wknh* (I,11) may be a list of terms used for female sooth-sayers. The *ʿnyl* is not a 'poor woman' but an 'answerer', the equivalent of the *apiltu*, known from an earlier period in the Mari texts, and of the *ragintu* "speaker" known from neo-Assyrian texts. The *rḡht mr* describes the specific function of preparing myrrh, probably for libanomancy, and the *knh* 'priestess' presents no problem. It may be assumed that in our text these three terms are used of one and the same person who performed rites for Bileam bar Beor. Needless to say this is all sheer speculation.

Bileam was revered by these people, the Gileadites or north Ammonites, if you wish, and his memory was preserved in this shrine. Who read this text? A priest, or a scribe, in all likelihood, rather than schoolboys, as has been suggested. This is surely not the way that reading was taught in the ancient world. Literacy was still very

<sup>31</sup> See M. DELCOR, "Le texte de Deir ʿAlla et les oracles bibliques de Bal-ʿam", *VTS* 32 (1981), pp. 52-73, esp. pp. 64-65.

<sup>32</sup> See O. GURNEY, "The Cuthean Legend of Naram-Sin", *AS* 5 (1955), pp. 93-113, esp. p. 104, l. 109.

limited in the mid-eighth century and inscriptions, when visible, were intended to make an impression by their presence rather than by their contents, since these were not readily available to the average attendant at a shrine.

A fruitful area of comparison with the Deir 'Alla inscription which has not been given the attention that it deserves is the Book of Job. It has been noted by various scholars that the author skilfully used dialect to place the various speakers in context. Even the Elihu speeches are characterized by usages that may be considered dialectal. It is only the voice of the lord from the whirlwind that is written in classical Hebrew, albeit one that is highly poetical and lexically rich. There is also an important religious context for the book of Job is dominated by El and Shaddai, divine names also present in the DAPT.

We have in the DAPT, be it a unified composition, or composed of two separate texts, be it an original composition, or the copy or reworking of an earlier text, a work in a local dialect. This dialect may have been affected by contact with Aramaic, or with greater likelihood shared certain innovative morphological, orthographic and lexical features with Aramaic. I would at this point abjure any pan-Canaanite bias. My preoccupation with Aramaic studies in its literary, linguistic, historical and cultural aspects would forestall that. My opposition to meaningless lists of Aramaic compositions is strong, especially if such works do not exist. The addition of the DAPT to the supposed corpus of Aramaic literature distorts the nature of this literature, and adds very little to our appreciation of it.



## WHAT DID THE GODS SAY? REMARKS ON THE FIRST COMBINATION OF THE DEIR 'ALLA-PLASTER TEXTS

J. HOFTIJZER

On one point those who occupy themselves with the Deir 'Alla-plaster texts will agree: there is no *communis opinio*. This is also true of the lines 8ff. (6ff.) of the first combination: the words spoken by the gods who were gathered in an assembly.<sup>1</sup> Most authors agree that these words were spoken to a goddess, but recently WESSELIUS has proposed that they were spoken to a city, namely Samaria.<sup>2</sup> Those who think the words were spoken to a goddess do not agree on the identity of this goddess nor on the tenor of the words. According to some the words were meant to restrain her from punishing, according to others they were meant to incite her to a severe punishment. Although it is clear that the direct discourse starts in l. 8 (6), there is also disagreement on which part of the following lines still belongs to it and which not. And I have not mentioned yet the difference of opinion on nearly every detail. The idea of this symposium is to evaluate the studies made so far on the Deir 'Alla-plaster texts. In this lecture on a special problem from these texts, I will try to discuss, as far as possible, every relevant opinion and to determine which solution is the most probable one. The words *skry šmyn* in l. 8 (6) can be explained in two ways, either as "the bolts of heaven" or said to a female person "close the heavens".<sup>3</sup> That the words are spoken to a "you" in the female singular is clear from the form *thby* in l. 9 (7) and from the forms *'bky* and *sm/krky* in ll. 8 (6) and 9 (7). It seems less probable that we are not dealing here with a pronominal suffix 2 p.s.f. but with the conjunction *ky*.<sup>4</sup> Between

<sup>1</sup> I quote the text in accordance with the numbering of the *editio princeps*. The new numbering proposed by CAQUOT and LEMAIRE (1977) 193f. is placed in brackets after it. Although the rearrangements of fragments proposed by them seems, at first sight, very convincing, I still have some doubts, cf. HOFTIJZER (1986) 140.

<sup>2</sup> Cf. WESSELIUS (1987) 593f.

<sup>3</sup> For the first interpretation, cf. already HOFTIJZER (1976) 194, for the second one, cf. already CAQUOT and LEMAIRE (1977) 197.

<sup>4</sup> For the interpretation of *ky* in those two instances as conjunction, cf. LEVINE (1981) 197f., (1985) 329, cf. also LEMAIRE (1985a) 318, (1985b) 280, PUECH (1986) 286, (1987) 21.

*b<sup>c</sup>b* and *sm/kr* on the one side and *ky* on the other there is no word divider. Word dividers can be left out in this text, but only between words which are closely related as *nomen regens* and *nomen rectum*, as verbal form and subject, and as a preposition with the following nominal form.<sup>5</sup> It seems less probable that a conjunction introducing a hypotactic clause should be followed by a word divider, but not be preceded by it. The question which interpretation of *skry smyn* is preferable largely depends on that of the word preceding *skry*. In the *editio princeps* I proposed to read *y/t/htp.ry*.<sup>6</sup> CAQUOT and LEMAIRE already proposed to read *tpry*, which most authors read now.<sup>7</sup> I am inclined to agree with them.<sup>8</sup> They have interpreted this *tpry* as a Qal Imper. s.f. of *tpr* "to sew" and many authors have followed them.<sup>9</sup> However H. and M. WEIPPERT have rightly stated that it remains completely unclear how a form of a root with this meaning would fit the context.<sup>10</sup> Therefore I prefer to interpret *tpry* as a form of the root *prr* as H. and M. WEIPPERT have done, translating "you may break".<sup>11</sup> If this translation is right we have to interpret *skry*

<sup>5</sup> Cf. HOFTIJZER (1976) 183.

<sup>6</sup> Cf. HOFTIJZER (1976) 173, 193f. The reading of the *h* is uncertain, cf. v.d. KOOIJ (1976) 110.

<sup>7</sup> Cf. CAQUOT and LEMAIRE (1977) 197.

<sup>8</sup> Cf. the remarks of HACKETT (1984a) 42f.; cf. also MÜLLER (1982) 224 n. 60. It is possible that the bit of ink which I and v.d. KOOIJ (1976) 111 read as a word divider is "the tip of the long tail of a letter lost in the break in the line above", cf. McCARTER (1980) 53.

I also disagree with WESSELIUS' proposal (1987) 596 not to read *ltpry* but */ts/tkry*. The top of the sign which nearly all authors read as a *p* stands on a fragment which "is joined to the main fragment (turned nearly 180°)", cf. v.d. KOOIJ (1976) 110. WESSELIUS himself agrees that this top has preferably to be identified with a *p*-top. Therefore I do not understand his considering the reading */tkry* (with a *k* instead of a *p*) "better, whether or not the fragment really belongs here".

Also against SASSON (1986a) 288, 290, 296, who proposes the reading *ytk./ry* (cf. SASSON (1986b) 149).

<sup>9</sup> Cf. CAQUOT and LEMAIRE (1977) 196f.

<sup>10</sup> Cf. H. and M. WEIPPERT (1982) 92; cf. also SASSON (1986a) 296. The remark of McCARTER (1980) 54 that *tpry* and *skry* are both imperatives (of *tpr* and *skr* respectively) and that we find here "an instance of verbal hendiadys, meaning "stop up by sewing" and thus "sew shut" does not help either in solving the problem of the use of a derivative of the root *tpr* in this context. The reference made by WEINFELD (1982) 143 to God's tearing the Heavens (Is xliii 19) is as such no justification for assuming the idea that the Heavens can be sewn up. The interpretation proposed by MÜLLER (1982) 218, 224 n. 60 for *tpry* as "verhülle(?)" is not based on argumentation.

<sup>11</sup> Cf. H. and M. WEIPPERT (1982) 92, 103. In Classical Hebrew we find the Hiphil of the this root used in a figurative sense: breaking a covenant (Gen. xvii



*šmyn* as "the bolts of heaven". The breaking of the bolts of heaven means that the mass of water held back by them will not be restrained anymore.<sup>12</sup> Doors/gates with bolts are a safeguard from danger outside, there being no doors or bolts means that one is not protected from that danger (cf. Jer. xlix 31, Ez. xxxviii 11f.); if the bolts are destroyed somehow, the enemy may come in (cf. Jer. li 30ff., Nah. iii 12f., cf. also Lam. ii 9). But doors/gates with bolts can also serve as a safeguard from the danger within. According to Job xxxviii 10, God put bars and doors for the sea, to prevent it from crossing its border (cf. v. 11), and in this way shutting it in (cf. v. 8).

If we interpret *skry šmyn* as "close the heavens" we have to ask ourselves what this would mean. CAQUOT and LEMAIRE have proposed that it would mean a covering of heaven with a cloud which prevents the light of the sun from penetrating to the earth (they connect *b<sup>c</sup>bky* with *skry šmyn* and do not consider it as the beginning of a new clause).<sup>13</sup> Although RINGGREN translates the words in the same way as they do, he utters some doubts for contextual reasons: "The expression 'close the sky' is strange, since similar expressions usually refer to the withholding of rain . . ." <sup>14</sup> Indeed, the closing of heavens where it occurs in the Bible means the stopping or withholding of rain: Gen. viii 2 (the shutters of heaven being closed; form of *skr* used), Dt. xi 17 (form of *šr* used), 1 Kings viii 35 (form of *šr* used; = 2 Chr. vi 26), 2 Chr. vii 13 (form of *šr* used). The opening of the heavens means rain: Gen. vii 11 (cf. v.

14, Lev. xxvi 15, 44, etc., etc.), a vow (Numb. xxx 9, etc.), the law (Ps. cxix 126), etc. But in Zech. xi 10 the breaking of a covenant by God is represented by His breaking one of the two rods He has in His Hand (xi 7) in two pieces, cf. also v. 11. After that He breaks the brotherhood between Judah and Israel by breaking the other rod in two pieces (v. 14). Moreover the Polel of the root is used in Ps. lxxiv 13 in the sense of "to crush" parallel with a form of the root *šbr*. In Job xvi 12 the Pilpel of the root is used in the sense of "to crush" (sc. a person) parallel with a form of the root *pss*. In Is. xxiv 19 the Hitpolel of the root is used to describe the destruction of the earth parallel with a form of the root *r<sup>cc</sup>* (to break). I do not think that we have two roots *pr* in Classical Hebrew, as supposed in e.g. BDB and HAL. One may also compare Arabic *farfar*, which among others can mean "to cut", "to break".

<sup>12</sup> Cf. HOFTIJZER (1976) 194f.

<sup>13</sup> Cf. CAQUOT and LEMAIRE (1977) 197. Most authors have followed them, also those who divide *b<sup>c</sup>bky* in *b<sup>c</sup>* and the conjunction *ky* (v. supra). Cf. recently LAYTON (1988) 184. However PUECH (1985) 361 utters some doubts about *b<sup>c</sup>bky* belonging to the preceding clause.

<sup>14</sup> Cf. RINGGREN (1983) 94 (translation), 95 (remark); cf. already KAUFMAN (1980) 173.

12), or it means God procuring food: Ps. lxxviii 23 (the manna which *rained* upon the people, cf. v. 24; cf. also Ex. xvi 4, where God let it *rain* food from heaven).<sup>15</sup> Whereas rain is related to clouds and darkness (see below), phenomena which are mentioned in the context, it would, in my opinion, be very strange if here an expression was used meaning the withholding of rain and at the same time indicating darkness. Combining *b<sup>c</sup>bky* with *skry šmyn* does not solve this problem ("close the Heavens with your cloud").<sup>16</sup> Cf. also the fact that clouds are said to *cover* something.<sup>17</sup>

In l. 9 (7) the third word up to and including the seventh were read in the *editio princeps* *w<sup>3</sup>l.smrky.thby.ht*.<sup>18</sup> WESSELIUS has proposed to read *thby* instead of *thby*.<sup>19</sup> There is however no doubt that a *h* has to be read here.<sup>20</sup> Instead of *smrky* the reading *skrky* proposed by HACKETT is possible.<sup>21</sup> It also remains uncertain whether one has to read a word divider after *ht* or not.<sup>22</sup> In deciding which reading is the most probable one, we also have to look at the degree of probability of the different interpretation proposals. HACKETT has proposed to read *w<sup>3</sup>l.skrky.thby.ht/m b hšk* and translates: "And put the dark [ sejal on your bolt". "The gist of the phrase is still that the

<sup>15</sup> Cf. also Mal. iii 10.

<sup>16</sup> For this interpretation, cf. already CAQUOT and LEMAIRE (1977) 196f. The alternative interpretation suggested by HACKETT (1984a) 29, (1986) 217, 220: "in your cloud" (instead of "with your cloud") does not solve the problem either.

<sup>17</sup> Cf. Ps. cxvii 8 (the heaven), Ez. xxxviii 9, 16 (the earth), cf. also Ex. xxiv 15, 16, xl 34, Lev. xvi 13, Numb. ix 15, 16, xvii 7, Ez. xxx 18 (cf also Ez. xxxii 7 the covering of the sun with a cloud).

<sup>18</sup> The preceding clause will be treated below.

<sup>19</sup> Cf. WESSELIUS (1987) 596.

<sup>20</sup> Cf. v.d. Kooij (1976) 107. The reason for WESSELIUS' hesitation to accept the reading *thby* is that in Hebrew and Aramaic there would not occur a Qal Imperfect of the root *yhb*. It is true that e.g. in Official Aramaic there only occurs an imperfect Qal of *ntn* and not of *yhb*. The way however in which WESSELIUS dismisses the imperfect form *thb* in KAI 222B 38 is too easy. In the Sfire texts no imperfect form of *ntn* occurs, and although the context is damaged, the translation of *lthb lthmy* with "you will not procure my bread/food" seems highly probable in a context speaking about the procurement of food. The situation we find in Official Aramaic (and elsewhere) must not for that reason be necessarily found in every older Aramaic dialect. Therefore I prefer to follow the majority of authors (e.g. DEGEN (1969) 74) in interpreting this *thb* as Qal Imperfect 2p.s.m. of *yhb*. That the Deir 'Alla dialect has also its own peculiarities is a reason the more to consider the possibility of the interpretation of *thby* as Qal Impf. 2p.s.f. of *yhb*. Moreover linguistic problems can be no real argument for proposing a new reading against palaeographical evidence.

<sup>21</sup> Cf. HACKETT (1984a) 45, (1986) 220.

<sup>22</sup> Cf. also v.d. Kooij (1976) 107.



goddess is being told to put a (dark) seal on her "bolt" in the heavens, a "bolt" that may be the cloud itself, or something applied to the cloud."<sup>23</sup> This is done "to seal up the sky forever".<sup>24</sup> This interpretation can be considered only if the text speaks of a closing of the heavens; in my opinion however this interpretation is not probable (see above).

McCARTER has been the first to read *h̄tm*.<sup>25</sup> But before I go into this point it is best to discuss the problems of *ʔl.sm/krky*. In the context there is no interpretation possible of *skrky* (provided one accepts this reading) than as a derivative of *skr* "to close" + a pronominal suffix 2p.s.f. "your closing" or "your bolt". This makes this reading less probable. Those who read *smrky* have proposed different interpretations. This difference in interpretation is (at least partly) connected with the different clause divisions they propose. Many authors consider *h̄šk.wʔl.ngh.ʕtm.wʔl.smrky* as two pairs which are more or less parallel.<sup>26</sup> It is clear that *h̄šk.wʔl.ngh* is a pair.<sup>27</sup> That *smrky* has a pronominal suffix may seem peculiar at first sight, nevertheless it cannot be used as an argument against the interpretation mentioned here.<sup>28</sup> The question is whether *ʕtm* and *smr* can be interpreted as nouns referring respectively to "darkness" and "light". The other question is whether *smr* can refer to "dread" and so could be a parallel to *h̄t*.<sup>29</sup> SASSON has said that "it is erroneous to equate *smr* with "fear" because the root *smr* denotes "the physiological reaction which a person experiences" as "the outcome of the psychological process denoted by *yrʔ* and *ph̄d*."<sup>30</sup> There is no doubt that *smr* denotes the physiological reaction to fear, cf. the parallel of *yrʔty* with *smr b̄stry* in Ps. cxix 120 (the last expression

<sup>23</sup> Cf. HACKETT (1984a) 29, 45.

<sup>24</sup> Cf. HACKETT (1986) 217.

<sup>25</sup> Cf. McCARTER (1980) 51, 54.

<sup>26</sup> Cf. already CAQUOT and LEMAIRE (1977) 197f. For the discussion whether the reading *ʕtm* is right, see below.

<sup>27</sup> Cf. the parallel pairs *h̄šk . . . wʔl-ʔwr wʔpl wʔl-ngh* in Amos v 20. Cf. also the pair *h̄šk wʔl-ʔwr* in Amos v 18, Job xii 25, Lam. iii 2.

<sup>28</sup> For a survey of the occurrence of the so-called double duty suffix, cf. e.g. DAHOOD and PENAR (1970) 429ff.

<sup>29</sup> So HOFTIJZER (1976) 198. The reading of *ʔl* before *smrky* cannot be considered as certain (cf. v.d. KOOIJ (1976) 106f., HACKETT (1984a) 45), nevertheless nearly everyone accepts this reading. GARBINI (1979) 171, 176 proposes to read *ʔ/d/* "mist, fog". For contextual reasons this proposal is not convincing.

<sup>30</sup> Cf. SASSON (1986a) 297, cf. already idem (1985) 102.

also in Job iv 15).<sup>31</sup> However this also implies that the physiological reaction to fear can stand in parallelism to fear itself. Cf. also the *wyr<sup>3</sup> wyhrd lbw* in 1 Sam. xxviii 5 and *hrd wphd* in Is. xix 16. Against this background the parallelism of two nominal forms denoting respectively "reaction to fear" and "fear" is not to be excluded. Cf. also the parallelism of *yr<sup>3</sup>h* and *r<sup>c</sup>dh* in Ps. ii 11<sup>32</sup> and of *phd* and *r<sup>c</sup>dh* in Job iv 14. One may also compare *qwl hrdh šm<sup>c</sup>nu phd w<sup>3</sup>yn šlw<sup>m</sup>* in Jer. xxx 5. Therefore one cannot exclude the possibility that one finds in our text a parallelism of *smr* (= "shuddering for fear") and *ht* (= "terror").

The question we now have to consider is, can *šm* and *šl smr(ky)* possibly be a pair which is more or less parallel to *hšk w<sup>3</sup>l ngh*? Formally there is much which pleads the case of this interpretation. However, if we accept it, there remains the problem of the interpretation of both words *šm* and *smr*. The interpretation of *smr* as an object (an interpretation which in itself is possible): pole, sceptre, does not fit the context.<sup>33</sup> McCARTER (1980) 51, 54 has hesitantly proposed to interpret *smr* with "radiance".<sup>34</sup> However his remark "that the verb *smr*, "bristle", might mean by extension "bristle with light" and thus "radiate"; hence the noun would mean "radiance",<sup>35</sup> in my opinion, tells us more about the semantics of the English "to bristle", than that it is an adequate description of the semantic possibilities of the root *smr*. PUECH (1987) 22, interpreting *smr* in the same way, refers to a corresponding word in Arabic meaning "light/radiance of the stars/the moon". An Arabic noun exists which indeed can have this meaning: *samar*.<sup>36</sup> Still I doubt whether it is right to adduce this noun as a possible help for the interpretation of the *smr* in the Deir Alla-plaster texts. The Arabic noun is probably derived from a root in which the idea of "night" is one of the semantic components: hence the meaning "light of the moon", etc.

<sup>31</sup> Here also the context speaks of fear (v. 14).

<sup>32</sup> Cf. the combination *yr<sup>3</sup>h wr<sup>c</sup>d* in Ps. lv 6.

<sup>33</sup> CAQUOT and LEMAIRE (1977) 196 translate hesitantly "ton timon", but they mention on p. 198 the contextual problems. MÜLLER (1982) 224 translates with "lance", but combines *šl smrky* with the next clause. His proposal *ibid.* 218 to take *smrky* as the subject of this clause seems impossible. Also RINGGREN (1983) 94 hesitantly proposes a translation "pole, sceptre".

<sup>34</sup> Some authors followed him, cf. LEVINE (1981) 197f.

<sup>35</sup> Cf. McCARTER (1980) 54.

<sup>36</sup> Cf. e.g. LANE (1872) 1425.



This is an insufficient basis to defend an interpretation with "radiance".<sup>37</sup>

The interpretation of  $^c\text{tm}$  is also very difficult. I completely agree with those who considered my original interpretation as a possible derivative from the root  $^c\text{zm}$  as less convincing.<sup>38</sup> However the reading can be considered as reasonably certain.<sup>39</sup> The reading proposals  $^c\text{dm}$  and  $^c\text{lm}$  (the last mentioned one would in itself fit the context very well) seem less probable.<sup>40</sup> H. and M. WEIPPERT, assuming the existence of the two parallel pairs mentioned above, propose for  $^c\text{tm}$  a negative interpretation ("Dunkel"?) without being able to give an etymological or semantic explanation.<sup>41</sup> SASSON (1985), 102 (cf. idem (1986a) 297) relates  $^c\text{tm}$  to the Arabic root  $^c\text{tm}$  derivatives of which have the idea of "darkness" as semantic component.<sup>42</sup> Although the meaning as such would fit the context, the proposed etymological relationship is unconvincing. PUECH (1987) 22 has proposed to relate  $^c\text{tm}$  to the Akkadian noun *etemmu* (= shade of a dead one).<sup>43</sup> In my opinion a possible etymological relationship of the  $^c\text{tm}$  in the Deir Alla texts with this Akkadian noun is an insufficient basis to defend an interpretation as "darkness". Another interpretation proposed by PUECH stands a better

<sup>37</sup> SASSON (1985) 103, (1986a) 288, 297f. has proposed to relate the *smr* of l. 9 (7) to the same Arabic root, referring to *samar*- meaning "night", "darkness" and *asmar*- meaning "(dark-)brown". To avoid however an interpretation of  $^2\text{l smr}$  as "light" (which would not fit the context) he proposes to read ...  $^c\text{tm}$  *w/srh.b/smrky* ... However, in my opinion, there is not enough room to justify the assumption that between *w* and *smrky* there once stood four graphemes and a word divider (also against SASSON (1986b) 149; cf. also V.D. KOOIJ (1976) 106f.).

<sup>38</sup> Cf. HOFTIJZER (1976) 197, 284. For the criticism, cf. e.g. NAVEH (1979) 136, ROFÉ (1979) 66 n. 28, H. and M. WEIPPERT 93 n. 77, PUECH (1985) 362.

<sup>39</sup> Cf. V.D. KOOIJ (1976) 106; cf. also H. and M. WEIPPERT (1982) 92, PUECH (1987) 22.

<sup>40</sup> For the reading  $^c\text{dm}$ , cf. CAQUOT and LEMAIRE (1977) 197 (cf. also GARBINI (1979) 171, 176, MÜLLER (1982) 224); for the reading  $^c\text{lm}$ , cf. McCARTER (1980) 54 (cf. also LEVINE (1981) 197f., HACKETT (1984a) 27, 44, GARR (1985) 27).

<sup>41</sup> H. and M. WEIPPERT (1982) 93.

<sup>42</sup> In this connection he also mentions the  $n^c\text{tm}$  in Is. ix 18. Cf. also SASSON (1986b) 149.

<sup>43</sup> In this article he justly withdraws a previous proposal (PUECH (1985) 361) to relate  $^c\text{tm}$  etymologically to the Hebrew root  $^c\text{ty}$ . The proposal of PUECH (1987) 22 to consider the  $^2\text{ty}$  in Is. xix 3 as etymologically related to *etemmu* is less probable, because for contextual reasons it is most probable that  $^2\text{ty}$  is a plural form, in which case the *m* is not a root radical. The proposal of ROFÉ (1979) 66 n. 28 to interpret  $^c\text{tm}$  as Piel Imper. s.f. + suff. 3p.pl.m. is not convincing for contextual reasons.

chance, in my opinion. It is his proposal (at a suggestion of J.C. GREENFIELD) to relate  $\text{ḥtm}$  with the Aramaic root  $\text{ḥml}$  which has "darkness" as semantic component. (In this case one has to presuppose a metathesis).<sup>44</sup>

At this stage of my argumentation it is impossible to give an answer to the question I asked with any reasonable certainty. It is very attractive to consider  $\text{ḥšk } w^{\text{ḥ}}l \text{ ngh}$  and  $\text{ḥtm } w^{\text{ḥ}}l \text{ smrky}$  as two parallel pairs. But the uncertainty of the interpretation of the second pair (especially of *smr*) precludes all certainty. In my opinion, it is also attractive to consider *smr* and *ḥt* as parallels; from the interpretational side there is nothing against it. But it remains uncertain whether we find here *ḥt* followed by a word divider.<sup>45</sup> What is certain is that no *k*, *m*, *n* or *t* followed.<sup>46</sup> This means that readings like *ḥtt* or *ḥtm* which are proposed by some authors can be considered as excluded.<sup>47</sup>

Different interpretations also have been proposed for the words  $w^{\text{ḥ}}l \text{ thgy } \text{ḥ}d \text{ ḥlm}$  in l. 9 (7). Some authors have proposed not to derive *thgy* from the root *hgy* "to make a certain kind of noise", but of the homonymous root *hgy* "to remove".<sup>48</sup> What the goddess is asked not to remove is either a cloud or darkness.<sup>49</sup>

Those authors who derive *thgy* from the root *hgy* "to make a certain kind of noise" give mutually different interpretations. The interpretation with "do not make noise forever" does not make much sense in the context, in my opinion.<sup>50</sup> The interpretation with "do

<sup>44</sup> Cf. PUECH (1987) 22 (n.39).

<sup>45</sup> This is considered probable by v.d. Kooij (1976) 107.

<sup>46</sup> Cf. also HOFTIJZER (1976) 198.

<sup>47</sup> For the reading *ḥtt*, cf. already CAQUOT and LEMAIRE (1977) 198 (cf. also LEVINE (1985) 329, PUECH (1985) 356, (1987) 17). For the reading *ḥtm*, cf. McCARTER (1980) 51, 54 (cf. also HACKETT (1984a) 27, 45; compare also WESSELIUS (1987) 597).

<sup>48</sup> Cf. GARBINI (1979) 176f., 185, McCARTER (1980) 51, 54, HACKETT (1984a), 29, 46, (1986) 220, (1987) 125.

<sup>49</sup> For the cloud as object, cf. McCARTER (1980) 51 (cf. also HACKETT (1987) 125). For the darkness as object, cf. GARBINI (1979) 177, 185. If one takes the cloud as object one has to presuppose a restoration  $\text{ḥ}^{\text{ḥ}}b \text{ ḥšk}$  in the preceding clause.

<sup>50</sup> Cf. CAQUOT and LEMAIRE (1977) 196, 198 (Also their explanation (p. 198) of these words as a request "à ne plus jamais faire de bruit adressée au soleil dont le voyage quotidien faisait croyait-on, un certain bruit", does not make much sense, even if one accepts that the words were directed to a sun-goddess). For this interpretation, cf. also PUECH (1985) 359, RINGGREN (1983) 94. There is no argumentation given for SASSON's interpretation (1987) 288 n. 14 of "and keep thou silent forever" as "and do not seek to rebel".



not be angry forever" would make sense in the context, but there are not enough arguments in support of it.<sup>51</sup> The interpretation with "and never raise your voice again" does not make much sense in the context.<sup>52</sup> Whether the gods are presented as trying to restrain the goddess from punishing or as trying to incite her to do so, a future speaking of the goddess is not excluded. The only possible interpretation, provided one derives *thgy* from the root *hgy* "to make a certain kind of noise" is, in my opinion, "will never say".<sup>53</sup>

Making a provisional assessment of what is said until now, it seems highly improbable that the words of the gods were spoken to a town (whether Samaria or another one). One would not ask a town to break (or: not to break) the bolts of heaven (etc.). What has been said until now does not allow us to choose between the two possibilities mentioned above: a) that the gods are trying to restrain the goddess from punishing b) that they are trying to incite her to punish. In the first case we have to assume that before *tpry* an <sup>2</sup>*l* was lost, that we have to take *smr* as a parallel to *ht*<sup>54</sup> and that <sup>2</sup>*lthgy* <sup>5</sup>*d* <sup>6</sup>*lm* has to be interpreted as "will never say". We also have to assume that a compound nominal form <sup>2</sup>*lngh* "not-light" is possible, as I proposed in the *editio princeps*. I still believe this possible with reference to Prov. xii 28.<sup>55</sup> In the second case we have to assume that originally there was no <sup>2</sup>*l* before *tpry*, that *hšk w<sup>2</sup>l ngh* and <sup>6</sup>*tm w<sup>2</sup>l smrky* are two parallel pairs and that <sup>2</sup>*lthgy* has to be interpreted as "will not remove". Up to now, the argumentation given does not provide us with any clue which makes it possible to make a choice between them with any reasonable certainty.

In the following lines ll. 10, 11 (8, 9) there two imperfect forms

<sup>51</sup> Against H. and M. WEIPPERT (1982) 93, 103. The fact that *hgy* can be translated with "knurren" (especially said of a lion defending his prey: Is. xxxi 4) is no argument enough. The root *hgy* is used to indicate the typical sounds of men and animals and of the shades of the dead (also against MÜLLER (1982) 218, 224).

<sup>52</sup> Cf. LEVINE (1981) 197f. The interpretation of SASSON (1986a) 208 (n.14) as "keep thou silent forever" i.e. "do not seek to rebel" does not have enough arguments in support of it. (Also against SASSON (1986b) 148, 149, 153).

<sup>53</sup> Cf. HOFTIJZER (1976) 179, 199 (see also the parallels mentioned there). The translation of PUECH (1987) 27: "et tu n'intrigues plus à jamais" is not convincing either in view of the available material.

<sup>54</sup> The clause *b<sup>6</sup>bky šm hšk w<sup>2</sup>l ngh* has in that case to be interpreted as being a nominal clause without modal function, but containing a statement/description.

<sup>55</sup> Against H. and M. WEIPPERT (1982) 93 n. 76. The *l<sup>2</sup>-ngh* and *l<sup>2</sup>-<sup>2</sup>ur* in some classical Hebrew texts (cf. n. 27) have in my opinion also to be interpreted as compound nouns.

(*y<sup>c</sup>nh*, *yybl*) occur. It is interesting that the first one has the ending *-h* and not *-y* (cf *yruy* in ii 6). It seems probable that in our text with this formal opposition a functional one corresponded.<sup>56</sup> This means that *y<sup>c</sup>nh* is an announcement of what will happen and not a wish.<sup>57</sup> What will happen is according to the clause *wql rhmn y<sup>c</sup>nh* something highly unpleasant.<sup>58</sup> The question is, what the relation is between this unpleasant future and those words of the gods by which they try to induce the goddess to punish (or: not to punish) the land/world severely. The function of the *ky* in l. 9 (7) is of interest here and the question whether the clause *wql rhmn y<sup>c</sup>nh* is dependent of it. It goes without saying that the interpretation of the word *hrpt* in the clause introduced by *ky* is of great importance. The question is whether it has to be interpreted as a noun or as a verbal form. That it is a noun indicating an animal is not very probable.<sup>59</sup> If we

<sup>56</sup> Cf. HOFTIJZER (1976) 297ff.; cf. also GARR (1985) 138.

<sup>57</sup> Some authors have not interpreted *y<sup>c</sup>nh* as a verbal form, but as a noun denoting the ostrich. In itself a bird would fit a context speaking of birds. (cf. GARBINI (1979) 177, H. and M. WEIPPERT (1982) 95, 103, LEMAIRE (1985a) 318, (1985b) 280, PUECH (1985) 359 (cf. however idem (1987) 22, 28)). In classical Hebrew the ostrich is not normally denoted by *y<sup>c</sup>nh* but by *bt y<sup>c</sup>nh* (cf. Lev. xi 16, Dt. xiv 15, Is. xiii 21, xxxiv 13, xliii 20, Jer. l 39, Micah i 8, Job xxx 29). H. and M. WEIPPERT (1982) 95 refer to Lam. iv 3 where one finds in the Qere *y<sup>c</sup>nym* denoting ostriches (Kativ *ky<sup>c</sup>nym*) and suggest that it is a plural form of a noun *ya<sup>c</sup>ane*. It remains possible that in Hebrew there existed a noun *y<sup>c</sup>nh* (and not *y<sup>c</sup>n*) denoting the ostrich. Still I prefer to interpret *y<sup>c</sup>nh* in the Deir 'Alla plaster text as a verbal form, because I agree with PUECH (1987) 22 that otherwise we would get a mere list of animals, which would be strange. I do not agree with PUECH *ibid.* that we find a parallel between *hrp* and *ny* in Ps. cxix 42 (in Prov. xxvii 11, also quoted by him, we do not find a derivative of *ny*).

<sup>58</sup> Cf. HOFTIJZER (1976) 202, 204, 206f., cf. also LEVINE (1981) 199, RINGGREN (1983) 95f. The reading *qn* instead of *ql* is palaeographically possible, cf. v.d. KOOIJ (1976) 107. This reading is proposed by GARBINI (1979) 177f., who interprets the word as "the young birds in the nest" (cf. also H. and M. WEIPPERT (1982) 94, PUECH (1985) 359, (1987) 22, 28). It is possible to interpret *qn* in this way, cf. Dt. xxxii 11 (where *qmw* is parallel with *gwzlyw*), cf. possibly also Job xxix 18. So one cannot deny the possibility that *qn rhmn y<sup>c</sup>nh* has to be read (cf. also the *prhy nph* in l. 10 (8)). The reading *q<sup>2</sup>* instead of *ql/qn* is less probable (cf. v.d. KOOIJ (1976) 107), against LEMAIRE (1985a) 318, (1985b) 280. GARBINI (1979) 177 has proposed to read *rhpn* instead of *rhmn*. This reading is possible (cf. v.d. KOOIJ (1976) 108). However the words *qn rhpn y<sup>c</sup>nh* ("la nidiata cova lo struzzo", cf. GARBINI *ibid.* 185) do not make much sense in the context.

<sup>59</sup> Against LEMAIRE (1985a) 318, (1985b) 280 interpreting it as "bat". Although the translation as such is not impossible (cf. *hārapā* in Jewish Aramaic) it seems to me less probable to use it in this context, because it presupposes a list of animals (birds) which as far as one can judge, does not make a clause (cf. n. 57). The ending *-t* also offers problems.



take *hrpt* as the singular construct of a noun meaning "reviling, contumely" we have to translate the context as "the swift has answered the reviling of the eagle and the voice of the vultures"<sup>60</sup> I do not think this translation is probable, because in that case we have to take *hrpt nšr* as "contumely/reviling uttered by the eagle". To the best of my knowledge the object of the reviling always has to be someone whose power/strength/importance the reviler wants to inveigh against. This means that the object must be someone who is considered by the speaker/writer as having power/strength/importance or as someone presenting himself as having it. Since in the translation mentioned above the swift (?), a weak unimportant bird, is the only one who can be the object of the eagle's reviling, the translation seems less probable.<sup>61</sup> The only possibilities left are to interpret *hrpt* either as Qal/Pael Impf. 3p.s.f. or as an active

<sup>60</sup> CAQUOT and LEMAIRE (1977) 199 propose this as a possible translation (cf. also RINGGREN (1983) 94, ROUILLARD (1985) 117).

<sup>61</sup> Cf. 2 Kings xix 4 (= Is. xxxvii 4) where Hezekiah is introduced saying that Rabshake has taunted the living God (cf. Rabshake's words that God will not be able to save Jerusalem (2 Kings xviii 35), cf. also 2 Kings xix 16, 22, 23 (= Is. xxxvii 17, 23, 24; cf. also 2 Chr. xxxii 17)). In Judg. viii 15 Gideon is introduced saying that the men of Succoth taunted him by refusing to help him when he pursued Zebah and Zalmuna. By the words they used they denied his power to cope with them (cf. also v. 6) at the moment he had already shown to be able to do so (cf. Judg. vii 21). In Neh. vi 13 Nehemiah introduces himself speaking and describing his enemies as looking for an opportunity to write an evil report about him and so to taunt him. They tried to do so by wrongly implying that he was not a really plucky man (cf. vv. 10ff.). Goliath (1 Sam. xvii 10) is introduced saying that he taunts the army of Israel, i.e. that by standing there in all his power (vv. 4ff.) he defies the Israelites, implying by his words that there is no one among them who is able to stand up to him (in other words that they standing there in array have no real power, i.e. that they and their God are powerless; cf. also David's reactions in 1 Sam. xvii 25, 26, 36, 45), cf. also 2 Sam. xxi 21 (= 1 Chr. xx 7)). In Ps. lxxiv 10, 18 the psalmist says that God's enemies have taunted Him (parallel with contemning His Name; forms of *nšr* used). It goes without saying that for the Psalmist God is a mighty God (cf. vv. 12-17) Whose power the enemies by their attitude and actions defy (cf. also Ps. lxxix 12, cf. also the question in v. 10 "where is their God?"; cf. Prov. xiv 31 where taunting God is opposite to honouring Him). In Ps. lxxix 52 the taunting concerns God's anointed who is rejected by God (vv. 39ff.). He is taunted by God's enemies who selfevidently present him as being worthless, whereas in the eyes of the psalmist he is special because of the promises made by God concerning David and his house (cf. vv. 20ff., 50). In Ps. xlii 11 his enemies taunt the psalmist who finds himself in adverse circumstances, but who hopes for God's help (cf. vv. 6, 9), the enemies question the efficacy or the realizing of this help (v. 11). Cf. also Ps. xlv 17, lv 13, lvii 4, cii 9, cxix 42 in their contexts (cf. also Zeph. ii 8, 10). In Is. lxxv 7 the Lord is introduced saying that those Israelites who commit idolatry taunt Him, holding His power in contempt by transgressing His prescriptions.

participle of the Qal in the construct state.<sup>62</sup> So, the only possible interpretation of the clause *ss<sup>c</sup>gr hrpt nšr* seems to be that it speaks about a small bird taunting/reviling a mighty one, the eagle.<sup>63</sup>

Some authors consider this clause as the first one of a group of clauses belonging together, in which in each instance something or someone is described as behaving in a way that is contrary to, indeed precisely antithetical to, its natural character.<sup>64</sup> However, I doubt whether clauses describing people or animals behaving contrary to their natural character, must for that reason be considered as necessarily belonging together and describing *one and the same* situation. The swift's reviling of the eagle is a description of an undesirable situation.<sup>65</sup> However, if we have to take the *šm<sup>c</sup>w* in l. 12 (10) as a form of the perfect and not as an imperative form, the clause *wqb<sup>c</sup>n šm<sup>c</sup>w muwr* would speak about the *qb<sup>c</sup>n* listening to exhortation. Whether we take the *qb<sup>c</sup>n* as "aggrieved" or as "hyenas" (as most authors do), this would mean the *qb<sup>c</sup>n* acting contrary to their normal way, but such a change would not be considered something undesirable, but a symbol of a situation which is better than the "normal" one.<sup>66</sup> Compare e.g. Is. xi 6–9 and Ixv 25 (here is described that in a happy future ferocious and dangerous animals will become harmless), cf. also Is. xliii 20 (here is described that in such a future jackals and ostriches will honour God and that there will be rivers in the desert). For these reasons, I do not believe that we find in ll. 9ff. (7ff.) of the first combination a description of one and the same situation.<sup>67</sup> This also means that the clauses in these lines do not all depend on the *ky* in l. 9 (7).<sup>68</sup>

This brings us back to the question about the function of this *ky*

<sup>62</sup> Against WESSELIUS (1987) 597 who interprets *hrpt* as probably being a form of the perfect 2p.s.f. But as he himself agrees in that case "the construction of the sentence seems somewhat awkward". On the ending *-t* as ending of the Perfect 3p.s.f. in this type of language, cf. e.g. GARR (1985) 60, KAUFMAN (1988) 51.

<sup>63</sup> Cf. already HOFTIJZER (1976) 201.

<sup>64</sup> Cf. McCARTER (1980) 58, cf. also HACKETT (1984a) 46, (1986) 217, (1987) 125, SASSON (1986a) 299, (1986b) 149.

<sup>65</sup> Cf. HOFTIJZER (1976) 201.

<sup>66</sup> On *qb<sup>c</sup>n* see below.

<sup>67</sup> SASSON (1986a) 301 also concludes that the hyena "a treacherous animal . . . will be willing to listen to words of chastisement and reform". But his way of letting this fact coincide with a description of bad times ("ironically, this will take place at a time when the forces of darkness have dominion over the earth") is unconvincing.

<sup>68</sup> Against e.g. HACKETT (1984a) 46.



and the relation of the clause *ky ss<sup>c</sup>gr hrpt nšr* to its context.

It is possible to interpret this clause as a hypotactic clause with causal function related to the preceding clause(s). The words *ss<sup>c</sup>gr hrpt nšr* can indicate a sin, mention being made by the use of "animal symbols" of a disobedience of mortal men to the gods (or to the central goddess).<sup>69</sup> If one accepts this interpretation one has to take *hrpt* as indicating an event in the past and one has to accept the interpretation that the gods ask the goddess to punish the land/earth severely. In that case the clause *wql rhmn y<sup>c</sup>nh* cannot be dependent on *ky*, because it speaks of a future event.<sup>70</sup>

PUECH has proposed to take the clause *ky ss<sup>c</sup>gr hrpt nšr* as having a consecutive/final function.<sup>71</sup> In that case the clause *ss<sup>c</sup>gr hrpt nšr* has to be taken as indicating a punishment.<sup>72</sup> The "animal symbols" are used to indicate the overthrow of the established social order.<sup>73</sup> The question is whether in such a context the form *hrpt* (taken as a form of the perfect or as a participle) can indicate a future event. In classical Hebrew one finds *ky*-clauses with consecutive/final function having either a perfect-form or a participle as "predicate". In those instances where a perfect is used the *ky*-clause describes a situation/action which has already taken place, which is already a reality.<sup>74</sup> In those instances where a participle is used the *ky*-clause also describes an action/situation which is presented as being a reality.<sup>75</sup> This makes it less probable that in the relevant lines of the Deir 'Alla-text a consecutive/final *ky*-clause is to be found with either a perfect-form or a participle as "predicate" and speaking about a situation which has not yet been realized.<sup>76</sup>

<sup>69</sup> Cf. already HOFTIJZER (1976) 201.

<sup>70</sup> I do not agree with the interpretation of LEVINE (1981) 197 who translates "For the swift [and] crane will shriek insult . . ." Also against RINGGREN (1983) 94, PUECH (1985) 359; unless they think of a consecutive/final interpretation.

<sup>71</sup> Cf. PUECH (1986) 37, (1987) 28.

<sup>72</sup> I do not agree with PUECH (1986) 37 that this clause and the following ones describe the birds (and the other animals) as panicking because of the darkness (also against Puech (1987) 22).

<sup>73</sup> For instances which prove that the overthrow of the established order could be felt as a punishment, cf. HOFTIJZER (1976) 215.

<sup>74</sup> Cf. GESENIUS-KAUTZSCH-COWLEY par. 166b. Cf. the following instances: Gen. xx 9, 1 Sam. xxii 8, Is. xxii 1, 16, xxxiv 5, Micah iv 9, Hab. ii 18. Cf. also Lachish letters ii 3ff., vi 2f.

<sup>75</sup> Cf. Judg. xiv 3, 1 Sam. xx 1, 1 Kings xviii 9, 2 Kings v 6, Ez. xxiv 19. Most *ky*-clauses of this type have an imperfect as "predicate".

<sup>76</sup> Selfevidently absolute certainty is excluded.

Another possibility is to take the words *ky ss<sup>c</sup>gr hrpt nšr* as a *ky*-clause with causal function being the *protasis* of the *apodosis* *wql rhmn y<sup>c</sup>nh*. The fact that the small bird has taunted the eagle is then given as the reason for the hopeless situation described in the next clauses.

It is also possible to take *ky* in l. 9 (7) as the introduction of a main clause. In the case one prefers the interpretations that the gods ask the goddess not to punish the land/earth, the clause *ky ss<sup>c</sup>gr hrpt nšr* has to be taken as the (first clause of the) direct discourse.<sup>77</sup>

All the interpretations of the clause *ky ss<sup>c</sup>gr hrpt nšr* which as such can be considered tenable, presuppose that the clause *wql rhmn y<sup>c</sup>nh* is a main clause. Those who prefer the interpretation that the gods ask the goddess to punish the land/earth severely, must take this clause as representing something the gods wish, namely that chaos will be realized and that the voice of the vultures will resound.<sup>78</sup> However, in that case one would expect, in my opinion, *y<sup>c</sup>ny* a form with modal function (as one also finds in the preceding clauses, cf. e.g. *lthgy*). This means that a solution which presupposes that the gods asked the goddess not to punish seems preferable. But it is better to consider some other points beforehand.

If we were to concur with this interpretation, this would mean that before the *tpry* in l. 8 (6) there originally stood an *l*. The question is whether there is enough room. CAQUOT and LEMAIRE have proposed a rearrangement of the fragments of the first combination which is accepted by nearly every author.<sup>79</sup> If we did not accept this

<sup>77</sup> See above, that in case one prefers this interpretation one has to interpret the preceding *lthgy* as "will not say". For the interpretation of *ky* as introducing a main clause, cf. HOFTIJZER (1976) 202, SASSON (1986a) 288 (n. 15).

<sup>78</sup> Cf. above. The interpretation of MÜLLER (1982) 218, 225f. of the clause *wql rhmn y<sup>c</sup>nh* with "und (umgekehrt) muss die Stimme der Geier sich ducken" is less convincing. He adduces Is. xxv 5 as reference: *zmyr rym y<sup>c</sup>nh* (cf. the parallel with *tkny<sup>c</sup>* in the preceding clause). In this case there can be no reasonable doubt that the text speaks of the singing of the ruthless ones being brought low. So, the clause *wql rhmn y<sup>c</sup>nh* as such could be translated as "the voice of the vultures will be brought low". But one has to ask oneself what function a clause with this meaning would have in the context, the vultures, in my opinion, being not the representatives of power and might, but of destruction.

<sup>79</sup> Cf. CAQUOT and LEMAIRE (1977) 193f. For a good survey, cf. also PUECH (1987) 15ff., where also the insertion of other fragments is discussed. In my opinion, also the probable insertion of fragments VIII d and XII c as proposed by HACKETT (1984a) 7, PUECH 1985) 359f., (1987) 15 does not prove the rearrangement mentioned here.



rearrangement (and I have still some doubts)<sup>80</sup> there would be no problem. But even if we accept the proposed rearrangement (which in itself is very attractive) there is, in my opinion, after  $w^2mrw \text{ } \check{s}f$  in l. 8 (6) room for more than two signs and a word divider, namely for around four signs and a word divider. Let me explain.

The main observation of CAQUOT and LEMAIRE is that the rests of red signs on the first line of fragment 1c do not belong to the third line of the first combination but to the first line.<sup>81</sup> They tried to determine the exact relation of fragments 1a and 1c by proposing the restoration  $wy^2mrw \text{ } l[bl^c]m \text{ } br \text{ } b^cr$ .<sup>82</sup> When this last proposal (which in itself is a very attractive one) is correct, there is no room between the  $\check{s}f$  and the  $tpry$  in l. 8 (6) for much more than two signs and a word divider. But I doubt whether this proposal is correct. If this were so, there would not be room between the first sign of l. 1 of fragment 1a ( $r$ ) and the  $[s]pr$  of l. 1 of fragment 1c for  $bl^cm \text{ } br \text{ } b^c$  to be inserted, even without any word dividers.<sup>83</sup> So, if we accept the central rearrangement proposal of CAQUOT and LEMAIRE we have to move fragment 1c somewhat more to the right than they proposed.<sup>84</sup> In that case there is enough room between  $\check{s}f$  and  $tpry$  in l. 8 (6) for around four signs and a word divider. Now the question remains what to do with the attractive restoration suggestion  $wy^2mrw \text{ } l[bl^c]m \text{ } br \text{ } b^cr$ . Perhaps I may propose  $wy^2mrw \text{ } l[h \text{ } bl^c]m \text{ } br \text{ } b^cr$ ?

The next problem is: of which name is  $\check{s}$  the first letter? CAQUOT and LEMAIRE have proposed  $\check{s}m\check{s}$  as the name of the goddess<sup>85</sup> If we leave aside for the moment the fact that Shamash is only attested as a female deity in Ugarit (which makes it less selfevident that in Deir 'Alla  $\check{s}m\check{s}$  was known as a female deity), there is also another reason for doubting whether this name originally stood here. As McCARTER has already said "it would be most curious for the Sun to be given the task of obscuring the sky"<sup>86</sup> and one

<sup>80</sup> Cf. HOFTIJZER (1986) 140.

<sup>81</sup> Cf. CAQUOT and LEMAIRE (1977) 193f.

<sup>82</sup> Cf. CAQUOT and LEMAIRE *ibidem*.

<sup>83</sup> Cf. the space needed by  $bl^cm$  (1.5 (3)) and  $br \text{ } b^c$  (1.2). Cf. also the drawing of PUECH (1987) 16 where nearly all room between the relevant signs is left out to make the restoration possible. Also if we take the tail (left of  $]prf$ ) in l. 1 of fragment 1c as the tail of the last sign of  $bl^cm$  there is not enough room for  $brb^c$  between  $bl^cm$  and the  $r$  of l. 1 of fragment 1a.

<sup>84</sup> According to my estimation more than a centimeter.

<sup>85</sup> Cf. CAQUOT and LEMAIRE (1977) 196f.

<sup>86</sup> Cf. McCARTER (1980) 53, cf. also PUECH (1985) 361, SASSON (1986a) 295f.

would add to be given the task of breaking the bolts of heaven. The most probable solution seems to me to take *šgr* as the name of the goddess, the more so as, in my opinion, the name of this goddess occurs in l. 16 (14).<sup>87</sup>

If we restore *w<sup>3</sup>mrw lšgr* there is, in my opinion, enough room for *l* after *šgr*. So the possibility that the gods did not ask the goddess to break the bolts of heaven, but did ask her not to break them, has to be seriously considered.

In this case we have to accept in l. 9 (7) the parallelism of *smrky* with *ht* and not the parallelism of *hšk w<sup>3</sup>l ngh* with *šm w<sup>3</sup>l smrky*. For the interpretation of *w<sup>3</sup>l smrky thby* as "do not spread the shuddering for you", one can adduce texts like Ez. xxvi 17, xxxii 23, 24, 26, 32, texts already quoted by CAQUOT and LEMAIRE.<sup>88</sup> It also means that *šm* has to be taken as an attribute to *l ngh*<sup>89</sup>; its meaning remains obscure. If the suggestion that *šm* has to be related to the root *šm* is true one could think of an adjective meaning something like "dark", "dense".<sup>90</sup>

The remarks of HACKETT (1984a) 41, that we nevertheless could think of *šmš* because ll. 8f. (6f.) "that the goddess in question does have the power to ordain light and that, in fact, that is her normal function" are less convincing, because they presuppose the closing of the Heavens and not the breaking of the bolts of heaven which not only means darkness but also heavy rains which does not plead the case of the goddess being a sun-goddess.

<sup>87</sup> CAQUOT and LEMAIRE (1977) 201 have argued that the *šgr* and *štr* of Dt. vii 13 (cf. also Dt. xxviii 4, 18, 51) prove that in the *šgr w<sup>3</sup>štr* of l. 16 (14) we are not concerned with two gods, but with the indication of the litter of cows and sheep. In my opinion, there can be no doubt that *šgr* can be the name of a goddess (the same is true of *štr*), cf. HOFTIJZER (1976) 273, MÜLLER (1978) 64f., and that we have to consider this possibility also for the Deir 'Alla text. We also have to consider the fact that in Ugarit the goddess *šgr* occurs in combination with *štr* and *štr* (on the relevant Ugaritic text, cf. XELLA (1981) 99). The interpretation of the *štr* in l. 16 (14) as the name of a god and not as "litter" could be supported by the feminine ending of *štr* in the Dt.-instances (on the problem, cf. MÜLLER (1978) 64 n. 48, H. and M. WEIPPERT (1982) 100f., cf. also RINGGREN (1983) 96). I do not believe that *šgr w<sup>3</sup>štr* is the indication of one female deity (against LEVINE (1981) 198f., (1985) 333: Shegar we<sup>3</sup>Ishtar), because *štr* clearly is a male deity (also against SASSON (1986a), 285, 307, (1986b) 148). The proposal of McCARTER (1980) 53 to restore a goddess *š<sup>3</sup>l* cannot be disproved, but I prefer to restore a name which is attested in the text itself.

<sup>88</sup> Cf. CAQUOT and LEMAIRE (1977) 198.

<sup>89</sup> For *ngh* being a masculine noun, cf. HOFTIJZER (1976) 197 (n. 32).

<sup>90</sup> What stood between *ht* and *hšk* remains uncertain (for the reading problems of the sign before *hšk*, cf. v.d. KOOIJ (1976) 112). The proposal to read *htf. b<sup>3</sup> hšk* (cf. already H. and M. WEIPPERT (1982) 93) is very attractive, but, in my opinion, it does not fill the space, even if one accepts the rearrangement proposals of CAQUOT and LEMAIRE.



Also if we give preference to the interpretation that the gods tried to restrain the goddess from punishing, we also have to take the clause *ky ss<sup>c</sup>gr hrpt nšr* as (part of) a direct discourse.

The question is whether the next clause belonged to this direct discourse. This could very well be the case, for then the goddess (having stated that the small bird has taunted the eagle, i.e. that mortal man has taunted the goddess/the gods) would announce what would happen.<sup>91</sup> In my opinion, it is even preferable to assume that the announcement of what would happen would belong to the direct discourse, for it seems probable that the gods would try to prevent the consequences of the sinful behaviour of the people: that means that they would especially try to prevent the goddess from saying that she would draw consequences from that behaviour.

Although I prefer (for the reasons given above) the interpretation that the gods seek to restrain the goddess from punishing, I am aware that this proposal remains based on hypothesis and probability reasoning. The opposite interpretations, although in my opinion less probable, cannot be said to be *disproved*.

I would like to end this lecture by some short remarks on the question which part of the following lines still can belong to the words of the gods and which not.

In ll. 10f. (8f.) the words *ṣprhy ṣnph* up to and including *wšpr* form a list of birds. Some authors have proposed to interpret the preceding words also as indications of birds. Instead of *šrh* they propose to read *šdh* indicating a type of owl.<sup>92</sup> However, the reading of the second sign as *d* instead of *r* is less probable.<sup>93</sup> The reading *nḥš* for the word preceding *wšrh* is ascertained.<sup>94</sup> The proposal to interpret *nḥš* as indication of a certain type of bird (unknown until now)<sup>95</sup> seems less probable because the *nḥš* and *šrh* both can be in-

<sup>91</sup> If we take the clause *ky ss<sup>c</sup>gr hrpt nšr* as a hypotactic clause with causal function related to the next clause, this last-mentioned clause *must* belong to the direct discourse.

<sup>92</sup> Cf. already CAQUOT and LEMAIRE (1977) 198, cf. also LEMAIRE (1984) 142, (1985a) 318, (1985b) 380, PUECH (1985) 359, 361, (1987) 17, 22 n. 41, SASSON (1986a) 288, 299.

<sup>93</sup> Cf. v.d. KOOIJ (1976) 113, cf. also GARBINI (1979) 178, McCARTER (1980) 54, H. and M. WEIPPERT (1982) 95.

<sup>94</sup> Cf. v.d. KOOIJ (1976) 113. The proposal of LEMAIRE (1985a) 318, (1985b) 280 (followed by PUECH (1985) 362, (1987) 17, 22 n. 41) to read *nšš* (indicating a falcon) can be excluded for palaeographical reasons.

<sup>95</sup> Cf. already CAQUOT and LEMAIRE (1977) 198, cf. also McCARTER (1980) 51, 55, RINGGREN (1983) 94, SASSON (1986a) 288, 299.

terpreted as indicating distress.<sup>96</sup> In the list of birds the word *ywn* has to be interpreted as "dove" as rightly indicated by CAQUOT and LEMAIRE (1977) 199 and not as "marsh" as I originally proposed.<sup>97</sup>

These birds serve as "auguries portending calamity".<sup>98</sup> The question is what is the relation of this list of birds to the context. To answer this question we have to look first at the end of l. 11 (9), the words *mth* up to and including <sup>3</sup>*klw*. It seems very probable that *mth* and *htr* which can both indicate a kind of staff or rod are parallels here, as the majority of authors assume.<sup>99</sup> The question is what kind of rod is meant. CAQUOT and LEMAIRE (1977) 199 already proposed to interpret it as a shepherd's staff.<sup>100</sup> This interpretation is preferable to the one proposed by me, namely to take *mth* and *htr* as words indicating "punishment".<sup>101</sup> A shepherd's staff fits better in a text speaking of ewes.<sup>102</sup>

There can be no reasonable doubt that *htr* is the subject of *yybl*.<sup>103</sup> The parallelism of *mth* and *htr* also makes it probable that there were two parallel clauses in which *mth* and *htr* respectively were subjects.<sup>104</sup> In that case the <sup>3</sup>*rnbn* have to be the object of *yybl*. This

<sup>96</sup> Cf. HOFTIJZER (1976) 202f.; cf. also GARBINI (1979) 178, H. and M. WEIPPERT (1982) 95. This semantic correspondence also pleads against interpreting *nhš* as a nominal form and *šrh* as a verbal form ("he ripped up") as proposed by McCARTER (1980) 51, 55 (also against HACKETT (1984a) 29, 48).

<sup>97</sup> Cf. HOFTIJZER (1976) 204; for the interpretation as "dove", cf. also H. and M. WEIPPERT (1982) 96. McCARTER's interpretation of *nšrt* as a verbal form ((1980) 51, 55) is unnecessary, because the word (as indicating a bird) fits perfectly in the context (also against HACKETT (1984a) 29, 49).

<sup>98</sup> Cf. RINGGREN (1983) 95f. It is interesting that the *ywnh* is described in Jer. xlviii 28, Cant. ii 14 as living in inhabitable surroundings.

<sup>99</sup> For this reason I doubt whether *mth* has to be interpreted as "below", although in itself this would be possible (against LEMAIRE (1985a) 318, (1985b) 280).

<sup>100</sup> For *mth* indicating a rod in the hand of a shepherd, cf. Ex. iv 2 where Moses (keeping the flock of his father-in-law: Ex. iii 1) has a *mth* in his hand. For *htr* indicating a shepherd's rod in some kinds of Aramaic, cf. DELCOR (1981) 57. For the Hebrew parallel *šbē* being a shepherd's staff, cf. Lev. xxvii 32, Micah vii 14, Ps. xxiii 4 (cf. v. 1).

<sup>101</sup> Cf. HOFTIJZER (1976) 205.

<sup>102</sup> For texts where a rod is described as "acting on its own", cf. Ps. xxiii 4, cf. also Numb. xxiv 17, Zech. x 11. PUECH (1987) 17, 23 proposes to place fragment IXk just before *mth* and to read the word preceding *mth* as *[b]qr*. This would fit the context very well. Unfortunately the reading of the *q* is uncertain, cf. v.d. Kooij (1976) 158.

<sup>103</sup> Cf. HOFTIJZER (1976) 205.

<sup>104</sup> For that reason I do not agree with those authors who propose to take *rhln* *yybl* *htr* as a relative clause to <sup>3</sup>*šr* (so already CAQUOT and LEMAIRE (1977) 199).



means that the parallel clause to which *mth* belonged must have had a more or less parallel meaning. If we accept the rearrangement proposed by CAQUOT and LEMAIRE this means that the *w* which probably has to be read after the *Jyn*. in l. 11 (9) is the introduction of this clause.<sup>105</sup>

Because a list of birds as such does not make much sense in the context, the list of ll. 11f. (9f.) must be part of a clause having a meaning more or less parallel to *wql rhmn y<sup>c</sup>nh*.<sup>106</sup> However uncertain the attempts to reconstruct the context may be, it seems probable that there were two clauses telling of the disastrous presence of certain birds and two clauses telling of what the rod did (in the last one telling that the rod would bring hares to the place fit for breeding ewes). It seems also probable that all these clauses contained an announcement of what would happen because of the sinfulness of the people. As such they were probably part of the words the goddess was asked not to speak.

The two clauses containing the verbal forms *klw* and *štyw* are very difficult to interpret because they are heavily damaged. The next clause *wqb<sup>c</sup>n šm<sup>c</sup>w mwsr*, in my opinion, cannot belong to the words of the goddess. Whether one interprets *qb<sup>c</sup>n* as "aggrieved" (as I have done)<sup>107</sup> or as "hyenas" as most authors do,<sup>108</sup> their

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Their translation "à l'endroit où le bâton (= la houlette) menait paître des brebis des lièvres mangent" denies the parallelism of *mth* and *htr*, the more so because they take *klw* and *štyw* in ll. 11, 12 (9, 10) rightly as parallels (cf. *ibid.* 200). One can try to avoid the problem by taking *yybl* as a verbal form with stative meaning (cf. LEVINE (1981) 197, 199, SASSON (1986a) 288, 300 who take the verbal form as a Yiphi form), but (leaving aside the peculiar interpretation of the verbal form) this is less convincing because it would not be clear what the function of the rod would be (the same problem exists if one proposes to interpret *yybl* as a passive verbal form, cf. e.g. McCARTER (1980) 51, 54: Pu<sup>c</sup>al, cf. also HACKETT (1984a) 49). LEVINE's interpretation "Here the text projects the beating and scattering of herds and flocks as a wrathful act of the gods" is unconvincing.

<sup>105</sup> For the probability that this *waw* has to be read, cf. v.d. KOOIJ (1976) 108.

<sup>106</sup> For such a proposal, cf. SASSON (1986a) 288, 299. It is possible that after *y<sup>c</sup>nh* there was a *b* (for the reading possibilities, cf. v.d. KOOIJ (1976) 108) introducing a prepositional phrase indicating the place where the voice of the vultures would resound. It is also possible that for *-ny* we have to read *bny* as already proposed by CAQUOT and LEMAIRE (1977) 198 (for the reading possibilities, cf. v.d. KOOIJ (1976) 112f.). Must one read *bny nhš wšrh*, "those who belong to the sphere of distress and trouble"? (cf. MÜLLER (1982) 218, 226).

<sup>107</sup> Cf. HOFTIJZER (1976) 210f.

<sup>108</sup> Cf. already CAQUOT and LEMAIRE (1977) 200. Both interpretations are, in my opinion, as such possible. For the grapheme *q* representing the phoneme \**q* in the Deir 'Alla plaster texts, cf. now e.g. HACKETT (1984a) 11f., (1984b) 61, 64,

listening to admonition is a change for the good. What the goddess is speaking about is a change for the worse. One can interpret  $\text{šm}^{\text{c}}\text{w}$  as a form of the perfect or as an imperative form. If it is a perfect-form it only can be interpreted as so-called *perfectum propheticum*.<sup>109</sup> As such it would announce a change for the good. But in l. 13 (11) we have in the words  $\text{lhkmn yqhk}$  to do with a change for the worse which is announced. For this reason I prefer, with all due reserve, to interpret  $\text{šm}^{\text{c}}\text{w}$  as an imperative. If this is right the clause  $\text{wqb}^{\text{c}}\text{n šm}^{\text{c}}\text{w mwsr}$  contained an exhortation and did not belong to the words spoken to the goddess.<sup>110</sup> Possibly those words ended already with  $^{\text{r}}\text{mbn}$ .

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GARR (1985) 24, LEMAIRE (1986) 323f., 328, 333f., HALPERN (1987) 122, KAUFMAN (1988) 48f. The interpretation of  $\text{qb}^{\text{c}}\text{n}$  as “bowls” (cf. H. and M. WEIPPERT (1982) 96f., 103) is in itself possible, but grammatically less convincing (also against WEINFELD (1982) 145).

<sup>109</sup> We do not know enough of the language of this text to be sure whether this grammatical phenomenon occurred in it or not.

<sup>110</sup> If I am right the interpretation of  $\text{gb}^{\text{c}}\text{n}$  with “hyenas” is less probable from a contextual standpoint.



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## RESPONSE TO J. GREENFIELD AND J. HOFTIJZER

G.I. DAVIES

I hope I shall not be misunderstood if I say that I was a little disappointed when I read Professor GREENFIELD's paper—not of course for any lack of learning, or wit, but because it mainly took us back into the *Sprachstreit* of yesterday evening, rather than grappling with the problems of interpretation of a particular section of the plaster texts. A full evaluation of his very carefully argued paper would be a rewarding exercise, but it would take more time than we have, and all that I could hope to do now would be to try to bend some of his missiles and exploit some of his concessions<sup>1</sup>. But I will pass over this, because I want to leave as much of our discussion time today

<sup>1</sup> For example (i) (a) the instances of final *-n* replacing final *-m* in Mishnaic Hebrew words that are not masculine plurals are of little relevance to the situation in the early 1st millennium B.C., when consistent m.pl. in *-n* remains predominantly, though not exclusively, an Aramaic feature; (b) the retention of *-t* in the 3rd sing. f. SC before suffixes in Hebrew does not alter the fact that, in the 1st millennium, final *-t*/final *-h* in the unsuffixed form is an Aramaic/Canaanite opposition; (c) the fact that the *-h/-y* distinction in the PC of third weak verbs was obscured in later Aramaic counts for nothing in the face of the clear attestation of the phenomenon in early Aramaic inscriptions (R. DEGEN, *Altaramäische Grammatik*, Wiesbaden, 1969, pp. 76–78); (d) I do not see how the genre in which *waw* consecutive plus PC occurs in the Zakkur inscription disqualifies it from being an admittedly rare indication that early Aramaic knew this construction, especially as it is not used with any greater consistency in the Deir 'Alla text (cf. I 8 *wnšbw*, *w<sup>3</sup>mrw*, and possibly I 15, *wšm<sup>c</sup>w*); (e) G.'s doubts about all the *\*q̄ > q* equivalences in the Deir 'Alla text are unduly cautious, and in the particular case of *qrq* the Hebrew and Arabic roots *crq* do not provide a suitable meaning for the (other) Aramaic occurrences; (f) whatever exactly the writing of *\*q̄* as *q* implies (and it is agreed on all sides that it does not mean that *\*q̄* was pronounced exactly like *\*q* in the communities which used this scribal convention), it is impossible to escape the fact that it is in Aramaic alone that secure examples of it are found (it is by no means certain that the *q* in Hebrew *mq̄* and *rbq̄* represents *\*q̄*); (g) surprising as it may seem, works were written in Old French in England after the Norman Conquest, for example the twelfth-century Arthurian romances of Walter Map, just as after Alexander the Great's conquest of Egypt much was written there in Greek! (ii) (a) *hd* is recognised by G. as Aramaic, and so also should be *qrq* (see above); (b) his acceptance of "shared innovations and some retentions from a common ancestor" in Aramaic and Canaanite leaves open a way in which features of the Deir 'Alla text which are hitherto unattested in Aramaic (e.g. the N-stem) could readily be accommodated by those who hold that the language of the text is Aramaic.

as possible for a consideration of what this text might mean, as distinct from complex general issues of linguistic classification. As a matter of fact I do not think it will make too much difference to the interpretation of the text whether it was written in a Canaanite dialect or a kind of Aramaic, and this is certainly not the only interesting issue which the text raises, as Professor GREENFIELD's brief concluding remarks indicate<sup>2</sup>. On the question of the language I will say only this: after reading and hearing the earnest protagonists on either side of the debate I find myself increasingly wondering whether a fight to the death (metaphorically speaking!) is ever going to settle the argument, and I incline to the view that it is our (already stretched) conceptions of Aramaic (especially) and Canaanite which need to be rethought further still in the light of the Deir 'Alla text, which (geographically and historically speaking) is very much in a border zone between the two language groups. Which side of the old categories the Deir 'Alla text falls down on is not necessarily a very important question, if it is even a possible one. Do we yet know enough about either early Aramaic or Transjordanian Canaanite to be sure that the distinction between them was always so clear c. 800 B.C. as it became later? "Intermediate" languages are not unknown, however different may be the ways in which they arise. The evidence we have suggests that in early times "real Aramaic" was a varied phenomenon, sometimes more like (certain of) the Canaanite dialects, sometimes less so. In general I should want to align myself with the balanced standpoint on these issues of Kyle McCARTER's lecture last night, which was in danger of being lost sight of in the debate which followed it<sup>3</sup>.

I turn now to Professor HOFTIJZER's paper. The editor of the text, to whom we are all so much indebted both for the *editio princeps* and its pioneering studies, and also now for the organisation of this symposium, has to a very large extent kept his thoughts to himself as other scholars have offered their various interpretations of the text

<sup>2</sup> I find his suggestion of literary, linguistic and even religious parallels with the book of Job particularly inviting, in view of its likely Transjordanian setting.

<sup>3</sup> Compare also the general line of argument in W.R. GARR, *Dialect Geography of Syria-Palestine, 1000-586 B.C.E.* (Philadelphia, 1985). The central issue for future discussion should be, I believe, whether the existing categories "Canaanite" and "Aramaic" should be widened, so that one or other of them can accommodate the language of the Deir 'Alla text, or whether early 1st millennium North-West Semitic should be seen as having more than merely two components, so that the language of the Deir 'Alla text as well, presumably, as "Samalian" are attributed to a separate category or categories altogether.



these past thirteen years. He broke his silence to some extent with the translation and notes included in a fascicle of *Texte aus der Umwelt des Alten Testaments* published in 1986, but that was of necessity brief in its treatment of the problems, and we all welcome the opportunity which we have just had to hear his considered and detailed evaluation of the proposals which have been made for the understanding of at least part of the First Combination. I myself hope that there will be an opportunity before long of hearing or reading his reflections on the interpretation of the remainder of the text as well!

No doubt each of us has been listening carefully to see how our own interpretations have fared in the editor's review! In fact it is clear that he has carefully studied all that has been written, and he has accepted a number of the newer proposals, even if within this particular section his overall understanding of the text has remained similar to that which he proposed in 1976. The most distinctive feature of this overall understanding, in which (as far as I can see) he remains opposed to nearly all other scholars who have written on the text, is his view that the gods in council are trying to dissuade or restrain the goddess whose name begins with Š- from causing judgement to fall on the earth. As we read the text with him we first encounter this view in his proposal to restore the negative particle ʾl in the lacuna in line 8 of the original numbering (6 if the CAQUOT-LEMAIRE realignment is accepted), but this proposal is not of course the basis of his interpretation—for that we must, and will, look elsewhere. I should like to spend most of the time allotted to me considering the validity of this overall interpretation of the lines under discussion, in the light of detailed points of translation and the wider context. I will begin with the wider context, which seems to me to raise a difficulty for HOFTIJZER's view. In the earlier part of Combination I we have been told that Balaam received a divine visitation in the night, in which the gods spoke to him. When he wakes in the morning he is found weeping and asked why. It is in reply to this enquiry that he utters the words which we are studying. My question is—against this background would it not be surprising to find Balaam offering an explanation that begins with the gods in council discouraging one of their number from sending judgement? Would we not expect him to begin his answer with a statement of the threat which caused him to weep? Another general point: do the references to the gods in the context lead us to expect a disagreement in the council of the kind that HOFTIJZER's interpretation implies? Dr. SAS-

son thinks so<sup>4</sup>. But it is after all "the gods" who in lines 1–2 bring Balaam the original message of doom.

But now we must turn to the details of the text under examination. In his discussion of the text HOFTIJZER acknowledged that what survives as far as near the end of line 9(7) is compatible with either view, the gods dissuading the goddess or encouraging her. But he believes that there are three aspects of lines 10–12 which, taken together, support his view that the gods are discouraging the goddess from bringing judgement. I will concentrate on these:

(1) The scope of what the gods say. He argues that "hyenas" or "aggrievers", whatever *qb<sup>c</sup>n* means in line 12(10), hearkening to chastisement or correction (*musr*) may be unnatural, but it would be a good thing, and is therefore unlikely to form part of either a metaphorical accusation or a threat of judgement such as we seem to have in the previous lines. By this point then the accusation or threat has ended, and "the clauses in [lines 9ff.] do not all depend on the *ky* in line 9". The observation is I think a correct one, and if so it means that what follows line 12 is not directly relevant to the assessment of what is going on in lines 9–11. That is important, because it narrows down the evidence which we have to consider at this point. But it does not by itself decide the question.

(2) The form *y<sup>c</sup>nh*: it is, HOFTIJZER argues, not to be taken as a noun meaning "ostrich", but as a verb, and the use of the "long-imperfect" points to a future meaning, not a jussive one<sup>5</sup>.

(3) The function of *ky* at the end of line 9: "All the interpretations of the clause *ky ss<sup>c</sup>gr hrpt nšr* which as such can be considered tenable, presuppose that the clause *wql rhmn y<sup>c</sup>nh* is a main clause", i.e. not dependent on *ky*. This is based in part on (2) and in part on a detailed review of some other possibilities, and it leads HOFTIJZER to the conclusion that the gods cannot be inciting the goddess to act, because in that case the "short-imperfect", *y<sup>c</sup>ny*, "may it answer/sing", would be required.

I think HOFTIJZER has overlooked an important possibility here which undermines his argument. This is that the "long-imperfect" may have an iterative or durative sense<sup>6</sup>, as McCARTER and

<sup>4</sup> *UF* xvii (1986) 292–95.

<sup>5</sup> See DEGEN, pp. 76–7, for the distinction between forms of final weak verbs ending in *-y* and *-h* in Aramaic generally, and *ywy* in Comb. II.6 of this text.

<sup>6</sup> Cf. DEGEN, pp. 108–9, and compare *ybk<sup>c</sup>* and *tbk<sup>c</sup>* in line 6 of this text.



HACKETT have understood it<sup>7</sup>. There is then no difficulty in taking this clause, like the previous one (though the SC is used there), as a metaphorical accusation which gives the reason why the gods incite the goddess to bring judgement. The following clauses (to the end of 11) could be understood in a similar way. The alternation of SC and PC is frequent in prophetic accusations (e.g. Hos iv 7-8, 12-13).

Even so, I believe that this passage is also compatible with HOFTIJZER's overall interpretation, especially if he is prepared to render *ky* by "although". He seems to prefer to see it as introducing direct discourse, but I find the idea that *hgy* I might introduce direct (or indirect) speech difficult to parallel. For that this text (and other texts) prefers *ʔmr*. Within the general approach which HOFTIJZER takes it seems to me that there is a better possibility, which he too quickly rejects on etymological grounds. M. and H. WEIPPERT proposed that *ʔlthgy* means "do not be angry"<sup>8</sup>; but HOFTIJZER (see his note 51) doubts whether even the use of *hgh* of a lion in Isa xxxi 4 justifies this rendering. The consonants of our text may, however, be from a root *hgg*, not *hgh*, and cognates in Arabic (*hajja*) and more especially Akkadian (*agāgu*) would strongly support a meaning "be angry". Then we could translate: "Do not be angry for ever, even though ..."

Thus far in the detailed examination of the text I think the honours are fairly even between the two overall approaches. In the earlier part of the passage so much is indeed uncertain that a decision one way or the other is certainly very difficult. Even the fact that HOFTIJZER has to supply a negative in the lacuna in line 8 is not too great a problem for him, in view of the negatives later on in line 9. I think there is room for it, because his observations about the placing of fragment 1c are very acute. I had myself at one time thought that the problem to which he alludes was an objection to the CAQUOT-LEMAIRE re-alignment as such, but this is not so provided that the horizontal placing of the fragments is carefully watched. HOFTIJZER's proposal to read *l/h.lbf/m* in line 2\* is one possibility. Alternatively we may do without the second *l* and regard the name

<sup>7</sup> P.K. McCARTER. *BASOR* cxxxix (1980) 51: "resounds": J.A. HACKETT, *The Balaam Text from Deir 'Alla*, Harvard Semitic Monographs 31, Chico, 1984, p. 29: "sings".

<sup>8</sup> *ZDPV* xcvi (1982) 93, 103.

as a vocative introduction to the speech of the gods: "O Balaam son of Beor . . ." (cf. Gen xxii 1, I Sam iii 4,6 etc., Amos vii 8, viii 2, Jer i 11)<sup>9</sup>.

The biggest problem in the passage for HOFTIJZER, I think, is what he sees as a compound negative expression, <sup>3</sup>l ngh in lines 8-9. To have this standing, as he wants it, in a nominal circumstantial clause would be most irregular. The parallel to which he appeals in Prov xii 28 is itself textually very problematic and may well be a late scribal creation<sup>10</sup>. It is more likely that the use of <sup>3</sup>l here in the Balaam-text is due to the precative context, and that the gods are asking for the removal of light.

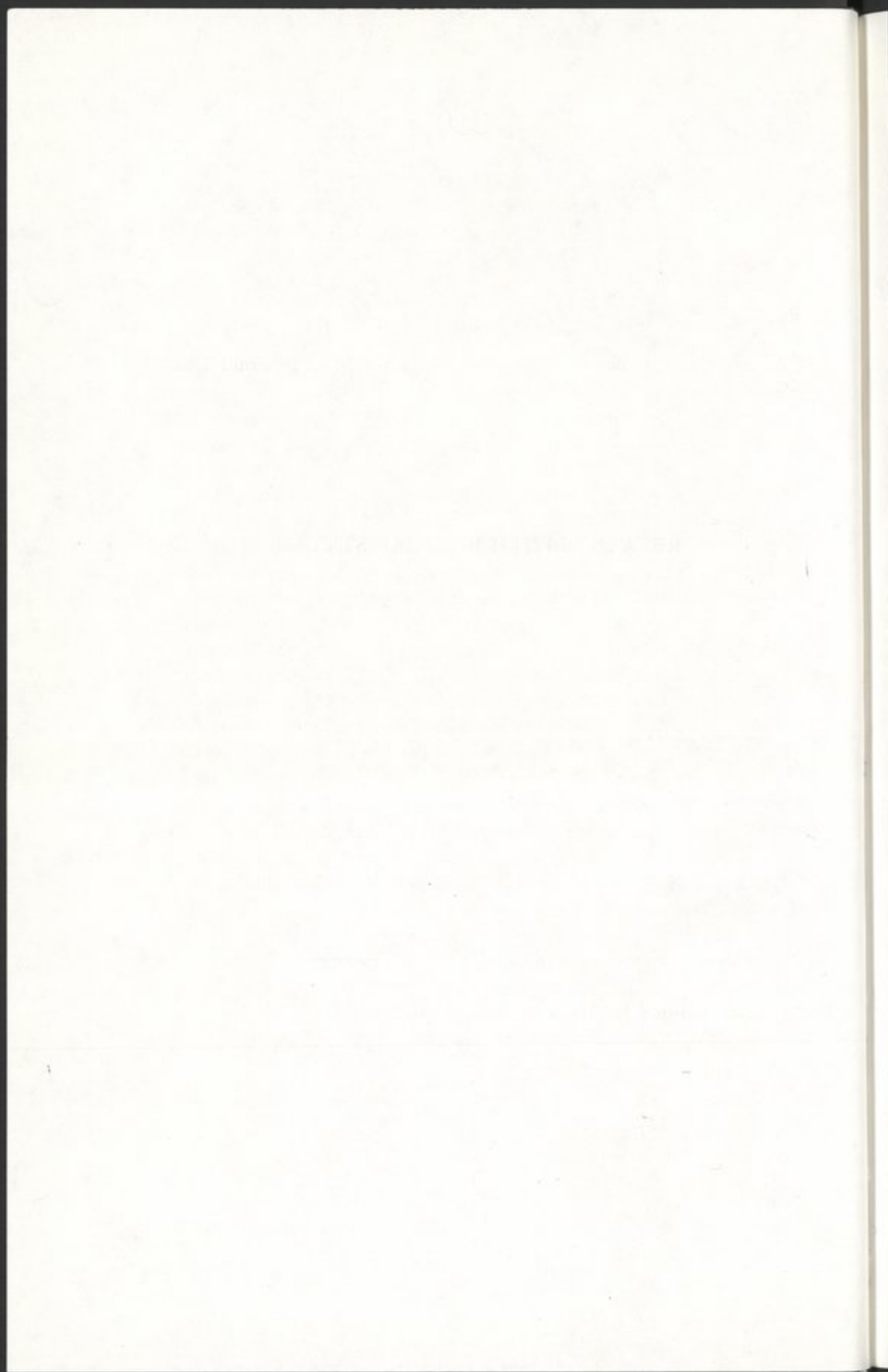
In conclusion, then, I believe that the choice between the two overall patterns of interpretation of this passage is even more finely balanced than HOFTIJZER does. But partly because of the overall context and partly because of the problem over <sup>3</sup>l ngh which I have just mentioned, I think it tips slightly in the opposite direction to him.

<sup>9</sup> In the discussion HOFTIJZER mentioned the possibility of reading *l/h.ybl<sup>c</sup>m*: cf. II 10 for the vocative particle *y*. Both this proposal and my own make the change from black to red ink occur within the speech of the gods, but this need not be a problem, as it is not certain that the change is syntactically significant.

<sup>10</sup> Cf. R. TOURNAY, *RB* lxix (1962) 495-7; W. MCKANE, *Proverbs: A New Approach*, London, 1970, p. 451.



RELATION WITH BIBLICAL STUDIES





## THE BALAAM TEXT FROM DEIR 'ALLĀ AND THE STUDY OF THE OLD TESTAMENT

Manfred WEIPPERT

### 1. INTRODUCTION

As the title I have chosen for my contribution is rather general I shall briefly state in this introduction what I want to deal with.

Before I began looking for indications to the impact of the Deir 'Allā inscriptions on biblical studies, I was convinced that almost everything that could be said on this subject had already been said in the course of the twelve and a half years which have elapsed since the publication, in 1976, of the monumental volume by Jacob HOFIJZER and Gerrit VAN DER KOOIJ, *Aramaic Texts from Deir 'Allā*. First and foremost, I had in mind all those articles and reviews from about 1980,<sup>1</sup> which pointed out a number of "parallels" between "combinations" I and II on the one hand, and the biblical Balaam pericope in Numb. 22-24 on the other. It seems to me that here, especially with regard to "combination" I, the possibilities of isolated comparisons have been exhausted to a large extent. Upon closer scrutiny it appears, however, that questions of a more general nature such as problems of literary and rhetorical criticism or of the history of Ancient Near Eastern religion have not been dealt with properly. There are, of course, a number of treatments of this kind. For these, I refer only to Prof. HOFIJZER's commentary on the Deir 'Allā texts in the aforementioned book and to articles by Hans-Peter MÜLLER, Mathias DELCOR, and Helga WEIPPERT.<sup>2</sup> My general impression is that the impact of the Deir 'Allā texts on the study of the Old Testament has been very restricted during the last decade, or, to speak more frankly, that Old Testament scholars until now have more or less ignored these texts. Even in the comprehensive volume by Hedwige ROUILLARD on the biblical Balaam pericope<sup>3</sup> the texts from Deir 'Allā play only a marginal rôle.

<sup>1</sup> Bibliography in AUFRECHT 1986, supplemented by LEMAIRE 1990, above, pp. 55ff.

<sup>2</sup> HOFIJZER-VAN DER KOOIJ 1976, pp. 173-282; MÜLLER 1978 and 1982; DELCOR 1981; H. WEIPPERT 1981b.

<sup>3</sup> ROUILLARD 1985, pp. 25-28 and *passim* (the references are, unfortunately, not registered in the indices).

Considering the fact that the interpretation of these inscriptions is extremely difficult this scholarly discretion may perhaps be grounded on a wise decision.

In this situation I shall refrain from repeating all the "parallels" between both sets of texts which can be gathered from previous publications. Nor shall I present a survey of the findings of the authors just mentioned. I shall rather focus on the importance of Deir 'Allā for the study of some of the literary and linguistic aspects of the Hebrew Bible and for the research into the religion of Palestine (or, Israel) in the first half of the First Millennium B.C. This paper, therefore, deals with questions of a more general nature, and I will try not to dwell too much on details.

One more restriction will be made in this paper. I shall base my considerations exclusively on "combination" I, the "Book of Balaam, son of Beor." The main reason for this is that I do not sufficiently understand "combination" II. In my opinion, it would make no sense to speculate upon the bearing of such a poorly-understood text on the interpretation of another group of texts. My decision to put aside, for the moment, "combination" II has been facilitated by André LEMAIRE's recent demonstration<sup>4</sup> that it has to be separated from "combination" I as a fragment of a distinctive literary work which has nothing to do with Balaam.

## 2. RECONSTRUCTION AND LANGUAGE OF THE BALAAAM TEXT

It must not be stressed among experts that, for obvious reasons, a scholarly accord with regard to deciphering and understanding the Balaam text has not yet been obtained. This causes some inconvenience; but it is a fact we have to live with—and which keeps discussion alive. Therefore, for still some time to come, scholars working on that inscription will have to base their researches and results on their personal reading of the epigraphical material and on their interpretation of what they think can be read. But it is essential, in this situation, that everybody who is going to discuss the text make explicit the *recensio* on which the discussion is founded. For this reason I am presenting here my latest version of the original text in transliteration together with an English translation. Both are revi-

<sup>4</sup> LEMAIRE 1986.



sions of those published in 1982.<sup>5</sup> It will be evident from the transliteration that I accept the joining of fragments IIIh.e.d to the beginning of line 1 proposed by André LEMAIRE,<sup>6</sup> and of fragments VIII d + XII c<sup>7</sup> to lines 3–5 as suggested by Jo Ann HACKETT,<sup>8</sup> as well as LEMAIRE's putting fragment Vh into line 4 at the spot where fragment Ic breaks off.<sup>9</sup> I hesitated to include in my transliteration LEMAIRE's proposal to restore fragment VIIa to the beginning of line 11,<sup>10</sup> and finally dropped it, though this is a possibility that should seriously be considered. On the other hand, I am not in a position to accept the suggestion by Gordon HAMILTON, published by Ms. HACKETT, to join fragments Ve and XVa to the end of line 1<sup>11</sup> as the former displays traces of characters above those claimed for line "1".<sup>12</sup> These joins<sup>13</sup> and, in addition, a reconsideration of the syntactical microstructures of the text have resulted in a certain increase in the number of sentences that can be isolated. In order to retain, for the present, the original numbering of the sentences as employed in the 1982 article I introduced numbers with the indices a, b, c where it was necessary.

#### Transliteration<sup>14</sup>

- I <sup>1</sup>[ZNH .] ŠPR [ .B]L<sup>Ċ</sup>M [ . BR B<sup>Ċ</sup>]R . ʔŠ . ḤZH . ʔLHN [ .]  
 II ḥ<sup>ʔ</sup> [ .]  
 III wy<sup>ʔ</sup>tw . ʔlwh . ʔlhn . blylh [ .]

<sup>5</sup> WEIPPERT-WEIPPERT 1982, pp. 83.102f.

<sup>6</sup> LEMAIRE 1985a, pp. 316f.; 1985b, p. 279. In 1985a, also IIIa was restored here, but was—correctly—dropped from 1985b (the first letter is <ṭ>, not <b>).

<sup>7</sup> Joined already by CAQUOT-LEMAIRE 1977, p. 193.

<sup>8</sup> HACKETT 1984a, p. 59; 1984b, p. 21.

<sup>9</sup> LEMAIRE 1985a, p. 317; 1985b, p. 279 (in both cases read "Vh" in place of "IIh").

<sup>10</sup> LEMAIRE 1985a, p. 317; 1985b, p. 279.

<sup>11</sup> HACKETT 1984b, p. 33.

<sup>12</sup> See already LEMAIRE 1985a, p. 279, fn. 45; PUECH 1985, p. 360; 1987, p. 17, fn. 12.

<sup>13</sup> Additional joins have been proposed by PUECH 1985, pp. 359f.; 1987, pp. 15.17, which are not included here and will be discussed at another occasion.

<sup>14</sup> Passages written in red are indicated by small capitals. Outside square brackets, a dash represents a letter traces of which can be seen, but cannot be identified. Between square brackets, the number of dashes corresponds to that of the letters missing which can be calculated on the basis of the average length of the lines of the inscription. A group of three points symbolizes an unknown number of characters missing.

- IV [wy<sup>3</sup>mrw . l]h<sup>2</sup>kmš<sup>3</sup> . ʔl .  
 V wy<sup>3</sup>mrw . l[bl<sup>c</sup>]m . br b<sup>c</sup>r . kh .  
 VI ʔP<sup>c</sup>L-G<sup>1</sup> . ʔHR<sup>3</sup>H . ʔš . L-[-----]<sup>c</sup>T  
 VII ʔwyqm . bl<sup>c</sup>m . mn . mhr̄ [.]  
 VIII [-]---[---]l . ʔymn . -[-----]-h .  
 VIIla wl ʔk[l . ʔkl .]  
 VIIlb [wys]m [.]  
 IX w<sup>b</sup>bk<sup>4</sup>h . ybk<sup>h</sup> .  
 X wy<sup>c</sup>l̄ . ʔmh . ʔlw<sup>h</sup> .  
 XI wy[<sup>3</sup>mrw .] l̄bl<sup>c</sup>m . br b<sup>c</sup>r̄ .  
 XII lm̄ : tsm̄ [.]  
 XIIa [wl]m̄ . tbkh .  
 XIII wy<sup>3</sup>mr̄ . lhm̄ .  
 XIV šbw̄ .  
 XV ʔhwkm̄ . mh̄ . šd[yn̄ . ---- .]  
 XVI wlk̄w̄ .  
 XVII r<sup>3</sup>w̄ . p<sup>c</sup>l̄t̄ . ʔlhn̄ .  
 XVIII ʔl[h]n̄ . ʔty<sup>h</sup>dw̄  
 XIX ʔwnšbw̄ . šdyn̄ . mw<sup>c</sup>d̄ .  
 XX w<sup>3</sup>mr̄w̄ . lš[mš̄ .]  
 XXI tpqȳ . skr̄ȳ . šmyn̄ .  
 XXII b<sup>c</sup>bkȳ . šm̄ hšk̄ . w<sup>3</sup>l̄ . n<sup>7</sup>gh̄ .  
 XXIIa ʔtm̄ . w<sup>3</sup>l̄ [.] šmr̄kȳ .  
 XXIII thbȳ . ht̄ . [b<sup>c</sup>]b̄ . hšk̄ .  
 XXIV w<sup>3</sup>l̄ thgȳ . ʔd̄ . ʔlm̄ .  
 XXV kȳ . ss̄ ʔgr̄ . hr̄<sup>8</sup>pt̄ . nšr̄ .  
 XXVI wq[n]̄ . rh[m]n̄ . y<sup>c</sup>nh̄ .  
 XXVII h[sd̄ . ---- .] bnȳ . nšš̄ .  
 XXVIII wšdh̄ . ʔpr̄hȳ . ʔnph̄ .  
 XXVIIIa dr̄r̄ : nšrt̄ . ʔywn̄ .  
 XXVIIIb wšpr[-----]j̄n̄ .  
 XXIX w[-----]- mth̄ .  
 XXX b<sup>3</sup>šr̄ . rhln̄ . ʔybl̄ . htr̄ . ʔrnb̄n̄ . ʔklw̄<sup>10</sup>[<sup>c</sup>]šb̄ .  
 XXXI hpš[-----(-)]  
 XXXII [---(-)]n̄ . štyw̄ . hmr̄ .  
 XXXIII wqb<sup>c</sup>n̄ . šm<sup>c</sup>w̄ . mwsr̄ [.]  
 XXXIV grȳ . š<sup>11</sup>[...]̄  
 XXXIVa [...]̄  
 XXXV [...]̄ l̄hk̄mn̄ . yq<sup>h</sup>h̄k̄ .  
 XXXVI w<sup>c</sup>nyh̄ . rqh̄t̄ . mr̄ [.]



- XXXVII wkhn<sup>h</sup> <sup>12</sup>[...]  
 XXXVIII [...]  
 XXXIX [...]lñš<sup>2</sup> . 2<sup>2</sup>zr . qrn .  
 XL ḥšb . ḥšb .  
 XLI wḥšb . ḥ<sup>13</sup>[šb .]  
 XLII [...]  
 XLIII wšm<sup>c</sup>w : ḥršn [.] mn . rḥq  
 XLIV <sup>14</sup>[...]  
 XLV wkl . ḥzw . qqn . šgr . w<sup>c</sup>štr .  
 XLVI <sup>15</sup>[...]  
 XLVIa [...]  
 XLVII [...] lñmr :  
 XLVIII ḥnyš . ḥqrqt . bn<sup>16</sup>[y . ---(-)]  
 XLIX [...]  
 L [...] -šn . 2<sup>2</sup>bdn . w<sup>c</sup>yn  
 (remainder broken off)

Translation<sup>15</sup>

- I [THIS IS] THE BOOK OF [BA]LAAM, [SON OF BEO]R, A SEER  
 OF THE GODS.  
 II.III And to that (man) came the gods at night.  
 IV [And they spoke to] him according to the utterance<sup>16</sup> of  
 El.  
 V And they spoke to [Balaa]m, son of Beor, thus:  
 VI ...[...]<sup>17</sup>

<sup>15</sup> Passages written in red are indicated by small capitals. Outside square brackets, a group of three points represents passages that I am not able to translate, within square brackets, passages destroyed completely.—The commentary in WEIPPERT-WEIPPERT 1982, pp. 83–102, should be compared for the interpretations, where still applicable. The translations of bird designations are conventional (for the problems see H.-P. MÜLLER, below, pp. 189ff.).

<sup>16</sup> Hebrew *maššā* (*ms*<sup>2</sup>) is, in my opinion, an abbreviation of an original \**maššā qōl*, a *nomen actionis* derived from *NS*<sup>2</sup> + *qōl*, “to utter.” The translations “oracle” (HACKETT 1984b, pp. 29.33; 1986, p. 220; PUECH 1985, p. 356; 1987, p. 27; WESSELIUS 1987, cols. 593f.) or “vision” (LEVINE 1981, p. 196) should be avoided.

<sup>17</sup> Sentence VI is still a play-ground of scholarly imagination as the wording of the passage is far from being assured. See the listing of earlier proposals in WEIPPERT-WEIPPERT 1982, p. 85, and further McCARTER 1980, p. 51 (*yp<sup>c</sup>l* [...] <sup>2</sup> . 2<sup>2</sup>hr<sup>2</sup>h . 2<sup>2</sup>š . lr<sup>c</sup>t . zy šm<sup>c</sup>t “let someone make a [...] hereafter, so that [what] you have he[ard may be se]en”); DELCOR 1981, p. 53 (“... fera de son avenir un feu pour ...”); LEVINE 1981, p. 196 (*kh yp<sup>c</sup>l* [...] <sup>2</sup> . 2<sup>2</sup>hr<sup>2</sup>h . 2<sup>2</sup>š . lr<sup>c</sup>h ... šm<sup>c</sup>t “this will they [...] do in the future. No man has s[een what] you have





- XXI Thou mayest break<sup>21</sup> the bolts of heaven,  
 XXII in thy clouds let there be gloominess and no brilliance,  
 XXIIa darkness (?) and not thy radiance (?)<sup>22</sup>,  
 XXIII thou mayest cause terror [by] the gloomy [cl]ouds—  
 XXIV but do not be angry<sup>23</sup> forever!  
 XXV For the swift is reproaching the eagle,  
 XXVI and the vultures' brood the ostrich.  
 XXVII The st[ork is . . .ing] the (young of the) hawk,  
 XXVIII and the owl the chicks of the heron.  
 XXVIIIa The swallow is . . .ing<sup>24</sup> the dove,  
 XXVIIIb and the sparrow the [ . . .].

<sup>21</sup> The first word of sentence XXI was read by CAQUOT-LEMAIRE 1977, pp. 196f., as *tpry* and interpreted as the imperative feminine singular of *TPR*, "to sew (up)." This proposal has been accepted almost unanimously by later commentators. WEIPPERT-WEIPPERT 1982, p. 92, however, suggested to understand *tpry* as 2nd person feminine singular (short) imperfect (= jussive) of *PRR H*, "to break." To this hypothesis can be objected (J.C. GREENFIELD, personal communication) that in Biblical Hebrew *PRR H* is used only with abstract nouns, first and foremost with *berit*, "treaty, covenant." The problem can be solved by reading the verbal form *tpq'y*, which is described as possible by G. VAN DER KOOIJ in HOFTIJZER-VAN DER KOOIJ 1976, p. 111, and recommended by HACKETT 1984b, pp. 42f. with fn. 29, following a suggestion by P.K. McCARTER, although both authors finally adopted the reading *tpry*. The form *tpqy* is a 2nd person feminine singular (short) imperfect G of *PQQ*, "to smash, break to pieces." The verb is attested in the G conjugation as *P<sup>cc</sup>* in Jewish Aramaic and Syriac and as *FDD* (*faḏḏa*) in Arabic. In Jewish Aramaic it occurs also in the R (*pa<sup>c</sup>pa<sup>c</sup>*), in Syriac in the D and A conjugations with similar notions. The Hebrew equivalent is *PSS*, used in the Old Testament only in the L (*pōšēš*) and R conjugations (Mishnaic Hebrew *P<sup>cc</sup> R* is obviously an Aramaism). The root, therefore, is */PDD/* (with lateral /d/) which in Old Aramaic orthography has to appear as *<PQQ>* (i.e., *<PQ<sub>2</sub>Q<sub>2</sub>>* following STEINER's writing convention; STEINER 1977, p. 38).

<sup>22</sup> The meaning of *ʿm* and *smr* is conjectured on the basis of the parallel sentence XXII; see WEIPPERT-WEIPPERT 1982, p. 93 with fn. 77.

<sup>23</sup> Usually the verbal form *thy* is explained as a 2nd person feminine singular (short) imperfect (= jussive) G of *HGI* "to utter a sound." This was interpreted by WEIPPERT-WEIPPERT 1982, p. 93, as "to growl" ("knurren, grollen") > "to be angry," thus perhaps introducing a Germanism (or Anglism) into a text much older than German or English. The form may, however, be derived from *\*HGG*; cp. Akkadian *agāgu*, "to be angry," the root of which may be */HGG/* on the strength of Arabic *HĠĠ* (*haḡḡa*), "to burn" (LEVINE 1981, p. 198, fn. 12; G.I. DAVIES, orally).

<sup>24</sup> *nšrt* has been explained as a verbal form by McCARTER 1980, p. 55; HACKETT 1984b, p. 49. The root most probably is *<NŠR>*, i.e., */NS<sub>2</sub>R/* (Hebrew *\*NŠR*), */NS<sub>2</sub>R/* (Hebrew *\*NŠR*), or */NTR/*. Both authors derive *nšrt* from */NTR/*, "to fall down" (Mishnaic Hebrew *NŠR*, Biblical and Jewish Aramaic, Syriac *NTR*; cp. Arabic *NTR*, "to disperse"); but their attempts to elucidate the semantic development of this verb seem to me somewhat forced.

- XXIX And [...]... staff.  
 XXX Where the stick would lead sheep, hares are (now) eating [the g]rass.  
 XXXI [...]...  
 XXXII The [...] are drinking wine.  
 XXXIII And the hyenas are listening to instruction.  
 XXXIV The young of the [...]...]<sup>25</sup>  
 XXXIVa [...],  
 XXXV while [the [...]...]<sup>26</sup> is laughing at wise men.  
 XXXVI And the poor woman is preparing an ointment of myrrh.  
 XXXVII And the priestess [...]  
 XXXVIII [...]  
 XXXIX [...]...  
 XL ...  
 XLI And ...  
 XLII [...],  
 XLIII while the deaf are hearing from afar.  
 XLIV [...]  
 XLV And all are beholding the oppression (exercised) by Šagar-and-<sup>c</sup>Aštar.<sup>27</sup>  
 XLVI [...]...  
 XLVIa [...]  
 XLVII [...] the leopard.  
 XLVIII The piglet is driving out the (you[ng] of the) [...]  
 XLIX [...]  
 L [...]... , destruction and ruins.<sup>28</sup>  
 (remainder broken off)

As the WEIPPERTS have been criticized for their silence, in the 1982 article, on the question of the language of the Balaam text<sup>29</sup>—in

<sup>25</sup> Here LEMAIRE 1985a, p. 317; 1985b, pp. 279f., restores *š-l*, "fox," with the help of fragment VIIa; see above, p. 153, and already CAQUOT-LEMAIRE 1977, p. 200; McCARTER 1980, pp. 51.56; HACKETT 1984b, pp. 25.51; 1986, p. 220.

<sup>26</sup> The person laughing at the wise cannot be the fool (CAQUOT-LEMAIRE 1977, p. 200; WEIPPERT-WEIPPERT 1982, p. 98), for this would be his normal behaviour (cp. Prov. 1:7B; 23:9); see SMELIK 1984, p. 86 = 1987, p. 82.

<sup>27</sup> Alternative translation: "And all are beholding the restriction of procreation and fertility." See LEMAIRE 1985b, p. 281.

<sup>28</sup> See LEMAIRE, *ibid.*

<sup>29</sup> LEMAIRE 1987, pp. 320f.



fact, we wanted to include a paragraph on this problem together with a grammatical sketch in the second part of our essay which we intended to publish in due course<sup>30</sup>—I shall this time take care to express my opinion. I am still convinced, however, that it is feasible to analyze the grammar and semantics of an obviously Northwest Semitic text like this one on its own merits without recourse to such preconceived categories as "Aramaic" or "Canaanite." But to let the cat out of the bag—I am sure that HOFTIJZER, VAN DER KOOIJ and others, *e.g.*, LEMAIRE, are basically right in calling the language of the Balaam text (and probably that of the other plaster text fragments as well) Aramaic.<sup>31</sup> But this is only basically so. All the attempts I have seen to determine the language of the Deir 'Allā plaster inscriptions seem to start from a static view of language which underestimates the universal phenomenon of linguistic change and development.<sup>32</sup> The only exceptions I have met with so far in this context are a brief remark by Hans-Peter MÜLLER who reflects on the possibility that the language of the plaster texts might be "ein Aramäisch, das seinen Gegensatz zum Kanaanäischen noch nicht voll ausgebildet hat,"<sup>33</sup> and Ernst Axel KNAUF's attempt to demonstrate the Proto-Aramaic character of the Balaam text<sup>34</sup> which seems to have been overlooked completely in the current discussion.

From the four linguistic categories which have to be considered in this connexion—orthography (often improperly termed "phonology"), morphology, syntax, and lexicon—two speak in favour of Aramaic: In spite of some minority statements to the contrary,<sup>35</sup> it is evident that the orthography of the Balaam text conforms with that of the Old Aramaic inscriptions from Northern and Central Syria<sup>36</sup> (with the sole exception of the Aramaic of the bilingual statue inscription from Tell Feheriye<sup>37</sup>). Also the morphology is

<sup>30</sup> See the "noch nicht" in WEIPPERT-WEIPPERT 1982, p. 83, fn. 26.

<sup>31</sup> See the summary description of the *status quaestionis* in LEMAIRE 1987, pp. 318–321, where the partisans of Aramaic are listed in pp. 318f.

<sup>32</sup> This has been underscored in many publications by R. HETZRON. For a recent application of this principle to Arabic, see KNAUF 1988, pp. 64–77.

<sup>33</sup> MÜLLER 1982, p. 215.

<sup>34</sup> KNAUF 1985b, pp. 189–191; 1988, pp. 64f., fn. 313.

<sup>35</sup> NAVEH 1979, pp. 135f.; GREENFIELD 1980, pp. 250f.

<sup>36</sup> J. HOFTIJZER in HOFTIJZER-VAN DER KOOIJ 1976, pp. 283f.; LEMAIRE 1987, pp. 233f.

<sup>37</sup> See ABOU ASSAF a.o. 1982, especially pp. 38–46.

much closer to Old Aramaic than it is to the Canaanite languages of the First Millennium B.C. (and to the younger Aramaic idioms). The case is, however, different with the syntax and the lexicon. It is the morphosyntactical phenomenon of the so-called "consecutive imperfect" that has induced scholars to hesitate about identifying the language of the Balaam text<sup>38</sup>—although it occurs in Old Aramaic at least in the Zakkūr stela inscription which is approximately contemporary with the Deir 'Allā plaster texts.<sup>39</sup> But when we look closer at the syntax of the Balaam text we can only conclude that it is in complete harmony with that of Moabite, Classical Hebrew (both epigraphical and biblical) and Edomite prose. It varies from the syntax of Old Aramaic, and differs fundamentally from that of the later Aramaic languages and dialects as well as from that of Phoenician and Post-Classical Hebrew. The vocabulary of the Balaam text has been described as "mixed."<sup>40</sup> By this it was meant that it contains side by side lexemes which are specific to only one of the established subdivisions of First Millennium Northwest Semitic, *i.e.*, Aramaic and Canaanite.<sup>41</sup>

At first sight this is disturbing evidence. But I believe it will become less disturbing or can even be explained if we consider the general history of the Northwest Semitic languages during the First Millennium B.C. under the aspects of continuity and change. Both the Aramaic and the Canaanite branches of that family are rooted in the closely related Northwest Semitic idioms of the Late Bronze Age. The phonetic and syntactical character of these languages can be deduced from the Old Canaanite of the Sinaitic and Palestinian inscriptions, from the Canaanite glosses of the Amarna letters and from Ugaritic. In the First Millennium, the Aramaic subdivision is in general the more dynamic and innovative of both. From the outset, it displays a remarkable tendency to simplification in regard to phonology and syntax. The reason for this is, in my opinion, that

<sup>38</sup> FITZMYER 1978, p. 94; McCARTER 1980, p. 50; LEVINE 1981, p. 195; HACKETT 1984a, p. 62; 1984b, pp. 118f.; PUECH 1985, p. 362. But see, on the other hand, J. HOFTIJZER in HOFTIJZER-VAN DER KOOIJ 1976, p. 296, fn. 23; HAMMERSHAIMB 1977, p. 241; RINGGREN 1977, p. 85; LEMAIRE 1987, pp. 327f.

<sup>39</sup> Zakkūr (DONNER-RÖLLIG 1968-71 no. 202; DEGEN 1969, pp. 5-7) A 11 ( $w^{\text{3}}\text{r}^{\text{2}}$ ,  $wy^{\text{c}}\text{ny}$ ).15 ( $wy^{\text{c}}\text{mr}$ ); see DEGEN 1969, pp. 114f. with fn. 21. For the dating of the inscription to about 796 B.C. see M. WEIPPERT forthcoming, chapter 4.

<sup>40</sup> McCARTER 1980, p. 51; cp. KAUFMAN 1980, p. 73; PUECH 1987, p. 29.

<sup>41</sup> See the word lists compiled by HACKETT 1984b, pp. 120-123; LEMAIRE 1987, pp. 328-331.



Aramaic always was in close contact with political and economic centres. The Old Aramaic literary language which we know from the inscriptions was formed in the great regional metropolises of Syria such as Arpad, Šamʿāl, Hamath and, probably, Damascus. For its wide geographical range and its simplicity Aramaic became the *lingua franca* of all of the Near East not later than the 8th Century B.C. and subsequently grew into a universal language under the Achaemenids. According to the evidence at hand the Canaanite branch developed along different lines, at least partially. Here we can distinguish between the development of language in the centre and in the periphery. In the centre there were without doubt the Phoenician city-states along the eastern coast of the Mediterranean which could keep and even strengthen the economic position they had occupied already in the Late Bronze Age. Accordingly, the development of the Phoenician language took a dynamic course and showed an early tendency to phonetic and syntactical simplification similar to that of Aramaic. The situation was different in the periphery. Here we find the languages of the Judaeans, Ammonites, Moabites, and Edomites from which we have epigraphical evidence and, of course, the Hebrew Bible. These were conservative languages with an archaic phonology and an old-fashioned syntactical system. I cannot go into detail here and, therefore, will confine myself to giving only the most obvious examples. For the phonology, mention can be made of the preservation of lateral /š/ in Moabite as evidenced by the Assyrian transcription *Kamāšhātā* of the royal name \**Kamōš-ʿašā* by the scribes of King Ashurbanipal (7th Century B.C.).<sup>42</sup> In addition, we have the possible preservation of interdental /t/ in Ammonite if the *B<sup>c</sup>lys<sup>c</sup>* (\**Ba<sup>c</sup>lyatā<sup>c</sup>*) of a recently found Ammonite seal impression<sup>43</sup> is the same person as the Ammonite king Baalis (*Ba<sup>c</sup>līs*) known from the Book of Jeremiah (early 6th Century B.C.).<sup>44</sup> The real phonetic conditions of all these languages are obscured by the basically Phoenician orthographical conventions employed. The crucial point of the syntax is the existence of the so-called "consecutive tenses" at least in Judaeans and Moabite. These reflect, *mutatis mutandis*, Second Millennium usage

<sup>42</sup> KNAUF-MA'ANI 1987, p. 93.

<sup>43</sup> HERR 1985.

<sup>44</sup> For the problems involved in this identification, see KNAUF-MA'ANI 1987, p. 91, fn. 3; M. WEIPPERT 1987, p. 101, fn. 51.

and were abandoned very early both in Phoenician and Aramaic. The position of Israelite among the Canaanite languages is not well known as no texts have survived from which we could glean sufficient information regarding its phonology, morphology, syntax, and lexicon. For the phonology, we could perhaps refer to the "Shibboleth" incident narrated in Judg. 12:6 the linguistic implications of which are much disputed. Since Hebrew *šibbōlet* most probably means "ear" (of corn) the salient point of the questioning of the Ephraimite fugitives by the Gileadites is a difference in the phonetic realization of the consonantal phoneme /s<sub>1</sub>/ (Masoretic Hebrew [š]) which seems to have been pronounced as [s] by the Ephraimites, but as [š] by the Gileadites and the Judaeans tradents of the story.<sup>45</sup> In Northwest Semitic, however, [s] and [š] were allophones of /s<sub>1</sub>/ already in the Late Bronze Age;<sup>46</sup> this prevents us from exploiting this dialectal difference in pronunciation for our problem of central and fringe languages. (By the way—the deadly pun would have been as effective even if the Gileadites spoke Aramaic, not Hebrew.) It can be assumed that the diphthong /ai/ was monophthongized in Israelite as in Phoenician and Moabite,<sup>47</sup> while the diphthong /au/ could still be heard in the late 7th Century B.C. as in Judaeans.<sup>48</sup> Thus the picture will remain indistinct until more inscriptions have been found.

If we now, with these observations in mind, cast a second look at

<sup>45</sup> The lexeme is attested in Biblical and Mishnaic Hebrew. Its cognates in other Semitic languages prove beyond doubt that the sibilant in *šibbōlet* is /s<sub>1</sub>/: Akkadian *šubu/iltu*, Jewish Aramaic *šə/ubaltā*, Syriac *šebbālā* and *šebbaltā*, Mandaean *šumbiltā*, Arabic *sabala* and *sunbula*, Ethiopian (G<sup>c</sup>z) *sabl*. The variant *šəbaltā* in Jewish Aramaic is, in my opinion, a case of hyper-correction and cannot decide, therefore, the issue of the etymology of *šibbōlet*. Its Biblical and Mishnaic Hebrew homonym *šibbōlet*, "stream, flow," may be etymologically related to *šibbōlet*, "ear." The identity of the sibilant of this word cannot be determined independently, however, on the basis of Syriac *šebbaltā dā-nahrā* Isa. 27:12, as this is only a *calque* of Hebrew *šibbōlet han-nāhār* which in itself is the result of a scribal mistake.

<sup>46</sup> KNAUF 1988, pp. 73f. with fn. 340.

<sup>47</sup> The Samaria ostraca (first half of the 8th Century B.C.) have *yn* = \**yēn*, "wine" (JEAN-HOFTIJZER 1965, p. 109, 19f.). Contrast Judaeans *yyn* = \**yain* (AHARONI-NAVEH 1981, nos. 1:9; 2:2,5; 3:2; 4:3; 10:2; 11:3; Bible *yyn* [Masoretic *yayin*, construct state *yēn*]).

<sup>48</sup> See the Assyrian transcription <sup>1</sup>A-u-si-<sup>2</sup> of the Israelite royal name \**Haušī<sup>c</sup>* (Masoretic *Hōšēā<sup>c</sup>*), RAWLINSON 1870, pl. 10:2 = M. WEIPPERT 1971, p. 490, text 38, line 17' (732 B.C.).—Regarding the pronunciation of /au/, the situation in Judaeans is ambiguous; see on the one hand, e.g., *Hwš<sup>c</sup>yhw* (NAVEH 1960, pl. 17, line 7) = \**Hauš<sup>c</sup>/ti<sup>c</sup>yahū* (personal name), on the other *ym* (AHARONI-NAVEH 1981, nos. 1:4; 24:19; 40:11) = \**yōm*, "day."



the language of the Balaam text from Deir ʿAllā, we may notice at once that it displays grammatical features that can be interpreted as characteristic of a fringe language. There is the archaic phonology which it shares with Old Aramaic, and there is the old-fashioned syntactical system which is identical with that of the Canaanite languages of the periphery. We could consequently identify this language as a species of peripheral Old Aramaic with traits which have already disappeared from "Standard" Old Aramaic or which are to disappear soon in the subsequent development of "Standard" Aramaic.<sup>49</sup> But perhaps we have to go one step further. There is also the lexicon which is neither distinctly Aramaic nor distinctly Canaanite. This can, in my opinion, only mean that the vocabulary of the inscription reflects a stage in the history of First Millennium Northwest Semitic when both of its subdivisions—or at least the Aramaic branch—had not yet developed their specific lexica.<sup>50</sup>

On the combined evidence of orthography/phonology, morphology, syntax, and lexicon I am inclined to identify the idiom of the Deir ʿAllā Balaam text as a peripheral language which is not yet Aramaic, but is about to become Aramaic.<sup>51</sup> The archaic character of this language<sup>52</sup> may also account for the rareness or even absence of the postpositive article which has been a matter of dispute among scholars. As the article was only gradually introduced into the Northwest Semitic languages in the early First Millennium B.C., its presence in or absence from the Balaam text would cause, in my

<sup>49</sup> Cp. KAUFMAN 1980, p. 73.

<sup>50</sup> Theoretically, the lexemes and roots shared by both the language of the Deir ʿAllā Balaam text and Canaanite could be explained as borrowings from neighbouring languages such as Ammonite or Israelite. See KAUFMAN 1980, p. 73; MÜLLER 1982, pp. 215f. Such loans are normal with languages in contact. But only those isoglosses are decisive which appear in Deir ʿAllā and in Canaanite (and perhaps in other Semitic languages, too) *to the exclusion of Aramaic*. When we apply this rule, only two instances remain: the verb *R<sup>2</sup>I*, "to see" (standard Aramaic *HZI*), and the root *P<sup>c</sup>L* (in \**p<sup>c</sup>lh*, "work," and perhaps in the verb *P<sup>c</sup>L*, "to do," if this has been correctly identified in sentence VI; standard Aramaic *BD*). These may indeed be borrowings from Canaanite literary language (see LEMAIRE 1987, pp. 320f., where also other explanations are discussed). But this is not sufficient to call the language of the Deir ʿAllā Balaam text "Canaanite." In the context of the hypothesis advocated here, even this assumption is not necessary.

<sup>51</sup> This translates E.A. KNAUF's definition, "eine westsemitische Sprache, die auf dem Weg ist, Aramäisch zu werden, ohne es schon ganz geworden zu sein." See KNAUF, 1985b, p. 191; 1988, pp. 64f., fn. 313.

<sup>52</sup> See KAUFMAN 1980, p. 73; KNAUF 1988, p. 65, fn. 313.

understanding of the linguistic position of this inscription, neither an ideological problem nor a problem at all.

### 3. REFLECTIONS ON FORM AND MEANING OF THE BALAAM TEXT

The general disposition of the Balaam text—as far as it is preserved—is clear. We have

- a. a heading (written in red) in sentence I;
- b. a narrative in the 3rd person (*Fremdbericht*) (written in black with sentence VI in red) in sentences II–XVII, and
- c. a relation of a vision and/or audition (written in black) in sentences XVIIIff.

Unfortunately we lack any evidence for the end of the text as it is badly broken already from line 10 on and completely breaks off after line 16.<sup>53</sup> Perhaps it once ended with the relation of what Balaam had heard in the council of the gods (section c) without returning to the narrative about Balaam (section b). It then would be similar to the story of Amos and Amaziah in Am. 7:10–17 which ends in a prophecy of doom addressed to Amaziah. If this comparison is right the literary genre of the Balaam text would be the apophthegma as described by Rudolf BULTMANN—a narrative told in order to introduce and hand down to posterity sayings of some important person, a god, prophet, philosopher and the like.<sup>54</sup>

Let us now consider in some detail the three sections of the Balaam text.

#### 3.1. *The Heading*

The first seven readable or restorable words of the inscription are written in red. Unless sufficient proof to the contrary can be presented, I hold that the rubrics of our text are meaningful. This would imply that the limits between the passages written in red and those in black constitute also syntactical boundaries.<sup>55</sup> Therefore, I agree

<sup>53</sup> G. GARBINI's attempt to join "combination" II directly to "combination" I (GARBINI 1979, pp. 168f.171f.185–188) has not been received with approval by subsequent commentators.

<sup>54</sup> BULTMANN 1958, pp. 8–73.

<sup>55</sup> McCARTER 1980, p. 52; KAUFMAN 1980, p. 73; implicitly, J. HOFTIJZER in HOFTIJZER-VAN DER KOOIJ 1976, pp. 179.184.186; HAMMERSHAIME 1977, p. 223;



with Hoftijzer that the eighth word  $h^2$ , written in black, does not belong to the first seven ones. The contents of the phrase (sentence I) suggest that it is a title or heading identifying the text which follows. Although the word *spr* has now been almost completely restored, there is still some space in front of it that would suffice for filling in two or three more characters. In 1982, we suggested to restore here the demonstrative pronoun *znh* which would result in the nominal clause "[This is] the Book of [Ba]laam, [son of Beo]r, the seer of the gods."<sup>56</sup> But the alternative proposed by Lemaire,<sup>57</sup> namely, an indentation at the beginning of the text, cannot be excluded.

It is not obvious that an ancient document like our text should be provided with a title in form of a heading. In the Ancient World, colophons were much more common.<sup>58</sup> They are sometimes found also in the Old Testament, e.g., in Ps. 72:20 at the end of a collection of Davidic psalms incorporated in the Psalter: *kālū tēpillōt Dāwīd ben-Yīšāy* "to an end are brought the prayers of David, son of Jesse." But book titles are much more numerous in the Old Testament. If we leave out of consideration the titles of originally independent literary works now constituting parts of larger books like the different collections of sayings which make up the Book of Proverbs,<sup>59</sup> almost one half of the thirty-nine books of the Hebrew canon is

RINGGREN 1977, p. 85; Rofé 1979, p. 65 with fn. 19; MÜLLER 1982, p. 220; WEIPPERT-WEIPPERT 1982, pp. 83.85 with fn. 29; LEMAIRE 1985a, p. 318; 1985b, p. 280.—The boundaries of the passages written in red are ignored by FITZMYER 1978, pp. 94f.; NAVEH 1979, pp. 134f.; GREENFIELD 1980, p. 250; DAHOOD 1981, p. 125; LEVINE 1981, pp. 196f.; F.M. CROSS *apud* HACKETT 1984b, p. 31, fn. 1; HACKETT 1984a, p. 60 with fn. 19; 1984b, pp. 30f., fn. 1; 1986, p. 221; PUECH 1987, p. 27. It must be admitted that in line 1 the resulting sentence  $ʔš hzh ʔlhn h^2$ , "a seer of the gods is he," is a good nominal clause; but form-critical considerations (see below) do not favour this segmentation and understanding. It must also be conceded that the observation by G. HAMILTON (*apud* HACKETT 1984b, pp. 30f., fn. 1) that "the rubric in I,2 begins at the same point in the line where the rubric in I,1 leaves off" is correct. But I cannot agree with the conclusion that "the red-ink sections in Combination I need not be complete statements in themselves, but instead simply extend approximately half-way across the line in which they are written." What would have been the reason for such a strange scribal "technique?"

<sup>56</sup> WEIPPERT-WEIPPERT 1982, p. 83; independently, MÜLLER 1982, p. 219. See also GARBINI 1979, pp. 172f.

<sup>57</sup> LEMAIRE 1985a, p. 317.

<sup>58</sup> KRECHER 1978, pp. 116f.; HUNGER 1980–83; OTTEN 1980–83; LUNDBOM 1986 (with earlier bibliography).

<sup>59</sup> Prov. 1:1; 10:1A; 22:17; 24:23; 25:1; 30:1; 31:1.

provided with a title.<sup>60</sup> The shortest form of these titles occurs only once, in Ob. 1, where it consists of the term *hāzōn*, "vision," and the name of the author to whom the book is ascribed. More often the author's name is given in a fuller form with filiation (8 times); it can also be accompanied by other personal data such as his place of origin and his "profession" (7 times). Genre indications like *hāzōn*, "vision," in Ob. 1 or *sēper*, "book," *dabārīm*, "words," *maššā*, "utterance," etc. are often extended by attributive ("relative") clauses (11 times). To seven titles dates are added, usually introduced by *bīmē*, "in the time of."

It is remarkable that—with the exception of Deuteronomy—only prophetic and sapiential books have titles in the Old Testament. They are lacking altogether from the historical books although these are quoting titles of some of their sources, e.g., "The Book of the Wars of Yahweh" (*sēper milḥāmōt YHWH*, Num. 21:14); but I doubt that these titles can really be traced back to headings of literary works—rather they have to be interpreted as secondary citation titles.

The heading of the Balaam text from Deir 'Allā fits well into the general picture of First Millennium book titles obtained from the Old Testament by employing the expanded simple form *spr* (\**sipr*) + author's name with filiation + an indication of his "profession." Like the biblical examples, it differs remarkably from Late Bronze Age Northwest Semitic book titles which, according to the sparse evidence from Ugarit, seem to have consisted only of the preposition *l-* and a name indicating the protagonist, e.g., *lb<sup>c</sup>l* (\**li-Ba<sup>c</sup>la*), "About Baal."<sup>61</sup> The element *sēper*, "book," cognate with the *spr*

<sup>60</sup> Deut. 1:1 (v. 2 is secondary); Isa. 1:1; Jer. 1:1–3; Hos. 1:1; Jo. 1:1; Am. 1:1; Ob. 1; Mic. 1:1; Nah. 1:1; Hab. 1:1; Zeph. 1:1; Mal. 1:1; Song of Songs 1:1; Koh. 1:1.—Neh. 1:1 is probably not the title of a separate "Book of Nehemiah" which would have started with a date like Ezra 1:1, but rather that of an earlier work incorporated into the greater Book of Ezra (= Ezra + Nehemiah).

<sup>61</sup> This is the scheme of the titles of the great epics (see further *lkt* DIETRICH-LORETZ-SANMARTÍN 1976, nos. 1.14 I 1; 1.16 I 21; [*l*] *aqht* *ibid.*, no. 1.19 I 1). The headings of smaller, non-literary works may contain the element *spr* (\**sipru*) as in *spr n<sup>c</sup>m ššum* (DIETRICH-LORETZ-SANMARTÍN 1976, no. 1.85:1), *s[pr]r ḥlmm* (*ibid.*, no. 1.86:1), or *spr dbḥ ḫlm* (*ibid.*, no. 1.161:1). The headings of administrative lists are often introduced by *spr* (*ibid.*, nos. 3.3:1; 4.33:1; 4.74:1; 4.93 I 1; 4.120:1[?]; 4.124:1; 4.134:1; 4.141 I 1; 4.144:1; 4.151:1; 4.160:1; 4.166:1; 4.181:1; 4.207:1; 4.215:1f; 4.245 I 1; 4.247:1; 4.261:1; 4.263:1; 4.264:1; 4.269:1; 4.273:1; 4.288:1; 4.322:1; 4.335:1; 4.337:1; 4.338:1–3; 4.348:1; 4.355:1; 4.367:1; 4.369:1; 4.370:1; 4.378:1; 4.385:1; 4.424:1; 4.427:1; 4.485:1; 4.515:1; 4.554:1; 4.561:1; 4.574:1; 4.609:1; 4.631:1; 4.636:1; 4.680:1; 4.683:1; 4.689:1; 4.690:1; 4.714:1).



of the Balaam text, occurs in the Old Testament only in the title of the prophetic Book of Nahum.

### 3.2. *The Narrative about Balaam*

The title (sentence I) is followed by a narrative about Balaam (sentences II–XVII). According to the interpretation advocated here it begins with the first word written in black, *i.e.*,  $h^3$  (sentence II), which I understand as the anaphoric personal pronoun of the 3rd person masculine singular.<sup>62</sup> It functions as a connecting link between the heading and the narrative referring both back to the personal name mentioned in the title and to sentence III which is a narrative verbal clause with which the story actually starts. By this simple means the heading is more closely annexed to the corpus of the Balaam text than are the biblical book titles to their respective books. In these books there normally does not exist any syntactical or logical connexion between title and corpus. The only exception is probably the Book of Amos in which the extended title in Am. 1:1 is followed up in v. 2 by the phrase *wayyōmer*, “he said,” the intrinsic subject of which can only be supplied by reference to the title.

The narrative may be divided into three scenes according to the persons appearing in them.

The *first scene* includes sentences (II.)III–VI. Here we are told that the gods paid a nocturnal visit to Balaam and communicated to him a message of El, apparently the highest god and head of the pantheon. El’s message is quoted verbatim by the narrator and, as the climax of the first scene or even of the Balaam narrative as a whole, has been accentuated by the use of red ink by those responsible for the manuscript *Vorlage* or for inscribing the text on the wall. It is a great disadvantage that precisely this divine oracle cannot be read and interpreted with certainty.

It is certain, however, from the continuation of the narrative in

<sup>62</sup> Syntactically, it is a nominal clause, “that (man) was it.”—The alternative explanation of  $h^3$  as “lo! behold!” (Old Aramaic, Elephantine  $h^3$ , Biblical and Jewish Aramaic  $hā$ , Biblical Hebrew  $hē$  < Aramaic) proposed by CAQUOT-LEMAIRE 1977, p. 194, and accepted by RINGGREN 1977, p. 85; GARBINI 1979, pp. 173.185.187; McCARTER 1980, pp. 51f. (with alternative “he”); KAUFMAN 1980, p. 73; LEMAIER 1985a, p. 318; 1985b, p. 280; 1987, p. 325; PUECH 1985, p. 356; WESSELUS 1987, cols. 593f. (undecided HAMMERSHAIMB 1977, p. 223; MÜLLER 1982, pp. 218.220) is less likely because of the following *wy<sup>3</sup>tw*.

the *second scene* (comprising sentences VII–IX) that the divine message was understood by the addressee as an oracle of doom. It is not clear what Balaam did immediately after he had risen in the morning (sentence VII) as the large lacuna counted by me as sentence VIII (perhaps rather two sentences) cannot be filled in at the moment. But we can reconstruct sentence VIIb, [he fast]ed, from XII, and guess what might have been contained in sentence VIIIa. If these restorations are right—at least approximately—, we are informed that Balaam reacted to his nocturnal experience by fasting and weeping (sentences VIIIa–IX). This is, of course, a manifestation of consternation and grief. But I believe it is more than that. Fasting and weeping are also rites of self-abasement (*Selbstminderungsriten*) which are performed to turn away threat and danger from the person in question or from the community represented by this person.<sup>63</sup> The ritual of the lamentation for the dead known in all countries of the Mediterranean may be quoted as an illustration. We can probably interpret as such rites also the strange behaviour of David during the deadly illness of his firstborn from Bathsheba (2 Sam. 12: 15B–23). After having sought a divine oracle, the king fasts and weeps and sleeps on the ground dressed in a *šaq*. This conduct is given up as soon as the child is dead since there is no longer any chance—and necessity—to divert God's wrath.

In the *third scene* (sentences X–XVII) the *amm* of Balaam appears on the stage. This is not his "uncle," as originally conjectured by Hoftijzer,<sup>64</sup> but most probably his "people," the group on which he exerts his authority. His fasting and weeping causes them to ask the reason for his behaviour. The "lachrymonous scene" (*Tränenszene*) is a common motive in the literatures of Ugarit and Israel. When king Kurit wept he was questioned by the god El about the cause of his distress (Krt A I 26–42<sup>65</sup>). Similarly, when Hagar had been expelled into the desert with her child, the Angel of the

<sup>63</sup> KUTSCH 1964 = 1986, pp. 78–95.

<sup>64</sup> HOFTIJZER-VAN DER KOOIJ 1976, pp. 179.190. On p. 190, Hoftijzer reflects on the possibility to understand *cmh* as "with him." This was taken up subsequently by WEIPPERT-WEIPPERT 1982, pp. 86f., who quoted evidence from Ugaritic that *verba eundi* could be constructed with *cm*, "with," to indicate the destination. There are also Hebrew examples for *BA* + *cm* et, "to come to;" Ps. 26:4; Prov. 22:24; Song of Songs 4:8. But McCarter's proposal (1980, p. 53) to read *lwh* instead of *lqh* is certainly right.

<sup>65</sup> DIETRICH-LORETZ-SANMARTÍN 1976, no. 1.14 I 26–42.



Lord asked her why she was weeping (Gen. 21:16f.). The motive is employed in like manner in dialogues between mortals. Thus Hannah is induced by her husband Elkana to tell him the cause of her weeping (1 Sam. 1:7f.) as is Elisa by Hasael (2 Kings 8:11f.) or the Gibeonites by Saul (1 Sam 11:4f.). This stereotyped question enables narrators to reveal hitherto untold motivations of the acting persons or the antecedents of the story. In our case a flashback to the beginning of the story is intended. It is now Balaam himself, not the narrator, who is to relate his nocturnal vision. That this report will be much more extensive than the initial summary quotation in sentence VI is already clear from Balaam's invitation to his audience to sit down (sentence XIV). Balaam can now start to describe in detail "what the gods are about to do" (sentence XVII).

### 3.3. *Balaam's Report about the Divine Council*

Everybody who has tried to interpret the Balaam text will agree that section 3 is the most difficult of all. There are several reasons for this. First and foremost, the text is in a bad condition showing many lacunae which grow larger and larger when we approach the lower end of the inscription fragment. This makes the segmentation of the sequence of words into sentences an arduous task with uncertain results. It is, therefore, hard, and sometimes impossible, to find out the syntactical structure of the text. In addition, there is a lot of disputed readings and words difficult to understand, especially animal designations.

What is certain, however, is that Balaam here gives an account of a council of the gods which obviously he had witnessed. His report must refer to the same experience that was described in sentence III as "the gods' coming to him at night," perhaps in a dream vision.<sup>66</sup> The fact that Balaam was honoured by the gods to observe the divine council makes him an authorized medium for conveying to his audience what they are intending to do or what they are actually doing. According to Jer. 23:18,22, the true prophet is required "to have stood *bəsōd YHWH*, in the council of Yahweh."<sup>67</sup> The divine assembly, a sort of parliament of the gods, is known from Meso-

<sup>66</sup> Cp. Gen. 20:3; 31:24.

<sup>67</sup> See, e.g., ROBINSON 1944; CROSS 1953; 1973, pp. 186-190; KINGSBURY 1964; POLLEY 1980.

potamia, Ugarit, Phoenicia, and the Old Testament<sup>68</sup> where, however, Yahweh became the only "real" god at the expense of the others who were made into lower heavenly beings and finally, angels. In the Balaam text, the gods, called *ʾlhn* (\**ʾilāhīn*) or *šdyn* (\**šaddayīn*<sup>69</sup>), are depicted as addressing a certain goddess whose name, unfortunately, has only been partially preserved. As its first letter is <š> it was restored by HOFTIJZER<sup>70</sup> as *š[gr]* (\**Šagar*) on the strength of sentence XLV. I accepted this proposal in 1982,<sup>71</sup> but I am now inclined to find here *š[mš]* (\**Šamš*),<sup>72</sup> the Sun divinity, who was usually conceived as a goddess in Greater Syria during the Second Millennium B.C.<sup>73</sup> (and therefore perhaps also in the First). This restoration is, however, still conjectural as I cannot discern in the photographs the final <š> of the divine name read here by LEMAIRE.<sup>74</sup> What the gods are saying to *Šamš* is a matter of dispute. In my opinion, in sentences XXI–XXIV they attempt to induce the goddess to limit the extent of a catastrophe she brought over

<sup>68</sup> See MÜLLER 1963; MULLEN 1980.

<sup>69</sup> For the *šdyn*, see still WEIPPERT-WEIPPERT 1982, pp. 88–92. That the *šdyn* (Masoretic *šēdīm*) of Deut. 32:17; Ps. 106:37 should be re-vocalized as \**šaddayīm* (HACKETT 1984b, pp. 88f.) is rather unlikely. These are late texts the authors of which certainly knew *Šadday* as an epithet of Yahweh and therefore might have been reluctant to use the plural of this theonym to denote beings "that are not god" (Deut. 32:17). On the other hand, they may well have known \**šēd*, "demon," borrowed from Akkadian *šēdu* (*lemnu*), probably via Aramaic (cf. Syriac and Jewish Aramaic *šēdā*; Mishnaic Hebrew *šēd*). On the question of Palmyrene *šdy*, see WEIPPERT-WEIPPERT 1982, p. 92, fn. 72. It is interesting to notice that WEINFELD 1982, p. 146, translates *šdyn* of the Balaam text by (Modern) Hebrew *šēdīm*.

<sup>70</sup> HOFTIJZER-VAN DER KOOIJ 1976, pp. 272–275.

<sup>71</sup> WEIPPERT-WEIPPERT 1982, pp. 92.101; see further HAMMERSHAIMB 1977, p. 225; MÜLLER 1978, p. 64 with fn. 49; 1982, pp. 217f.223; ROFÉ 1979, pp. 61.66; WEINFELD 1982, p. 146; PUECH 1985, pp. 356.361; 1987, pp. 17.21.26f. *š[gr]* *w[štr]* is restored by LEVINE 1981, p. 196.

<sup>72</sup> First proposed by CAQUOT-LEMAIRE 1977, pp. 196f.; see further GARBINI 1979, p. 176; HACKETT 1984b, pp. 14f.; LEMAIRE 1985a, pp. 317f.; 1985b, p. 280. Undecided whether to restore *š[gr]* or *š[mš]*: RINGGREN 1977, p. 86; DELCOR 1981, p. 55. Mention can also be made of McCARTER's reconstruction *š[ʔl]*, "Sheol" (1980, p. 53). WESSELIUS' suggestion *š[mm]*, "Samaria" (1987, cols. 593f.), seems far-fetched.

<sup>73</sup> M. WEIPPERT 1969, pp. 204f. (Ugarit, Tyre, Ascalon). The personal names with the theophoric element *šmš* quoted by PUECH 1987, p. 21, fn. 33, cannot prove the masculine gender of the sun deity as in names of males the verbal element may be congruent with the sex of the name-bearer, not with that of the god(ess) invoked. See EDZARD 1962 (Akkadian). In the Hellenistic and Roman periods, the sun deity had become masculine in all of Syria.

<sup>74</sup> See fn. 72.



the world, while in sentences XXVff. they try to add weight to their wish by describing the disastrous consequences of her acts.

Breaking the bolts of heaven (sentence XXI) most probably means to open the road for the water of the supercaelian sea in order to pour out upon the earth<sup>75</sup> causing heavy rains and inundations. The dark clouds and the absence of the light of the sun (sentences XXIIa-XXIV) fit well into this scenario. These meteorological phenomena, as well as the terror spread upon the earth by the gloomy clouds, call to mind the biblical depictions of the Day of Yahweh and similar events<sup>76</sup> which are, according to Am. 5:18,20, "darkness and not light." Since water and darkness are attributes of chaos the situation provoked by Šamš can also be interpreted as the re-transformation of the earth into chaos. It is the will of the majority of the gods that this should not last forever (sentence XXIV).

The limitation of cosmic catastrophes by the gods is a common feature in Ancient Near Eastern mythology. In the Mesopotamian flood narratives contained in the Atra-ḫasīs<sup>77</sup> and Gilgameš<sup>78</sup> epics the original decision of the divine assembly is to exterminate all of mankind from the earth. In the course of events this is thwarted by one dissident god so that in the outcome the other gods are compelled to accept the fact that some human beings have survived the disaster. Subsequently the gods agree about reducing mankind by smaller catastrophes without destroying man completely.<sup>79</sup> A similar attitude is reflected in God's decision never again to bring about a flood to annihilate mankind in the Priestly document in Gen. 9:15. It is found also, in a more general sense, in the persuasion of Old Testament authors that God's wrath will not last *lāʿōlām*, forever, which is pronounced both in God's own words in Jer. 3:12; Isa. 57:16, and in confessions of the community in Ps. 103:9; Lam. 3:31 (cp. Jer. 3:5). That the limitation of divine judgement may be caused by conflicts in the heavenly sphere is clear from the Ancient Near Eastern examples cited (to which could be added the Era

<sup>75</sup> Opening the windows or doors of heaven means rain: Gen. 7:11; Isa. 24:18f.; Ps. 78:23ff. See J. HOFTIJZER in HOFTIJZER-VAN DER KOOIJ 1976, p. 195.

<sup>76</sup> See, e.g., LEVINE 1981, pp. 204f.; H. WEIPPERT 1981a, pp. 49-54.

<sup>77</sup> LAMBERT-MILLARD 1969.

<sup>78</sup> SCHOTT-VON SODEN 1958, pp. 86-94; E.A. SPEISER in PRITCHARD 1969, pp. 93-95.

<sup>79</sup> Not in Gilgameš.

epic<sup>80</sup>). This conception may also be reflected in the narrative framework of the Book of Job in which God twice grants the Satan to place Job at his disposal but in both cases only under certain restrictions (Job 1:12; 2:6).

We now come to the, as it were, Hitchcockian vision of birds and other animals, including also some human beings, behaving in an irregular and abnormal manner. Since there is no indication that the speech of the gods ends with sentence XXV, and no other speaker is appearing on the scene, I think that the gods continue to address Šamš also in the following until the end of the preserved text. The words contained in sentences XXV–XXVIIIb are interpreted by LEMAIRE<sup>81</sup> as a list of birds (with the exception of *ky* in sentence XXV, naturally). But, though I accept most of his new readings, I cannot agree with his interpretation. There are several problems involved here. It is tempting, at first sight, to identify *hrpt* (sentence XXV) as the bat (Jewish Aramaic \**harpā*, st.emph. *harpātā*) which might be included among the birds as it is in Lev. 11:19; Deut. 14:18, or is the bee in Ben Sira (Sir. 11:3). But since the feminine ending of nouns in the absolute state is *-h* in <sup>2</sup>*nph* (sentence XXVIII), <sup>c</sup>*nyh* (sentence XXXVI), and *knhh* (sentence XXXVII), it would be strange if it were *-t* in the case of *hrpt* and *nšt* (sentence XXVIIIa). In my opinion, both words have to be understood as verbal forms, i.e., 3rd person feminine singular perfect,<sup>82</sup> on the strength of *rqht* (sentence XXXVI) and *hqrqt* (sentence XLVII). Thus the syntactical structure, as I understand it, speaks in favour of a series of sentences, not a list. In terms of English grammar, the construction of sentence XXV could be expressed by S–P–O,<sup>83</sup> the predicate being a finite verbal form in the perfect. In Northwest Semitic grammar, I would call this a nominal clause in which a verbal clause has been substituted for the rhema ("predicate") which is normally nominal. If we look around in the "birds" section of the Balaam text we can detect some more examples of this construction, namely, sentences XXVIIIa, XXXII, XXXIII, XXXVI, XLV, and XLVIII, to mention only the certain ones. This seems to be a

<sup>80</sup> CAGNI 1969 and 1977.

<sup>81</sup> LEMAIRE 1985a, p. 318; 1985b, pp. 280f. with fn. 50.

<sup>82</sup> For *hrpt* this is almost universally accepted (with the exception of LEMAIRE, see fn. 81). For *nšt*, see above, fn. 24.

<sup>83</sup> This is the normal order of constituents in the so-called "composite nominal clause" in Old Aramaic; see DEGEN 1969, p. 122 § 82 (b). 136 § 98.



typical feature of this section, especially, if we take into account also those sentences which most probably were constructed in the same manner, but where lacunae prevent us from being more definite (XXVII, XXXIV, XXXVII, XLVI). Another syntactical-stylistic feature can be recognized in this section, too. This is the formation of pairs of sentences similar to the "parallelismus membrorum" in biblical poetry and literary prose (*Kunstprosa*).<sup>84</sup> There are two schemes for this. In the first scheme, the verb is deleted in the second sentence of the pair, as in sentences XXV + XXVI (if in XXVI *y<sup>c</sup>nh* is a noun signifying "ostrich"), XXVII + XXVIII, and XXVIIIa + XXVIIIb. The second scheme is less certain. Here in the second sentence of the pair a finite verbal form is employed which is not a perfect, but a so-called "consecutive perfect" in verbal clauses, a (long) imperfect in nominal clauses. In the "birds" section I count two examples of this construction, i.e., sentences XXXV and XLIII, which only have the disadvantage that the first component of the pair has not been preserved. Sentence XXVI, forming a pair with XXV, could here be included if *y<sup>c</sup>nh* in XXVI had to be understood as a verbal form. A certain, complete and even expanded example can, however, be found in sentences XVIII-XX where the nominal clause XVIII, with the verb in the perfect, is followed up by two verbal clauses (XIX-XX) beginning with verbal forms in the "consecutive perfect." This all is paralleled in Classical Hebrew prose not only in the Old Testament, but also in the reaper's petition inscribed on an ostrakon found at Mašad Ḥašavyāhū.<sup>85</sup> The perfect of those sentences can be translated into English either in the past or the present tenses. Personally, I prefer the present describing something that is actually going on<sup>86</sup> (in English, probably, also the perfect would be possible). My argument is that in the whole narrative the goddess Šamš who seems to have caused the prevailing situation has not had leave to speak in the divine assembly and, therefore, has not announced the disaster still to come. For me this means that it is already extant, at least in the vision of the seer. The sentences with "consecutive perfect" or imperfect fit well into this picture. They have to be interpreted, in my

<sup>84</sup> For "Kunstprosa" in Classical Hebrew literature, see H. WEIPPERT 1973, pp. 74-81.

<sup>85</sup> See M. WEIPPERT, 1990a.

<sup>86</sup> Similar to the so-called *Koinzidenzfall*, but not restricted to the first person.

opinion, as circumstantial clauses which are constructed in exactly the same way in Classical Hebrew and are used there to denote the simultaneousness of the action described in the second sentence with that expressed by the first one. In microsyntactical structures like these here they cannot be understood as referring to the future.

I refrain from going into the factual details of the "birds" section. I will only state my continuing conviction that we have here something of the sort classical and medieval scholars would call the "adynata," the world turned upside-down,<sup>87</sup> as pointed out for the first time by Helmer RINGGREN.<sup>88</sup> This is a motive already known from Ancient Near Eastern literature, especially from Egyptian texts from the First and (perhaps) Second Intermediate Periods, as stressed by Jo Ann HACKETT,<sup>89</sup> but also from the Bible.<sup>90</sup> All those texts depict a reversal of natural and political conditions regarded as "normal" which is interpreted as a disturbance of world order and a re-transformation of the earth into chaos.

I believe that, with these prospects in mind, Balaam had enough reason to fast and weep even if the extent of the disaster was to be restricted by order of the gods.

<sup>87</sup> DUTOIT 1936; CURTIUS 1965, pp. 104–108.

<sup>88</sup> RINGGREN 1977, p. 86. In HACKETT 1984b, p. 46, McCARTER (1980, pp. 58f.) is credited with the priority regarding this idea. See also KOENIG 1983, p. 81.

<sup>89</sup> HACKETT 1984b, p. 75.

<sup>90</sup> See, e.g., Isa. 3:1–5; 10:15; 24:1–3. Also the irreal statements in Am. 6:12AB (in 12B read *ʾim yaḥārōš/yaḥrōšū bā-bāqār yām*, but see LORETZ 1989) may belong in this context.—Without the cosmic perspective, the motive occurs also in modern literature. See, e.g., the chanson "Les quat' cents coups" by Léo FERRÉ (ESTIENNE 1962, pp. 123–125; RIEGER 1987, pp. 236–241), especially the fourth stanza:

"Donner aux brébis des bergères  
Aux chevaux des maquignons frais  
Aux chiens les flics de la fourrière  
Aux baleines les baleiniers  
Aux oiseaux le permis de chasse  
Aux enfants les parents mineurs  
Aux souris le matou d'en face  
Aux matous les toits du bonheur  
(Refrain)  
S'il faut tirer par tous les bouts  
Copains tirons les quat' cents coups."



## 4. THE BALAAM TEXT AND THE STUDY OF THE OLD TESTAMENT

In chapters 2 and 3 of this paper the language and some of the contents of the Old Testament were used to support my view of the meaning of the Balaam text from Deir 'Allā. That this can be done is not at all surprising as the Hebrew Bible contains the largest collection of Northwest Semitic texts from the First Millennium B.C. that have survived the vicissitudes of time. Moreover, they belong to literary genres related to that of the Balaam text. If the other peoples speaking Northwest Semitic languages had bequeathed to us Bibles the picture would probably be more or less the same, since there is nothing in the Balaam text which is foreign to the Near East during the First Millennium B.C. In this chapter I will deal with the significance of our text for the study of the Old Testament in the sense sketched in the introduction. There are only two main subjects: literary and historical questions, the latter including also religion.

## 4.1. Literary Questions

1. The Balaam text from Deir 'Allā proves that the Balaam tradition is rather ancient, though in the Bible it appears only in late compositions none of which can be dated with certainty to the Pre-exilic Period. Nowhere in the Old Testament is Balaam called a seer (Hebrew *hōzē* or *rō'ē*) as in the title of the inscription; but his self-characterization in Numb. 24:4, 16 comes close to it. He is rather presented as a *qōsēm*, a soothsayer,<sup>91</sup> in Josh. 13:22, and is given *qōsāmīm*, the soothsayer's fee,<sup>92</sup> according to Numb. 22:7. What he does and says in Numb. 22-24 shows him in two rôles: that of a mighty "man of god" who is able to pronounce effective curses,<sup>93</sup>

<sup>91</sup> A *qōsēm* may originally have been a practitioner of technical oracles as *belomancy* is called *qēsem* in Ezek. 21:26 (cp. DAVIES 1980). But there, as in *qōsāmīm* in Numb. 22:7 (see fn. 92), a more general notion may be implied.

<sup>92</sup> Others interpret the *qōsāmīm* as "instruments of divination" (e.g., Gross 1974, pp. 141-143). According to GROSS, the sentence Numb. 22:7B does not make sense in its context, and should be understood as an addition. This conclusion is not necessary, in my opinion, as Balaam's rôle is described ambiguously in Numb. 22-24, and *qsmym* might easily be re-vocalized as *\*qōsāmīm*. See the discussion of Numb. 22:7B in ROUILLARD 1985, pp. 62-66.

<sup>93</sup> See (besides Numb. 22-24) Deut. 23:5f. (> Neh. 13:2); Jos. 24:9f.; Mic. 6:5.

and that of a prophet who can foretell even events of much later times as, e.g., the rise of David (Numb. 24: 17f.).<sup>94</sup> As there are only a few literal parallels in the Deir 'Allā Balaam text and Numb. 22-24 there is probably no direct literary connexion between both narratives. The best parallel is that between Numb. 22:9,20 and Deir 'Allā sentence III, while that between Numb. 23:13,21 and Deir 'Allā sentence VII has biblical analogies also in Judg. 19:27 and 2 Sam. 24:11 (cp., in addition, Gen. 22:19; Judg. 20:19). One could perhaps argue that the mention of El and Šadday in Numb. 24:4,16 reflects Balaam's relations to El and the Šaddayin in the Deir 'Allā text; but since both theonyms are common designations of Yahweh in post-exilic literature this is at best ambiguous evidence. In Numb. 31:16, a late text, the rôle of Balaam has been developed into that of the false prophet who seduces Israel to apostasy as all false prophets do;<sup>95</sup> but this is an internal question of Old Testament theological historiography which has no longer to do with the Balaam we know from Deir 'Allā.<sup>96</sup>

2. The example of the Balaam narrative from Deir 'Allā—which is doubtless a literary text—demonstrates *ad oculos* what a level of literacy can be expected in early 8th Century Palestine even in a rather remote place. The date can probably be raised by several decades to the second half of the 9th Century B.C. It is by no means unlikely that the text was handed down in manuscript form for some time before it was made public as a sort of "poster" in the building where the inscribed plaster fragments were excavated. This would make the Balaam text approximately contemporary to the stela inscription of King Mesha of Moab from Dibon which is, in my opinion, another testimony to the existence of a literary tradition in the Transjordan already in the 9th Century B.C.<sup>97</sup> It should also be

<sup>94</sup> This is, in my opinion, the obvious meaning of this passage. It is possible, however, that in the post-exilic period when the fourth Balaam oracle was composed also the hope for the future restoration of "Israel" was expressed in this manner in historical guise. See ROUILLARD 1985, pp. 419-448.

<sup>95</sup> The text is unclear. Perhaps it means that the Midianite women seduced the "Israelites" (i.e., the Israelite men) to apostasy at <Baal-> Peor on the instigation of Balaam (*bi-dbar Bil'am*). For the implicit "ratio" of Numb. 31, see KNAUF 1988, pp. 167f.

<sup>96</sup> For the development of the Old Testament presentation of Balaam, see in general DONNER 1977; ROUILLARD 1985, pp. 483-485; KNAUF 1988, pp. 167f.

<sup>97</sup> That the scribes of King Mesha used a literary language almost identical with that of the Hebrew Bible should be evident to every reader attentive to syntax and style.



mentioned that the lay-out of both "combinations"—writing in columns, the use of rubrics—gives evidence of a scribal art which transcends the qualifications necessary for everyday writing.<sup>98</sup> It presupposes a tradition of professional scribes which can ultimately be traced back to Second Millennium Egypt. I am stressing these rather trivial facts in order to challenge the present tendency among Old Testament scholars to postulating very low dates for large portions of the Hebrew Bible. That there were no "real" literary activities in Palestine prior to the middle of the 8th Century B.C. is a hypothesis<sup>99</sup> that has been definitively called in question by the plaster texts from Deir 'Allā although the Mesha inscription could already have taught us a similar lesson.

3. The Balaam text is the earliest example so far of the literary genre of the prophetic narrative or rather, the prophetic apophthegma as described in chapter 3 of this paper. It is similar to the slightly later story about Amos and Amaziah in Am. 7:10–17, and can to a certain degree be compared with that about Micah ben Imlah in 1 Kings 22.<sup>100</sup> From this can be deduced that the prophetic narrative of the Old Testament is rooted in a tradition older than its earliest occurrence in the Hebrew Bible, and likewise, that the prophetic oracle of doom is not specific to the Old Testament.

4. The Deir 'Allā plaster inscriptions also throw a light on the formation of literary collections. It is well known that the Old Testament is a small library comprising books of different authors from different times. But also most of the individual books that constitute the Hebrew canon are again collections of often heterogeneous materials. Compiling such collections was a common literary activity in the Ancient Near East during the second half of the Second and the First Millennia B.C.<sup>101</sup> If "combinations" I and II of Deir 'Allā belong together as originally surmised by HOFTIJZER and

<sup>98</sup> A. LEMAIRE has convincingly argued that the lay-out of the inscription(s) reflects that of a leather or papyrus manuscript (scroll); see LEMAIRE 1986, p. 89; 1989, pp. 37f.

<sup>99</sup> See, e.g., SMELIK 1977, pp. 84–99; 1984, pp. 25–27 = 1987, pp. 22f.; KNAUF 1985a, pp. 35–37. In this connexion, it is important to make clear the conception of "literature" on which the judgement is founded. For KNAUF's view, see *ibid.*, p. 35, fn. 146.

<sup>100</sup> For a recent treatment of this narrative, see H. WEIPPERT 1988.

<sup>101</sup> Cf. the "canonization" of Mesopotamian literature beginning in the Kassite period (REINER 1978, p. 205).

accepted by most of the later commentators then the master manuscript from which the wall inscription was copied must have been such a literary collection. We would have to admit then that there existed at Deir 'Allā in the late 9th or the early 8th Century B.C. a compilation of the literary legacy of a seer or prophet called Balaam, son of Beor, analogous to the prophetic books of the Old Testament. If LEMAIRE is right in separating both "combinations" the picture would be only slightly different. In this case, the copying of "combinations" I and II side by side either in the manuscript *Vorlage* of the wall inscriptions or on the wall would have resulted in the formation of some kind of "Bible" *in nuce* comprising in one literary collection religious texts by different authors. It would also provide an analogy to the compilation of *Sammeltafeln* from various prophetic oracles addressed to King Esarhaddon of Assyria in the 7th Century B.C.<sup>102</sup> In the present situation of the study of the Old Testament a rather early date for the formation of prophetic books or of collections of such in the same area from which the Hebrew Bible originated is most helpful.

#### 4.2. Historical Questions

Is the Deir 'Allā Balaam text or are the plaster inscriptions Israelite? Usually, the absence of the divine name Yahweh and the undeniable presence of a pantheon are adduced in favour of an answer to the negative. I believe, however, that the matter is not so easy. It is certainly true that Yahweh was the national god of both Israel and Judah. As such he was honoured by a national cult performed in the state sanctuaries of Jerusalem, Bethel, Dan, and some smaller places. But this does not imply that he was venerated with the same intensity by all Israelites and Judaeans, or at every local or regional *bāmā*. Thus for me the absence of Yahweh even from an Israelite religious text would not be amazing. Nor would I be disturbed by the apparent presence of a pantheon here, called "the gods" (\**ilāhīn*) or "the Šaddayīn," with El as its head. We have ample evidence both from irenic and polemic passages of the Old Testament and from inscriptions like those from Hīrbet el-Qōm and Kuntilet 'Ağrūd<sup>103</sup> that the Israelites of the Pre-exilic Period worshipped a number of

<sup>102</sup> See M. WEIPPERT 1981, pp. 72f.; 1988, pp. 303.317f.

<sup>103</sup> Texts and references in M. WEIPPERT, 1990b, pp. 171f., fn. 40.



gods—and goddesses—besides Yahweh. I am strongly convinced that Israelite religion until the Persian (or even the Hasmonaean) Period was polytheistic.<sup>104</sup> A true pantheon, with Elyon as its head, can be found in Deut. 32:8f., where Yahweh is not yet the highest god. In Ps. 89 he is; but the pantheon is still there.<sup>105</sup> Thus a polytheistic inscription from a region which according to the Bible had an Israelite population would not *per se* be non-Israelite. The “nationality” of the Balaam text, therefore, cannot be determined by internal criteria.

External criteria also fail, such as the political allegiance of the Deir ʿAllā region during the 9th and 8th Centuries B.C., or the language of the text.

It may be true that most (if not all) of the northern part of the Transjordan including Deir ʿAllā was in Aramaean hands in the second half of the 9th and at the beginning of the 8th Century. The Balaam text, therefore, may have been monumentally published on wall no. 36 of stratum IX (ex-M) at Tell Deir ʿAllā under Damascene rule. On the other hand, there are the allusions to the re-conquest of the ʿAḡlūn (Lodebar) and the Ḡōlān (Carnaim) by Jeroboam II in Am. 6:13f. which, in my opinion, are historically reliable. This would imply that the Deir ʿAllā area returned under Israelite control—at least temporarily—during the reign of this king.<sup>106</sup> There is no evidence that the inscription was destroyed at this occasion. Apparently the authorities of the Israelite kingdom did not interfere in these matters, and there were still no *ḥiṣḥullāh* activists—I refer to what Morton SMITH, Bernhard LANG and others have termed the “Yahweh-alone Movement”<sup>107</sup>—at this remote place to wipe out the text they would certainly have abhorred as “heterodox.”

If the language of the Balaam text is indigenous—and I am convinced that it is—this would mean that in the late 9th Century B.C. the inhabitants of the northern part of the Transjordan claimed by the Bible as Israelite territory, or groups among them, spoke a (Proto-) Aramaic language,<sup>108</sup> and not Hebrew. There is no hint at

<sup>104</sup> See M. WEIPPERT, 1990b, especially chapter 2.

<sup>105</sup> See M. WEIPPERT, *ibid.*, chapter 1.

<sup>106</sup> A brief survey of the sources in M. WEIPPERT 1976–80, p. 203.

<sup>107</sup> See, e.g., SMITH 1971, chapter II; LANG 1981; 1983, pp. 13–56; M. WEIPPERT, 1990b, chapter 3.

<sup>108</sup> For “Proto-Aramaic,” see also KNAUF 1985b, pp. 190f.; 1988, pp. 64f. with

all that the tradents of the text have been Aramaean immigrants to the region as suggested by Al WOLTERS.<sup>109</sup> On the contrary, the character of their idiom as a peripheral language strongly speaks in favour of their autochthony. Since language and ethnicity must not be confused the question is only whether these people identified themselves as Israelites or not. This is primarily a political problem, and we know nothing about the attitudes of those concerned.

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fn. 313. At the Leiden symposium, we were criticized for employing this term by J. HUEHNERGARD with the argument that "proto-languages" are not spoken. In my opinion, this is another example of the static view of language discussed above in chapter 2. If the phenomenon of linguistic change is taken in earnest (as HUEHNERGARD himself does in his genealogical system of the Semitic languages) all languages, even the modern ones we speak, have to be regarded as "proto-languages" in relation to their future development.

<sup>109</sup> WOLTERS 1987.



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# DIE FUNKTION DIVINATORISCHEN REDENS UND DIE TIERBEZEICHNUNGEN DER INSCHRIFT VON TELL DEIR 'ALLÄ\*

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## I

Es war eine von der alttestamentlichen Wissenschaft kaum registrierte Sensation, als J. HOFTIJZER und G. VAN DER KOOIJ 1976<sup>1</sup> die Inschrift von Tell Deir 'Allā veröffentlichten. Und doch enthielt die vermutlich aus dem 8. oder 7. Jh.v.Chr. stammende<sup>2</sup> Inschrift nicht weniger als eine Unheilsankündigung des aus der Bibel

\* Abkürzungen nach *Zeitschrift für Althebraistik* 1, 1988, 2ff.

<sup>1</sup> *Aramaic Texts from Deir 'Alla* (Documenta et Monumenta Orientis Antiqui 19); Zeilenzählung im Folgenden nach einem Vorschlag von A. CAQUOT und A. LEMAIRE wie bei Vf., "Die aramäische Inschrift von Deir 'Allā und die älteren Bileamsprüche", *ZAW* 94, 1982, 214–244.

<sup>2</sup> So die von mir aaO. (Anm. 1) 214 angenommene Datierung. Wegen der mangelnden Trennung von lexikalischen und grammatischen Elementen, die sich später auf das Aramäische und Kanaanäische verteilen – ein Phänomen, das an das in einer Randlage gesprochene "Ja<sup>2</sup>üdische" erinnert –, könnte der Text freilich älter sein; mit einem zugrundeliegenden Bileambuch aus dem 10./9. Jh.v.Chr. rechnet A. LEMAIRE, "Les inscriptions de Deir 'Alla et la littérature araméenne antique", *CRAIBL* 1985, 270–285 u.ö. – Möglicherweise läßt sich der Mangel an Differenzierung später aramäischer oder kanaanäischer Sprach-elemente auch aus dem israelitischen – genauer: gileaditischen – Charakter der Inschrift erklären, insbesondere wenn man diese relativ früh datiert; so E.A. KNAUF ("War 'Biblisch-Hebräisch' eine Sprache? Empirische Gesichtspunkte zur linguistischen Annäherung an die Sprache der althebräischen Literatur", *Zeitschrift für Althebraistik* 3, 1990, 11–23, bes. 15–18.23), der auf den Vorgang von B.A. LEVINE, B. HALPERN, H. WEIPPERT (*Palästina in vorhellenistischer Zeit* [Handbuch der Archäologie: Vorderasien II 1], 1988, 626f.) und M. WEIPPERT (in diesem Band) verweist. Nach H. WEIPPERT wurde die Inschrift vor dem Erdbeben von 762 v.Chr. angebracht, also bevor die israelitische Stadt 733 der assyrischen Provinz Gal<sup>2</sup>ad eingegliedert wurde – was ein höheres Alter des Textes allerdings nicht ausschließt. Nach KNAUF handelt es sich beim "Sukkoth-Israelitischen" im Gegensatz zum Gezer- und Samaria-Israelitischen um populäre Erzählsprache der ritardierenden Peripherie. Oder soll man an das archaisch-lokale bzw. archaisierende Idiom (vgl. B. HALPERN, "Dialect Distribution in Canaan and the Deir Alla Inscriptions", in: D.M. GOLOMB [ed.], *Working With No Data. FS Th. O. Lambdin*, 1988, 119–139, bes. 137ff.) einer poetisch-gehobenen Sakralerzählung denken, wofür u.a. das Fehlen des Artikels bzw. Determinationsmorphems und der Nota accusativi (vgl. zu diesem Archaismus und anderen *KAI* 181, 21b–31b außer 30b) geltend gemacht werden könnte? Dagegen kennen die gleichzeitigen

wohlbekannten Sehers Bileam, die dieser namens ostjordanischer Götter über deren Verehrer ausbrachte. Damit gewinnt nicht nur die schon früher vom Typus des "Propheten" (*nābiʿ*)<sup>2</sup> unterschiedene Gestalt des "Sehers" (*rôʾā, hōzā*)<sup>3</sup> schärfere Konturen; auch die Phraseologie des Seherspruchs, die sich besonders durch ihre Legitimationsfloskeln von Äußerungen prophetischer Berufungsgewißheit abhebt, kehrt hier weitgehend wieder<sup>4</sup>. Aber auch eine eigentlich prophetische Wendung aus der sog. Denkschrift Jesajas hat im Munde des heidnischen Bileam eine Parallele: wenn dieser in I 12/3 seine offenbar als "Feinde (Šagars)" titulierten Hörer mit einer doppelten figura etymologica *hšb' hšb' whšb' h/[šb]* "bedenkt ein Bedenken" oder "plant einen Plan", wie es scheint, zur Umkehr ruft<sup>5</sup>, so erinnert uns dies an die ebenfalls paronomastische, freilich ironische Aufforderung *ʿûšû ʿēšā* "plant einen Plan" Jes 8,9. – Wichtiger aber ist eine funktionelle Übereinstimmung mit der biblischen Prophetie: die Unheilsankündigung gegen das eigene Volk, bislang für apologetischen Eifer als ein proprium biblicum verwendbar, als Erweis der besonderen Souveränität Jahwes, der sein Schicksal und seine Existenz von denen seiner Verehrer zu lösen vermag, hat nun eine außerbiblische Parallele. Auch Jahwes kanaänäische Vorgänger samt ihren Divinatoren fungieren nicht einfach als Heilsgaranten ihrer Völker; sie bewähren vielmehr gegenüber einschlägigem Bedarf an Sicherheit eine Freiheit, die Auswirkung von Heiligkeit der betr. Götter, von so etwas wie göttlicher Unbestechlichkeit ist. Während E. NOORT<sup>6</sup> sich in dem auf die Veröffentlichung der Inschrift folgenden Jahr noch bemühte, die Mari-Prophetie möglichst

profanen "short texts on clay and stone" 1 und 2 (HOFTIJZER – VAN DER KOOIJ, aaO. [Anm. 1] 267) das Determinationsmorphem – ʿ.

<sup>3</sup> Vgl. nach dem Vorgang von G. HÖLSCHER u.a. C. WESTERMANN, *Grundformen prophetischer Rede*, 1960, 14, D. VETTER, *Untersuchungen zum Seherspruch*, Diss. theol. [masch.] Heidelberg 1963; Ders., *Seherspruch und Segensschilderung* (CThM A 4), 1974.

<sup>4</sup> Vgl. Vf., aaO. (Anm. 1) 239–241.

<sup>5</sup> Die Auffassung von *qrn* I 12 als Vokativ "(ihr) Feinde" in Analogie zu *qbʿn* "(ihr) Räuber (?)", und *grjʿj/[gr]* "(ihr) Gegner der Ša[gar]" Z. 10/1 sowie die Ableitung von *qr* oder *qr* von einer ursemitischen Basis \*DR (vgl. althebr. *qr*) sind freilich unsicher. – Zur Übersetzung von *hšb' hšb' whšb' h/[šb]* vgl. Vf., aaO. (Anm. 1) 218.229, H. und M. WEIPPERT, ZDPV 98, 1982, 99.103; ähnlich J. HOFTIJZER, TUAT II 1, 1986, 144:4c, der andere Übersetzungen zurückweist. Vgl. aber auch S. 188 mit Anm. 15.

<sup>6</sup> *Untersuchungen zum Gottesbescheid in Mari. Die "Mari prophetie" in der alttestamentlichen Forschung*, AOAT (S) 202, 1977. Eine in vielem vorbildliche Materialzusammenstellung zur Divination in Mari bietet jetzt J.-M. DURAND, *Archives épistolaires de Mari I/1* (ARM XXVI), Paris 1988, bes. 377ff.



weit von der Prophetie Israels fortzurücken, stellt sich uns nun in unmittelbarer Nähe Israels, dazu etwa gleichzeitig mit dessen frühen Propheten nicht nur ein Sehertum dar, dessen Redeformen den biblischen entsprechen, sondern auch ein Verkündigungsinhalt, der dem der biblischen Unheilspropheten funktionell ähnlich ist. Wir erinnern daran, daß die Funktionsbestimmung für die biblische Unheilsprophetie in der alttestamentlichen Wissenschaft – zwischen M. BUBER<sup>7</sup> und G. FOHRER<sup>8</sup> auf der einen, C. WESTERMANN<sup>9</sup> und vor allem W.H. SCHMIDT<sup>10</sup> auf der anderen Seite – kontrovers ist: ist die prophetische Unheilsankündigung *Umkehrpredigt*, die die Katastrophe im Wort vorwegnehmen sollte, um sie – mittels geforderter und geschehener Umkehr Israels, dazu eines Gnadenerweises Jahwes – in Wirklichkeit zu erübrigen, oder gilt die Unheilsankündigung *vorbehaltlos*, weil die Katastrophe auch durch Umkehr nicht mehr abwendbar ist oder mit Umkehr nicht ernstlich gerechnet werden kann<sup>11</sup>? Ergeht also die Unheilsankündigung, damit ihr Inhalt – mittels einer Entscheidung der Angesprochenen und der Antwort Jahwes auf diese – geradezu falsifiziert werde, oder soll sie sich so erfüllen, wie sie ausgesprochen wird? Ist der Ankündiger der κατέχων (2 Th 2,7) oder umgekehrt ein Vollstrecker des Gerichts, letzteres insbesondere, wenn er das Angekündigte durch die Ankündigung sogleich magisch herbeiführt<sup>12</sup>. – Mir scheint im Blick auf gelegentliche Drohworte in der Mari-Prophetie<sup>13</sup> und die Unheils-

<sup>7</sup> *Der Glaube der Propheten*, 1950, wieder abgedruckt in: *Werke II: Schriften zur Bibel*, 1964, 231–484.

<sup>8</sup> Etwa in: *Studien zur alttestamentlichen Prophetie*, 1967, 36.240; *Geschichte der israelitischen Religion*, 1969, 274–276; *Theologische Grundstrukturen des AT*, 1972, 79f.

<sup>9</sup> *Grundformen prophetischer Rede*, 1960, 45f., wo der Terminus "Drohwort" abgelehnt wird, weil er "das Eintreffen des Angedrohten offen läßt"; stark abgeschwächt in: *Theologie des AT in Grundzügen*, 1978, 110.124.

<sup>10</sup> Vor allem in: *Zukunftsgewißheit und Gegenwartskritik* (BSt 64), 1973, 15ff. Ähnlich vorher H.W. WOLFF, "Die Begründung der prophetischen Heils- und Unheilssprüche", *ZAW* 52, 1934, 1–22, wieder abgedruckt in: *Gesammelte Studien zum AT*, 1964, 9–35; Ders., *Die Stunde des Amos*, 1969. WOLFF hat später, in: "Die eigentliche Botschaft der klassischen Propheten", *FS W. Zimmerli*, 1977, 547–557, bes. 552.555, eine dritte Position gesucht.

<sup>11</sup> Vgl. dazu für Amos die Position R. SMENDS in: "Das Nein des Amos", *EvTh* 23, 1963, 404–423, wieder abgedruckt in: *Die Mitte des AT. Gesammelte Studien Band 1*, 1986, 85–103.

<sup>12</sup> Zum Faktor, der das Angekündigte unmittelbar, d.h. durch die Macht des Ankündigers bewirkt, wird die Unheilsprophetie nach Hos 6,5; Jer 23,29; vgl. die Symbolhandlung Jer 51,59ff. und die Reaktion des Oberpriesters Amasja Am 7,10bß.

<sup>13</sup> Ein bedingtes Drohwort liegt vor in A.1121, Z.16–18: "Wenn er (scil. der

ankündigung der Tell-Deir-<sup>c</sup>Allā-Inscription das Verständnis auch der biblischen Unheilsprophezie als Umkehrpredigt, ihrer Ankündigung als heilsamer Drohung weithin wahrscheinlicher<sup>14</sup>. Dafür sprechen, was die Unheilsankündigung der Tell-Deir-<sup>c</sup>Allā-Inscription angeht, die bereits zitierte Aufforderung zu besonnenem Bedenken I 12, aber auch, falls diese Wendung doch anders zu verstehen sein sollte<sup>15</sup>, Bileams Fasten und sein Ausbrechen in Tränen I 3/4 – offenbar Tränen des Mitleids mit seinem vom Gericht der Göttin bedrohten Volk, das er am liebsten von ihm abwenden möchte –, ferner die Fürbitte auch der Götterversammlung gegenüber der zürnenden Göttin Šagar I 7<sup>16</sup> und vor allem die Paränese des Sehers šm<sup>c</sup>w'musr "hört die Mahnung" I 10. Die Unheilsankündigung hat also Erfolg, wenn sie das Angekündigte verhindert: wenn sich das Ankündigen *pragmatisch* als wirksam erweist, wird das Angekündigte *inhaltlich* zur Unwahrheit; so greift der Mensch als Dialogpartner der Gottheit in sein Schicksal und damit in den Weltprozeß ein. – Zwar entscheiden außerbiblische Analogie nicht eo ipso über biblische Inhalte und Funktionen. Umgekehrt aber kann das alttestamentliche Problem, das wir hier natürlich nicht

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König) nicht geben will (scil. Opfertiere für Adad; vgl. Z. 3f.), so bin ich (Adad) der Herr von Thron, Erde und Stadt: Ich werde, was ich gab, wegnehmen!" (W. VON SODEN, "Verkündigung des Gotteswillens durch prophetisches Wort in den altbabylonischen Briefen aus Mari", *WdO* 1, 1950, 397–403, bes. 403, mit Erweiterungen wieder abgedruckt in: *Bibel und Alter Orient*, 1985, 19–31, bes. 29). – Zur Verbindung von Mahnrede und Unheilsankündigung vgl. K. KOCH, "Die Briefe 'prophetischen' Inhalts aus Mari", *UF* 4, 1972, 53–77, bes. 65, wieder abgedruckt in: *Studien zur alttestamentlichen und altorientalischen Religionsgeschichte*, 1988, 153–188, bes. 171.

<sup>14</sup> Vgl. hierzu und zum Folgenden Vf., "Mythos – Kerygma – Wahrheit. Zur Hermeneutik einer biblischen Theologie", in: Ders. (ed.), *Was ist Wahrheit?*, 1989, 53–67, bes. 57–59.

<sup>15</sup> Vgl. etwa die Interpretation von J.A. HACKETT, *The Balaam Text from Deir 'Allā* (HSM 31), 1980, 27.29.53.

<sup>16</sup> Diese Deutung von I 7 beruht zunächst auf der Übersetzung von *w<sup>2</sup>l thgj<sup>c</sup>d<sup>c</sup>lm* durch "und grolle nicht ewig!". Sie wurde außer vom Vf. (aaO. [Anm. 1] 218.224f.) von H. und M. WEIPPERT (aaO. [Anm. 5] 93.103) vertreten; anders etwa P.K. McCARTER, *BASOR* 239, 1980, 51.54: "and you will not remove it forever!" Aber auch HOFTIJZER, der anders übersetzt, bemerkt in *TUAT* II 1, 1986, 142:9e, daß die Götter die Göttin vom Bösen abzuhalten versuchen. Ob auch die vorangehenden Wendungen . . . *hšk'w<sup>2</sup>l'n/gh<sup>c</sup>d/ym'w<sup>2</sup>[l]smrkj 'thbj'ht'* ]b' *hšk* "Finsternis und kein Glanz, . . . und nicht dein . . . du magst Schrecken bereiten . . ." I 6/7 die Göttin beschwichtigen sollen, ist mir nicht mehr sicher. Vielleicht liegt in den Worten "du magst Schrecken bereiten" die Einräumung vor, die der Göttin Grund und Recht für einen begrenzten Zorn, der Finsternis und Schrecken brächte, zugesteht.



als solches entfalten können, schon darum nicht ohne die außer-biblischen Analoga angegangen werden, weil es sich im Grunde um eine linguistische Frage im Themenkreis der Sprechakttheorie handelt.

Zunächst interessieren uns darum zwei methodisch-hermeneutische, die Pragmatik des Textes betreffende Fragen, die sich aus dem Detail der Tell-Deir-<sup>c</sup>Allā-Inschrift ergeben, und damit freilich gerade diesem eine theoretische Bedeutung verschaffen. Es werden nämlich, wie es scheint, der Göttin Šagar seitens der fürbittenden Götterversammlung Gründe zur Abwendung ihres Zorns vorgehalten, so wie Amos in seinen ersten beiden Visionen (7,1-6) Jahwe im Interesse einer Verschonung Israels begründet entgegentritt. Unter diesen Gründen sind – nach der erwähnten abmahnen-den Fürbitte an die Göttin I 7 – Beispiele bedenklichen Verhaltens in der Tierwelt, eines Zustands mithin, der infolge des bevorstehenden Gerichts eintritt, wenn dieses nicht durch göttliche Fürbitte sowie durch die Umkehrwirkung der mit der Ankündigung verbundenen Mahnung und den darauf antwortenden Gnadenerweis der zürnenden Göttin eben gerade noch verhindert wird. Die beiden methodisch-hermeneutischen Fragen sind:

- Wie sind Tierbezeichnungen in den altorientalischen Sprachen überhaupt zu deuten? Daß die Einzeldeutungen weithin unbefriedigend bleiben, wie schon die Vielzahl einander widersprechender Identifikationen zeigt, hat m.E. einsichtige Gründe.
- Welche Funktion hat die Benennung der betr. Tiere und die Schilderung ihres bedenklichen Verhaltens innerhalb einer Unheilsankündigung? Welches ist das dieser Funktionswahl zugrundeliegende Wirklichkeitsverständnis, das es gestattet, Tier- und Menschenwelt in der für die Tell-Deir-<sup>c</sup>Allā-Inschrift charakteristischen Weise zu verbinden?

## II

1. Wie Tier- (und Pflanzen-)Bezeichnungen in den altorientalischen Sprachen zu deuten sind, ist ein Problem der kontrastiven Linguistik, das alle altorientalistischen Philologien betrifft. Es lassen sich dazu zwei Positionen einander gegenüberstellen, von denen die eine von allgemeinerer Relevanz, die andere speziell althebraistisch bezogen ist.

a. Einerseits hat C. LÉVI-STRAUSS in *La pensée sauvage* (1962) von einer besonderen "Logik der totemistischen Klassifikation" im

Blick auf die "Taxonomien" der Indianer Südamerikas gesprochen<sup>17</sup>, d.h. im Blick auf die Klassifikation und Benennung der diesen bekannten und von ihnen verwendeten Pflanzen: die "Taxonomien" der Indianer seien Mittel eines Denkens, das sich zwar wie das unsere von Zufälligkeit und Schematik gleich weit entfernt halten möchte, das aber anders als das unsere die mit gefühlsbetonten Bedeutungen besetzten Bezeichnungen einzelner Arten so gebraucht, daß diese Bezeichnungen die Verwandtschaft oder Nichtverwandtschaft der Arten mit dem Menschen darstellen können; dabei seien diese "Taxonomien" weder Mittel eigennütziger Verwertung noch mystischer Partizipation, sondern vielmehr Medien ordnenden Denkens innerhalb eines System von einander ausgleichenden Bedeutungsbeziehungen. Zwar sind die Altorientalen keine Indianer; vor allem ist ein nennenswertes Vorkommen von Totemismus im Alten Vorderen Orient zumindest zweifelhaft<sup>18</sup>, obwohl ich allerdings gern wüßte, wie die Häufigkeit von nicht immer naheliegenden Tiervergleichen in Stammes- und Völkersprüchen religionsgeschichtlich zu erklären ist. Aber auf die alt-amerikanische Herkunft des Paradigmas und auf die Klassifikation der indianischen "Taxonomien" als totemistisch kommt es nicht an, sondern vielmehr darauf, daß die heutigen Naturzivilisationen ebenso wie die frühantiken Völker Tiere und Pflanzen nach anderen Kategorien als wir benannten und klassifizierten. Ihre "Taxonomien" waren nach komplexen, ja polyvalenten (widerspruchstoleranten) Beobachtungsprinzipien organisiert; ihnen lagen andere Beobachtungskriterien zugrunde als den abendländischen Benennungen und Klassifikationen, die Carl von LINNÉ in seinem *Systema naturae* (1735) begründet hat. Die abendländische Taxonomie knüpft allgemein an logische Verfahren an, die seit Parmenides und Platon bewußt gepflegt werden, während als Begründer speziell einer regelmäßigen zoologischen Klassifikation nach Genus und Spezies letztlich Aristoteles gilt<sup>19</sup>. C. von LINNÉ aber hat zugleich

<sup>17</sup> Deutsch: *Das wilde Denken* (stw 14), 1981, 49–91.

<sup>18</sup> A. SALONEN (*Vögel und Vogelfang im alten Mesopotamien* [AASF-B 180], 1973, 9.79) weist allerdings auf mesopotamische Städtenamen hin, die auf Vogelbezeichnungen zurückgehen, vielleicht den Namen des Totemvogels der betreffenden Stadt; hierfür ist etwa die Übereinstimmung von bu r u<sub>4</sub> (ŠIR.BUR) "Krähe" und ŠIR.BUR.LA<sup>ki</sup> als Schreibung für "Lagaš" bezeichnend.

<sup>19</sup> Vgl. B. SNELL, *Die Entdeckung des Geistes*, 1975, zu Parmenides S. 223, zu Platon 178. Zu Aristoteles u.a. vgl. die unten genannte Diss. von P.C. WAPNISH, S. 32ff. (Lit.).



für die spätere evolutionäre Klassifikation, d.h. für eine linear strukturierte, im Idealfall monovalente (widerspruchsintolerante) Theorie, mithin für die Evolutionslehre von Charles DARWIN Bedingungen und Maßstäbe geschaffen. Zumindest uns scheint eine lineare, monovalente Konstruktion systemtheoretisch überlegen.

Auch der Altorientale hat – darin mit den Indianern vergleichbar, anders aber als der Abendländer – bei klassifikatorischen Identifikationen offenbar nach der Rolle gefragt, die die einzelne Gattung oder besser: der von dieser repräsentierte Typus innerhalb eines Systems von Bedeutungen spielte; um dieser typischen Rollen willen werden bestimmte Tiere in der Tell-Deir-<sup>c</sup>Allā-Inscription, in den alttestamentlichen Unheilsankündigungen oder den Gottesreden des Hiobbuches genannt. – Nach welchen Regeln sind solche rollenorientierte Typisierungen in einer altorientalischen Naturkunde erfolgt? Über Leistung und Grenze dessen, womit zunächst die sumerisch, dann die akkadisch sprechenden Mesopotamier Wissenschaft antizipierten, angefangen mit diversen Zeugnissen einer "Listenwissenschaft", hat bekanntlich W. VON SODEN mehrfach gehandelt<sup>20</sup>, ohne doch nach der Rolle der "Taxonomien" im Bedeutungssystem zu fragen.

Es gibt daneben m.W. nur einen ersten größeren Versuch, die vorwissenschaftliche Logik in den Tierbenennungen und -klassifikationen einer orientalischen Kultur systematisch aufzuzeigen: P.C. WAPNISH hat sich in ihrer 1984 vorgelegten Dissertation der Columbia University über *Animal Names and Animal Classifications in Mesopotamia* einen "interdisciplinary approach based on folk taxonomy", wie es im Untertitel heißt<sup>21</sup>, zur Aufgabe gemacht; der rezentes ethnographisches Material vergleichende Entwurf zielt auf eine die mesopotamische Kultur betreffende Theorie des Wissenserwerbs und der Kategorienbildung in bezug auf die Tierwelt. "A discussion of scientific classification shows the development of logic and modern systematics, and especially its interface with philology. A parallel

<sup>20</sup> Leistung und Grenze sumerischer und babylonischer Wissenschaft, *Die Welt als Geschichte* 2, 1936, 411–434. 509–557, mit Ergänzungen wieder abgedruckt zusammen mit B. LANDSBERGER, "Die Eigenbegrifflichkeit der babylonischen Welt" (*Islamica* 2, 1926, 355–372), als Libellus 142 der Wissenschaftlichen Buchgesellschaft, 1965 = 1974, hier zu "Naturwissenschaften", speziell zur "Zoologie" S. 69ff.; Ders., *Sprache, Denken und Begriffsbildung im Alten Orient* (Akademie der Wissenschaften und der Literatur [Mainz], Abh. d. geistes- und sozialwiss. Kl. 1973:6), 1974, bes. 11ff.; Ders., *Einführung in die Altorientalistik*, 1985, 138–164, zu Ansätzen von "Naturwissenschaften" 152–154.

<sup>21</sup> University Microfilms International, Ann Arbor 1985 (8511567); die nachfolgenden Zitate finden sich S. 1/2.161. – Vfin bezieht sich nicht auf Lévi-STAUB.

discussion of folk classification shows how it can act as an integrative tool between philology and systematics." Die Arbeit zeitigt eine Reihe von Einzelergebnissen zur Soziologie der Schriftkulturen und in bezug auf eine in sich nicht widerspruchsfreie "taxonomic organization through the linguistic encoding and arrangement of terms employed by the scribes", die wir hier nicht darstellen wollen.

Als Material solcher Analysen altorientalischer "Taxonomien" bieten sich eben die in frühe Zeiten zurückreichenden Listen aus Mesopotamien an<sup>22</sup>, vor allem die zweisprachigen (sumerisch-

<sup>22</sup> Einem umfassenden Überblick bietet A. CAVIGNEAUX, "Lexikalische Listen", *RLA* VI, 1980-1983, 609-641. Das hohe Alter und die weite Verbreitung der Gattung Tiernamenliste (dazu CAVIGNEAUX 612f.) zeigt etwa die seinerzeit von A. DEIMEL veröffentlichte einsprachig sumerische Liste von Vogelnamen aus präargonischer Zeit VAT 9124 (*Die Inschriften aus Fara II: Schultexte aus Fara* [WVDOG 43], 1923, Nr. 58 VI 11ff.), wozu jetzt ein Duplikat aus Ebla vorliegt (G. PETTINATO, "Liste presargoniche di ucelli nella documentazione di Fara ed Ebla", *OrAnt* 17, 1978, 165-178 + Tafeln XIV - XVI; Neubearbeitung Ders., *Testi lessicali monolingui della biblioteca L. 2769* [MEE III], Neapel 1981, 105-120, dort S. 120/1 Hinweis auf drei weitere Vogellisten aus Ebla); speziell zu anderen Vogellisten aus dem 3. Jt. (Uruk III, Lagaš, Nippur) vgl. CAVIGNEAUX 613a.

Zu *ĤAR-ra* = *ĥubullu* aus Emar vgl. D. ARNAUD, *Recherches au pays d'Aštata. Emar VI 4: Textes de la bibliothèque, transcriptions et traductions* (Mission archéologique de Meskéné-Emar), 1987, 38-160; die Texte sind zum Teil bereits zweisprachig.

<sup>23</sup> Kommentierte Editionen: LANDSBERGER, *Die Fauna des alten Mesopotamien nach der 14. Tafel der Serie Ĥar-ra* = *ĥubullu* (Abh. d. phil.-hist. Kl. d. Sächs. Akademie der Wissenschaften XLII, Nr. VI), 1934; Ders., *The Fauna of Ancient Mesopotamia I/II* (Materialien zum sumerischen Lexikon [= MSL] VIII 1/2), Rom 1960/2. - LANDSBERGER bemerkte (1934, 45) zu der zweisprachigen Liste von Schlangennamen *Ĥh XIV 1-47.407-409*, daß die von einer sumerischen Klassifikation ausgehenden "Einteilungen nach anderem als zoologischen Prinzip" erfolgte. Die Beispiele, die seine Skepsis erregten, sind die Gleichsetzung von *m u š. s i g<sub>7</sub> s i g<sub>7</sub>* "die gelbe (grüne) Schlange" offenbar mit dem wenig schlangenhähnlichen *urnu* "Waran(?)" *Ĥh XIV 15* (dazu Kommentarzusatz *muš a[r-qu]* *Ĥg 265* [MSL VIII 2, 45] sowie *mut-tal-lik* : *muš<sup>s</sup>ur-nu* : *še-ru ár-qa* *Fauna* 51 c7, worin das Determinativ *muš* bei *urnu* zu beachten ist) und das Verständnis von *m u š. ĥ u l* "böse Schlange" als Artbezeichnung, nämlich *ĥulmiĥtum* (AHw: etwa "Drache" mit hebr., syr. und arab. Isoglosse; CAD: a snake or lizard) und *ĥulmāĥu* (AHw: eine Schlange; CAD: a snake) *Ĥh XIV 21f.* (vgl. *m i r. ĥ u l* = *ĥulmiĥtu*, *ĥulmāĥu* 407f., ferner *m u š. ĥ u l* : *ĥul-miĥ-tum* *Fauna* 51 c3). Liegt in ersterem Fall eine Subsumtion des Waran (?) unter den Oberbegriff *m u š* nur an der Armut des Sumerischen an nominalen Wortwurzeln, oder bestand für eine genauere Begrenzung des unter *m u š* Begriffenen kein Bedarf? - Beruht im zweiten Fall die Wahl von sumerisch *m u š. ĥ u l* und *m i r. ĥ u l* in *Ĥh XIV 21f.407f.* für zwei akkadische Reptilbezeichnungen, von denen freilich das isoglossenlose *ĥulmāĥu* eine ad-hoc-Bildung sein kann (*Fauna* 62), einfach auf der Suche nach einem semantisch sinnvollen sumerischen Lautanalogon zu den akkadischen Lexemen? Offenbar mußte in diesem Fall für ein vorgegebenes akkadisches Wort, damit es in die Liste aufgenommen werden konnte, eine sumerische Entsprechung gefunden werden (vgl. von SODEN, "Leistung und



akkadischen) Aufreihungen von Tierbezeichnungen in der späteren Serie HAR-ra = *hubullu* (Hh), u.zw. Tafeln XIII, XIV und XVIII, wozu eine parallel laufende Kommentarserie wie HAR-gu d (Hg) gehört. Bei B. LANDSBERGER, der diese Texte, z.T. zweimal, ediert hat, findet sich bereits eine ganze Reihe wertvoller Einzelbeobachtungen<sup>23</sup>. Deren Zufälligkeit entspricht vielleicht gerade einer den frühantiken Benennungen und Klassifikationen eigentümlichen Systemschwäche, die sich zum Ordnungswillen der Listen<sup>24</sup> kontrafaktorisch verhält. Reiche Auswertungen verschiedener Texte zur mesopotamischen Naturkunde finden sich in den Arbeiten A. SALONENS<sup>25</sup>.

b. *Andererseits* ist es in der althebräischen Lexikographie, aber auch in den Lexikographien anderer altsemitischer Sprachen üblich geworden, Tier- und Pflanzennamen im Idealfall mit den lateinischen Doppelbezeichnungen der modernen Zoologie und Botanik zu bestimmen. So ist die im übrigen sehr verdienstvolle Monographie von F. S. BODENHEIMER, *Animal and Man in Bible Lands*, das Werk eines Zoologieprofessors an der Hebräischen Universität, so stark an der modernen zoologischen Taxonomie orientiert, daß am Ende ein umfangreicher "Index of the Latin

Grenze" [Anm. 20], 70<sup>81</sup>). Das sumerische Element, *hul* "böse", ist zumindest in Hh XIV 21f. 407f. für die beiden lautanalogen akkadischen Lexeme *hulmiṭtu* und *hulmāḫu* die Konstante, das zweite sumerische Element, *muš* "Schlange" und *mir* "?" (vgl. *m u š. mir* 11f.), im Wechsel zwischen 21f. und 407f. die Variable; so verdankt sich das konstante sumerische Element *hul* wohl ebenso der Lautanalogie, wie dies bei der Wahl des Wortzeichens *muš. hul* für *hulmiṭtu* der Fall ist. Umgekehrt fügt Hg 264 zu *m u š. hul* = *hul-miṭ-tu* als dritte Angabe *muš-ḫu[š-šu]* hinzu; ein medizinischer Kommentar zu Hh (*Fauna*, 42:65f.) stellt vor *m u š. hul* = *hul-miṭ-tu* die nicht durch Lautähnlichkeit motivierte alternative Gleichung *m u š. i di m* = *hul-miṭ-tu*. Der Begriff *m u š* war also auf *hulmiṭtu* anwendbar, obwohl dessen Beschreibung als vierfüßig (4 GİR.MEŠ-ŠU *Fauna* 53:26) wie offenbar im Fall von *urnu* "Waran (?)" eher an eine Echsenart denken läßt.

<sup>24</sup> Daß schon die sumerischen Zeichen- und zugleich Wortlisten der Urukperiode von einem "den Sumerern seit alters in ganz einzigartiger Weise eigenen Ordnungswillen" gelenkt waren, "der alles, Sichtbares und Unsichtbares, in einer höheren Ordnung zusammenzufassen und zusammenzudenken sich bemüht," und daß auch die jüngeren ausschließlich nach Sach- statt nach Schriftgesichtspunkten (Zeichenformen) geordneten Gegenstandslisten die Aufgabe hatten, "eine systematische Ordnung der gesamten Gegenstands- und Erscheinungswelt zu ermöglichen", hat VON SODEN, "Leistung und Grenze" (Anm. 20), 29.31f. u.ö., betont; vgl. zum Problem WAPNISH, aaO. 163f.

<sup>25</sup> *Hippologica Accadica* (AASF B 100), 1956; *Die Fischerei im alten Mesopotamien nach sumerisch-akkadischen Quellen* (AASF B 166), 1970; *Vögel und Vogelfang im alten Mesopotamien* (AASF B 180), 1973; *Jagd und Jagdtiere im alten Mesopotamien* (AASF B 196), 1976.

Names of Animals and Species of Animals" (S. 223–228) zu erstellen lohnt<sup>26</sup>. L. KÖHLER vollends hat seinem vielbenutzten *Lexicon in Veteris Testamenti libros* im *Supplementum* ein Register wissenschaftlicher – botanischer und zoologischer – Wörter beigegeben (S. 119f. = 1265f.), das jeweils von dem betr. Begriff der modernen biologischen Nomenklatur zu einem entsprechenden althebräischen Begriff führen soll; W. BAUMGARTNER empfiehlt im Vorwort des postum erschienenen *Supplementum* (p. VII) u.a. diese Zusammenstellung als ein besonders willkommenes Novum, woran man zumindest heute füglich zweifeln kann.

c. Es scheint mir offenkundig, daß man zuerst die altorientalischen, mithin auch die althebräischen Klassifikationskriterien kennen müßte, die den betreffenden "Taxonomien" zugrunde liegen, ehe man über die Bedeutungen oder besser: über die Bedeutungspotentiale althebräischer Tier- und Pflanzennamen und damit über den Platz der einzelnen Signifikate im Bedeutungssystem der althebräischen Kultur eine Entscheidung trifft. Wir brauchen eine Art "Systemarchäologie", die an Listen sicherer ablesbar ist als an literarischen Gestaltungen, deren "Denkform" erst durch viel risikoreichere Analysen zu gewinnen ist. Vielleicht greift schon der Begriff "Taxonomie" zu weit, insofern er an eine mit unseren zoologischen Klassifikationen zumindest vergleichbare Systematik denken läßt, während in Wirklichkeit mehr oder weniger zufällige Eindrücke und Assoziationen, vor allem aber wechselnde Lebensinteressen nicht nur zu Benennungen (die ohnehin die divergentesten Ursprünge haben), sondern auch zu Klassifikationen und den ihnen entsprechenden Ordnungsentwürfen führen, an denen, wenn sie erst einmal – etwa in Listen – literarisiert waren, beharrlich festgehalten wird. Wir haben den Begriff "Taxonomie" deshalb, wenn wir ihn auf das Denken des Alten Orients beziehen, in Anführungszeichen gestellt. Eine Wertung liegt darin nicht: sollte, was uns als kontingent erscheint, nicht unter anderen Aspekten auch seine Notwendigkeit haben?

2. In die gleiche Richtung weist die bekannte Schwierigkeit, althebräische, aramäische oder akkadische Tier- oder Pflanzenbezeichnungen, speziell die Liste von Vogelnamen in I 7–9 der

<sup>26</sup> Leiden 1960. – Vgl. etwa auch SALONEN, *Vögel* 296–302, der dort in einer "Liste der mit modernen Namen identifizierten alten Vogelnamen" sumerische und akkadische, moderne (deutsche, englische) und lateinische (zoologische) Bezeichnungen zusammenstellt.



Tell-Deir-<sup>c</sup>Allā-Inschrift, in einer uns geläufigen Terminologie wiederzugeben.

a. Für viele der einschlägigen Lexeme stellt das Deutsche mehrere bedeutungsmäßig unterschiedene Äquivalente zur Verfügung: diese altsemitischen Lexeme erscheinen im Blick auf die Zielsprache der Übersetzung als polysem; sie haben verglichen mit den Begriffen, durch die sie übersetzt werden, ein breiteres Bedeutungspotential. So ist, um mit etwas verhältnismäßig Einfachem, Unstrittigem zu beginnen, das in I 8 gebrauchte *nšr* und das zugehörige Nomen unitatis *nšrt* am Ende der gleichen Zeile – als *nāšār* – nach der althebraistischen Semantik sowohl mit "Adler"<sup>27</sup>, als, zumindest wegen wichtiger Einzelstellen, auch mit "Geier"<sup>28</sup> wiederzugeben. Für eine stellenweise Übersetzung von *nāšār* mit "Geier" spricht, daß nur auf den Geier das in Ijob 39,(27-)30 vermerkte Aasfressen<sup>29</sup> (vgl. Spr 30,17; 1QpHab 3,11) und die in Mi 1,16 erwähnte Kahlköpfigkeit paßt. Wie aber verhält sich dann *nšr* zu dem in I 8 fast unmittelbar folgenden *rh[m]n*, das, wenn die Lesung

<sup>27</sup> So herkömmlicher Weise, in der althebräischen Lexikographie bei F. ZORELL, offenbar nach *šstōc* LXX und Aquila V, und zum Biblisch-Aramäischen *nšar* KBL<sup>2</sup>, E. VOGT; zum Nabatäischen DISO.

<sup>28</sup> G.R. DRIVER, "Birds in the OT", PEQ 87, 1955, 5–20. 129–140, bes. 8f.: "primarily the vulture, in all probability the griffon vulture" mit der Einschränkung: "At the same time the *nešer* undoubtedly includes the eagle"; vgl. Ders., PEQ 90, 1958, 56–58, bes. 56f. Beide Bedeutungen nebeneinander haben auch GesB, BDB, E. KÖNIG, KBL<sup>2</sup>, HAL. Ausschließlicher für "eine große Geierart, wahrscheinlich den Gänsegeier (*Gyps fulvus*)" ist O. KEEL (*Jahwes Entgegnung an Ijob*, 1978, 69<sup>234</sup>) eingetreten, wenn auch ebenfalls nicht ganz ohne Einschränkung. Ähnlich geht T. KRONHOLM (Art. *nāšer*, ThWAT V, 1986, 680–689) davon aus, "daß es sich normalerweise nicht um den Adler handeln kann, sondern um eine Geierart" (682), den *Gyps fulvus*; KRONHOLM gesteht aber zu, daß nicht "alle altlichen *nāšer*-Belege eindeutig vom Geier sprechen" (683), wobei er n.a. KEEL zitiert. In bezug auf das Südsemitische bemerkt KRONHOLM zu Recht, daß arabisch *nāšar* u.ä. "als konturschwache Bezeichnung für die Gattung der Geier-Vögel (mit Ausnahme von *raḥam*, 'adlerähnlicher Geier') fungieren kann, ausnahmsweise auch für den 'Adler'" (681); zu äthiopisch *nešer*, das nach KRONHOLM ebenfalls "sowohl 'Adler' als auch 'Geier'" bezeichnet, vgl. jetzt W. LESLAU (*Comparative Dictionary of Ge'ez*, 1987, 403): "eagle, vulture, hawk". – KEEL und KRONHOLM führen die durchgängigen LXX- und V-Wiedergaben von *nāšār* als "Adler" auf eine von den Griechen im Alten Orient eingeführte Geringschätzung des Geiers und Hochschätzung des Adlers als eines königlichen Vogels zurück. Wieweit solche Wertungen die Übersetzung beeinflussen, bedürfte freilich einer breiter angelegten Untersuchung.

<sup>29</sup> Fraglich ist aber, wie sich *nāšār* Hi 39,27 zu folgendem *w'ki* verhält, da 11QtgJob und LXX in *ki* einen weiteren Vogelnamen finden, nämlich *ʿwz* "Geier(?)" (vgl. ATTM 296.625) bzw. *γύψ*. Vgl. G.R. DRIVER, PEQ 104, 1972, 64–66, und KEEL, aaO. (Anm. 28).

richtig ist, wie althebräisch *rāḥām* Lev 11,18 und das Nomen unitatis *rāḥāmā* Dtn 14,17 entsprechend seinen beiden arabischen Isoglossen mit "Geier, Schmutzgeier"<sup>30</sup> wiederzugeben ist? Umfaßt *ḥšr* als konturschwacher Begriff die Bedeutungen "Geier" und "Adler" zugleich, so daß sich der Übersetzer lediglich mit Rücksicht auf die Zielsprache entsprechend dem jeweiligen Kontext für eine der beiden Übersetzungen entscheiden mußte<sup>31</sup>? Ist also in I 8 der Tell-Deir-<sup>c</sup>Allā-Inscription nur wegen der kontextuellen Differenz zu *rḥ[m]n* deutsch an "Adler" zu denken? Und wie verhält sich dann althebräisch *nāšār* zu *pārās* (gewöhnlich:) "Lämmergeier", zu *ʿoznijā* oder zu *ʿajit*, zu Lexemen mithin, die die althebräische Lexikographie ebenfalls für große Raubvögel festlegt<sup>32</sup>?

Umgekehrt kennt die althebräische Lexikographie zahlreichere, dafür jeweils enger begrenzte Begriffe, wo wir pauschaler kategorisieren. Man beobachtete anders, weil man andere Interessen verfolgte oder weil andere Weisen der Beobachtung es gestatteten, andere Interessen zu verfolgen. So hatte die Unterscheidung von Adler und Geier für das Leben wohl kaum eine Bedeutung. Wohl aber scheint es darauf angekommen zu sein, mit den in Joel 1,4 und – in gleicher Reihenfolge, aber anders abgetrennt – in Joel 2,25 gebrauchten vier Termini<sup>33</sup>, zumindest nach einer Hypothese<sup>34</sup>, vier verschiedene Stadien der Heuschreckenmetamorphose zu

<sup>30</sup> Vgl. zu arabisch *raḥam*<sup>am</sup> Anm. 28; das arabische Nomen unitatis *raḥam*<sup>am</sup> wird schon in *GesB* erwähnt. – Zur Übersetzung "Schmutzgeier" vgl. KRONHOLM, aaO. (Anm. 28) 684 (Lit.).

<sup>31</sup> Das gleiche Problem ergibt sich für akkadisch *erū* (I) = *arū* (II) (*AHW*: gewöhnlich "Adler"; vgl. *CAD*: *erū* C "eagle"), wozu SALONEN (*Vögel* [Anm. 18], 104–106, vgl. 160f.292) mit Zitat LANDSBERGERS (*MSL* VIII 2, 130) einen Text nachweist, der das Aasfressen des *erū* (Λ. MUŠEN) voraussetzt: Maqlū VIII 85. – Während akkadisch *našru* in der Äquation [Λ]<sup>(te-[e])</sup> = *e-ru-ú* = *na-āš-ru* Hg C 26 offenbar Kanaanismus ist (vgl. W. VON SODEN, *AfO* 18, 1957/8, 393), fragt es sich, ob nicht in *nadru* = [Λ].u š. bzw. [Λ.u š] g u. l a Hb XIV 137b.c. = Λ.u š<sup>mušen</sup> Hg B IV 241, = Λ.u š<sup>mušen</sup> KBo I 47 + 57 + KUB IV 96:II 25' (*MSL* VIII 2,60) eine ferne Isoglosse zu *nāšār* u.ä. vorliegt; zur Deutung von *nadru* als "Gänsegeier" (*Gyps fulvus*) SALONEN, *Vögel* (Anm. 18), 107.292; anders *AHW*: "wild, aggressiv" als Adjektiv zu *nadāru(m)*; *CAD*: "ranging, furious", was natürlich für die übrigen Belege zutreffen wird.

<sup>32</sup> Vgl. KRONHOLM, aaO. (Anm. 28) 683/4, S.P. TOPEROFF, "The Eagle in Bible and Midrash", *Dor leDor* 15, 1986/7, 260–263.

<sup>33</sup> In Joel 1,4 folgen *gāzām*, *ʿarbā*, *jālāq*, *ḥāšil* aufeinander, in 2,25 *ʿarbā*, *jālāq*, *ḥāšil*, *gāzām*. *gāzām* steht also einmal am Anfang, einmal am Ende der gleichen Reihenfolge (vgl. L. KÖHLER, "Die Bezeichnungen der Heuschrecke im AT", *ZDPV* 49, 1926, 328–333, bes. 328); es handelt sich offenbar um einen Regelkreis. Da *ʿarbā* Lev 11,22 (s. sogleich) u.ö. als Speziesbezeichnung vorkommt, bedeutet es wohl die entwickelte Form und steht darum in Joel 2,25 am Anfang der Reihe.

<sup>34</sup> So nach dem Vorgang von CREDNER (1831) I. AHARONI, *Hāʿarbā*, Jaffa 1920, 12ff.37 (vgl. das Referat bei DALMAN, *AuS* I 2, 394), L. BAUER, "Die



unterscheiden. Dagegen kennt das Reinheitsgesetz bei der Aufzählung eßbarer, d.h. reiner Heuschreckenarten in Lev 11,22, als Spezies dort jeweils durch *l'minô* und *l'minēhū* "nach seiner Art" ausgewiesen, wiederum vier Termini, von denen nur *ʿarbā* auch in der Nomenklatur von Joel 1,4; 2,25 vorkommt.

Auf einer etwas anderen Ebene wieder liegt die Vielzahl hebräischer Begriffe, die dem deutschen Wort "Löwe" entsprechen<sup>35</sup>.

b. Zurück zur Liste von Vogelbezeichnungen in I 7-9 der Tell-Deir-<sup>c</sup>Allā-Inschrift! - In einigen Fällen scheint hierzu die Schwierigkeit einer Identifikation durch den onomatopoetischen Charakter der Bezeichnungen, für den es in Vogelnamen vieler Sprachen Parallelen gibt, relativ gering.

Dies gilt schon in bezug auf die erste Bezeichnung der Reihe, nämlich *ss<sup>c</sup>gr*. Für die Deutung dieses Wortes als Bezeichnung einer Spezies der Gattung Schwalbe spricht zunächst der Befund der Versionen zu *sūs ʿāgūr* Jes 38,14 MT. So hat LXX offenbar nur *k'sūs* gelesen und dies mit *ὡς χελιδὼν* "wie eine Schwalbe" wiedergegeben, während offenbar Symmachus *k'sūs ʿāgūr* durch *ὡς χελιδὼν ἐγκεκλεισμένη* übersetzt, was wiederum *kswsj<sup>c</sup> d-ʿhjd* "wie eine eingeschlossene Schwalbe" im Targum Jonathan entspricht; Vulgata hat für *k'sūs ʿāgūr*: *sicut pullus hirundinis*<sup>36</sup>. Sodann fällt für "Schwalbe" zu *sūs ʿāgūr* Jes 38,14 MT in die Waagschale, daß von dem bezeichneten Wesen im Folgenden eine Lautäußerung ausgesagt wird, nämlich *špšēp* "wispern", die auf die Schwalbe,

Heuschreckenplage in Palästina", ZDPV 49, 1926, 168-171, bes. 170, KÖHLER, das. (vgl. Anm. 33) 332 [zu *jālāq* und *sof-ām* Lev 11,22], S. KRAUSS, ZDPV 50, 1927, 244ff., F.S. BODENHEIMER, *Animal Life in Palestine*, 1935, 309ff., O.R. SELLERS, *AJSL* 1935/6, 81ff., und J.A. THOMPSON, *JNES* 14, 1955, 52ff., denen H.W. WOLFF, *Dodekapropheten* 2 (BK XIV 2), 30-32, unter Einbezug z.T. einschränkender zoologischer Informationen zugestimmt hat. - Zurückhaltender äußerte sich W. RUDOLPH, *Joel, Amos, Obadja, Jona* (KAT XIII 2), 1971, 42. Vgl. zuletzt M. TAAM-AMBEY, *BiTrans* 36, 1985, 216-220.

<sup>35</sup> Hier erscheint eine ursprünglich konturschwache Bezeichnung wie *ʿari* > *ʿarjē*, deren semitische Isoglossen verschiedene große und gefährliche Tiere benennen, neben engeren Begriffen verschiedener Herkunft und z.T. altersspezifischer Bedeutung, was aber zu semantischen Überschneidungen führt; vgl. G.J. BOTTERWECK, Art. *ʿari*, *ThWAT* I, 1973, 404-418, bes. 405-407.

<sup>36</sup> H. WILDBERGER (*Jesaja* 3 [BK X 3], 1982, 1443f.) nimmt metri causa an, daß *ʿāgūr* von einem Glossator hinzugefügt sei, um das richtige Verständnis von *sūs* zu sichern, was dem Befund bei LXX zu entsprechen scheint. - Hieronymus zitiert Symmachus mit *sicut hirundo inclusa* (vgl. zum Gesamtbefund J. ZIEGLER, *LXX Jesaja*, 1983, 263); er selbst hat *pullus hirundinis*, wie aus dem Zitat bei F. FIELD (*Origenis Hexapla* II, 1875 = 1964, 506<sup>39</sup>) hervorgeht, für die richtige Wiedergabe von hebräisch *SUS AGUR* im Gegensatz zu LXX gehalten.

freilich nicht nur auf sie paßt. Entscheidend ist aber, daß *sús*, mehr aber noch das zu *wsws* <sup>c</sup>*gwr* Jer 8,7 bezeugte offensichtlich richtigere *Q<sup>e</sup>rê sís* (vgl. *SIS* Theodotion bei Hieronymus zu Jes 38,14, σεις Aquila zu Jer 8,7) als eine onomatopoetische Bezeichnung der Schwalbe<sup>37</sup> plausibel ist, was darüber hinaus durch *snōnūtā* "Schwalbe" S zu Jer 8,7 eine weitere Bestätigung findet. – Als Bezeichnung einer *Spezies* der Gattung Schwalbe ist *ss<sup>c</sup>gr/sís* <sup>c</sup>*āgúr* wohl auch durch seine Struktur als Kompositbezeichnung aus Nomen + Attribut (Dehnungsstufe des beschreibenden Adjektivs nach *qatul*) kenntlich, der in Jer 8,7 freilich (*w<sup>e</sup>tōr*) *w<sup>e</sup>sís w<sup>e</sup>(!)* <sup>c</sup>*āgúr* gegenübersteht. Die lexikalisch-grammatische Möglichkeit der Verbindung *sú/ís* <sup>c</sup>*āgúr* Jes 38,14 wird durch *ss<sup>c</sup>gr* in I 7 der Tell-Deir-<sup>c</sup>Allā-Inscription auch dann bestätigt, wenn <sup>c</sup>*āgúr* in Jes 38,14 Glosse sein sollte. Woran aber dachte der überlieferte hebräische Text zu dem durch *w<sup>e</sup>* – abgetrennten <sup>c</sup>*āgúr* Jer 8,7<sup>38</sup>?

Die Annahme einer onomatopoetischen Wortbildung hilft wohl auch bei der Identifikation von *dr̄r* in I 8 der Tell-Deir-<sup>c</sup>Allā-Inscription, dem im Alten Testament *ḏr̄r* < \**dur̄r* entspricht; dagegen gibt es für *ḏr̄r* m.W. keine semitischen Isoglossen. Daß *dr̄r* in I 8 "Schwalbe" bedeutet, wie aufgrund der jüdischen Auslegung ein Großteil der althebräischen Lexikographie<sup>39</sup> zu *ḏr̄r* annimmt, ist schon deshalb unwahrscheinlich, weil dieser Begriff bereits durch *ss<sup>c</sup>gr* I 7 besetzt ist. Für die Übersetzung mit dem im ersten Teil ebenfalls onomatopoetischen "Turteltaube" oder "Tauben" sprechen Wiedergaben in antiken Versionen wie τρυγών LXX (= turtur Hieronymus sec. LXX), und ṣ(w)pnjn<sup>3</sup> Targ. und

<sup>37</sup> F. ZORELL (*Lexicon Hebraicum et Aramaicum Veteris Testamenti*, 1955 = 1968, s.v. *sús* II) kennt nach TRISTRAM und L. KÖHLER (ZAW 54, 1936, 289; *Kleine Lichte*, 1945, 35–39) modern-arabisches *sís* "Mauersegler": "*si-si* est clamor huius avis". Vgl. DRIVER, aaO. (Anm. 28) 131f. (weitere Lit.). Eine onomatopoetische Deutung, freilich anderer Art, hatte auch Symmachus mit τέτιξ "Zikade" vor Augen.

<sup>38</sup> KÖHLER (aaO. [Anm. 37]; KBL<sup>2</sup>) dachte an die Kurzfußdrossel. – Zu akkadisch *igirú* "Reiher (?) " scheint wegen des /-ú/ als möglichem vierten Radikal zunächst keine Beziehung zu bestehen. Die Gleichsetzung [x]<sup>i-gi-ra</sup> IGI.MUŠEN = [i-gi-ru-ú] Hh XVIII 150 (vgl. CAD I/J, 49; MSL VIII 2, 122) läßt aber zu akkadisch *igirú* eher an ein sumerisches Lehnwort denken (vgl. AHw s.v.), so daß für eine Übernahme ins Hebräische nicht dieselben Voraussetzungen wie im Fall ursprünglich semitischer Wörter beständen.

<sup>39</sup> Vgl. zu mittelhebräisch *šippōr ḏr̄r* = "Schwalbe" L. LEWISOHN, *Die Zoologie des Talmuds*, 1858, 206–209; anders *Diet Talm* ("a free bird") und *WTM* ("schnellfliegende Vögel, Sperlinge"), vgl. DALMAN, *AuS* VI 97f.; VII 267. Zur neueren Lexikographie *GesThes*, *GesB*, ferner *BDB*, *KBL*<sup>2</sup>.



S, beides zu Ps 84,4<sup>40</sup>. Zwar ließe sich für eine Identifikation von *ḏrōr* als "Sperling" ebenfalls auf antike Übersetzungen hinweisen, nämlich auf στρουθός Aquila und passer V zu Ps 84,4 sowie auf στρουθοί LXX und passer V zu Spr 26,2; doch fällt dagegen ins Gewicht, daß onomatopoetisch vermittelte Bedeutungen in allen semitischen Sprachen und darüber hinaus vorrangige Wahrscheinlichkeit haben, während umgekehrt manche Wiedergabeweisen der Versionen mehr auf Gelehrtentradition als auf Anschauung beruhen dürften<sup>41</sup>. *jwn* I 9 ist offenbar – wie *tōr* (?) – eine andere Taubenart.

Ist auch *špr* in I 9 der Tell-Deir-<sup>c</sup>Allā-Inschrift – wegen des w- "und" offenbar das letzte Glied in der Kette der Speziesbezeichnungen vor einer Textlücke<sup>42</sup> – eine onomatopoetische Bildung? Hilft auch hier die klangmalende Funktion bei der Identifikation? – Die Wurzel *špr* hat im Semitischen verbale und nominale Realisierungen. Zweifellos erwecken die Verben, akkadisch *šabāru(m)* (I) "sich schnell bewegen, zwinkern, tuscheln, zwitschern"<sup>43</sup>, aramäisch *špar* (I) "pfeifen"<sup>44</sup> und arabisch *šafara* "pfeifen (von Menschen, Vögeln)"<sup>45</sup>, den Eindruck des Onomatopoetischen, insofern Vogelgezwitscher nachgeahmt zu werden scheint<sup>46</sup>. In welchem Verhältnis aber stehen dazu die Nomina mit den Radikalen *špr*? Akkadisch *šibāru(m)* (II) "Sperling (?)" und *iššūru(m)* "Vogel", welches letztere W. BAUMGARTNER von *\*(i)špūrum* entsprechend arabisch *ʿuṣfur* ableiten wollte<sup>47</sup>, lassen sich nicht als *pirās-* bzw. *gar iprūs-*Bildungen auf das Verb *šabāru(m)* (I) zurückführen. Noch weniger sind die o.g. Verben als Denominative zu verstehen, zumal man in diesem Fall in einer Sprache, die den zweiten Radikal des Nomens *špr* schärft (aramäisch *šippār* / *šippārā*; vgl. die ägyptisch-aramäische Dissimilationsform *šnpr*), die Grundbedeutung im D-Stamm erwarten

<sup>40</sup> An eine wilde Taubenart dachten zu *šippōr ḏrōr* schon S. BOCHART und E.F.C. ROSENMÜLLER; vgl. LEWYSOHN, aaO. (Anm. 39) 206.

<sup>41</sup> Formulierung nach KÖHLER, aaO. (Anm. 33) 329.

<sup>42</sup> Vgl. aber auch H. und M. WEIPPERT, aaO. (Anm. 5) 96.

<sup>43</sup> AHW 1065; CAD *šabāru* A: "1. to be voluble, to prattle (said of lips), to flit, move quickly ...".

<sup>44</sup> ATTM 677; WTM IV 212/3 mit dem Derivat *špār*, *špārā* "Pfliff"; DictTalm *špar* I: "to whistle".

<sup>45</sup> A. WAHRMUND, Handwörterbuch der neu-arabischen und deutschen Sprache I 2, 1898, 35; vgl. LANE I 4, 1697: "He, or it, ... whistled ... [It is mostly said of a bird]", ferner DOZY I 835.

<sup>46</sup> Vgl. SCHWAB, Art. "šippōr", ThWAT VI 8–10, 1989, 1102–1107, bes. 1103.

<sup>47</sup> "Das semitische Wort für 'Vogel'", ThZ 5, 1949, 315f. – Vgl. AHW 390; anders LANDSBERGER, MSL VIII 2, 145.

würde<sup>48</sup>. Ist also wegen des Nomens die Annahme einer onomatopoetischen Bildung doch mit Unsicherheiten belastet<sup>49</sup>, so verwundert es vollends in bezug auf die Bedeutung, daß *spr* in I 9 der Tell-Deir-<sup>c</sup>Allā-Inschrift wie in Ps 84,4 und Spr 26,2, wo *šip-pôr* jeweils parallel zu *d'rôr* "Tauben" steht<sup>50</sup>, offenbar ebenso wie *drr* in I 8 eine Speziesbezeichnung ist; dabei bezeichnet *spr* I 9 nach den semitischen Isoglossen und nach LXX am ehesten den "Sperling"<sup>51</sup>. Dagegen ist *šippôr* (II) im Alten Testament sonst Allgemeinbezeichnung für "(kleine) Vögel"<sup>52</sup> oder überhaupt für kleinere flugfähige Lebewesen<sup>53</sup>.

c. Faktisch ungedeutet ist bislang die in der Wendung *b]nj<sup>54</sup> nḥš' wšrh<sup>55</sup> prh<sup>56</sup> nph* I 8 begegnende Speziesbezeichnung *nph*, die offensichtlich der des in Lev 11,19; Dtn 14,18; Tempelrolle 48,1cj. *nāpā* genannten unreinen Vogels entspricht. Die an den auf-

<sup>48</sup> Vgl. C. BROCKELMANN, *VG I*, § 257 Bby, zum Aramäischen S. SEGERT, *Altaramäische Grammatik*, 1975, § 6.6.1.6.6.

<sup>49</sup> SALONEN, *Vögel* (Anm. 18) 7.93f., vermutet in \*sib ar < sib + -ar "ein spätneolithisches Substratwort", das er, anknüpfend an eine vorsichtige Äußerung LANDSBERGERS (MSL VIII 2, 145), mit englisch "sparrow" und deutsch "Sperling" in Zusammenhang bringt.

<sup>50</sup> In Koh 12,4 sind *ḥšippôr* und *kol-b'nôt ḥššr* einander parallel: hier scheint zu *ḥšippôr* an so etwas wie "der Singvogel" gedacht; die Bedeutung stände in der Mitte zwischen dem Oberbegriff "Vogel" und einer Speziesbezeichnung.

<sup>51</sup> Ausführlich zu den Isoglossen SCHWAB, aaO. (Anm. 46) 1103. LXX übersetzt *šippôr* 7 von 37 mal mit στρούθιον "Sperling" (SCHWAB 1104), welche Übersetzung umgekehrt in Spr 26,2 im Unterschied zu *šippôr* = ὄρνεια hebräischem *d'rôr* vorbehalten ist; die anderen Wiedergaben der LXX, πετεινόν, ὄρνειον und ὄρνιθιον, sind keine Speziesbezeichnungen. Auffällig im Sinne der Polysemie von Speziesbezeichnungen ist übrigens, daß ὁ bzw. ἡ στρούθιος einerseits wie das Deminutiv στρούθιον den "Sperling", andererseits allgemein den Vogel (Aischylos Ag 143), auch große Vögel, vor allem den "Strauß", bezeichnet – letzteres nicht nur in Verbindungen wie ἡ μεγάλη σ., σ. κατάγειος oder in dem Kompositum στρούθοκάμηλος, sondern auch als Simplex (Aristophanes Ach 1105; LXX für *bat* [ḥaj]ja<sup>c</sup>nā und ja<sup>c</sup>nā); vgl. O. BAUERNFEIND, Art. στρούθιον, *ThWNT VII*, 1964, 729–732). – Sehr unsicher ist auch die zoologische Identifikation von passer.

<sup>52</sup> Für *spr* I 9 der Tell-Deir-<sup>c</sup>Allā-Inschrift hat B.A. LEVINE, "The Deir 'Alla Plaster Inscriptions", *JAOS* 101, 1981, 195–205, bes. 197: "birds of [ ]" vorgeschlagen; ähnlich V. SASSON, "The Book of Oracular Visions of Balaam from Deir 'Alla", *UF* 17, 1986, 283–306, bes. 288. – Nicht an kleine Vögel ist natürlich bei *šippôr* "Raubvögel" Ez 39,4 zu denken.

<sup>53</sup> In Hb XVIII 296–299 werden auch die Fledermäuse unter die Vögel gerechnet (MSL VIII 2, 143). Auch Lev 11,19; Dtn 14,18; Tempelrolle 48,1cj. erscheint die "Fledermaus" (*šallēp*) am Ende einer Liste von Vögeln, die Lev 11,13 als *šp*, Dtn 14,11 als *šippôr* eingeführt werden (vgl. *šp* Dtn 14,20).

<sup>54</sup> Zur Ergänzung *b]nj* oder *bnj* vgl. A. CAQUOT – A. LEMAIRE, "Les textes araméens de Deir 'Alla", *Syr* 54, 1977, 189–208, bes. 198, HACKETT, aaO. (Anm. 15) 48, u.a.



gezählten Stellen unmittelbar vorher erwähnte ḥ<sup>a</sup>śīdā ("Storch, Reiher") läßt eventuell auch zu ʿānāpā an einen "Reiher" denken<sup>55</sup>; aber auch das bleibt Vermutung, die etwa durch die Versionen nicht gedeckt wird. Für die akkadische Isoglosse *anpatu* hat A. SALONEN – aufgrund einer Gleichsetzung mit sumerisch *giš<sup>(num)</sup>* šīr.MUŠEN als "Flammenvogel" Hh XVIII 337 – die Übersetzung "Flamingo" vorgeschlagen; *anpatu* schien als Unglücksvogel zu gelten<sup>56</sup>. Dagegen führt syrisch ʿanpā nach R. PAYNE SMITH auf "Wiedehopf", wobei zugleich eine ganze Reihe anderer, sehr verschiedener Deutungen notiert wird<sup>57</sup>; C. BROCKELMANN, der sonst zu ʿanpā (auch zu S Lev 11,19; Dtn 14,18) an den "Geier" denkt, gibt zu Elias von Nisibe 43,86 "Chameleon" an<sup>58</sup>. Mag hier auch manches korrekturbedürftig sein – gerade bei Speziesbezeichnungen haben Isoglossen oft einzelsprachlich sehr divergierende Bedeutungen.

3. Das Problem einer Übersetzung altorientalischer Tier- (und Pflanzen-)bezeichnungen und das ihrer Klassifikation innerhalb eines umfassenderen Bedeutungssystems kann natürlich anhand der kurzen Liste von Tiernamen in I 7–9 der Tell-Deir-<sup>c</sup>Allā-Inschrift nur in groben Konturen angedeutet werden. Es kommt hier vor allem auf ein Bewußtmachen der Frage an; weitere Untersuchungen sind ohnehin erforderlich.

### III

Wir kommen zu unserem zweiten methodisch-hermeneutischen Problem: welche Funktion hat die Benennung bestimmter Tiere und ihres bedenklichen Verhaltens innerhalb einer Unheilsankündigung? Inwiefern sind gerade diese Tiere, auch in ihrem Verhältnis zueinander, für die Unheilssituation symbolisch? – Die Tell-Deir-<sup>c</sup>Allā-Inschrift bestätigt hier manches, was auch dem Alten Testament und Texten aus dessen Umwelt als Grundmuster eines altorientalischen Wirklichkeitsverständnisses ablesbar ist.

<sup>55</sup> Vgl. KBL<sup>2</sup>, Ges<sup>18</sup>, J. FELIKS, BHHW, 1578. – Ebenso zu ʿnph I 8 Caquot – LEMAIRE, GARBINI, HACKETT, McCARTER, LEVINE, E. PUECH und SASSON, aaO. (Anm. 52) 288. – Zu dem in HAL verzeichneten griechischen ἀνοπαία bzw. ἀνόπαια vgl. dagegen E. MASSON, *Recherches sur les plus anciens emprunts sémitiques en grec* (Études et commentaires 67), 1967, 99f.

<sup>56</sup> *Vögel* (Anm. 18), 21.110.120.

<sup>57</sup> *Thesaurus Syriacus* I, 1879, 277f.

<sup>58</sup> *LexSyr* 30a.

1. Die Symbolik der Tiere und ihres Verhaltens ist ambivalent.

a. *Einerseits* sind die aus der Tierwelt gegebenen Verhaltensbeispiele insofern bedrohlich, als Niederes sich gegenüber Höherem als überlegen darstellt. Ein kleiner Vogel wie *ss<sup>c</sup>gr*, offenbar Spezies der Gattung Schwalbe, schmäht einen majestätischen Vogel wie *nšr*, den "Adler" oder "Geier" (I 7/8); offenbar ist die Wendung mit ihrer gnomisch verwendeten Affirmativkonjugation<sup>59</sup> sprichworthaft und bezeichnet schon insofern etwas in Unheilssituationen Immer-Wiederkehrendes, für sie Typisches. Das Tierverhalten ist dann aber symbolisch für die Menschenwelt, wenn infolge des Unheils die soziale Hierarchie und damit – für den frühantiken Menschen – ein wesentliches Stück sinnhafter Weltordnung gefährdet scheint. Entsprechend beklagt I 11, daß man über die Weisen lache, womit zugleich ein Mißerfolg der in I 10 und 12 ausgesprochenen seherischen Mahnungen antizipiert sein kann. Auch die Nennung ritueller Funktionsträgerinnen in I 11 scheint paradigmatische Störungen der sozialen Ordnung im Auge zu haben, die eben in den vorher bezeichneten Zuständen der Tierwelt ein symbolisches Analogon finden.

b. *Andererseits* scheint die Aufzählung von Tierarten in I 7ff. der Tell-Deir-<sup>c</sup>Allā-Inschrift solche Gattungen zu wählen, die eine gegenmenschliche Welt repräsentieren: die Tiere stehen für einen Typ innerhalb des Systems von Tierbenennungen, der für eine Chaotisierung der kosmischen Ordnung charakteristisch ist<sup>60</sup>. Vögel wie die in I 7–9 aufgezählten sind offenbar wegen der Rolle gewählt, die sie in einer Tiertypologie spielen: sie nehmen die Stätten ein, von denen die Menschen im Fall einer Katastrophe wie der von Bileams Weissagung angekündigten vertrieben werden, d.h. die sie verlassen müssen, wenn sie göttlichen Gerichten verfallen. Insbesondere *nšr*<sup>61</sup> bzw. *nšrt* und *ṣnph*<sup>62</sup>, vielleicht auch *ss<sup>c</sup>gr* und *jwn* (vgl. zu beidem Jes 38,14) spielen diese Rolle (I 8): sie mögen allgemein als Unheilsboten angesehen worden sein, als "auguries

<sup>59</sup> Vgl. SEGERT, aaO. (Anm. 48) § 6.6.3.2.2.1; zum Hebräischen GKa § 106k; C. BROCKELMANN, *Synt.* § 41k. – Oder liegt ein Partizip vor?

<sup>60</sup> KEEL, aaO. (Anm. 28) 61ff., hat die Repräsentanz ähnlicher, z.T. gleicher Tierspezies für eine "menschliche Gegenwelt" an einigen der in den Gottesreden des Ijobbuches dargestellten Tieren, aber auch an deren Vorkommen in prophetischen Unheilsschilderungen aufgewiesen.

<sup>61</sup> KEEL, aaO. (Anm. 28) 69f.

<sup>62</sup> Zu dem *anpatu* der akkadischen Isoglosse als Unglücksvogel s.o. und SALONEN, *Vögel* (Anm. 18), 120.



portending calamity", wie H. RINGGREN sagt<sup>63</sup>; auch in Mesopotamien galten Vögel – neben der Leber von Opferschafen und neben Schlangen – als Vorzeichenträger<sup>64</sup>. – Charakteristisch für die von den aufgezählten Vögeln repräsentierte Atmosphäre ist die unmittelbar darauf folgende Schilderung: wo früher der Weideplatz von Mutterschafen war, werden nach einer wenig umstrittenen Wendung in I 9 jetzt Steppentiere wie die im Alten Testament als unrein geltenden Hasen hervorgebracht<sup>65</sup>; eine entsprechende Funktion hat wohl auch die Benennung vom *nmi* "Panther(?)" im leider zerstörten Kontext von I 15<sup>66</sup>. Man vergleiche die Erwähnung von Heuschrecken und anderen gefährlichen Tieren, dazu von Ruinenbewohnern wie u.a. Hasen, Eulen<sup>67</sup> und Elstern in dem Fluch, der nach der Sfire-Stele I den Vertragsbrecher treffen soll (*KAI* 222 A 32ff.)<sup>68</sup>; vorher wird unter den Vernichtern des eidgebückten Volkes u.a. der Panther genannt (31). – Nicht erst das bedenkliche Verhalten der aufgezählten Tiere, schon deren Auswahl liefert also Gründe, die die Göttin Šagar zur Abwendung ihres Zorns zu bewegen vermögen. Der Gegensatz zwischen dem Menschen und gewissen Tieren, die etwas Gegenmenschliches, Chaotisches repräsentieren, ist ein Unordnungsmodell, etwas, das den Ordnungswillen des Menschen prinzipiell in Frage stellt, wobei

<sup>63</sup> "Balaam and the Deir 'Alla Inscription", in: A. ROFÉ – Y. ZAKOVITCH (edd.), *Isac Leo Seeligman Volume III: Non-Hebrew Section*, 1983, 93–98, bes. 95f. – Wenn die Übersetzung von phönizisch *ršp šprn* in der Inschrift des Azitawadda als "Ršp der Vögel" *KAI* 26 II 10/11 richtig ist und nicht etwa nach der bildluwischen Fassung an *Ršp* als Hirschgott zu denken ist, so hat *Ršp* möglicherweise als Bringer von Krieg und Krankheit seine Beziehung zu Vögeln. Die "Mühsal" (*āmāl*) bringenden "Kinder des *Rāšāp*" von Ijob 5,7 sind geflügelt gedacht: sie fliegen hoch (*jagbiḥū āp*) wenn Krankheit den Menschen trifft; in Sir 43,17 bezeichnet *ršp* appellativ "(die) Vogel(welt)". Im vorislamischen Heidentum haben Vögel nach J. WELLHAUSEN "zwar allesamt etwas Dämonisches, besonders Rabe, Specht, Wiedehopf und Eule, doch gelten sie nicht ... als Incarnation der Ginn" (*Reste arabischen Heidentums*, 21897 = 1961, 152).

<sup>64</sup> Vgl. VON SODEN, "Leistung und Grenze" (Anm. 20), 71; Ders., *Einführung*, 145–149.

<sup>65</sup> Vgl. zur Unreinheit Lev 11,6; Dtn 14,7.

<sup>66</sup> In Hld 4,8 werden Löwen und Panther in den unwegsamen Bereichen des Hochgebirges vorausgesetzt.

<sup>67</sup> CAQUOT – LEMAIRE, aaO. (Anm. 54), wollten mit Blick auf *KAI* 222 A 33 *šdh* "Eule" auch in I 8 der Tell-Deir-'Alla-Inschrift finden; vgl. A. LEMAIRE, in: *Biblical Archaeology Today. Proceedings of the International Congress of Biblical Archaeology Jerusalem 1984*, 1985, 318; dagegen HACKETT, aaO. (Anm. 15) 48.

<sup>68</sup> Vgl. KEEL, aaO. (Anm. 28) 64 u.ö.

freilich das Analogieverhältnis von Menschen- und Tierwelt, das eins für das andere symbolisch sein läßt, wieder einem Ordnungsmodell entspricht<sup>69</sup>.

2. Welches ist schließlich das Wirklichkeitsverständnis, das für eine solche ambivalente Tiersymbolik innerhalb einer Unheilsankündigung die Voraussetzung ist? Ich setze dabei voraus, was sich bislang an den Texten bewährt hat, daß der grundsätzliche Zugang zur Wirklichkeit – im Gegensatz zu spezifisch-israelitischen Vorstellungen und Begriffen sowie deren heidnischen Opposita – der biblischen Prophetie und einem Text wie dem Bileamorakel aus Tell Deir 'Allā gemeinsam eigen ist. – Für dieses Wirklichkeitsverständnis scheint mir charakteristisch zu sein, daß nach ihm Ordnung und Chaos, Kosmisierung und Dekomposition in der Welt ein schwebendes, labiles Gleichgewicht, ein System einander ausgleichender Beziehungen bilden – mit einer Option für Ordnung und Kosmisierung beim menschlichen Handeln, Sprechen und Denken, insbesondere aber im religiösen Glauben, wobei selbst die seherische oder prophetische Unheilsankündigung ihrer Funktion nach gegen die angekündigte Katastrophe optiert. – An einem labilen Gleichgewicht zwischen Ordnung und Chaos, an einer Dialektik gegenseitiger Aufhebung zwischen Kosmisierung und Dekomposition haben nach dem Wirklichkeitsverständnis des frühantiken Menschen<sup>70</sup> einerseits Tier- und Menschenwelt in gleicher Weise Anteil. Andererseits aber vermag es allein der

<sup>69</sup> Welche Bedingungen erfüllt sein müssen, damit ein Syndrom von Vorstellungen und Begriffen nach frühantikem Verständnis ein befriedigendes System bildet, ist eine noch gar nicht gestellte Frage.

<sup>70</sup> Nach moderner Anschauung sind Kosmisierungen und Chaotisierungen Vorgänge, die sich ständig schon in der anorganischen Welt abspielen und infolgedessen auch die Welt des Lebens beherrschen, wobei allerdings im engeren Bereich des Lebens die Kosmisierung im Sinne der Entropieverminderung eine zunehmende Optimierung erfährt. Einer Selbststrukturierung der Materie in offenen Systemen, zu denen vor allem die lebenden gehören, die zugleich selbstreproduktiv und mutabel sind, scheint universal eine Tendenz zur Dekomposition gegenüberzustehen. Ein "Denkprozeß", wie ihn G.W.F. HEGEL dem Weltprozeß unterstellte, den er damit spiritualisierte, scheint sich in molekularen Organisationen vorabzuzeichnen, ohne daß die Frage nach einer Priorität des Denkens oder seiner materiellen "Basis" sinnvoll wäre: das System einander ausgleichender Beziehungen, das im Austausch offener (dissipativer) mit relativ geschlossenen Systemen den Kosmos durchwaltet, ist subjektiv und objektiv zugleich; es gibt, wenn überhaupt einer Inbrunst, weder der materialistisch-mechanistischen noch der idealistisch-dezisionistischen, sondern allenfalls einer solchen den Anlaß, die den cartesianischen Dualismus von Denken und Natur überwinden hilft.



Mensch, im Dialog mit den Göttern bzw. mit Gott auf sein Schicksal und damit auf den Weltprozeß im Dienst von Ordnung und Kosmisierung einzuwirken: so ermöglicht es eine Unheilsankündigung, die funktionell Umkehrpredigt ist, daß die Umkehr der Bedrohten auf Götter bzw. Gott einwirkt, damit diese in einem Gnadenerweis vom Unheil verschonen<sup>71</sup>; die Vorstellungen und Begriffe, die in solche Unheilsankündigungen eingehen, ihre semantischen Faktoren, die wir in Abschnitt II an einem Teil untersuchten, tragen je auf ihre Weise zur Funktionalität der betreffenden Unheilsankündigung bei.

Eine direkte – magische oder technische – Einwirkung auf die Welt und ihre Bedrohungen dagegen steht weder in der Tell-Deir-<sup>c</sup>Allā-Inschrift noch in der biblischen Unheilsankündigung im Vordergrund der Reflexion. Man mag sich darüber wundern, da doch außer Zweifel steht, daß auch der frühantike Mensch einschließlich Israels magisch und vor allem technisch auf die Wirklichkeit einwirkte; die religiöse Einwirkung auf die Welt stand für das Denken jedenfalls im Vordergrund. Über die Götter bzw. Gott als Mittelinstanz, d.h. im Dialog mit göttlichen Personen, optiert der Mensch für Ordnung; eine Kosmisierung der ständig ins Chaotische abdriftenden Realität, auch der Realität im Menschen, an den sich Unheilsankündigung und Mahnung richten, kann nicht in erster Linie durch direkten Zugriff des Menschen auf die Wirklichkeit erfolgen. Man mag, ja man muß m.E. ein Wirklichkeitsverständnis, das göttliche Personen zwischen den Menschen und seine Welt setzt, ein mythisches nennen; diese Kategorisierung hat zur Folge, daß die Differenz zwischen biblischer und außerbiblischer Religion, wenn beide am Wirklichkeitsverständnis als ihrer unbewußten Voraussetzung gemessen werden, nicht so sehr ins Gewicht fällt. Alttestamentliche und altorientalische Religion sind am Ende verschiedene Realisierungen desselben Organisationssystems, eines menschlichen Handlungskontinuums, das bereits außerhuman, ja außerorganisch präfiguriert sein mag, also am Ende Teil eines gesamt natürlichen Prozesses ist.

<sup>71</sup> So konnten deuteronomistische Redaktionen nicht nur der Prophetenbücher die prophetischen Unheilsankündigungen unter die Bedingung ausbleibender Umkehr stellen und auf Mahnungen zur Umkehr hinauslaufen lassen (2 Kön 17,13; Jer 3,12f.14.22; 4, 1f.; 18,11; 25,5; 35,15; 36,3.7. u.ö., vgl. Sach 1,4).

## RESPONSE TO H.-P. MÜLLER AND M. WEIPPERT

Meindert DIJKSTRA

Ladies and gentlemen<sup>1</sup>

Let me start to express my appreciation, not only for both lectures presented today, but for all the contributions which Prof. MÜLLER, Prof. WEIPPERT and also Mrs. WEIPPERT have written to date concerning the interpretation of the Balaam-text<sup>2</sup>. When I worked my way through all those contributions, their wealth of information in addition to HOFIJZER's extensive commentary made it very clear to me how Old Testament scholarship should be convinced of the crucial importance of our text for the study of the nature of biblical prophecy. Perhaps, it is too harsh to say that our text has been neglected, if not ignored in the last two decades, but I agree, that it certainly did not get the attention it deserved from the outset. It is difficult to find reasons for that cautious attitude. Fact is, indeed, that so many aspects of the Balaam-text seem to be unorthodox. If we only reconsider the struggle – almost the battle – which is still carrying on to assess the text epigraphically and linguistically, we can imagine that it took quite a while for Old Testament scholarship to change their tacks. I agree with Prof. MÜLLER and Prof. WEIPPERT in this respect that apart from an initial restraint after the publication of the *editio princeps*, no Old Testament scholar should be excused for overlooking the Balaam-text five or more years later<sup>3</sup>.

<sup>1</sup> The conversational, somewhat unpolished style of my original responses was not changed. Scholarly discussion and support for my own views were referred to the notes.

<sup>2</sup> Cf. H.-P. MÜLLER, "Einige alttestamentliche Probleme zur aramäischen Inschrift von Der 'Alla'", ZDPV 94 (1978) 56–67; idem, "Der Neu Gefundene Bileam-Text aus Deir 'Alla'", ZDMGSup (1980) 128–130; idem, "Die aramäische Inschrift von Deir 'Alla und die älteren Bileamsprüche" ZAW 94 (1982) 214–244; H. WEIPPERT, "Der Beitrag ausserbiblischer Prophetentexte zum Verständnis der Prosareden des Jeremiabuches", in *Le livre de Jérémie. Le prophète et son milieu, les oracles et leur transmission*, P.-M. BOGAERT (ed.), Louvain 1981, pp. 83–104; H. and M. WEIPPERT, "Die 'Bileam'-Inschrift von Tell Der 'Alla." ZDPV 98 (1982) 77–103; H. WEIPPERT, Palästina in Vorhellenistischer Zeit, in: *Handbuch der Archäologie. Vorderasien II. Bd. II*, München 1988, pp. 625f.

<sup>3</sup> To mention only a few examples. Despite of the extensive description of the



I am glad that I can plead not guilty. I quoted my own reconstruction and translation of the first lines of the first Combination in my Ezekiel commentary, published in 1986 to illustrate Ezekiel's emotional behaviour and symbolic actions as part of his prophetic performance<sup>4</sup>. I assume that it has been one of the reasons why Prof. HOFTIJZER invited me to join this happy family of "Balaamites".

Secondly, I was pleased to discover once more in both lectures of today the continuous effort of lifting the discussion above the mere level of linguistic and epigraphic research to the assessment of the Balaam-text for the study of the Old Testament. Of course, I do not deny the necessity for the determination of its paleographic and linguistic affiliation. Prof. WEIPPERT devoted quite a long and in my opinion illuminating section on the language of the text, but I will skip that subject today, hoping to be able to say something more about it tomorrow. It becomes more and more clear that even if we succeed to piece together all the preserved plaster-fragments and so the text and finish this symposium by taking a vote on the language-issue, interpretational problems will remain. So for instance, the birds passage to which Prof. MÜLLER devoted almost his entire lecture and Prof. WEIPPERT a considerable part of his.

Our text mentions a number of birds. So far so clear, but as soon as we try to define more closely what family, species, what subspecies etc., we discover how arbitrarily, how faulty even many of our identifications still are. The Balaam text, like every newly found text helps us to a better understanding of certain lexical items, like for instance the *ss<sup>c</sup>gr*, but provides us at the same time with the tantalizing problem of a number of new words, of which I mention only

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nether world in Combination II no word about it is found in K. SPRONK, *Beatific Afterlife in Ancient Israel and in the ancient Near East*, (AOAT 219) Neukirchen-Vluyn 1986. L. RUPPERT, article Y<sup>c</sup>Š, *TWAT III* (1982), cols. 719vv does not mention *šh* and *y<sup>c</sup>š/wš* in Combination II.9., which are very important for the understanding of Y<sup>c</sup>Š in Numbers 24:14, a text cited by RUPPERT to support an alleged original meaning "to ask/give an oracle".

<sup>4</sup> Cf. M. DIJKSTRA, *Ezechiël I*, (Tekst en Toelichting), Kampen 1986, pp. 116f. Text and translation were firstly discussed with Dr. G. VAN DER KOOIJ in a letter of January 26th 1985, who drew my attention to the work of André LEMAIRE, P. Kyle McCARTER and Jo Ann HACKETT.

<sup>5</sup> About *smr* see below. The reading *NHŠ* seems to me well established and should not be replaced for *NŠŠ* *pace* A. LEMAIRE, "L'inscription de Balaam trouvée à Deir Alla: épigraphie", in: J. AMITAI (ed.) *Biblical Archaeology Today. Proceedings of the international congress on Biblical Archaeology*, Jerusalem 1985, 313ff; E. PUECH, "Le texte << ammonite >> de Deir 'Alla: Les admonitions de Balaam (première

SMR and NHŠ<sup>5</sup>. Nevertheless, Prof. MÜLLER encourages us to ask further for the literary, cultural and religious horizon of the Balaam-text where its fragmentary state allows such a comparative approach. And Prof. WEIPPERT refers us to the study of such problems as literary and rhetorical criticism and the impact for the history of ancient Near Eastern religion. The mutilated condition of the text, which defies a scholarly accord concerning decipherment and interpretation is of some inconvenience (I like that understatement made by Prof. WEIPPERT), but it is a fact we have to live with.

In general, I agree with Prof. MÜLLER's view that the Balaam-text provides additional proof of the fact that we cannot draw an exact border-line between biblical and extra-biblical prophecy. Of course, some of the texts from Mari and Assur already made us feel uneasy about the classic distinction between prophecy of salvation and doom, accrediting the latter with the mark of authenticity<sup>6</sup>. In the Balaam-text of Deir 'Alla the seer is indeed presented as the "storm petrel" of judgment, a designation so often applied to the Old Testament prophets. I am also convinced that the biblical prophecy of doom from the outset intends to be *Umkehrpredigt*, preaches repentance and return as the late professor Walther ZIMMERLI said concerning Ezekiel's preaching "Gott will töten um zu heilen"<sup>7</sup>. Still, I do not think the controversy concerning the function of prophetic speech implies such an absolute either-or as is formulated by Prof. MÜLLER. Prophets who preach repentance, make intercessions and look for ways to divert judgment, like Amos and Ezekiel did, often also knew about a point of no return (e.g. Amos 7:8; Ez. 9:9; 21:6-7; 22:30 etc.)

The first observation, that Balaam is depicted as a prophet who takes action to divert disaster is probably true. In particular, when Prof. WEIPPERT is correct in his supposition that Balaam's ritual of fasting and mourning intends to be a *Selbstminderungsrite* performed to divert judgment<sup>8</sup>. Whether the words quoted from the mouth of the

partie), in: *La vie de la Parole, de l'Ancien au Nouveau Testament. Etudes d'exégèse et d'herméneutique bibliques offertes à Pierre Grelot* . . . , Paris 1987, p. 22; WEIPPERT, *The Balaam Text* 1989.

<sup>6</sup> Cf. my thesis *Gods Voorstelling. Predikatieve expressie van zelfopenbaring in Oudoosterse teksten en Deuterioesaja*, (Dissertationes Neerlandicae. Series theologica 2), Kampen 1980, pp. 146f, 165f.

<sup>7</sup> Cf. W. ZIMMERLI, *Ezechiel I*, (BKAT XIII/1), Neukirchen-Vluyn 1969, p. 313.

<sup>8</sup> Beside the reference to David's behaviour 2 Sam. 12:15b-23 one could also refer to the inhabitants of Nineveh in Jona 3:6ff.; also Ezra 9:4ff., Joel 1:13ff.



Shadayin are a request to bring about judgment or not – Prof. DAVIES made that perfectly clear yesterday – doom from the side of the goddess Shagar is pending<sup>9</sup>. One may object that the double *figura etymologica* *hšb hšb*<sup>10</sup> is still far remote from a biblical call to repent, it is clear that it has a crucial function in the structure of the text, as Prof. MÜLLER and others have noticed. Even, if we cannot grasp exactly what these words mean, they stand out as a syntactic cluster in the context. In my opinion the prophet ends the quotation of the heavenly speech here and turns himself again to his audience. More about that below.

Whether Balaam diverted judgment or brought it about, is – I am afraid – an unanswerable question and perhaps even irrelevant, because either outcome could create the reason to hand down his story and words. The fact that Balaam's prophecy was publicized on a plastered wall in a sanctuary at Deir Alla, that is, if it was a sanctuary<sup>11</sup> seems to imply that it gained a certain recognition of the people who lived there ca. 800 BC. More on that topic will be said at the end of my response. At this point I would like to continue on the meaning of the bird-list and the character and function of Balaam's divinatory speech, in particular the part that was called by

<sup>9</sup> The interpretation of the words *w'l.thgy. d. l'm* by MÜLLER and WEIPPERT as "aber grolle nich ewig", supports in my opinion rather strongly HOFTIJZER's view that the gods try to intercede and request Shagar to withhold disaster. See WEIPPERT's reference to Jer. 3:5,12, Is. 57:16, Ps. 103:9; Lam. 3:31. Therefore HOFTIJZER's rendering [*'l.thgy. skry. šmyn*] as a balance to the just mentioned sentence is acceptable too. It might be true that the verb *PRR* does not exactly mean "break, destroy" (GREENFIELD), but certainly an interpretation "take apart, dissolve etc." will provide the desired result. The *skry šmyn* could mean the bolts of heaven (cf. in particular CAD(S), 256ff, 258b), but also the lock, the dam (post) or something similar, cf. LEVY, *Wörterbuch III*, 589; JASTROW, *Dictionary*, p. 993a s.v. *su/ikrā* "lock, sluice (in river)". Gilgamesh XI.101 "Erragal tears out the posts (of the world dam)" (PRITCHARD, *ANET*<sup>3</sup>, 94a).

<sup>10</sup> I see no real objection to read it as an imp. with absolute infinitive emphasizing the imp., cf. GESENIUS-KAUTSCH, *Grammar*, 113r. *pace* MÜLLER, ZDPV 94 (1978) 61v: ZAW 94 (1982) 229; WEIPPERT, ZDPV 98 (1982) 99. If Balaam's people (*mh* 1.4) is the supposed audience such a collective could easily be addressed in the plural and occasionally in the singular.

<sup>11</sup> About the nature and extent of the building in which the plaster-text was discovered and which with some hesitation was identified as a sanctuary, cf. FRANKEN, ATDA, pp. 12–13; HOFTIJZER, ATDA, 269, cf. however IBRAHIM/VAN DER KOOIJ, ADAJ 30 (1986) 141, who suggest a religious use for the "bench" room (B/C 5,58) only. See also H. WEIPPERT, Palästina in Vorhellenistischer Zeit, in *Handbuch der Archäologie, Vorderasien II. Bd. II*, München 1988, pp. 625f, who thinks of a "Wahlfahrts-oder Memorialstätte" comparing it to the religious center of Kuntilet Agrud.

Prof. WEIPPERT: Reflections on form and meaning of the Balaam-text. Between a pure linguistic explanation and a comparative evaluation of its religious contents, still a number of stages have to be passed through in text linguistics. For instance, the study of particular stylistic devices and formulas, which structuralize our text and its genre. These are aspects of the Balaam-text, which in my opinion also should have an impact on the study of prophetism in the Old Testament.

Firstly, it is Prof. MÜLLER's taxonomic treatment of the bird-list, which poses a question, not so much on the level of what Prof. MÜLLER called the "kontrastiven Linguistik", which he handles impeccably to my knowledge, but on the level of the textform. Is it indeed a list? A number of scholars, of whom some are present, opted for a certain poetic structure in the birds-passage<sup>12</sup>. Originally, Prof. WEIPPERT also read a list with the exception of the beautiful bicolon in XXV/XXVI: "Denn die Turmschwalbe verhöhnt den Adler, und die Geierjun[gen] den Strausz". This time he has chosen for a poetic division of the text<sup>13</sup>. We all know very well that our text shows gaps, which causes any analysis of its parallelism to be arbitrarily. On the other hand parallelism teaches us something about the meaning of the text even, if we cannot complete or understand all the words used. To ascribe a meaning to a word does not always succeed by discovering a more or less obscure cognate, from an even more obscure lexicographical stock. For example, the translation of the enigmatic words  $\text{t}^*m.w^*[l].smr$ . If, indeed according to the very suggestive parallel in Amos 5:18  $h\check{s}k w'l ngh$  means "darkness and no light", in which the negative part simply emphasizes the first, a stylistic device often found in the Bible, e.g.  $\text{qārāh lō yālādāh}$  (Is. 54:1),  $\text{sōcārāh lō nūhāmāh}$  (Is. 54:11); in prose e.g.  $w'habbōr rēq \text{ } ^2\text{ēn bō māyim}$  (Gen. 37:24), it is feasible that the parallel words  $\text{t}^*m.w^*[l].smr$  expressed a similar idea<sup>15</sup>. It is an

<sup>12</sup> Cf. the treatments of McCARTER, HACKETT, PUECH et al. Also the WEIPPERTS understand  $ky ss^cgr hr/pt.n\check{s}r.wqn.rhmn.y^c nh$  as a bicolon, cf. ZDPV 98 (1982) 93ff.

<sup>13</sup> WEIPPERT accepts the questionable readings  $N\check{s}\check{s}$  instead of  $NH\check{s}$  and  $w\check{s}d^*h$  instead of  $w\check{s}r^*h$ , see note 17.

<sup>14</sup> Cf. also Is. 50:10; Hos. 11:9; Job 12:25.

<sup>15</sup> Cf. WEIPPERT, ZDPV 98 (1982) 93. For this reason SASSON's deliberations concerning  $smr$  "darkness" ("Two Unrecognized terms in the Plaster Texts from Deir 'Alla," PEQ 117 (1985) 102f, idem, "The Book of Oracular Visions of Balaam from Deir 'Alla", UF 17 (1985) 297) cannot be accepted. The evidence



extended colon like the one in Amos 5:18<sup>16</sup>.

The poetical structure of the beginning of Balaam's description has already been pointed out by several students of our text. But there is more. We find some word-pairs like *nšr/rhmn*; *bny NHŠ* // *ʔprhy ʔnph* and in particular, *dr̥r//špr*<sup>17</sup>. I mentioned *nšr/rhmn* as a wordpair, though I am aware of the fact that this pair does not occur somewhere else. MÜLLER's discussion about the problem *nšr* eagle or vulture is somewhat off the mark. Especially, as far as it concerns ancient classification. Of course, an eagle and a vulture are different species, but they belong to the overall family of birds of prey and that is what the word *nšr* actually means. They are specialized species of the same family and have often been confused and are still confused by untrained observers because of similarities in appearance and behaviour<sup>18</sup>. Occasionally, an eagle feeds himself on carrion and a vulture may rob birds nests<sup>19</sup>.

"Nach welchen Regeln sind solche rollenorientierte Typisierungen in eine altorientalische Naturkunde erfolgt?", asks Prof. MÜLLER. Earlier this week Mrs. HACKETT remarked that the description might indicate unusual behaviour, but not necessarily impossible or completely unnatural behaviour. Indeed, a swallow challenging an eagle or a vulture, might occur as an ominous sign in the eye of the observer, but is easily explainable in terms of

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of Arabic *samara* is very confusing due to the fact that two or three homonym roots are involved. The root *samara* used to express a tawny, reddish brown colour, has probably nothing to do with *samara* "to spend the night awake etc.". The well-known Arabic song quoted by SASSON (*UF* 17 (1985) 298 n. 32) implies a pun between these verbal roots. The classical expression *al-samara wal-qamara* refers to the clarity of a star- and moonlighted night, cf. PUECH, *La vie de la Parole*, 22. Here seemingly contrary to a pitch-dark night = *ʕtm*?

<sup>16</sup> Cf. on this type of extended colon M.C.A. KORPEL/J.C. DE MOOR, "Fundamentals of Ugaritic and Hebrew Poetry", *UF* 18 (1986) 173ff.

<sup>17</sup> In the Old Testament they appear twice in the reversed, but seemingly fixed order *špr/dr̥r* (Ps. 84:4; Prov. 26:2).

<sup>18</sup> In this respect the picture of the *nšr(m)//diy(m)* in the legend of Danel (*KTU* 1.17-19) is exemplarily confusing. Translations choose between either vulture (GINSBERG in: PRITCHARD, *ANET*<sup>3</sup>, 152ff) or eagle (*//diym* "kites" DE MOOR, *ARTU*, pp. 245ff etc.), but behavioral descriptions in the tale move between both species; not illogically because both species often appear and act together. An eagle snatches a prey and after eating his fill the vultures, kites etc. will come to finish it off.

<sup>19</sup> Cf. in general P.A.D. HOLLAND, R.F. PORTER, S. CHRISTENSEN, Ian WILLIS, *Birds of the Middle East and North Africa*, Calton 1988, pp. 50ff; K.H. VOUGS/H.J. SLIJPER, *Roofvogels en uilen van Europa*, Leiden 1986, e.g. the description of the golden eagle (p. 97ff.) and the black vulture (p.35).

natural behaviour. For instance, when a swallow shows the unbelievable, but nevertheless instinctive courage to divert a bird of prey much larger than himself from its nest. The same might be true when a brood or group of vultures challenges a giant bird like an ostrich, breaking its eggs with a stone<sup>20</sup>. There is not only internal, but also external parallelism because the fem. verbal form *hrpt* is almost certainly paralleled by the fem. form *nšrt* found between the *dr* and *ywn*<sup>21</sup>. Mrs. HACKETT (and in a way also Mr. McCARTER) translated here ingeniously "The swallow tears at the dove"<sup>22</sup>. I agree with Dr. MÜLLER that the translation "swallow" is hardly maintainable and I wonder whether it is a coincidence to find here the species "dove" distinguished as female *d'rôr* "pigeon" and male *yawn* "cock-pigeon", or not. If a female pigeon tears at a cock-pigeon (as Mrs. HACKETT has it) or even acts as a *nšr*, behaves vulturelike<sup>23</sup> as my ornithological informant says<sup>24</sup>, it is not so difficult to figure out along what lines of ancient folk taxonomy our text thinks. The latter case is even more interesting from ornithological point of view because a male and female dove are almost impossible to distinguish from birdwatcher's view. However, one dove attacking another is explainable in terms of natural behaviour (defense of breeding-place), but it seems that the observer read the situation in terms of distorted marital behaviour.

Without specifying all the details, it is possible to say something more about the description of birds here when using modern ornithological information. The same is true for a more close analysis

<sup>20</sup> Cf. J.C. WOOD, *Bible Animals*, 1869; VOOUS, *Roofvogels*, p. 27. If this observation applies to our text, it supports strongly the identification of *rh*m Hebr. *rāhām/rāhāmāh* Arab. *raḥamu/raḥamtu* etc. with the Egyptian Vulture/Schmützgeier (*Neophron percnopterus*), see also KRONHOLM, *TWAT V*, col. 684.

<sup>21</sup> Cf. also WEIPPERT and already McCARTER, HACKETT, PUECH, (note 22) see further I.11 *w<sup>c</sup>nyh rḡht mr.wknh*, which in my opinion could be completed by fragment V(d) *nzyt ʿt[nh]* "and the priestess sprinkles [her] donkey (?) [with it]".

<sup>22</sup> Cf. HACKETT, *The Balaam Text*, p. 49; McCARTER, *BASOR* 239(1980) 55 who says that the overall pattern of the passage suggests a verbal form. PUECH, in: *La Vie de la Parole*, p. 28: "la colombe dépèce(?) 9)le pigeon".

<sup>23</sup> *nšr* is probably a primitive noun in the West- and South Semitic languages (Akk *našru* only attested in lexical list is probably a loan, cf. WEIPPERT, *ZDPV* 98 (1982) 94 n.84). However, a verb derived from such a noun is possible, compare e.g. *dāʾāh/dayyāh* with *D<sup>3</sup>H* and *ʿayit* with *ʿYT*. A connection between *nšr* and M.Hebr. J.Aram. *NŠR* cannot be excluded.

<sup>24</sup> I owe the majority of these ornithological observations and references to my son Klaas-Douwe DIJKSTRA, who after eight years of living in Egypt knows a great deal about birdlife in the Middle East by his own observations.



of the poetic structure of these and other lines of this visionary description. Following the lead of both lines of information the text reveals some features of ancient taxonomy indeed, describing violations of the natural order according to ancient Near Eastern etiquette as some scholars noticed before, the *adynata* or "Umwertung aller Werte": small against great, female against male etc.<sup>25</sup>

One more question, one step further on the textlinguistic staircase, concerns the literary form or genre of our text. The title of the work, though in itself interesting is not very informative about its genre. We are pretty certain about *spr Bl<sup>m</sup> br B<sup>r</sup>*. Prof. WEIPPERT refers to parallel book titles from the OT and marks the lack of such titles in the Late Bronze period. As yet, I do not know of titles such as *spr PN etc.*, but in some Ugaritic documents we find *spr n<sup>c</sup>m ššwm*; *spr hlmm* and *spr dbh zlm*, in which *spr* certainly indicates something more than an administrative document<sup>26</sup>.

I start my genre-critical remarks from the observation that Balaam's words were not accepted without resistance by his audience. The audience of the prophet is very lively introduced; on purpose, as Dr. WEIPPERT remarks. The narrator needs them to create a decor, a background-sheet which helps Balaam to act according to his assignment, to relate his vision and finally to hammer the message home. The framework of the story and the description of the prophet's vision are larded with calls to look and to listen. A call like "Hear you, deaf from afar", which has an almost literal parallel in Second Isaiah (Is. 43:18), indicates something of the prophets despair to make his folk listen<sup>27</sup>. As I said before, the enigmatic

<sup>25</sup> Such a text division, suggests that between *y<sup>c</sup>nh* and *drr* another verse existed, of which the second colon is complete *wšrh ʔprhy ʔnph*. The rendering *wšdh* is epigraphically impossible (tail of *rēsh* preserved!). *wšrh* is most probably a noun (cf. HOFTIJZER, *ATDA*, p. 307 a.o.). A verb of which *srh* could be the subject is *ʔHZ* (Jer. 49:24) or *HZQ* (Jer. 6:24, 50:43); *h/zqt. ht. jb\*ny. nhs. wšrh. ʔprhy. ʔnph* or *h/lt. ʔhzt. jb\*ny. nhs. wšrh. ʔprhy. ʔnph*, "terror seizes the young of the N<sup>H</sup>S-bird and fear the chickens of the heron". It is difficult to figure out precisely what this verse wants to express. Seemingly a pun on the bird-names *N<sup>H</sup>S* "tormentor (?)", (cf. *L<sup>H</sup>S lōhēs*) and *ʔnph* < *ʔNP* "to rage, fulminate" is intended, i.e. the brood of those birds which usually inspire terror and fear become panic-struck themselves?

<sup>26</sup> Dr. PUECH, *La vie de la Parole*, p. 15 placed fragment III(f) in the upper right corner and completed *ysry spr*. I suggested a similar completion in my *Ezechiel I*, p. 117, but although such a reconstruction is suggestive, it cannot serve yet as an starting-point for a discussion about the form and meaning of the text.

<sup>27</sup> It is also tempting to understand the words *hzw qqn šgr w<sup>c</sup>štr* in a similar way. The word preceding *hzw*, most probably *w\*kl* (cf. VAN DER KOOIJ, *ATDA*,

words *hšb hšb wšb h/[šb]* represent in all probability a stylistic break, indicating the end of the visionary description and the beginning of Balaam's immediate address to his people. The singular is, of course, unexpected, but if the *am*, the clan or family is the audience, it may happen that they are sometimes addressed as a body of people, especially, when a plural vocative is left out<sup>28</sup>.

Now, if the prophet is the speaker in lines 13–16 and his people the audience, one wonders whether this situation continues in the second Combination. I know I am now entering a very slippery area, but even if we cannot understand all words and sentences of the second Combination in their connection, a provisional structural analysis may help us on the way. The classic questions in form-criticism are always: who is the speaker, who is the listener, what is the ambiance. Perhaps twice or three times a question introduced by *lm* "why" is found in the second Combination<sup>29</sup>. In II.16 – just before a new chapter indicated by writing in red ink – we find the warning *rhqf.m/n\*k.šltk.lm.nf* "Keep/remove far [from] you your request, lest we [ ]"<sup>30</sup>. According to the male pronominal suffix *-k* there is clearly someone addressed in II.11ff, be it a group or an individual, but it seems to me out of the question that this

p. 117) or *s\*kl* (HACKETT, *The Balaam Text*, p. 54) is better taken to the lost preceding sentence, which helps us to read *hzw qqn šgr w<sup>c</sup>štr* in the usual syntactic order. I take *qqn* as a real plural from a participle/adjective *qq = qāq* "frightened, dejected". For the godhead as object of *hzh* cf. Balaam's title *hzh šln* and Ex. 24:11, Job 19:26f, Ps 63:3. It is probable that Balaam urges his people to share his vision of Shagar-we-<sup>c</sup>Ashtar.

<sup>28</sup> The transition from plural to singular when addressing a body of people is also often found in Deuteronomy, the Second Isaiah and also the Sfire-treaties. See J.H. HOSPERS, *De Numeruswisseling in het boek Deuteronomium*, Utrecht 1947; G. MINETTE DE TILLESSE, "Sections 'tu' et sections 'vous' dans le Deutéronome," *VT* 12 (1962) 29–87. Though both authors investigated and figured out the literary- and tradition-historical connections between the plural and singular sections in Deuteronomy, they acknowledge that in certain texts the transition is better explained stylistically, see also A. WEISER, *Einleitung in das Alte Testament*, 1949<sup>2</sup>, pp. 101ff; D.J. MCCARTHY, *Treaty and Covenant* (Analecta Biblica 21a), Roma 1978, p. 158 n. 2, 166. For a similar transition in Hittite and Aramaic treaties, cf. K. BALTZER, *Das Bundesformular*, (WMANT 4), Neukirchen-Vluyn 1964<sup>2</sup>, p. 43 n. 1; J.A. FITZMYER, *The Aramaic Inscriptions of Sefire* (Biblica et Orientalia 19), Roma 1967 18f Sf.I.B.31f etc; F. ROSENTHAL in: PRITCHARD, *ANET*<sup>6</sup>, 659ff.

<sup>29</sup> Cf. Combination II.5 and 16 and perhaps also on fragment VIII(h) located in the area between Combination I and II, cf. VAN DER KOOIJ, *ATDA*, p. 154.

<sup>30</sup> *lm* could be introduction of direct speech (HOFTIJZER, *ATDA*, p. 222), interrogative "why" or following a prohibition etc. "lest", cf. 1 Sam. 19:17b, Eccl. 5:5, 7:16f; Neh. 6:3.



"you" is identical with the person addressed in the rhetorical questions of line II.9. The words *hl<sup>c</sup>sh* < *bk* > *lyl<sup>c</sup>š*. *šwlmkh*. *lytmk* "Did not someone consult you for an oracle, or did not someone ask for advice?"<sup>31</sup> have the character of an objection said by or quoted from the mouth of Balaam's audience. If so, it means that something like a dialogue or dispute is going on. I might complete these observations with the suggestion, that in one or two instances the "I" of the prophet appears on the scene again. Just preceding the objection in line II.9 I read *n\*[.ly]* which completed into [*'mr*]/*n\*[.ly]*. "those saying to me . . ." seems to strengthen the idea of a quotation from the mouth of the prophet's audience by the prophet himself. In II.12 I would suggest to read *šhp\*q/r\*.k[l].trdk\**. "I will frustrate all your motives . . ."<sup>32</sup>. May be not all my suggestions will stand a test, but I hope enough will hold to support my view that the first part of Combination II (ll. 1–16) is the continuation of Balaam's dispute started in C.I.12ff. Within the framework of a tale about the prophet, which Prof. WEIPPERT very convincingly compared to the *apophthegmata* we find as subgenres a report of a vision (Visionsbericht) and a prophetic disputation, a combination also found in the story of Micaiah son of Imlah (1 Kings 22). So far my reflections on the form and meaning of the text developed in response to today's lectures.

One question was not discussed in both lectures. The question which should have haunted students of our text from the outset<sup>33</sup>.

<sup>31</sup> The use of *Y<sup>c</sup>š* in the story of Balaam Num 24:14 shows how it comprises the connotation of revealing a future plan, i.e. to provide an oracle, cf. L. RUPPERT, TWAT III, 720vv. (who even thinks it to be the original meaning of the verb, cf. however W. WERNER, *Studien zur alttestamentlichen Vorstellung vom Plan Jahwes*, (BZAW 173), Berlin/New York 1988, pp. 2f). In the book of Isaiah the term *šāh* notwithstanding its wisdom background comes close to revelation of Yahweh's future plan by the prophet, cf. e.g. WILDBERGER, *BKAT X/1*, pp. 188f. In particular his connection of Gods work/action to this future plan is of significance for the relationship between the vision of the *p<sup>c</sup>lt*. *šlhn* and their *šh/mlkh* revealed to Balaam!

<sup>32</sup> H-stem of the verb *PRR*, cf. Hebr. *hēpar*. Retention of the prefixed *hē* in the imperfect like in Old-Aramaic, cf. DEGEN, *Altaramäische Grammatik*, par. 55, p. 66. For this verb in the semantic field of *Y<sup>c</sup>š*, cf. for example Is. 14:27;44:25f.

<sup>33</sup> A question, which according to J.W. WESSELIUS, "Thoughts about Balaam: The Historical Background of the Deir Alla Inscription on Plaster", *BiOr* 44 (1987) 598 should have plagued students of the text from the outset why it was publicized this way. His own solution: governmental propaganda of the kingdom of Judah at the end of the 8th c. BC seems to me ill-founded (questionable reading *lš[mrn]*) and quite farfetched. It falls through in any case, if our text proves to be a hundred years

Why did people in the valley of Sukkoth (Ps.60:6) copy Balaam's story on plastered wall nr. 36?<sup>34</sup> Because I got no direct answer to that question, I would like to connect my final remarks with Prof. WEIPPERT's description of the literary genre of our text. Referring to the story of Amos and Amaziah (Amos 7:10-17), he classifies our text as an *apophthegmon*, a genre described by R. BULTMANN - a narrative told in order to introduce and to hand down to posterity sayings of some important person, a god etc. I consider this to be a very important pointer into the right direction. It is an insight in the function of the text quite different from the usual one. In order to explain the reproduction of Balaam's book on the plastered wall, scholars often refer to Habakkuk's instruction to write the *hāzôn* on the *lūhôt* so that whoever runs along may read it, or whoever reads it may run (Hab. 2:20) and similar texts (Is.8:1ff; 29:18f; 30:8; Jer.36:6f; Ez.24:1f)<sup>35</sup>. LEMAIRE even thought of a copy for educational purposes, like the copy of the torah in Deut.27. In my opinion these references are of little value, because our text as far as readable neither contains such an instruction nor is, as far as I see, the result of such an instruction<sup>36</sup>. I am not convinced that

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older (C14 datings), cf. G. VAN DER KOOIJ "The Identity of Trans-Jordanian Alphabetic Writing in the Iron Age", in: A. HADIDI (ed.), *Studies in the History and Archaeology of Jordan* 3 (1987) 109 and E. PUECH, "Approches paléographiques de l'inscription sur plâtre de Deir 'Alla", in this volume.

<sup>34</sup> Because the text presents itself as a part of the book (*spr*) of Bileam and contains a story, a presentation of the seer, the text on the wall was most certainly copied from a Vorlage. Also the copying mistakes point in that direction.

<sup>35</sup> Of course, it is impossible to discuss here the enigmatic words *lm<sup>c</sup>n yrwš qwr<sup>2</sup> bw*. My first translation is inspired by NBG 1951 and Jerusalem Bible, KBS, but cf. other translations: "so he may run who reads it" (AV, RSV), or even "so that a herald may run with it" (NIV, also NEB), which seems to me quite farfetched. For similar, still unanswered questions, cf. HOFTIJZER, *ATDA*, p. 271. So much is now clear that the inscription was written on the eastern side of wall B/C 5.36, inside the "bench" room, which was covered by reed matting and had a "bench" along the southern, the eastern and maybe the northern wall. The entrance was in the south-west corner, cf. IBRAHIM, VAN DER KOOIJ, *ADAJ* 23 (1979) 49f; 30(1986) 132 fig.1. Room B/C 5.34 where combination I was discovered, had originally nothing to do with the location of the plaster-text.

<sup>36</sup> The second red line in Combination I (I.2) is unfortunately too broken to allow certain conclusions. McCARTER's rendering is suggestive, cf. *BASOR* 239 (1980) 51f, see also PUECH, *La vie de la Parole*, p. 27. I would suggest: *yp<sup>c</sup>l b<sup>1</sup>l<sup>2</sup> h<sup>2</sup>r<sup>2</sup>h<sup>2</sup> š<sup>2</sup> lr<sup>2</sup>jt<sup>2</sup>.mh<sup>2</sup>.š<sup>2</sup>m<sup>2</sup>c<sup>2</sup>t* "Let someone take action without delay to reveal (?) the tidings", but I do not think that such an instruction refers to a record in writing. I read it as a polite instruction to Balaam to take action reporting his vision to his people.

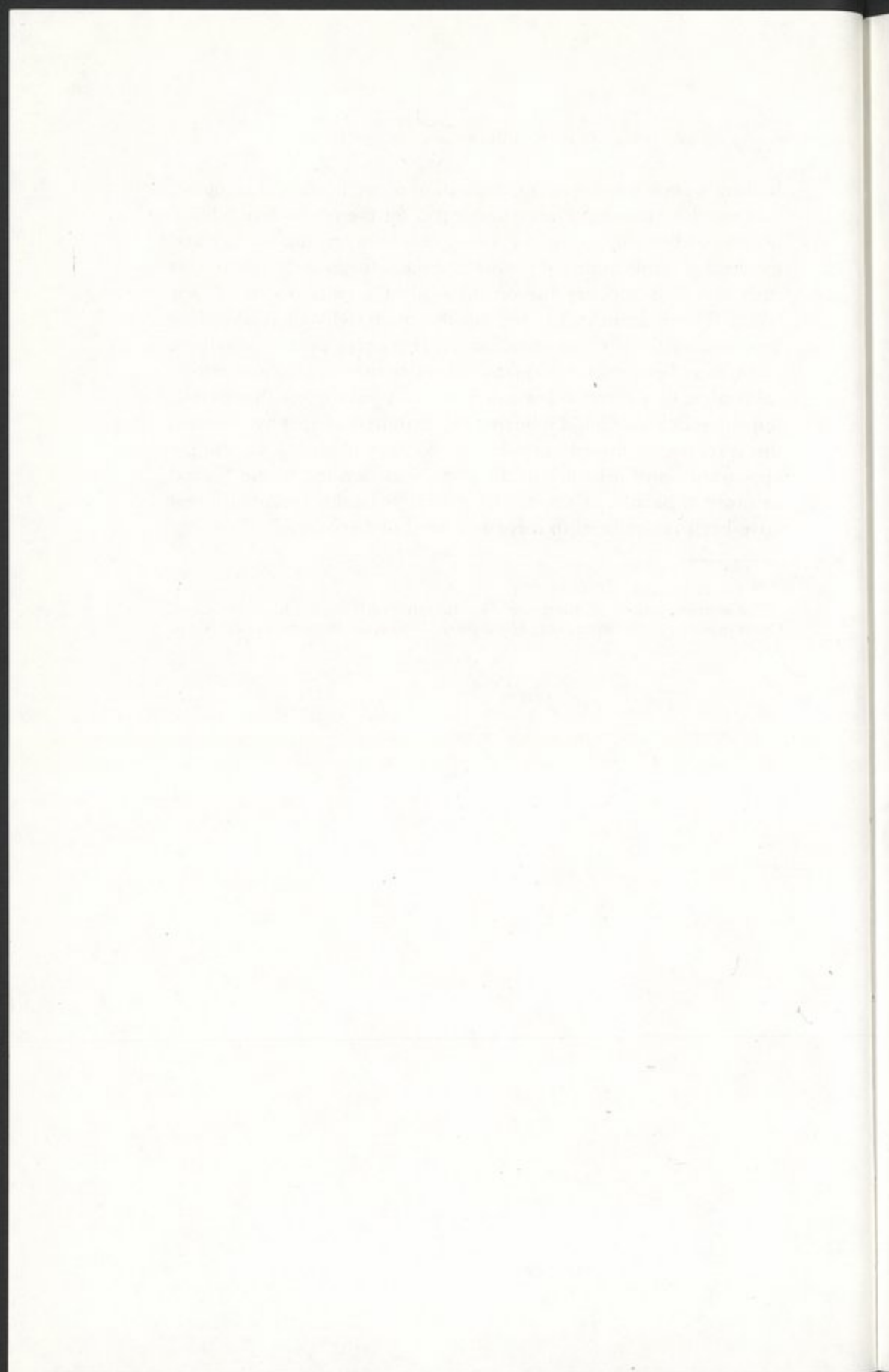


Balaam's book was copied on the wall to prove his case. Of course, I do not deny the importance of our text for the research of biblical prophecy. But should we not distinguish between the literary and theological implications of the *Sepher Balaam* for biblical research as such and its secondary use on the wall of a cultic room in Deir 'Alla? The Balaam in the text on the plastered wall is already a hero of a relatively remote past, as Dr. HOFTIJZER puts it<sup>37</sup>. So there might have been another explanation which matches the archeological context of a very modest cultic room. A reason less theological, but still religious. Could it be that this primitive chapel by means of this text, which proved not only his visionary abilities, but also the appearance and presence of the gods, was devoted to the blessed memory of Balaam as a sort of local saint<sup>38</sup>? In short, could the text have been reproduced to serve as a kind of *hieros logos*?

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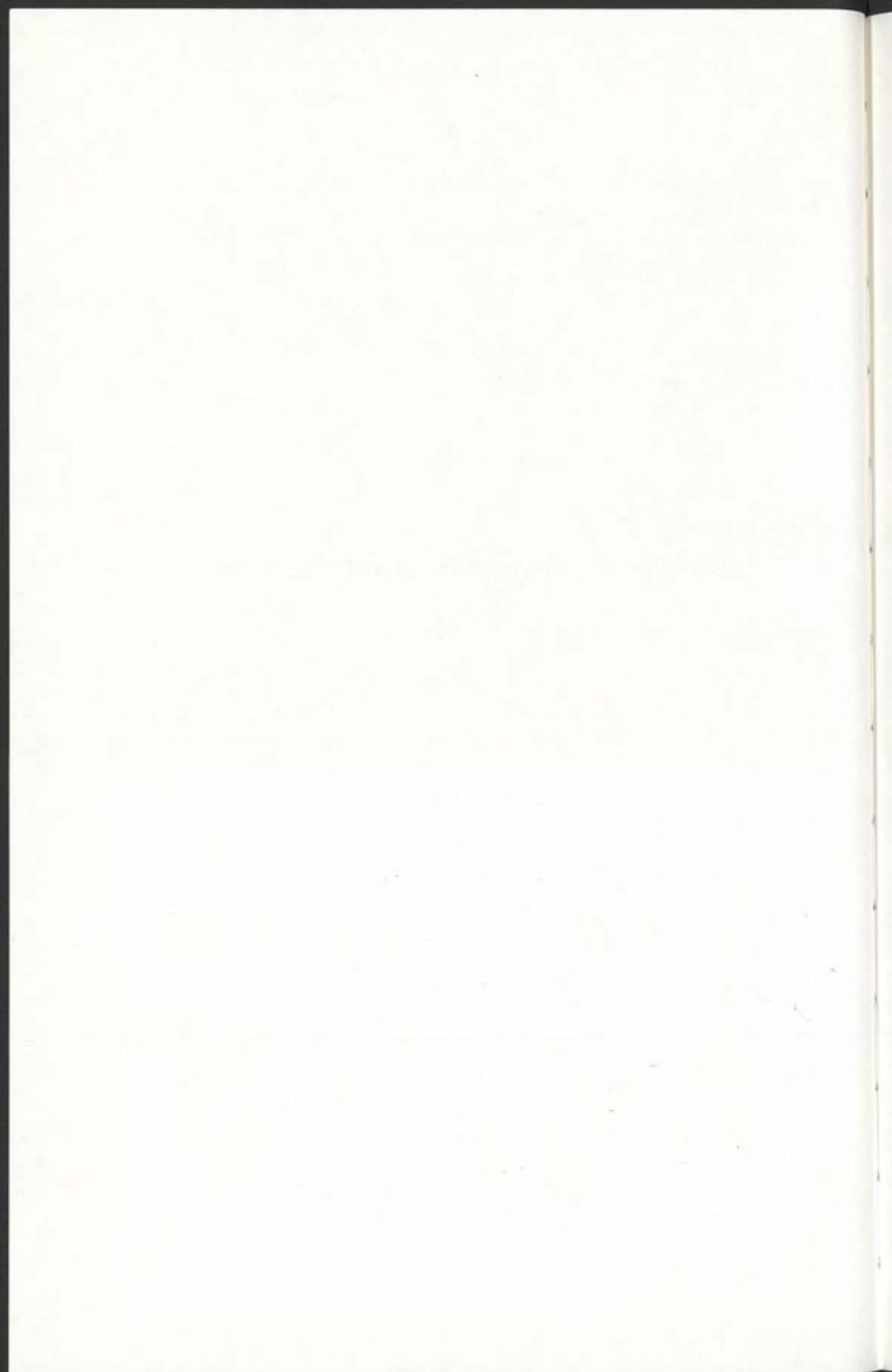
<sup>37</sup> Cf. HOFTIJZER, *ATDA*, p. 271.

<sup>38</sup> Compare centers of pilgrimage like the tomb of Nabi Saleh, or Sheikh el-Qerai in Sinai; cf. H. WEIPPERT, *Handbuch der Archäologie. Vorderasien II. Bd II*, pp. 625f.





PALAEOGRAPHICAL AND RELATED ASPECTS





## APPROCHES PALÉOGRAPHIQUES DE L'INSCRIPTION SUR PLÂTRE DE DEIR 'ALLĀ

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L'étude diligente et précise de l'inscription sur plâtre si fragmentaire et souvent mal lisible de Deir 'Alla fait honneur aux éditeurs.<sup>1</sup> Elle a permis à un grand nombre de chercheurs de se pencher avec le plus grand profit sur ce texte si important et difficile à la fois parce qu'unique. Le personnage de Balaam fils de Beor, déjà connu dans cette région-là par les textes bibliques, ajoute à la complexité des données que les auteurs s'efforcent de débrouiller de leur mieux.

Comme l'a fort bien précisé l'édition, la science paléographique a pour but de déchiffrer les caractères ou ce qu'il en reste mais aussi de situer une écriture dans le temps et l'espace et de lui trouver une filiation.<sup>2</sup> Ce point n'est pas sans importance puisqu'à l'aide des premiers résultats archéologiques, le fouilleur datait l'inscription en écriture araméenne de l'époque perse.<sup>3</sup> Mais une étude plus approfondie et une stratigraphie plus précise par la poursuite des recherches sur le terrain suggèrent de situer dans le VIII<sup>e</sup> s. la strate IX ou M, confirmant la destruction par un tremblement de terre et par le feu.

Dès 1967 il a été proposé par la méthode paléographique de remonter cette écriture sur plâtre vers le milieu du VIII<sup>e</sup> s. ou même un peu avant, conclusion obtenue par l'insertion de ce texte dans la séquence du développement de l'écriture araméenne, première étape de la cursive araméenne, en particulier le *he*, mais la langue peut ne pas être araméenne.<sup>4</sup> D'autres voix se sont élevées pour abaisser la datation au début du VII<sup>e</sup> s. mais dans la tradition ammonite.<sup>5</sup> Dans une étude comparative détaillée, l'édition arrive à la

<sup>1</sup> *Aramaic Texts from Deir 'Alla*, edited by J. HOFTIJZER and G. VAN DER KOOIJ, with Contributions by H.J. FRANKEN, V.R. MEHRA, J. VOSKUIL, J.A. MOSK, Leiden 1976, cité *ATDA*.

<sup>2</sup> G. VAN DER KOOIJ, *ATDA*, 29-170, spéc. 42 ss.

<sup>3</sup> H.J. FRANKEN, *Texts from the Persian Period from Tell Deir 'Alla*, *VT*, 17, 1967, 480 s.

<sup>4</sup> J. NAVEH, *The Date of the Deir 'Alla Inscription in Aramaic Script*, *IEJ*, 17, 1967, 256-58.

<sup>5</sup> F.M. CROSS, *Notes on the Ammonite Inscription from Tell Siran*, *BASOR*,

conclusion suivante: écriture araméenne, ca 700?,  $\pm 25$  ans.<sup>6</sup> Par la suite certains ont proposé d'identifier l'écriture de "galaadite" mais la langue d'araméenne.<sup>7</sup> D'autres estiment que l'écriture araméenne incline *a priori* à retrouver une langue araméenne<sup>8</sup> ou proche de l'araméen.<sup>9</sup> D'autres sont partisans d'un dialecte cananéen local avec des aramaismes.<sup>10</sup> D'autres enfin y verraient même de l'hébreu, ca 700, mais sans prendre en considération la paléographie,<sup>11</sup> du nord-arabique<sup>12</sup> ou une nouvelle langue, le midianite.<sup>13</sup>

Les désaccords, on le voit, ne peuvent être plus grands ni les

212, 1973, 12-15, spéc. 13-14; du même, Ammonite Ostraca from Heshbon. Heshbon Ostraca IV-VIII, *AUSS*, 13, 1975, 1-20, spéc. 11-17; J.A. HACKETT, *The Balaam Texts from Deir 'Alla*, Harvard Semitic Monographs 31, Chico 1980, spéc. 9-19; V. SASSON, *The Book of Oracular Visions of Balaam from Deir 'Alla*, *UF*, 17, 1986, 283-309, 284 s.

<sup>6</sup> *ATDA*, p. 96, mais HOFIJZER hésite davantage à en faire une langue araméenne, voir aussi Aramäische Prophetien, dans *Texte aus der Umwelt des Alten Testaments*, II,1, Gütersloh, 1986, 138-148, p. 139.

<sup>7</sup> P. Kyle McCARTER, *The Balaam Texts from Deir 'Alla: the First Combination*, *BASOR*, 239, 1980, 49-60, p. 50.

<sup>8</sup> A. CAQUOT-A. LEMAIRE, Les textes araméens de Deir 'Alla, *Syria* 54, 1977, 189-208, spéc. 190-192, préféreraient une datation dans la deuxième moitié du 8e s. en suivant NAVEH, *cit.* (1967) et F.M. CROSS (*BASOR* 193, 1973, 13-19, p. 14, n. 2); A. LEMAIRE, L'inscription de Balaam trouvée à Deir 'Alla: épigraphie, dans *Biblical Archaeology Today*, ed. by J. AMITAI, Jerusalem 1985, 313-325, spéc. 315 et 320; du même, Les inscriptions de Deir 'Alla et la littérature araméenne antique, *CRAI* 1985, 270-285, spéc. 272 et 282 s; S. KAUFMAN, Review Article. The Aramaic Texts from Deir 'Alla, *BASOR*, 239, 1980, 71-74, p. 73.

<sup>9</sup> W. Randall GARR, *Dialect Geography of Syria-Palestine, 1000-586 B.C.E.*, Pennsylvania 1985, spéc. 229 ss, et même, avec des nuances, un dialecte araméen: H.P. MÜLLER, Die aramäische Inschrift von Deir 'Alla und die älteren Bileam-sprüche, *ZAW*, 94, 1982, 214-244, spéc. 215-216.

<sup>10</sup> B. LEVINE, The Deir 'Alla Plaster Inscriptions, *JAOS*, 101, 1981, 196-205; du même, The Balaam Inscription from Deir 'Alla: Historical Aspects, dans *Biblical Archaeology Today*, *cit.*, 326-339; J. HACKETT, *cit.*; F.M. CROSS, *cit.* et *Biblical Archaeology Today*, *cit.*, p. 367-369; J.C. GREENFIELD, *JSS*, 25, 1980, 248-52 et dans *Biblical Archaeology Today*, *cit.*, 369 s; E. PUECH, L'inscription sur plâtre de tell Deir 'Alla, *idem*, 354-365; du même, Le texte "ammonite" de Deir 'Alla: Les admonitions de Balaam (première partie), dans *La vie de la Parole, De l'Ancien au Nouveau Testament. Etudes d'exégèse et d'herméneutique bibliques offertes à Pierre Grelot Professeur à l'Institut Catholique de Paris*, Paris 1987, 13-30; du même, L'inscription de Deir 'Alla: Admonitions de Balaam, l'homme qui voit les dieux, *Le Monde de la Bible*, 46, 1986, 36-39 (étude postérieure à la précédente).

<sup>11</sup> J.W. WESSELIUS, Thoughts about Balaam: the Historical Background of the Deir Alla Inscription on Plaster, *BO*, 44, 1987, 589-99.

<sup>12</sup> G. GARBINI, L'iscrizione di Balaam Bar-Beor, *Henoch*, 1, 1979, 166-88.

<sup>13</sup> A. ROFE, *The Book of Balaam ('ivrit)*, Jerusalem, 1979, spéc. 59-70.



opinions plus variées. Bien qu'écriture et langue soient théoriquement deux phénomènes différents, il importe cependant de regarder de près car les indications paléographiques ne peuvent pas ne pas jeter toute leur lumière sur le langage: date et peut-être origine, même si un scribe peut être bi- ou multi-lingue, ou subir l'influence d'une autre école scribale. Trois courtes inscriptions (sur cruche,  $\text{šr}^{\text{c}}$ , sur pierre,  $\text{bn šr}^{\text{c}}$ , et sur bol, alphabet?) provenant de la strate M sont manifestement de langue et/ou écriture araméennes. Mais le rapport linguistique de ces objets mobiles, trouvés à une dizaine de mètres au nord, avec l'inscription sur plâtre sur un support fixe reste posé et non résolu pour autant.

Les derniers résultats archéologiques qui tendraient à dater la strate IX/M de la première moitié du VIII<sup>e</sup> s. ou même avant, *circa* 800  $\pm$  70 ans par les indications de C14<sup>14</sup> et demanderaient donc de remonter quelque peu la datation habituellement proposée pour les textes, favorisent-ils une filiation linguistique? L'édition et un certain nombre d'auteurs ont opté pour une étude paléographique de ce texte dans la famille araméenne, géographiquement proche et la mieux attestée paléographiquement dans les horizons chronologiques entrevus, à l'exclusion de l'aire phénicienne.<sup>15</sup> Ainsi sont-ils à la recherche de formes plus anciennes ou contemporaines les plus proches possibles de celles de l'inscription. A défaut, ont été retenues les écritures lapidaires qui semblent refléter une influence de la cursive. La plupart des modèles se retrouveraient dans l'une ou l'autre inscription araméenne du VIII<sup>e</sup> s., la stèle de Zakkur *ca* 800<sup>16</sup>, mais quelques uns font figure d'innovation, tels *šadé*, *he* et

<sup>14</sup> H.J. FRANKEN, *ATDA*, p. 16, note: *ca* 800  $\pm$  70 ans; M.M. IBRAHIM-G. VAN DER KOOIJ, *Excavations at Tell Deir 'Alla. Season 1982, ADAJ*, 27, 1983, 577-85, p. 581, Phase VI, C 14, *ca* 650 (ou 750 avec correction), les mêmes, *idem*, *Season 1984, ADAJ* 30, 1986, pp. 131-143: Phase M/IX, C 14, *ca* 770-880.

<sup>15</sup> *ATDA*, pp. 72 ss. L'exclusion de la famille phénicienne, p. 73, est sans doute un peu trop rapide. Ce n'est pas parce que la tradition phénicienne est différente dans les textes tardifs qu'il faut négliger l'époque ancienne, à preuve les inscriptions phéniciennes à l'encre de Kuntillet 'Ajrud, fin du 9 s. ou *ca* 800, voir Z. MESHEL, *Kuntillet 'Ajrud. A Religious Centre from the Time of the Judaean Monarchy on the Border of Sinai*, Jerusalem 1978, fig. 9. Sans doute faut-il attendre la publication *princeps* mais les 5 lettres publiées dans *yth yhw* montrent une parenté non négligeable des *b, y, w*, et même *h* avec le 1<sup>er</sup> trait de gauche détaché qui ne peut qu'amorcer l'évolution de la cursive qu'on retrouve en CIS 86 B à Kition (voir PUECH, *RB*, 82, 1975, 446-52). Le *tet y* est encore en forme ovale mais ouverte avec un croisillon.

<sup>16</sup> *ATDA*, pp. 83 s. DA postérieure à Zakkur, Kilamuwa, Sfiré, Panamuwa I mais antérieure à l'ostrakon de Nimrud, Arslan Tash.

probablement *ṣet* dont la forme ovale avec une barre ne se retrouve pas clairement sur ces inscriptions.<sup>17</sup> Mais les nouveaux modèles des *samek* (la hampe ne coupe plus les 3 horizontales), de *qof* (tête ouverte à droite des inscriptions de Panamuwa et Barrakib) et d'abord de *kaf* et *zān* ont déjà commencé à s'imposer avec l'inscription de Zakkur. Malgré les difficultés subsistantes, les paléographes acceptent généralement une parenté, dépendance ou tradition scribale araméenne à partir de la Syrie centrale (Hama), dans la deuxième moitié du VIII<sup>e</sup> s., situant l'emprunt des formes modèles *ca* 725, mais ils doivent postuler un écart de quelques décennies pour permettre des innovations, d'où l'estimation *ca* 700 ± 25 ans.<sup>18</sup> Vendraient corroborer cette conclusion l'influence politique et militaire assyrienne, sous Téglatphalasar III avec la création des provinces dont le Galaad, et l'utilisation de l'araméen comme *lingua franca*.<sup>19</sup>

D'une part, une telle conclusion n'est possible que si on néglige ou refuse d'admettre l'existence d'une tradition scribale locale ancrée dans la région depuis de longs siècles, même si les témoignages épigraphiques font encore défaut. Ceux-ci se multiplient depuis 20 à 30 ans et ils nous paraissent d'ores et déjà suffisants pour postuler l'existence multiséculaire d'écoles de scribes dans la région des *bené Ammon*. Les maigres témoins épigraphiques de l'alphabet linéaire et cunéiforme au II<sup>e</sup> millénaire en Canaan et au Levant suffisent pour donner une idée de la répartition de cet art d'écrire un peu partout dans la région,<sup>20</sup> la rive gauche du Jourdain pouvant difficilement

<sup>17</sup> Les *ṣet* de l'inscription de Barrakib, KAI 216, ne sont pas si clairs. La forme, l.15, semble arrondie avec une barre oblique mais ll.12 et 16, la forme est ovale et des traces des croisillons ne sont pas impossibles, voir aussi les réserves de VAN DER KOOIJ, *ATDA*, p. 92. Pour des *ḥet* à deux barres, on peut en douter dans la même inscription, ll.11 et 14, la reproduction n'y est pas très favorable, voir KAI III, Pl. XXXII, et sur la courte inscription de Barrakib, le *ḥet* a bel et bien 3 barres, voir VON LUSCHAN-SACHAU, *Ausgrabungen in Sendschirli*, I-IV, Berlin 1893, Pl. LX, malgré *ATDA*, pp. 85 et 93. La graphie des stèles de Barrakib est en tout semblable à celle de la stèle de Panamuwa, *id.* Pl. VIII.

<sup>18</sup> *ATDA*, spéc. 94-96. VAN DER KOOIJ ajoute l'innovation du *taw* avec 2 barres horizontales parallèles. Ce schéma, s'il peut exister dans l'inscription en question, n'est pas le plus fréquent ou même habituel. Ce dernier n'a qu'une barre courte mais épaisse, due au trait plein avec un calame fendu. Dans une autre étude, VAN DER KOOIJ date DA *ca* 800 (C 14) sans explication paléographique, voir *The Identity of Transjordanian Alphabetic Writing in the Iron Age*, dans *Studies in the History and Archaeology of Jordan*, III, edit. by A. HADIDI, Amman 1987, 107-121, p. 109.

<sup>19</sup> *ATDA*, p. 96.

<sup>20</sup> Voir par exemple, E. PUECH, L'origine de l'alphabet. Documents en alphabet linéaire et cunéiforme du II<sup>e</sup> millénaire, *RB*, 93, 1986, 161-213.



échapper à ce phénomène culturel. La stèle de Balu'a au sud du wadi Mujib et les tablettes de Deir 'Alla, bien que non déchiffrées, ne devraient pas interdire la formulation de cette hypothèse de travail, ni restreindre l'apparition de l'alphabet dans la région au Ier millénaire. Ainsi, l'art d'écrire en Moab, Edom ou Ammon n'aurait *a priori* rien d'un emprunt direct aux écoles des petits royaumes araméens du nord ou cananéens de l'ouest au Ier millénaire. Mais il faut aussi s'empresse d'ajouter que ces écoles n'étaient pas totalement isolées, que des échanges existaient inévitablement pour des motifs politiques, commerciaux, diplomatiques, culturels ou autres et que des influences pouvaient s'exercer dans les deux sens.

D'autre part, il semble que la logique de la conclusion précédente (dépendance d'école scribale araméenne) ait réagi fortement sur les conclusions historico-politiques et entraîné une datation paléographique de l'inscription de Deir 'Alla à l'époque de la domination assyrienne, la période de la domination syrienne de Galaad sous Hazaël s'avérant par trop haute.

Ces remarques générales devraient permettre d'apprécier différemment les idiosyncrasies dans l'art d'écrire fixées sur la pierre de la Citadelle d'Amman très certainement antérieure: a-t-on affaire à des formes secondaires de modèles araméens<sup>21</sup> ou à des modèles de l'écriture locale, donc 'nationale, ammonite'? Comme des formes particulières de lettres se retrouvent sur plusieurs siècles dans les écrits mis au jour dans la région et qu'elles ne peuvent être le fait d'un seul scribe, ne faut-il pas postuler l'existence d'une "école" scribale ammonite<sup>22</sup> dont les premiers essais font encore défaut, avant de chercher une filiation plus ou moins

<sup>21</sup> Comme l'affirment J. NAVEH, *The Development of Aramaic Script*, Jerusalem, 1970, spéc. 66 s, ou *Early History of the Alphabet*, Jerusalem 1982, 107-111; G. VAN DER KOOIJ, *cit.* note 18, pp. 109-115, et même F.M. CROSS, *AUSS*, 13, 1975, 10-11, qui considère l'existence d'une tradition scribale ammonite, mais attribue l'inscription de la Citadelle à l'écriture araméenne.

<sup>22</sup> Comme je l'ai proposé dans *Biblical Archaeology Today*, *cit.*, et dans L'inscription de la statue d'Amman et la paléographie ammonite, *RB*, 92, 1985, 5-24. NAVEH, *Early History* . . . , *cit.*, p. 109 s, ne considère pour la première moitié du Ier millénaire que trois écritures nationales alphabétiques: phénicienne, araméenne et hébraïque, car "nationale ne s'applique (selon lui) qu'aux traditions qui se sont développées indépendamment sans aucune influence étrangère significative". Mais c'est oublier que même l'écriture hébraïque tout comme la langue n'est pas aussi unifiée qu'on veut le dire d'une part, et que d'autre part, l'une comme l'autre sont un héritage du IIe millénaire au nord, en Israël, comme en Juda au sud, avec leurs développements propres. En Transjordanie, la différenciation doit remonter aussi au IIe millénaire.

lointaine et hypothétique, et de toute façon bancale et donc non satisfaisante? Il n'y a, semble-t-il, aucun critère décisif qui demande de rattacher l'inscription de la Citadelle à la famille araméenne de la Syrie méridionale ou centrale.

Cela dit, il importe de passer en revue les inscriptions du pays des *Bené Ammon* qui ont un air de parenté afin de leur comparer ensuite l'inscription de Deir 'Alla. Les unes relèvent de l'écriture lapidaire, les autres de la cursive.

La plus ancienne inscription lapidaire retrouvée est actuellement celle de la Citadelle d'Amman que l'on peut dater de la fin du IX<sup>e</sup> s. d'après les données à présent connues<sup>23</sup> (voir figure 1). La seule inscription araméenne qui se rapprocherait de celle-ci serait l'ivoire de Hazaël trouvé à Arslan Tash, mais manquent les lettres les plus caractéristiques d'une part, et d'autre part, les *taw* ont tous une longue hampe et, en général, les hampes des lettres présentent un angle différent.<sup>24</sup> Aussi l'inscription de la Citadelle n'a-t-elle rien de typiquement araméen surtout pas les formes du *ʔet* ovale avec une seule barre à droite et du *ʕade* à longue hampe munie d'un 'v' couché en haut à droite, formes inexistantes en araméen,<sup>25</sup> ni celle de *ʕaïn* ouvert à une date aussi haute. Les *taw* sont plus proches de ceux de la stèle de Mésha de Moab, un X plus ou moins réguliers, que des inscriptions araméennes avec le trait à gauche nettement allongé.

<sup>23</sup> Je renvoie à ma note, *RB*, 1985, p. 10 et n. 24 pour les références où je signale mon dernier dessin après examen de la pierre paru dans *The Interpreter's Dictionary of the Bible, Supplementary Volume*, Nashville 1976, p. 433, *pace* VAN DER KOOIJ, 1987, p. 115. Le dessin de VAN DER KOOIJ, *cit* 1987, p. 111, n'est pas exempt d'inexactitudes: 2e *taw*, l. 6, le trait oblique de droite à gauche est tracé en deux fois, non dans le prolongement, le *ʕaïn*, l. 2, n'est pas aussi arrondi, etc.

<sup>24</sup> E. PUECH, L'ivoire inscrit d'Arslan Tash et les rois de Damas, *RB*, 88, 1981, 544-562.

<sup>25</sup> Les *ʔet* de Barrakib ne sont pas clairs comme on l'a signalé plus haut, n. 17. Un seul serait de forme ronde avec une oblique à droite, mais la stèle est de la fin du 8<sup>e</sup> s. On peut rapprocher le *ʔet* d'un poids de Hama, voir P. BORDREUIL, BAALIM, *Syria*, 62, 1985, pp. 174s, ovale avec une barre horizontale et un autre considéré comme probablement sidonien par les éditeurs, mais peut-être araméen par l'écriture, A. LEMAIRE-F. BRON, Poids inscrits phénico-araméens du VIII<sup>e</sup> siècle av. J.-C., *Atti del I congresso internazionale di studi fenici e punici*, Rome 1983, 763-770, 765s, mais le poids de Sidon, p. 765, n° 2, porte un *ʔet* avec le croisillon. Tous ces poids sont de la fin du 8<sup>e</sup> s. Le plat de bronze de Nimrud *ʔbpt* est d'origine inconnue, R.D. BARNETT, Layard's Nimrud Bronzes and their Inscriptions, *Erls*, 8, 1967, 1\*-7\*, n° 19, *ʔet* ovale avec une oblique à droite. BARNETT émet comme hypothèse une origine de Hama. Des preuves solides font défaut même pour une origine araméenne, un d'entre eux porte un nom israélite, etc. Les *ʔet* de Neirab (7<sup>e</sup> s.), de l'ostracon d'Assour (7<sup>e</sup> s.) sont ronds avec une barre à gauche. Pour des sceaux araméens avec *ʔet* à une seule barre, voir HERR, *cit.*, n°s 48 et 79 (fin du 8<sup>e</sup> s.).



Les yeux des têtes à double face portent gravées au dos des lettres de l'alphabet ammonite qui n'ont rien de typiquement araméen, surtout pas le *samek* inconnu, etc.<sup>26</sup>

La statue inscrite d'Amman porte sur le piédestal une inscription de deux lignes que certains rangent parmi l'écriture araméenne.<sup>27</sup> Mais le *het* à deux barres n'est pas typiquement araméen, voir Siran, etc *infra*, et le *ʿaïn* n'est pas un rond parfait, même si l'angle supérieur droit a été recoupé. Le *bet* ouvert suppose une date vers le milieu du 7e s.

Le tessou inscrit de la Citadelle d'Amman se lit au mieux *lʿbdʿ*, la haste du *dalet* ne coupe pas le pied du *bet*, de préférence à *lʿbrʿ*.<sup>28</sup> Le *ʿaïn* légèrement ovale ne semble pas fermé. Datation *ca* 700.

L'ostracon incisé de Heshbon, *Jtnʿl.f.*, se situerait aussi parmi les écritures ammonites, *ca* 700 ou peu après.<sup>29</sup>

Le fragment inscrit du théâtre d'Amman avec ses *ʿaïn* largement ouverts dont l'un assez étalé, les *bet* ouverts, n'est pas d'écriture spécifiquement araméenne, d'autant qu'en araméen ces formes ne côtoient jamais le *mem* à tête horizontale en forme de "w". Il est plus logique par le lieu de la découverte et la forme de ces lettres bien connues en ammonite jusqu'au 6e s. de le classer dans l'écriture ammonite, ce que le contenu vient appuyer.<sup>30</sup> Datation probable dans la deuxième moitié du 7e s.

Les sceaux ammonites, soit trouvés dans la région, soit attribués

<sup>26</sup> Voir PUECH, *RB* 1985, p. 11 avec références, VAN DER KOOIJ, 1987, p. 111, admet cette particularité.

<sup>27</sup> Voir VAN DER KOOIJ, 1987, p. 111. A. LEMAIRE, Notes d'épigraphie nord-ouest sémitique, *Syria*, 61, 1984, pp. 251-56, 251-54, qualifie l'inscription d'araméenne spécialement par la lecture *dmw*, mais très peu vraisemblable paléographiquement et linguistiquement, *waw* très douteux et *lamed* bien préférable; *dmw* ferait au moins *dmwt* au cas construit, on ne peut invoquer le mandéen ou le syriaque, dialectes orientaux tardifs pour une époque ancienne en Ammon! On préfère toujours notre proposition *sml NP* et *šnp* (de même, VAN DER KOOIJ) à *šnb* de LEMAIRE. Le mot *br* n'est pas suffisant pour une qualification d'écriture araméenne, il est bien connu même des inscriptions phéniciennes (Kilamu-wa ...).

<sup>28</sup> Voir PUECH, 1985, p. 11 (n. 26, corriger p. 26 et 33). L'éditeur a lu *lʿbrʿ*, plus difficile; il faudrait alors supposer *lʿbrʿyʿ*! VAN DER KOOIJ, 1987, p. 111, n. 22, suit l'éditeur.

<sup>29</sup> PUECH, 1985, p. 11.

<sup>30</sup> Voir E. PUECH, 1985, pp. 11 s (avec les références), malgré VAN DER KOOIJ 1987, p. 111. La 1e lettre de la deuxième ligne est sûrement *bet*: lire 1) *ʾbʿl.ʿbndʿb* 2) *ʾbn ʿmʿn(db)*. L'onomastique est des plus connues en ammonite.





à ce corpus par l'onomastique et/ou la paléographie et l'iconographie forment maintenant un ensemble assez conséquent mais difficilement datable vu leur courte inscription. La grande majorité d'entre eux doit dater de la fin du 8e s. au début du 6e s. On peut cependant utiliser avec une certaine précision ceux qui portent la mention d'un roi, *lbyd<sup>l</sup> <sup>c</sup>bd <sup>pd</sup>l<sup>31</sup>* ca 700 ou début du 7e s., *l<sup>pdnr</sup> <sup>c</sup>bd <sup>c</sup>mndb*, *l<sup>dnpl</sup> <sup>c</sup>bd <sup>c</sup>mndb* deuxième quart ou milieu du 7e s.<sup>32</sup> et la bulle de *mlkm<sup>wr</sup> <sup>c</sup>bd b<sup>c</sup>lys<sup>c</sup>*, début du 6e s.<sup>33</sup> Pour ne pas dépasser les limites raisonnables de cette communication, on ne retient ici que certaines lettres caractéristiques de la paléographie sigillaire ammonite. Dans tous les exemples contrôlés, le *ʔet* est toujours un ovale, le plus souvent en position verticale et fermée avec une seule barre médiane.<sup>34</sup> L'inclinaison de celle-ci importe moins vu l'écrit-

<sup>31</sup> F.M. CROSS, *Leaves from an Epigraphist's Notebook*, CBQ, 36, 1974, 486-94, A Forgotten Seal, pp. 493 s, vers 700.

<sup>32</sup> G. LANKASTER HARDING, *Four Tomb Groups from Jordan*, *Palestine Exploration Fund Annual*, VI, 1953, pp. 51 ss, Pl. VI,1; Ch. C. TORREY, *A Few Ancient Seals*, *The Annual of the American Schools of Or. Res. in Jerusalem*, II-III, 1923, 103-108, 103s.

<sup>33</sup> L. GERATY et alii, *Madaba Plain Project: A Preliminary Report of the 1984 Season at Tell el-Umeiri and Vicinity*, *BASOR Suppl.* n°24, 1986, 117-144, p.135s, 138.

<sup>34</sup> Voir *l<sup>dnpl</sup>*, cité n. 32; L.G. HERR, *The Scripts of Ancient Northwest Semitic Seals*, HSM 18, Missoula 1978, fig. 43 (n°s 17,19,24,31); A. LEMAIRE, *Syria*, 63, 1986, p. 317s, mais lire le patronyme comme hypocoristique *ʔdny* (*yod* inversé, à l'endroit sur le sceau, corrigé en *ʔdn<sup>2</sup>*, *alef* avec une tête plus arrondie, voir le n°13, p. 321 ...), au lieu de *ʔdnr*, fautif, de l'éditeur; P. BORDREUIL-A. LEMAIRE, *Semitica*, 26, 1976, pp. 59-60 et 63; K. YASSINE et P. BORDREUIL, *Two West Semitic Inscribed Stamp Seals*, dans *Tell el Mazar I, Cemetery A*, Amman 1984, 132-134, n°183. Le sceau d'Umm Udheina près d'Amman porte un *ʔet* à une barre, de type ammonite mais d'autres lettres ont des caractéristiques moabites, M. ABU TALEB, *The Seal of p<sup>lty</sup> bn m<sup>2</sup>s the mazkir*, ZDPV, 101, 1985, 21-29, Pl. 1,B-C.

Fig. 1. La lapidaire ammonite (p. 228).

- Cit. : inscription de la Citadelle d'Amman, fin du 9e s.  
 Yx : yeux gravés de têtes à double face, début du 7e s.  
 O.A. : Ostracon d'Amman, circa 700.  
 H.v : Ostracon v de Heshbon, ca 700.  
 Sc. : Sceau de *byd<sup>l</sup> <sup>c</sup>bd <sup>pd</sup>l*, début du 7e s.  
 St. : Inscription de la statue d'Amman, Ière moitié du 7e s.  
 Sc. : Sceaux de *l<sup>dnnr</sup>* et *l<sup>dnpl</sup> <sup>c</sup>bd <sup>c</sup>mndb*, ca 650.  
 Sceaux : sceaux ammonites de fin du 8e s. au début du 6e s.  
 Th. : Inscription du théâtre d'Amman, dernier quart du 7e s.  
 Sir. : Inscription de la bouteille de Tell Siran, fin du 7e s.  
 Um. : cachet de Tell <sup>c</sup>Umeiri, début du 6e s.

ture en négatif de la matrice. Le *šade* très spécifique de l'inscription de la Citadelle semble se retrouver sur un sceau dont on ne possède qu'un dessin au trait, récemment identifié comme ammonite.<sup>35</sup> Les autres exemples sans doute plus tardifs rappellent ceux de la bouteille de Tell Siran, tout comme d'ailleurs le *kaf* à tête triangulaire.<sup>36</sup> Le *qof* si caractéristique de Siran se retrouve sur des sceaux,<sup>37</sup> de même la forme du *ʿaïn* y est carrée,<sup>38</sup> anguleuse ou ouverte. Les *samek* des sceaux ammonites diffèrent des formes araméennes, excepté ceux des sceaux tardifs.<sup>39</sup> Le *he* des sceaux ne possède que 2 des 3 barres.<sup>40</sup> Les *het* ont deux ou une barres.<sup>41</sup>

L'inscription de la bouteille en bronze de Siran est des plus importantes tant par le contenu (généalogies royales) que par la paléographie. Qu'on accepte l'existence de 3 ou 2 Amminadab sur le trône ammonite au 7<sup>e</sup> s. affecte peu la datation de cette inscription, deuxième moitié du 7<sup>e</sup> s., vers la fin?<sup>42</sup>

Parmi les écritures cursives ammonites, outre celle du plâtre de Deir ʿAlla dont l'attribution est à préciser, on doit ranger l'ostra-

<sup>35</sup> A. LEMAIRE, *Syria* 62, 1985, pp. 44s, f), voir E.J. PITCHER, A Moabite Seal, *PEF*, 1915, 42 (mais assez étrangement cette lettre la plus caractéristique n'a pas été invoquée par LEMAIRE).

<sup>36</sup> Voir HERR, *cit.*, n°s 36, (5), et S. ABBADI, Ein neues ammonitisches Siegel, *ZDPV*, 95, 1979, 36 ss, proviendrait de Deir ʿAlla; *kaf* à tête triangulaire.

<sup>37</sup> *Syria* 63, 1986, p. 319, et pour une étape antérieure, HERR, *cit.*, n°s 25 et 18.

<sup>38</sup> Les dessins de HERR, *cit.*, ne sont pas toujours fidèles.

<sup>39</sup> Voir HERR, *cit.*, n° 22, la haste ne recoupe que 2 des 3 horizontales, n°s 11, 33, les 3 horizontales sont reliées en zigzag, les n°s 13, 14 sont plus proches des formes araméennes de la tablette d'Assur, de même ceux des sceaux de Tell el-Mazar, YASSINE-BORDREUIL, *cit.*, n° 185.

<sup>40</sup> Voir HERR, *cit.*, n°s 12, 14 et 36. *Semitica*, 29, 1979, p. 83; 32, 1982, p. 33s, n° 16 (ammonite). Sur un sceau de Tell el-Mazar, YASSINE-BORDREUIL, *cit.*, n° 184, lire sûrement *lhml rdh* ou *rdy*, hypocoristique, Rad(d)ah ou Radday, voir 1 Ch. 2, 14, au lieu des hypothétiques *šlt* ou *ddy/rry* des éditeurs. Le *he* aurait soit 2 parallèles, soit un trait rattaché à l'oblique de gauche, comme en araméen et sans doute déjà aussi en ammonite.

<sup>41</sup> Le scaraboïde de Deir ʿAlla, n° 2550, a été lu dernièrement *lmtnʿl bn ʿwrʿ*, LEMAIRE, *Syria*, 61, 1984, 255s au lieu de *lhnnʿl* proposé précédemment. On doute sérieusement de l'une et l'autre lecture, mais le caractère ammonite paraît incontestable.

<sup>42</sup> Par exemple, F.M. CROSS, Notes on the Ammonite Inscription from Tell Siran, *BASOR*, 212, 1973, 12-15, ca 600 (3 Amminadab), ou J. NAVEH, *Early History...*, *cit.*, pp. 110s (2 Amminadab, le petit-fils du roi régnant en 667). Une datation ca 620-600 paraît tout à fait acceptable dans l'un et l'autre cas.



con de Nimrud.<sup>43</sup> Celui-ci portant peut-être deux mains différentes à l'avant et au revers se situe entre la campagne de Sennachérub et la destruction du palais en 612, probablement dans la première moitié ou le milieu du 7<sup>e</sup> s. (voir figure 2). Noter le trait inférieur de la tête du *kaf* en forme de "v", avers 1.3 et revers 1.2.

Vient ensuite l'ostrakon de Heshbon IV (= A(mmonite)1), daté par l'éditeur *ca* 600.<sup>44</sup> Peu distants dans le temps, les ostraca Heshbon XI (= A 2), probablement de la 1<sup>re</sup> moitié du 6<sup>e</sup> s.<sup>45</sup> et Heshbon XII (= A 3), *ca* 550 ou peu après<sup>46</sup> sont à présent les derniers témoignages de la cursive ammonite. H XII est déjà marqué par des tendances aramaïsantes, mais les *z/h/q* en particulier ont encore conservé les caractéristiques ammonites. Les ostraca H II (= A 4) et H I (= A 5) de la fin du 6<sup>e</sup> s. ou *ca* 500 sont en écriture araméenne même si pour l'un d'eux (A 5) la langue est ammonite.<sup>47</sup> Sont aussi à classer dans la cursive ammonite quelques ostraca trouvés à Tell el-Mazar, l'ostrakon n°3 dans la 1<sup>re</sup> moitié du 6<sup>e</sup> s. et probablement l'ostrakon 5.<sup>48</sup>

Parmi les ostraca mis au jour à Deir 'Alla, l'ensemble appartient d'après les indications fournies aux époques perse et hellénistique.<sup>49</sup>

<sup>43</sup> J.B. SEGAL, *Iraq*, 19, 1957, 139-145. Pour d'autres références, voir PUECH, 1985, pp. 12s, malgré B. BECKING, Kann das Ostrakon ND 6231 von Nimrud für ammonitisch gehalten werden?, *ZDPV*, 104, 1988, 59-67.

<sup>44</sup> F.M. CROSS, Ammonite Ostraca from Heshbon. Heshbon Ostraca IV-VIII, *AUSS*, 13, 1975, 1-20; PUECH, 1985, p. 13.

<sup>45</sup> F.M. CROSS, Heshbon Ostrakon XI, *AUSS*, 14, 1976, 145-148, le situerait *ca* 575. En *RB* 1985, p. 14, j'ai proposé de le rajeunir quelque peu mais comparé à H.XII, l'éditeur a sans doute raison dans la datation.

<sup>46</sup> F.M. CROSS, An Unpublished Ammonite Ostrakon from Hesbān, dans *The Archaeology of Jordan and Other Studies Presented to Siegfried H. Horn*, ed. by L.I. GERATY-L.G. HERR, Berrien Springs 1986, 475-489. Ligne 7, on pourrait préférer une lecture *ʿ/qrš* à *ʿ/psr* de l'éditeur mais la reproduction n'est pas suffisante pour se faire une opinion plus arrêtée.

<sup>47</sup> Références en PUECH 1985, pp. 14-20, mais le *ʿaīn* carré de A 5 rappelle un trait ammonite. F.M. CROSS, cité n.46, p. 484, est d'avis que la cursive araméenne de chancellerie s'est imposée *ca* 500. Dans le même sens P. BORDREUIL, Perspectives nouvelles de l'épigraphie sigillaire ammonite et moabite, dans *Studies in the History and Archaeology of Jordan*, III, Amman 1987, 283-86, p. 284, estime qu'on peut considérer l'aramaïsation de l'écriture d'abord, puis du dialecte ammonites comme réalisée vers 500.

<sup>48</sup> K. YASSINE-J. TEIXIDOR, Ammonite and Aramaic Inscriptions from Tell el-Mazar in Jordan, *BASOR*, 264, 1986, 45-50. Les reproductions ne permettent pas une étude paléographique précise d'autant que la lecture pose des problèmes.

<sup>49</sup> H.J. FRANKEN-M.M. IBRAHIM, Two Seasons of Excavations at Tell Deir





Ayant regroupé le maximum de données écrites de la région, classées autant que possible chronologiquement selon des critères internes ou à défaut selon l'évolution des caractères, il importe alors de reconsidérer l'écriture de D.A. pour une authentification plus exacte. Un simple tableau synoptique montre que toutes les formes caractéristiques des lettres trouvent leur place normale et attendue dans l'évolution de l'écriture régionale ammonite (voir figures 1 et 2).

L'écriture lapidaire n'évoluant que sous l'influence de l'écriture (semi) cursive, principalement à l'encre, les innovations se produisent d'abord dans la (semi) cursive avant de se traduire dans l'écriture lapidaire. Ainsi, les particularités affichées dans l'inscription de la Citadelle, particulièrement *he* (angle des traits), *tet* (ovale à une barre à droite),<sup>50</sup> *kaf* (première exemple de trait inférieur à gauche), *šade* (à longue hampe et 'v' couché à droite)<sup>51</sup>, mais aussi *waw*, (ouverture de la tête à gauche), *samek* (angle des barres horizontales avec la hampe),<sup>52</sup> *ain* (plutôt ovale et ouvert),<sup>53</sup> *taw* (début d'allongement du trait à gauche)<sup>54</sup> supposent une écriture (semi)

<sup>50</sup> Alla, 1976-1978, *ADAJ*, 22, 1978, 57-80, p. 79. Pour l'ostracon n°2755, *ADAJ*, 27, 1983, p. 581, Pl. 128,2, niveau VI, une bonne reproduction est nécessaire pour décider de l'écriture. L'ostracon n°2768, niveau V, est aussi qualifié d'araméen par les fouilleurs, *idem*.

<sup>51</sup> Malgré VAN DER KOOIJ, 1987, p. 109. Cette forme n'est pas spécialement connue ailleurs à haute époque dans les écritures araméennes du Levant (pas avant la 2e moitié du 8e s.), voir *supra* notes 17 et 25, mais pp. 109-111, il reconnaît la similitude du *tet* de DA et de la Citadelle.

<sup>52</sup> Voir VAN DER KOOIJ, *idem*, p. 111: même *šade* à DA et à Amman. Ce *šade* reflète une forte influence de la cursive, inconnue de la lapidaire araméenne.

<sup>53</sup> *Idem*, mais le *samek* d'Amman ne représente pas une forme ancienne, car l'inclinaison à gauche des 3 barres parallèles et celle à droite de la hampe supposent une influence de la cursive.

<sup>54</sup> *Idem*, "représente une forme évoluée en araméen", si on le compare à l'araméen! Mais il dépareille dans une écriture araméenne du 9e s.

<sup>55</sup> Alors que l'allongement du trait à gauche est attesté par toutes les lapidaires araméennes, le *taw* d'Amman serait cette fois très en retrait dans le processus évolutif.

Fig. 2. La cursive ammonite (p. 232).

Deir 'Alla: Inscription sur plâtre de Deir 'Alla, 1ère moitié du 8e s.

Os.Nim : Ostracon de Nimrud, avers et revers, 7e s.

H.iv : Ostracon iv de Heshbon, ca 600.

H.xi : Ostracon xi de Heshbon, 1ère moitié du 6e s.

O.Ma. : Ostracon 3 de Tell el-Mazar, 1ère moitié du 6e s.

H.xii : Ostracon xii de Heshbon, ca 3e quart du 6e s.

H.ii : Ostracon ii de Heshbon, ca 525.

H.i : Ostracon i de Heshbon, ca 500.





cursive qui a suivi son propre chemin et évolué à son rythme, comparé à l'écriture lapidaire ou semi-cursive araméenne, (voir figure 3), phénicienne ou moabite. Ainsi le *ṣet* spécifique de Deir 'Alla qu'on ne retrouve pas avant la fin du VIII<sup>e</sup> s. au plus tôt en araméen,<sup>55</sup> n'est plus un *unicum* puisqu'il est déjà connu à Amman, le *ṣade* de DA inconnu ailleurs, même en araméen, ne l'est pas à Amman. Le *ṣaïn* ovale et ouvert d'Amman supposait déjà une écriture semi-cursive en deux traits courbes ou arcs de cercle. Le *he* de DA n'est pas d'un ductus unifié: la première oblique à gauche peut croiser la haste, la toucher ou en être détachée, les deux autres traits obliques à gauche peuvent être parallèles au premier (*wrmh* II a-b 8), ou, reliés entre eux en forme de 's', être rattachés au trait supérieur, à la haste, ou même sans attache. Cette forme de DA se comprend mieux dans la séquence ammonite, annonçant et expliquant la forme particulière des *he* de la bouteille de Tell Siran (les traits supérieur et gauche rendant un trait supérieur courbe, et les traits droit et inférieur rendant la forme cursive en 's'). De même le *kaf* de DA se situe parfaitement bien entre ceux de la Citadelle et ceux de Siran, même si la forme araméenne de Zakkur pourrait aussi l'expliquer, la proximité géographique l'emporte dans ce cas de figure, sans avoir à chercher plus loin. Le *samek* de DA à la tête très ramassée diverge de ceux des écoles araméennes, mais il explique ceux de l'école ammonite (yeux gravés, ostracon ammonite de Nimrud, ...). Le *qof* de DA, dans la logique du tracé du *ṣet*, annonce la forme de Siran, très distincte de l'école araméenne. Le *ṣade* de DA ne surprend plus entre celui de la Citadelle et ceux de l'ostracon de Nimrud. Le *taw* de DA se coule parfaitement dans l'évolution de l'écriture ammonite. Ces mêmes constatations valent pour toutes les lettres de l'alphabet de DA en regard de l'alphabet ammonite, alors que des iatus inexplicables ou des formes aberrantes sautent aux

<sup>55</sup> Poids et bronze de Nimrud, mais une origine précise est inconnue.

Fig. 3. Écriture araméenne (p. 234).

- |                |  |
|----------------|--|
| Zak.           | : Inscription de Zakkur, ca 800 ou début du 8e s.    |
| Sfi.           | : Inscription de Sfiré, ca milieu du 8e s.           |
| BrR.           | : Inscriptions de BarRabib, ca 730.                  |
| Br.Ha.         | : Briques de Hamat, milieu ou 2e moitié du 8e s.     |
| Br.-P.Nim.-Ha. | : Bronze et Poids de Nimrud et de Hama, fin du 8e s. |
| Tab.Ass.       | : Tablettes d'Assour, milieu du 7e s.                |
| O.As.          | : Ostracon d'Assour, milieu du 7e s.                 |
| P.Sa.          | : Papyrus de Saqqarah, fin du 7e s.                  |
| T.St.          | : Tablette 'Starcky', 571/70.                        |

yeux si on les compare à l'évolution de l'écriture araméenne, l'écriture de Damasène étant à ce jour à peu près inconnue et celle de Hama guère mieux attestée.<sup>56</sup> Ces innovations de la semi-cursive ammonite ont donc fait leur apparition à Amman indépendamment de l'écriture araméenne et bien avant, qu'on date l'inscription de la Citadelle de la fin du IX<sup>e</sup> s. ou peu avant.

L'écriture araméenne évolue parallèlement et souvent différemment, voir *qof* (tête en forme de 's' couché), *šade* (avec un 'z' très étalé), *mem* (avec un trait médian), *šet* (arrondi et ouvert, avec une barre à gauche, stèle de Neirab, tablette et ostracon d'Assour, à droite sur les poids), *het* (à une barre dès le milieu du 8<sup>e</sup> s.), *ʿaïn* (ouvert en même temps que *bet*, *dalet* et *reš*), *samek* (plus évolué), ... On peut même se demander dans quel sens va l'influence, mais trop peu est encore connu des écoles des royaumes araméens pour répondre.

Quoi qu'il en soit de cette question à laisser pour le moment sans réponse, force est donc d'admettre l'existence d'une tradition d'écriture locale à Deir ʿAlla même par une guilde de scribes probablement attachés au temple de Penuel (= Deir ʿAlla), tradition très fortement influencée par la tradition ammonite, si elle n'est pas elle-même ammonite. Mais rien ne l'en distingue dans l'état actuel de la documentation. Il faut conclure à un développement propre de l'écriture ammonite, parallèlement à l'écriture araméenne dont elle ne peut pas ne pas subir des influences inévitables du fait du voisinage.<sup>57</sup>

D'une part, si comme tout semble l'indiquer, l'ostracon ammonite de Nimrud qui doit être daté entre la destruction du palais en 612 et la campagne de Sennachérib (donc probablement de la 1<sup>ère</sup> moitié du VII<sup>e</sup> s.) est bien ammonite par le contenu et l'écriture

<sup>56</sup> Voir *supra* note 24. On ne comprend pas comment VAN DER KOOIJ, 1987, pp. 114-115, peut déduire que les scribes (ammonites) de la région nord ont appris à écrire d'après les traditions de Damas et d'autres villes ou états araméens, probablement pas plus tard que la fin du 9<sup>e</sup> s., alors que tant de particularités mettent à part l'écriture ammonite dès le 9<sup>e</sup> s.! (Soit dit en passant, le *šet* édomite de VAN DER KOOIJ, *idem*, 114s, qui serait à rapprocher de la forme ammonite, n'existe pas; il faut lire un *dalet* certain par la paléographie et le contexte. On peut apporter de nombreux autres exemples de cette forme de *dalet* en édomite même et ailleurs, en moabite, ...).

<sup>57</sup> Voir déjà E. PUECH, *Biblical Archaeology Today*, cit., p. 355; *Le Monde de la Bible*, cit., p. 36. Dans un sens assez proche, F.M. CROSS, *Studies ... Horn*, cit., pp. 480-481.



(qu'il y ait une ou deux mains importe peu), un repère important est assuré pour le développement de la cursive ammonite. Comme celle-ci est beaucoup plus évoluée que celle de Deir Alla, cette dernière ne peut être située dans le VIIe s. D'autre part, si l'inscription de la Citadelle avec ses particularités ammonites est bien à situer dans le IXe s., l'évolution de la semi-cursive de D.A. plus avancée que celle supposée par l'inscription de la Citadelle situe l'inscription sur plâtre dans le VIIIe s. Une datation dans le milieu ou même la 1ère moitié du VIIIe s. paraît tout à fait raisonnable et vraisemblable, plus proche de l'inscription de la Citadelle que de l'ostracon de Nimrud. Je remonte donc d'un quart à un demi-siècle la proposition que j'avais faite précédemment.<sup>58</sup> Une datation par la tradition araméenne a contraint l'éditeur à situer cette écriture de DA vers 700, tout en concédant une série d'anomalies de formes locales sans aucune correspondance (*sade*, *he*, *kaf*, *taw*, *het*, *qof*).<sup>59</sup>

Cette datation dans la première moitié du 8e s. paraît mieux s'adapter à l'évolution de la graphie ammonite et serait appuyée par les dernières données archéologiques: antérieure au grand tremblement de terre du milieu du VIIIe s. sous Ozias (Am. 1,1 et Za. 14,4-5), et historique: sous la domination israélite, après une domination araméenne en Galaad sous Hazaël. Le texte fondamentale cananéen se comprend mieux dans ces circonstances, même si des aramaismes ne sont pas absents.

En définitive, il semble donc qu'il y ait un rapport certain entre "langue" et "écriture" dans ce texte régional du territoire ammonite.<sup>60</sup> Quelle que soit la domination politique antérieure ou contemporaine, l'école scribale nationale ou locale y transmet ses propres acquis. Mais la domination araméenne de la fin du IXe s. expliquerait mieux les aramaismes du texte qui peut être plus ancien que la copie sur le plâtre. C'est dire que les habitudes des scribes sont plus stables ou plus lentes et moins perméables aux change-

<sup>58</sup> *Biblical Archaeology Today*, cit. et RB, 1985, p. 12: deuxième moitié du 8e s.

<sup>59</sup> ATDA, pp. 92-95: cependant le *bet* n'est plus fermé en araméen après le milieu du 8e s., à l'exception de sceaux en écriture lapidaire. J.A. HACKETT, cit., pp. 10-11, tout en suivant une tradition ammonite, arrive à un même résultat, mais elle considère encore l'ostracon de Nimrud comme araméen et se fonde sur les traits caractéristiques de l'*alef* et du *yod* archaïques, des *dalet*, *ʿain*, et *res* fermés, du *he* inhabituel, du *het* à deux(?) barres, ...

<sup>60</sup> La découverte dans des couches postérieures du site, phases VI, ..., d'ostraca ou inscriptions en écriture araméenne ne doit pas surprendre, étant donné la création de la province du Galaad lors de la conquête assyrienne du 8e s.

ments et influences externes que des emprunts linguistiques qui, eux, touchent toute une population et sont donc plus vite adoptés et assimilés. Le milieu culturel ainsi dégagé et la domination israélite de cette première moitié du 8<sup>e</sup> s. sur la région n'expliqueraient-ils pas au mieux l'intérêt porté au devin Balaam par la tradition biblique postérieure?



## BOOK AND SCRIPT AT DEIR 'ALLĀ\*

Gerrit VAN DER KOOIJ

In this paper I should like to deal with palaeographical and other material aspects of the Deir Alla Plaster Text:

1. The original location and general lay-out of the text;
2. The relations between the separate fragments and groups of fragments;
3. The palaeography;
4. Some letter identifications.

### 1

The subject of original location and general lay-out of the plaster text has, of course, been extensively dealt with by this writer in the *editio princeps* (pp. 23–28) as well as in the preliminary report of the 1976 dig with additional archaeological information (in FRANKEN, IBRAHIM, 1977/78, esp. pp. 60–71). Subsequent studies about the inscription did not deal with the original location—or hardly so—except those by A. LEMAIRE and E. PUECH.

The *original position of the plaster* was studied from the way the plaster fragments had spread and been deposited in the soil. In fact a scatter map was made during the excavation, but it was not possible to include all the fragments on it. Photographs and memory could fill almost all gaps. This information, including the orientation of the fragments, has been included in the *editio princeps* (esp. pp. 26f.). It had already been established by then, that all the plaster fragments had come from the E face, or perhaps also a S side, of the mudbrick built structure (a wall or something else) between the rooms 34 and 35, the upper part of the plaster having fallen to the SW. This reconstruction has been supported by the excavations of 1984 and '87 recovering much of the phase IX remains. Several of the N-S walls had collapsed to the W, especially to the SW, with a twist in the wall's line. This happened apparently due to the direc-

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\* During the symposium the first paragraph of this lecture was read in the archaeology-session.

tion of the clearly severe earth shock that probably destroyed the last remains of the settlement<sup>1</sup> after the first shock had destroyed quite a bit already, partly helped by fire. The 'mudbrick structure' could not be identified at first: a kind of stele (not stone cut) or wall. Clearly the remains show a wall, but it was thought that it may have had a S-face also, with a doorway with the fragments of the upper column (combination I) on it, while the E face bore the lower combination II column. The word "stele" has to be abandoned, but the original shape of the wall is not completely certain. In any case it bordered a room to the E of it, with a strange floor and a bench at the sides, partly made of old wall stumps, as well as a blocked "back-door". This floor was found covered by a mat of woven reed, that originally roofed the room (cf. fig. 1).

The idea of a stele-shaped supporting structure had originated from the contour of the right-hand and upper edges of the plaster surface. LEMAIRE, 1986, p. 82, dealt with this and prefers to connect the backward curve of the right-hand edge with an opening in the wall (doorway, window). This is possible, assuming a high threshold; although one would expect the plaster to curve around the edge somewhat. Also the sloping upper edge remains unexplained in this way.

New information has to be added now. In the *ed. pr.* (p. 28, n. 7) the comparative problem has been mentioned, i.e. whether the application of the layer of lime plaster was connected with the inscription or just with the architecture in order to provide a special finish of the wall. The recent excavations make it likely that the plaster was primarily connected with the architecture, because several other fragment groups of the same kind of plaster have been found in different locations, not inscribed and without drawings. Apparently walls, or, in fact, only parts of them, had been covered with this material and in one room this was used for a text.<sup>2</sup> It appears that

<sup>1</sup> There is no proof of an earth shock being responsible for the last stage of destruction. It is possible that men tore down the walls, but the fact that the SW inclination occurs with several walls running N-S suggests a second shock.

<sup>2</sup> The lime plastered walls at Kuntillet Ajrud, dated by the excavator to c. 800 BC, are comparable with the Deir Alla material, since at places they too were painted with drawings ("colourful floral motifs", "red and black", "linear design") and inscriptions (see MESHEL, 1978, paragraph "The Buildings"). On the other hand, apparently in this case the decorative elements are much richer than the inscriptional ones, of which very little was found. Furthermore the painted and inscribed plaster was located at doorways.



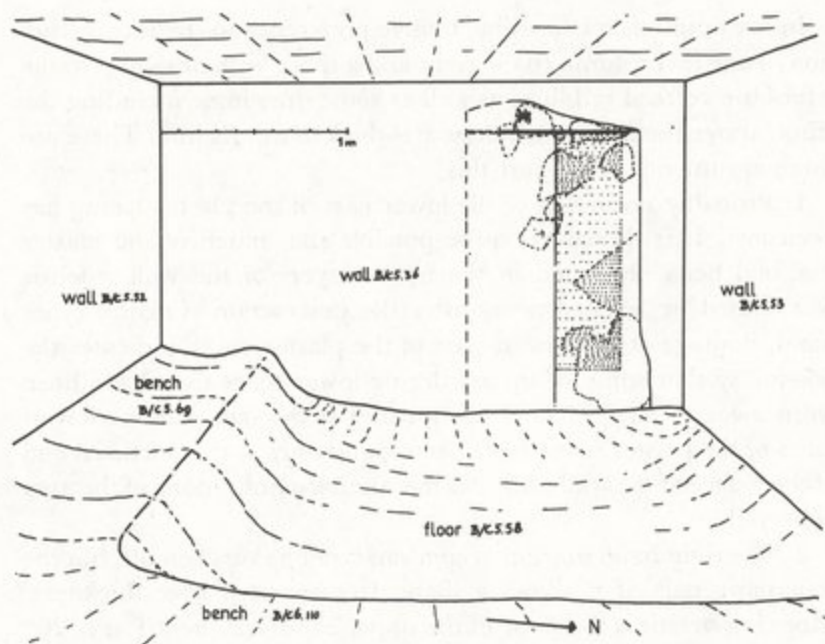


Fig. 1. Schematic reconstruction of room EE335 (see fig. p. 19), looking WSW, towards wall B/C5.36.

the inscribed plaster had been applied in the same way as the other plaster was, sometimes with irregular edges.

The *general lay-out of the inscription* has received some more attention, but again not as detailed studies. GARBINI (1979, p. 168), followed by DELCOR (1981, p. 60), suggested one text column with combinations I and II overlapping, mainly based on the textual possibility. PUECH (1978, pp. 114f, 1987, p. 14 and 1986, p. 38) also favors one text column, partly based on physical and partly also on internal textual evidence. LEMAIRE has reconstructed step by step a complex of two text columns to the right (comb. II to the right of comb. I) and two unwritten ones to the left, based both on internal and physical evidence (1986, pp. 86ff.). In the *ed. pr.* one text column was suggested, as well as the possibility of two, in which case comb. I would have been to the left of comb. II, but facing south (comb. II facing east) so at right angles to comb. II, supposing a passage way between rooms 34 and 35. Even the possibility of more text columns was proposed on internal evidence.

In my opinion, it is possible to give preference to the reconstruction of one text column to the right and a space with drawings to the left of the vertical red line, as well as some drawings, including the sphinx, above them and the horizontal thick marginal line. There are three arguments to support this:

1. Probably not much of the lower part of the plaster facing has been lost. It is, however, quite possible that much of the plaster that had been deposited in the upper layers of the wall's debris was eroded or levelled away after the destruction. On the other hand, damage at the lowest part of the plaster cover indicates the possibility that some of the bordering lower parts may have been worn away in situ, hit and trampled. Yet the fact that the lowest parts of the plaster cover have been found only at the northern end of the E-face of the wall indicates the southern limitations of the area plastered.

2. The right hand margin of combination I has broken off, but the remaining part of it shows a slight decrease of plaster thickness. More important is the slope of the upper edge (fragment I a; c. 20° from horizontal), where plaster thickness slowly decreases to zero,<sup>3</sup> forming the upper right hand edge of the plaster cover. If all the plaster was facing the same direction (east) this fact would suppose a one column text. The sphinx fragment was found very close to the comb. I fragments and has to be connected with the upper edge, and to be located above and to the left of the text column. The fragment also has the oblique upper edge of the plaster and the red, string-made, horizontal line has the composition and width of the line to be seen to the left of the writing on fragment i b (4 + 1 thin lines) contrasting with the line on top of the text column (3 + 1). This 4 + 1 composition is also seen on three other fragments (one published before, fragment xiii a, cf. p. 165;<sup>4</sup> another one added here) that have

<sup>3</sup> See *ed. pr.* p. 24. The gradual decrease of thickness of the plaster can, of course, not be seen on the surface only (the plaster being embedded in artificial plaster now), because the plaster surface does not curve backwards as on the right hand edge of comb. II (cf. LEMAIRE, 1986, p. 82).

<sup>4</sup> The position of fragment xiii a is indicated by the position of the elements of the composed horizontal stroke (4 + 1 below), identical to that on fr. i b.



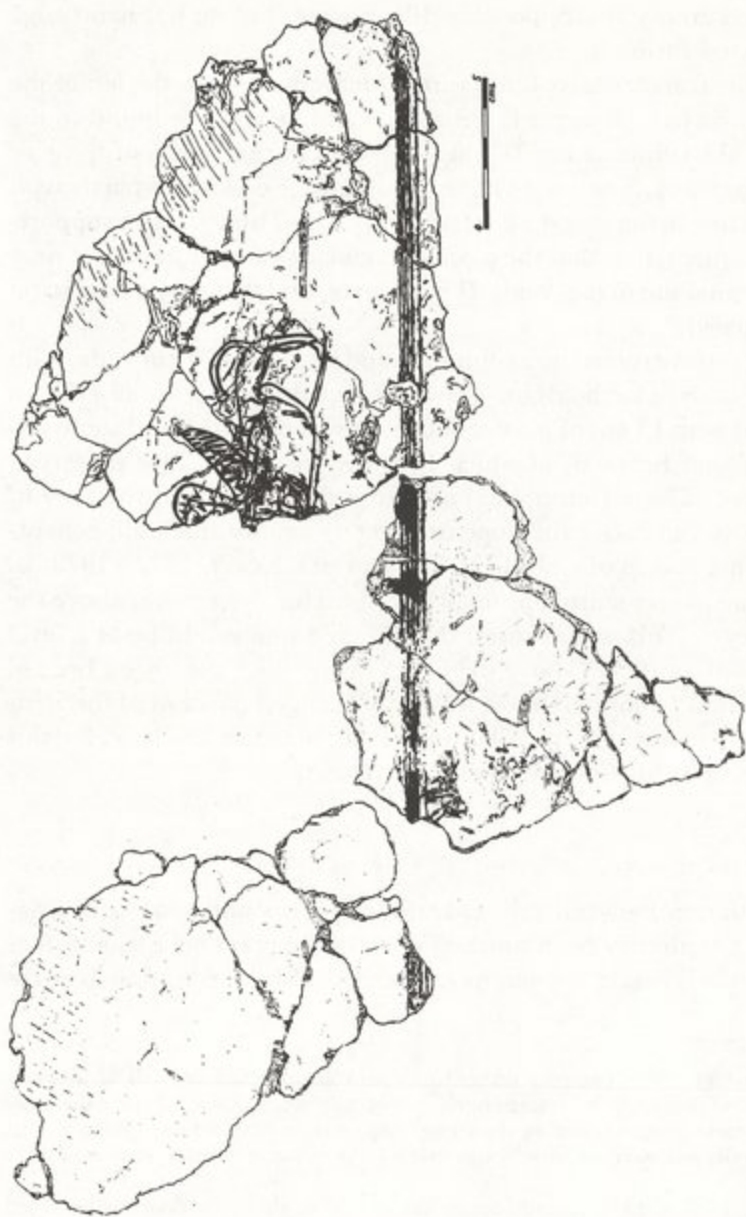


Fig. 2. Three fragments with horizontal thick marginal line (4 + 1) in a conjectural relation to one another. Probably the right hand end of the line was connected with the left hand end of the horizontal line of combination I (fig. 3).

to be placed in the position shown here, although their precise location is not certain (cf. fig. 2). This would mean a column for drawings and empty space, possibly 40 cm wide, but no left hand edge was found fitting here.

3. The fragments with red vertical marginal line to the left of the written surface (fragments viii a, b, c and xii a) were found to the SW of the combination II fragments and to the N/NE of those of combination I. They were lying partly upside down and partly with the red line to the E and SE (cf. *ed. pr.* p. 27). This evidence supports the reconstruction that the plaster fragments came from a spot near the original site of the comb. II fragments, and that only one vertical line existed.<sup>5</sup>

The reconstructed text column would be at least 33 cm wide, with a 9 cm margin to the right. The height would be at least 29 + 63 cm (92 cm) with 13 cm of plaster above it (with drawing) and some unused plaster below it, of which 17 cm is preserved. This preserved height of 122 cm (minimally) of plaster did not start at the floor of the room, but above the slope running up against this wall, consisting of the stump of wall B/D5.51 (VAN DER KOOIJ, 1977-1978, p. 63). The plaster started probably c. 50 cm (or even more) above the floor level. This would mean that the first line would be at a level of 1.50 m (or more) above the floor. In any case, the lowest lines of the column (comb. II) show a slightly changed position of the writing hand (see *ed. pr.* p. 25f.), indicating a rather low level for this line, such as 60-70 cm. above the floor.<sup>6</sup>

## 2

The relation between the separate fragments and groups of fragments has already been touched upon in the previous paragraph as far as the general lay-out is concerned. The inscription has the

<sup>5</sup> LEMAIRE, 1986, prefers a rather westward collapse, the E-W wall 42 preventing a SW direction. It is, unfortunately, not certain whether wall 42 had a doorway at its eastern part, weakening the corner. Apart from that, a force giving a twist to the walls as described, would also make a SW collapse of both connected walls possible.

<sup>6</sup> The level of the horizontal upper line at 1.50 m above the floor, as proposed by LEMAIRE, 1986, p. 88, may be used. It still makes it possible, however, to locate combination I above combination II, and to have the lowest line at 0.50 m from the floor.



appearance of an illustrated book, but many lacunae still have to be filled in.

The *editio princeps* located a large number of fragments, both those that are actually fitting physically and those that are grouped together on the basis of different physical criteria, but do no longer actually join because of damage at the edges. Naturally, one or two joining possibilities may have been overlooked. Several proposals for more precise locations have been made and I would like to mention and discuss just a few of them, concerning combination I. Some of those locations have been discovered independently by several scholars, such as CAQUOT-LEMAIRE, HACKETT, HAMILTON and DIJKSTRA—the last two not being published, but known to me from correspondence. In fact the job of finding locations for strayed fragments seems to act as an addiction for many of us.

The most influential rearrangement within combination I is the one published promptly by CAQUOT and LEMAIER (1977, p. 193), namely the upward move of fragments i c and i d by two lines; this has subsequently been accepted by almost all reviewers. The main reason that prevented us from doing this in the *editio princeps*, was the lack of a word divider at the end of line 2, after the *taw* in red ink. We had chosen a distance between the fragments thought to be a maximal one, so as not to impose interpretations that could not be proved. The location of fragment xii c in between the approaching fragments i a and i d appears physically possible to me<sup>7</sup> and supports the rearrangement. Fragment viii d has been placed to the right of this xii c because of internal evidence. In fact a small part of the edge of the fragment, namely at the *mem*'s tail, is surprisingly well fitting on to the spotted edge of i d, probably making a joint (fig. 3). Furthermore, fragment i c has been pushed to the left, (e.g. H.&M. WEIPPERT, 1982, p. 82) leaving little room between i c and i a + d.

This new composition poses two questions: Firstly, the missing word divider referred to above, but this is a philological problem

<sup>7</sup> The surface character of this fragment xii c, as described in the *ed. pr.* p. 163, differs from that of the other comb. I fragments, but this may be due to it having been embedded in a different type of soil (probably more moist only), as it lay somewhat separate from the other comb. I fragments.

The proposal to insert the two combined fragments xii c with viii d between fragments i a and i d was made independently by HACKETT, HAMILTON and DIJKSTRA (through writing to me).



Fig. 3. Rearrangement of fragments of combination I; the distance between the right hand part (i c) and the main part is not clear.





which I am not concerned with here. The second question is that it is difficult to locate fragment iv a, with the red tail at the top and two black line fragments below it. The fragment was found with combination I, but can no longer be placed there, unless i c is pushed to the right.<sup>8</sup>

Apart from this the rearrangement is promising, since it slightly diminishes the possibilities of locating other fragments that certainly belong within or very close to combination I. In fact it has to be expected that the gaps can be almost completely filled with the fragments preserved.

Let us review some of the proposed locations that are placed not only for internal reasons. In fact most of the fragments are located because of internal reasons only. A few of them, however, thus have to join physically as well. There are three cases in combination I:

– At the top of fragment i c, fragment iii h seems to fit the location so as to form a *pe* (*spr*) rather well (HAMILTON in HACKETT, 1984, p. 31, and LEMAIRE, 1985, p. 316): it is almost joining at the plaster's surface (parallel crack and straw traces), however, further down, below the surface, the plaster edge (drawn in the *ed. pr.*) probably does not allow such a closeness, but I cannot be certain.<sup>9</sup>

– The joining of fragment iii a at the top of i c, so as to complete the head of *reš* of the proposed reading *spr* (LEMAIRE, esp. 1985, p. 317, followed by PUECH, 1987, p. 15) is impossible: the plaster edges do not fit at all and the first character on ii a definitely is a *tet* and not part of a *reš* head.

– It is proposed by HAMILTON in HACKETT (1984, p. 33) to add fragment v c line 2 near the end of line 1, but this is impossible since this fragment shows the remains of a character of a preceding line, as has been mentioned already by LEMAIRE (1984, p. 142) and PUECH (1986 a, p. 17, n. 12).

<sup>8</sup> Fragment iv a is a bit problematic because the lines of writing seem to have an irregular distance. It is composed of three separate fragments of which the left hand and middle one join well (see *ed. pr.* p. 147), but doubt may remain concerning the third one.

<sup>9</sup> It is not possible to check the edge on the original plaster without removal of the artificial plaster around the fragment. A close look at the fragments on the infra red photographs (the fragments lying in trays 42 and 83) does not decide the point.

Tell Deir 'Alla - IX - lime plaster; smooth - nib 5:1 - angle c. 53°			
1	2	3	4
5	6	7	8
9	10	11	12
13	14	15	16
17	18	19	20
21	22	23	24
25	26	27	28
29	30	31	32
33	34	35	36
37	38	39	40
41	42	43	44
45	46	47	48
49	50	51	52
53	54	55	56
57	58	59	60
61	62	63	64
65	66	67	68
69	70	71	72
73	74	75	76
77	78	79	80
81	82	83	84
85	86	87	88
89	90	91	92
93	94	95	96
97	98	99	100



## 3. PALAEOGRAPHY

The description and classification of a script naturally is an important aspect of the study of inscriptions, since script is a physical expression of human behaviour, like any archaeological artefact. Since it is also an expression of a language, script is a bridge between philologists and archaeologists. However, we have to be careful using script as a bridge between the language of a population group and the other aspects of its culture.

In this paragraph three tasks of palaeography will be considered: to describe, to classify, which partly includes explanation, and to date scripts.

*Description of the writing (fig. 4).*

The writing of the scribe, his artefacts, has not received much attention after the *editio princeps*, except for comparative reasons.

Knowledge of the *ductus* is not only necessary to understand the letter shapes as found, but also to understand the developments in the shapes (cf. DANIELS, 1984, SEGERT, 1980). So it would be useful to reach certain agreements about this. The subject has been discussed briefly by PUECH in his first publication about the texts (his review of the *ed. pr.*, 1978). I do agree with his observation concerning the *ductus* of the 'alef, namely with the order of strokes *b-d-a*<sup>10</sup> (but *b-d* not in one movement), but his thoughts about *het*, *samek*, *šade*, and *taw* cannot be accepted:

- the *het* always has three unconnected cross bars, so are not written in an S-move
- the cross bars of *samek* also are always written unconnectedly (e.g. line ii 15);

<sup>10</sup> It has become clear to me that, in fact, the strokes of the 'alef have almost always been written in this sequence in NW Semitic writing (see this writer, 1986, e.g. p. 196). The ideas of PUECH concerning *het*, *šade* and *taw* are shared by CAQUOT, LEMAIRE, 1977, p. 191, but their example of *het* in ii 11 probably was suggested to them by some damage at the spot. Probably PUECH changed his ideas about the shape of *šade* comparing it, later on, with the Amman Citadel shape.

Fig. 4. (p. 248). Script table of the Deir ʿAlla plaster script, with, from left to right, the general shapes, deviating shapes (the locations of those occurring only once are given without brackets), nib-tip movements on the writing surface, and the skeleton forms (old and new types) (from this writer, 1986).

- the right hand part of *šade* is never written with a Z-move;
- the cross bar of *taw* was not written with one stroke (so also CAQUOT, LEMAIRE 1977, p. 191), but with two, partly overlapping, as described in the *ed. pr.*

The *shapes* of the letters have been discussed mainly for comparative reasons, but some remarks have to be made here about a few characters:

- *he*; the shape of *he* in line ii 9, with the top of the S-move touching the upper cross bar (see *ed. pr.* Pl. 27) was taken as representative by NAVEH, 1967, p. 257, and CROSS, 1975, p. 15 (both were working from a partial photograph of fragment ii b published by FRANKEN, 1967), and still as one of two representative shapes by HACKETT, 1984, pp. 11f., 147, and PUECH 1985a, p. 357 and 1985b, p. 19. However, this shape is, as described in the *ed. pr.* p. 62, most exceptional and was not intended, but had come about by ink flow, which, on the other hand, was caused by starting the S-move too close to the upper bar.

- *het*; all *hets* have three thin cross bars; a Z-stroke has been mistakenly seen by CAQUOT, LEMAIRE, in line ii 13 (*rh̄m*), but even the photos show three bars there. NAVEH (1967, p. 257f), CROSS (1975, p. 15f) and HACKETT (still in 1984, pp. 12f) took a two bar *het* as the normal shape.

- *kaf*; the little "nose" was written with a narrow V-movement, but not in such a way that a triangular nose was intended, as described by HACKETT, 1984, p. 14. On the other hand the triangular head, occurring elsewhere (e.g. Siran bottle), has been developed by this V-move (see below).

#### *Classification of the writing.*

A major task of the palaeographer is to locate a script in a typological series and, based on this, to connect it with a specific cultural tradition. Classification is based on comparative studies (see the *ed. pr.*, as well as this writer, 1986 and 1987). The first comparative study of the script of the plaster text was published by NAVEH in his prompt note in 1967, in order to correct a hasty preliminary date by the excavator. He connected the script with Aramaic writing traditions and still maintained this opinion in 1979. A close relation with Aramaic script traditions has, in fact, been advocated in several studies (e.g. LEMAIRE 1984 and 1985, p. 315), but a connection with "Ammonite" traditions has been stressed by, for example,



HACKETT and PUECH.<sup>11</sup> HACKETT (1984, p. 18) gave herself the choice to connect the typologically rather "early" script to an early period in the Aramaic tradition or to a later period (end of 8th century BC) in an Ammonite one, partly characterised, according to CROSS, by the retention of older Aramaic script types. She has chosen the second alternative, stressing shapes of *het* and *kaf* to be compared with the script of the Tell Siran bottle, and those of *šade* and *tet* with the Amman Citadel script. PUECH appreciates the similarities of shape with especially *tet* and *šade* of the Amman Citadel inscription (1985a, p. 355) by placing the plaster script in a "direct line" with this stone inscription, which he labels Ammonite.

In my opinion the limited material available, both from the Ammonite and Aramaic regions, does not affirm the hypothesis of a continuous Ammonite script tradition. It is preferable to take the scripts used in the Ammonite region (and surroundings) as standing in one or more specific Aramaic writing traditions, contrasting with Moabite and Edomite scripts. The main reason to do so is the fact that the most influential agent for the development of the Aramaic scripts, namely the gradual increase of the writing angle, also occurs in the Ammonite scripts and, furthermore, the new shapes developed by this increase are shared. If Ammonite script traditions had been isolated for quite some time this common character would most probably not have appeared; there are no reasons to expect that a parallel development would be responsible for this similarity in often related regions.<sup>12</sup>

However, within this framework it is also suggested by the data available, that the scripts of the Ammonite region and the eastern Jordan Valley occasionally show characteristics that were not common in Aramaic and Phoenician regions. This concerns three letters that also play a role in the characterisation of the Deir Alla plaster script as Ammonite:

<sup>11</sup> McCARTER, 1980, p. 50, sees the script as a "Transjordanian sub-family of the Old Aramaic cursive sequence", but does not want to use the term "Ammonite" for it.

<sup>12</sup> Cf. this writer, 1987, pp. 109-115. PUECH, 1985a, p. 355, deals with Aramaic features in a slightly different way, assuming independent developments in the Ammonite tradition parallel to the development in the Aramaic tradition, at the same time supposing Aramaic influences. The difference in points of view is a matter of accent and terminology. The more influence from Aramaic traditions is discerned the less parallel the development in Ammonite has to be called.

– *ṭet*; in the development of the *ṭet* two alternative changes occurred, dropping one of the two bars of the cross inside. The upper left to lower right bar was maintained in the BRRKB stele from Zinjirli (c. 730 BC), as well as in a developed oval shape in the broad nib and ink writing of the Deir Alla plaster script, the Kuntillet Ajrud plaster script and that of ostrakon no. 3 from Tell el-Mazar. The oval shape undoubtedly developed in the broad nib and ink writing, because only there does the wide cross bar ask for such a shape, but it was also used with other writing, namely the stone script of the Amman Citadel inscription, a jar inscription (soft clay writing) from Tell el-Qeda (Dan), a Nimrud bronze bowl (N19), the Adoni-pelet seal, and perhaps also on a clay tablet from Quyunjik (CISII 38), dated 682 BC.<sup>13</sup>

The alternative shape with the upper right to lower left cross bar is generally used in later Aramaic writing (in ink and argillary writing from mid 7th century BC onwards), but not (yet) found in the Ammonite region.<sup>14</sup> In Phoenician writing the cross was maintained.<sup>15</sup>

This evidence makes it possible to characterise the shape of the Deir Alla plaster text and the Amman Citadel inscription as “northern Trans Jordanian”, but not exclusively, since the type is shared with Aramaic traditions and perhaps Phoenician ones too if the two texts mentioned from Palestine are to be called thus. Thus not in an isolated tradition, but in a closely connected one.

– *kaf*; the development of the *kaf*, to be explained from ink writing, shows a direction shift of the third (left hand) stroke. Within this series the triangular move and shape has been found in the plaster script, possibly in the Nimrud ostrakon (convex side), and clearly in

<sup>13</sup> Most of these inscriptions have been referred to in the *ed. pr.* as well. Subsequent study of the BRRKB inscription made the existence of three examples of the *ṭet* shape clear to me. The non-Hebrew texts from Kuntillet Ajrud are in part preliminary published by MESHEL, 1978. The Tel Dan jar inscription (only sherd preserved) has also been published by DELAVAU, LEMAIRE, 1979, Pl. I.1. The inscriptions from Tell el-Mazar are published by YASSINE, TEIXIDOR, 1986. Perhaps the bulla from Buseirah (this writer, 1987, p. 114) and some other seals may be added here.

<sup>14</sup> During the Persian period, however, this type was spread all over the empire including the S. Levant.

<sup>15</sup> There are two examples of Phoenician texts with a *ṭet* with one cross bar, namely the plaster text from Kuntillet Ajrud and the jar sherd from Tel Dan (see above), but perhaps the two have to be labelled Aramaic.



the bronze script of the Siran bottle. The type is also found in Phoenician inscriptions from Sidon (Tabnit, Eshmunazar), Cyprus and Pyrgi—all probably dating from the 5th/4th century BC and possibly related.<sup>16</sup> On the other hand the type is not found in Aramaic writing, but the shape developed from it, with the third stroke almost vertically placed from the top down (supposing the development as sketched above is correct), did, for example on the Sfire steles (c. 730 BC) and the clay tablet from the Louvre (635 BC; BORDREUIL, 1973). This developed form is used too on the concave side of the Nimrud ostrakon.

The alternative development, with a decreasing angle between the second and third strokes, is used both in Aramaic and Ammonite writing.

— *šade*; the shape of the *šade* used in the Amman Citadel inscription, has, in my opinion, been developed by the use of the broad nib-ink ductus, since there the dropping of the connecting stroke of the Z-shape is hardly noticeable because of the contour of the one curved stroke. The reduced writing of the shape occurs with ink, apart from Deir Alla plaster, in the Nimrud ostrakon (convex side), a later ostrakon from Deir Alla, possibly Tell Hesban ostrakon no. IV, as well as on the clay tablet from Quyunjik mentioned above (CIS II 38, 682 BC). The shape of the Citadel inscription is more angularly shaped (less stretched) than the ones just mentioned. The character is not often used in texts, but in any case another type was used on the Siran bottle.

The conclusion from this and other evidence may be summarised thus:

1. The writing of pre-Persian inscriptions from Aramaic regions, with Aramaic language, and those from N-Transjordan, have a common characteristic not shared with other NW-Semitic scripts, namely the gradually expanding writing angle up to 80°, in broad nib-ink writing,<sup>17</sup> as well as the main letter shape changes caused by this angle (mainly the *'alef*, and the open *bet*, *dalet*, *ʿayin*, *qof*

<sup>16</sup> One may add a seal with this shape, i.e. HERR, 1978, p. 71, no. 36, from unknown provenance, but connected by him, following CROSS, with the Ammonite script tradition which, however, is not necessary (= AUFRECHT, 1989, *Corpus*, no. 56).

<sup>17</sup> The beginning stage of the expanding writing angle (up to c. 60°) is shared by Phoenician writing (see this writer, 1986, p. 91).

and *reš*). The similarity is better understood as stemming from contact rather than parallel development, because of the good possibilities for cultural relations between the regions, and because the expanded angle could easily have resulted in alternative shapes in isolated traditions.

2. The pre-Persian inscriptions from N-Transjordan have scripts stemming from several somewhat separate script traditions or branches.<sup>18</sup>

3. The script traditions represented in N-Transjordan are also represented in Aramaic regions where, however, other branches also existed. This means that close tradition relations existed.

4. It is striking that during the Iron Age II period the political boundaries of the territorial states, or groups of states, using the same language, are, by and large, also bordering the local writing traditions, as is explained elsewhere (see this writer, 1986, pp. 244ff, and 1987). This probably means that writing traditions are closely connected with the central administration, the court. Deductively this suggests the existence of a "national" script in Amman, whatever its border was.<sup>19</sup> But it is clear that this writing and script existed and developed in close contact with Aramaic writing, several traditions crossing the border at different times.

5. The Deir Alla Plaster Script has the closest typological and probably traditional relations with scripts that show no alternative or opposite developments (cf. the *ed. pr.* pp. 77ff; uncertainty exists where diagnostic characters are not used). They include:

a. With identical and some preceding types:

<sup>18</sup> The traditions of two inscriptions are labelled "separate" as soon as aspects of the writing pattern show dissimilarities in ductus or shapes which have to be considered opposite developments. This is obvious where alternative changes have developed, e.g. with the *ṣet*, but also where the stages of development of the characters are not parallel, but opposed to one another. Thus the script tradition of the Amman Citadel inscription is separate from the Tell Siran inscription, because the Siran script shows an alternative development with *ṣade*, although several other characters show the same or a later stage of development. Thus the Amman statue script is separate from the citadel script showing a later stage of development with *ḥet*, but an earlier one with *ayin*, and also separate from the Siran bottle script, which has a further developed *yod*, but less, or differently, developed *waw*.

<sup>19</sup> PUECH, 1985a, p. 355, postulates *a priori* "l'existence de scribes ammonites, moabites, édomites aussi bien qu'israélites, judéens, araméens ou phéniciens, dans ces diverses entités géographiques et régionales, de scribes attachés soit au pouvoir central (palais), soit à des temples, soit à d'autres institutions plus ou moins publiques, écoles, notariats, etc."



Kilamuwa inscriptions from Zinjirli (but *ʔet* not represented), as well as the Örddek Burnu stele; perhaps the Honeyman inscription from Cyprus, but the *ʔet*, *ʃade*, *kaf* and *samek* are not present; the Zakir inscription from Afis (*ʔet* not represented), in a closer stage of development; the same applies to the Melqart stele from Brej and the Hazael ivories, as well as some of the Hama brick graffiti, but in all these cases several characters are not used. Opposite development stages occur with Sfire (*kaf*), Zinjirli BRRKB, some Phoenician inscriptions from Cyprus and Carthage (*yod*), as well as probably the Amman Citadel inscription (*ʿayin*, and *samek*).<sup>20</sup>

b. With identical and following types:

Nimrud ostrakon convex side (but *ʔet* and *samek* missing; the difference of the word divider may not be significant); with the concave side the *yod* has an alternative development; Mazar ostrakon no. 3 (including *ʔet*, but *kaf*, *samek* and *ʃade* are not used); Deir Alla ostrakon reg. no. 2755 (VAN DER KOOIJ, HOFTIJZER, 1989, pp. 66, 69) possibly, but the *ʿalef* probably shows an alternative development, and *ʔet*, *samek* and *ʃade* are not used; Hesban ostrakon no. IV possibly, or hardly (because of *yod*), and more remotely (the shapes of *yod*, *samek*, *ʿayin* and *ʃade* are difficult to see and *he* and *ʔet* are missing); clay tablet Qyunjik CIS II 38, 682 BC (including *ʔet* and *ʃade*).<sup>21</sup>

<sup>20</sup> The open *ʿayin* has preferably to be explained by diffusion from the broad nib-ink writing, where it easily develops by the use of the expanded writing angle, indications of which are also to be recognised in other characters (see this author, 1987, pp. 111, 115f). However, if necessary for other reasons, the use of the open *ʿayin* may in this particular case be explained by the fear of the scribe that otherwise the encircled stone surface might chip away, as occurred with the first *ʿayin* he made in this flaky stone surface, thus invented independently but informally. However, I hesitate to accept this reconstruction, because, on the one hand, several *ad hoc* examples of informal writing exist in NW Semitic writing, influenced by a particular local situation of the writing surface, but there are no repeats. Furthermore the Amman Citadel scribe could have reached the same result by only enlarging the circle, as in fact he did too. On the other hand the Phoenician ivory box from Ur shows an informality in writing the *ʿalef*, with a break in carving the vertical line, apparently in order to avoid the tiny triangle between the three strokes chipping away, but in this inscription this action is formalised.

The abstract model of *samek* of Deir Alla has almost horizontal upper strokes, written from left to right, but those of the Amman Citadel stone are oblique, like the cross bars of *he* and *ʔet* – clearly representing the thin *b*-strokes of the broad nib-ink writing. Therefore I take this *samek* as an alternative or later development.

On the other hand, the shapes of *ʔet* and *ʃade* are both developed in a broad nib-ink writing tradition very close to the Deir Alla plaster script tradition.

<sup>21</sup> The developed type of *samek* is alternative for the type usual in East Jordan. On the other hand both types developed from the Deir Alla one.

*Dating the writing.*

Dating preferably has to be done with examples from the same tradition or branch, using the typological order of developing elements of the writing pattern, mainly the writing angle, strokes for a character and shapes. Typological order, of course does not automatically mean a chronological order of the inscriptions concerned even if the pattern relations are very close, because, in practice, the same writing pattern may be used longer by one scribe than by another one from the same "school."<sup>22</sup> In our case the closest, less developed related inscriptions and the closest further developed, possibly closely related, ones suggest a date roughly between 850 and 650 BC.

A further comparative study with scripts from possibly (or clearly) more remote but still related traditions or branches, makes it possible to use more dated material resulting, in this case, in a more defined time margin. A *terminus ante quem* may be based on the last dated appearance of the older types that die out relatively early; in this case the *het* with three cross bars. This type is not used any more on the clay tablets (all from the beginning of the 7th c. BC. onwards), having one cross bar only, as well as on other 7th. c. BC inscriptions. The last uses occur on:

- the polished and burnt brick tiles from Hama used at the entrance floor of Building III of Period E, destroyed in 720 BC. (FUGMAN, 1958, p. 176);
- Hazael ivory from Arslan Tash, probably from c. 800 BC. (BRON, LEMAIRE, 1989);
- two ivories from Nimrud, Fort Shalmaneser, to be dated before 720 BC.<sup>23</sup>;
- bronze bowls from the NW-Palace at Nimrud; probably before 740 BC.<sup>24</sup>;

<sup>22</sup> In this connection it may be useful to refer to the fact that the corpus of dated clay tablets with Aramaic writing from N-Syria and Mesopotamia clearly indicates, on the one hand, that newly developed shapes in ink writing may be quickly adopted in other writing patterns, but on the other hand, relatively ancient types, that had already changed considerably elsewhere up to 150 years previously, may continue to be used alongside modern types of other characters in the same inscription. (For a chronotypological schema, see this author, 1986, fig. 13, pp. 342-346) In those cases, of course, separate schools/branches, instead of just scribes, are concerned.

<sup>23</sup> The ivories are ND10151 (*hmt*) and ND12049, cf. e.g. MILLARD, 1962.

<sup>24</sup> The bronzes include N50 and N75; see BARNETT, 1967, pp. 2\*ff.



- Sfire basalt steles, c. 750 BC., using more old types, including *zayin*;
- Zakir basalt stele from Afis, c. 780 BC.

This gives a preliminary *ante quem* term of 720 or even 750 BC.<sup>25</sup>

The earliest examples of the developed types, all originating in broad nib-ink writing, in traditionally related scripts, make this *terminus ante quem* possible. The types of the *he*, *zayin*, *kaf*, *samek*, *šade*, *qof*, *taw* and probably *šet* were already generally used (apart from shapes of alternative developments), all of them even often in a further developed stage, in the early 7th c. BC. clay tablets from N-Syria, N-Mesopotamia. Examples of most of these developed types, but not those of *he* and *šade*, are found in use earlier: *zayin* and *kaf* both in the Afis stele, c. 780 BC.; *samek* and *taw* both in Hama bricks before 720 BC., Zinjirli BRRKB & PNMW, c. 750 BC. or a bit earlier; *qof* in Zinjirli BRRKB. One would expect that the new curved cursive strokes of *he* and *šade* are less easily adopted in non-ink writing.

This evidence, in fact, does not really indicate a *terminus a quo*, because the shapes have developed in ink writing, which is not represented in the list of dated inscriptions, and necessarily appeared some time earlier. All this leaves a rough margin of dating for the plaster script between 800 and 720 BC.<sup>26</sup>

#### 4

*Letter identification* is part of the palaeographer's tasks and has, of course, to be accomplished primarily without philological bias. To

<sup>25</sup> It is, of course, possible, that new discoveries give a later result, but it is not very likely that closely related writing deviates very much from the picture based on a relatively large number of texts. Phoenician writing consistently maintains the 3 cross bars for centuries, but the 7th c. BC material (mainly from Cyprus) stems from a separate tradition.

<sup>26</sup> The same dating method was used in the *ed. pr.*, pp. 94ff., but the disuse of *šet* was not considered. Apart from that an effort was made (p. 96) to limit the margin by using historical considerations. The end of the 9th c. BC and the end of the 8th c. BC were both taken as periods in which a contact resulting in script diffusion between Aram (-Damascus) and Deir Alla (region) could have easily occurred. The last period was chosen as fitting the palaeographical data somewhat better. However, it became clear to me from additional palaeographic study, that the text category and the social position of the inscription do not usually indicate a slowly developing pattern (cf. this author, 1986, e.g. pp. 124f.), thus also making the earlier period possible. Apart from that, new historical evidence seems to allow a possibility for diffusion during the intervening period.



Fig. 5. Detail-drawings of three problematic letter remains:  
 a. i 4(6) c-19 *waw* instead of *qof*?  
 b. i 8(10) d-6 *dalet* instead of *res*?  
 c. ii 17-6 *samek* instead of *waw*?

identify fragmentary characters one has to identify the (fragmentary) separate strokes also (preferably first), in order not to be influenced by a false suggestion of a mutilated shape.

Since the *editio princeps* quite a number of scholars have attempted alternative identifications of (not very clear) characters or remains



of characters, differing from the readings offered in the catalogue of the first edition. Several of them have consulted the original inscription in the museum of Antiquities in Amman, often using magnification glasses, some also a binocular. Others worked with the original prints of the infra red photographs made before restauration, and with glossy prints of the colour slides which had been used for the colour plates in the *ed. pr.* It is interesting to note that the number of alternative identifications decreases with the increase of years of study.

Because of the way we felt obliged to study and publish the text for the *editio princeps* (cf. p. 97) serious alternative identifications are of great interest to me. Let me review some influential ones:

- i 1(3) (i c 1-7) *pe* instead of *kaf* (or *mem*) to read *spr*: because of the high position of the head the *pe* is a better reading. The thickness of the point (the *m*-part in the *ed. pr.*, p. 99) does not occur elsewhere with *pe* and therefore suggested the *mem/kaf*, but may be explained here because of an excess of ink (it is doubtful whether fr. iii h fits here to complete the *pe*'s head; see above).
- i 2(4) (i c 2-3) *šin* instead of *taw* (or *lamed*) to read *kmš'*; the thin stroke indeed is better explained as the right hand stroke of a *šin* rather than the top stroke of a *taw*, because a *taw*'s head would have too little space to the right; the stroke is too short for a *lamed*. It is possible to explain the little bit of ink visible to the right on the crack's edge as secondarily deposited.
- i 2 (i a 2-15) *nun* instead of *lamed* to read *yp<sup>c</sup>n* ... is impossible; the upper parts of the *lamed-b* stroke are clearly visible.
- i 3 (i b 4) end of line: *kaf* instead of proposed *šin*; *kaf* is possible, although hardly any other instance exists where its *b*-stroke reaches above the *a*-stroke (viii b line 3); in this case the scribe may have tried to avoid the red tail of the *taw* of the preceding line; I am not sure whether the lower part of the tail can be seen—it was not seen when the *ed. pr.* was prepared, and surface damage is involved.
- i 4(6) (i c 4-19) *waw* instead of *qof* to read *'lwh*; for a *qof* the position of the upper stroke is unique; there would have been little but still enough room to the right for a more appropriate position (cf. the *qof* on fragment x c); it is possible to explain the ink of this stroke as being secondarily deposited—in any case much of this ink, blotted with dirt, is involved; the left hand short stroke can be easily seen as a *waw-c* stroke, so a *waw* is preferable to *qof* (fig. 5a).
- i 5(7) (i d 3-22) *nun* instead of *taw* to read *'lh)n*; the following features rather indicate a *nun*: length of short *b*-stroke at top; hardly any

or no extension of a cross bar to the left of this *b*-stroke (surface damage prevents the upper right and upper part of a long *b*-stroke from being seen; the infra-red photograph -*ed. pr.* Pl. 10- suggests these parts due to dirt in a straw pock); the *k*-part does not indicate a *taw* as clearly as mentioned in the *ed. pr.* (some blotting is involved). All this makes a *nun* preferable, not certain.

- i 6(8) (i d 4-4,5); in order to read *ṭp̄ry* the suggestion of *qof* instead of *reš*, with word divider between *pe* and *reš*, can easily be avoided by taking the thin bit of ink at the spot as the end of a long *b*-stroke from the preceding line (McCARTER, 1980, p. 53).

- i 7(9) (i c 7-5); a *dalet* or *lamed* instead of *ṭet* (<sup>c</sup>*tm*) are impossible, because *ṭet* is perfectly clear (*ed. pr.* Pl. 9 as well as Pl. 2).

- i 7(9) (i c 7-10); a *lamed* with wd. to read |w'l| is not impossible; some ink of a wd. may be visible (cf. also *ed. pr.* Pl. 9, but surface damage may be misleading).

- i 7(9) (i c 7-24) *taw* or *mem* instead of suggested wd. to read *ḥtt* or *ḥtm*; both proposals are possible, but only the upper right hand top would be preserved; in fact a wd. would be rather unusually low on the line.

- i 8(10) (i d 6-6) *ṣade* instead of *ḥet* to read *nšš* is impossible; the *ḥet* is clear (upper part of right hand *a*-stroke as well as the thin cross bars are clear) and also identifiable on Plts. 2 and 10 of the *ed. pr.*

- i 8(10) (i d 6-11) *dalet* instead of *reš* to read *ṣdh*; some secondarily deposited ink with dirt is clearly involved at the long tail, but the ink of the lower tail part differs from that and is most probably primarily deposited; a *dalet* however cannot be completely ruled out because of the slight possibility of the ink being secondary (fig. 5b).

- i 9(11) (i c 9-14) a *mem* to the right of *yod*, to read *myn*, is possible (together with *nun*, *kaf*, *ḥet*, etc.), with ink of the uppermost tip visible (point *gl* in the *ed. pr.*).

- i 10(12) (i c 10-2) *šin* or '*alef*' instead of *ḥet*, to read '*sb* or '*b*', are not possible; the *ḥet* is perfectly identifiable from e.g. Pl. 9 in the *ed. pr.*

- ii 6-12 *reš* instead of *dalet*, to read *y<sup>c</sup>br*, is not possible; ink of the tail would have been visible among the brown dirt; also there is no wd. to the left of *dalet*.

- ii 13-31 *reš* instead of *qof*, to read *ṛḥm*, is not possible; the ink of part of the upper right hand stroke of *qof* is clear (see *ed. pr.* Pl. 11); also the wd. after *ḥet* cannot be avoided.

- ii 17-6 *samek* instead of *waw*, to read *spr*; there is one detail giving



preference to a *samek* (or *yod*), namely the probable left hand opening between *d1* and *d2* strokes, but damage cannot be excluded for certain; apart from that the head (unclear shape) and tail would be exceptionally small. However, some straw-pock damage is involved and clearly also much removal of ink occurred without surface damage (as general on this part of comb.ii). All this makes a *samek* possible, but it has no clear support (fig. 5c).

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RESPONSE TO LECTURES OF PROF. E. PUECH AND  
DR. G. VAN DER KOOIJ

Meindert DIJKSTRA

Before some of you start to think I am a jack of all trades, I have to confess that in your illustrious company I am more the eye of the observer, than a trained paleographer. I am grateful that our hosts extended the title of today's subject to aspects related to the paleographical aspects of the Balaam-text. I cannot add anything substantial to the paleographic deliberations brought forward by Prof. PUECH and Dr. VAN DER KOOIJ. I had only a three-page summary of Prof. PUECH's lecture and my notes of the first part of Dr. v.d. KOOIJ's lecture concerning book and script, which prevents me from discussing their work in depth. Moreover, my response yesterday was certainly too long, so that today I will try to meet the time-limit.

Firstly, I will go back to a part of the lecture, which Dr. v.d. KOOIJ presented on Tuesday, the part about the location and lay-out of the inscription. This lecture, combined with the one by Dr. IBRAHIM was extremely helpful to clarify the circumstances under which the plaster text was destroyed and to reconstruct its position within a reasonable degree of probability. In my opinion further reconstruction and study of the text can continue on a stronger basis than before. From the outset the one-column lay-out appeared to me the most plausible one from epigraphic point of view—Prof. PUECH will agree on this point—, but the all-out effort to present the evidence as complete as possible is in my opinion one of the real assets to this symposium.

On the basis of the lay-out as presented by Dr. VAN DER KOOIJ, I suggest another placement for the fragment, which LEMAIRE thought in his "disposition" to belong to the end of a second column<sup>1</sup>. I mean fragment V(d), which was added to Combination V, but actually was found isolated from the other fragments of this Combination<sup>2</sup>. HOFTIJZER and LEMAIRE agree that the blank after

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<sup>1</sup> Cf. André LEMAIRE, "La disposition originelle des inscriptions sur plâtre de Deir 'Alla", *SEL* 3 (1986) 79–93. See also Prof. WEIPPERT's lecture.

<sup>2</sup> Cf. VAN DER KOOIJ, *ATDA*, 152. It might be noted that ascription of fragments to a certain combination is very helpful in most cases, but cannot always be

the word *kšd* probably indicated the end of a part of the inscription<sup>3</sup>. Perhaps, the rest of the line was left uninscribed, though we cannot be certain of it. On the other hand we do not find small blanks between textual unities<sup>4</sup>. It struck me that HOFTIJZER said: "It is excluded that this line was the last line of a column"<sup>5</sup>. If Prof. HOFTIJZER expresses himself so strongly, I take it almost for the gospels truth. The question remains where it was located in the column.

A new aspect or new chapter in the inscription is introduced by writing in red ink. In his reconstruction Prof. PUECH placed all the fragments with red ink in the top lines of the inscription (Combination III)<sup>6</sup>. This seems to indicate that no other red lines were written in the inscription. That is, if one assumes that the one in II.17 is complete<sup>7</sup>. Dr. VAN DER KOOIJ however pointed out that fragment III(a) written in red ink contains clearly *tb*, rejecting Prof. PUECH's reconstruction on this particular point. This fragment and also fragment IV(a) suggest together the existence of another line written in red ink somewhere in the inscription.

Is it possible that fragment V(q) contained the end of the chapter above the red line II.17? Unfortunately, it does not join, though neither colour nor condition of the surface seems to contradict such a location. When given a position after lines II.15–16 there is still space for a few words following the blank. Because no *setuma*'s are found in the preserved text, it seems reasonable to assume that the text had a *petucha*. There is, in my opinion, no real difficulty in understanding the words *ld<sup>c</sup>t.s\*pr.dbr.fnh.<sup>c</sup>l.lšn* to be the opening or title of a new section, certainly if the reading *s\*pr* (CAQUOT/LEMAIRE, McCARTER, HACKETT etc) is accepted. In that case it could introduce another *spr*, a document containing a "word

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taken as a definite clue. Also a fragment such as XII(c) was found isolated from the combination it belongs to (I a–c). In general, the groups III–VIII are related to Combination I (above the slanting line, which forms the top edge of Combination II), whereas group IX–XI are to be located somewhere between the upper and lower part of Combination II (private communication of Dr. G. VAN DER KOOIJ).

<sup>3</sup> Cf. HOFTIJZER, *ATDA*, 259,269; LEMAIER, *SEL* 3(1986) 86f.

<sup>4</sup> HACKETT et al. assumed a *vacat* at the beginning of the inscription, see my response 1 n. 26 for the possibility to restore fragment III(f) at the beginning.

<sup>5</sup> Cf. HOFTIJZER, *ATDA*, p. 269.

<sup>6</sup> Cf. PUECH, *La vie de la Parole*, pp. 15ff.

<sup>7</sup> HOFTIJZER, *ATDA*, p. 244 however assumes that line II.17 is the continuation of a sentence beginning in II.16. In that case the writing in red ink may have started in II.16. Also McCARTER, HACKETT and LEVINE translate II.17 as if it is the continuation of a preceding sentence/verse (LEVINE).



against wormwood on the tongue", i.e. an incantation or charm<sup>8</sup>. *ldt* at the beginning reminds me of the exclamation/warning found at the beginning of *KAI II* "Attention!"<sup>9</sup>.

But does the fragment make sense in this context? In II.15 we find the words *šlt.mlk.ssh.wš[?]/l\*[t]*. HOFTIJZER's translation: "What a king asks for is a horse, what [...] asks for ..." reflects his opinion that the sentence has proverbial character<sup>10</sup>. Is it pure coincidence that in this fragment V(q) the word *ssh* "a horse/his horse"<sup>11</sup> turns up again? I would like to suggest the following completion:

*šlt.mlk.ssh. The kings desire is a horse,*  
*wš[?]/l\*[t].ssh.r[x]x the desire of a horse is a ...*<sup>12</sup>.

The proverbial character of these words is certainly strengthened by such a completion. The big problem remains: what exactly could the meaning of such a proverb be in this context. Most probably its quotation has something to do with the prophet's warning through which he asked his people to abandon their request, whatever that request was (II.16)<sup>13</sup>.

Prof. PUECH commented in passing on the divergent opinions concerning the language. I quote "Les désaccords, on le voit ne peuvent être plus grands ni les opinions plus variées." Well, after three days of discussion I am not so sure about the extent and intensity of our disagreements. Before this symposium, the paleographic and linguistic assessment of the text had already engendered a broad spectrum of classifications such as that the script was Aramaic and the language Aramaic, or that the script was Aramaic but not the language. The classification Aramaic is often modified to peripheral Aramaic (even a dead end in the development) or "ein von Ka-

<sup>8</sup> See also the word *lš[t?]* II.37, perhaps *bqr.lš[t]* "by intoning the incantation"?

<sup>9</sup> Cf. DONNER/RÖLLIG, *KAI II*, p. 4; GIBSON, *Syrian Semitic Inscriptions I*, p. 17; JEAN/HOFTIJZER, *DISO*, p. 105.

<sup>10</sup> Cf. HOFTIJZER, *ATDA*, p. 244.

<sup>11</sup> Cf. HOFTIJZER, *ATDA*, pp. 243, 259, 289. If it is *ssh* "a horse", it is another Aramaic isogloss (cf. remark D. PARDEE), but a rendering "his horse" or even "mare" (Hebr. *sūsah* Song of Songs 1:9) is feasible as well, cf. RINGGREN, HACKETT etc.

<sup>12</sup> The word starting in all probability with a *resh* can only be guessed, but it was certainly something desirable to a horse. Possibilities? *rbq/rbš* "crib, resting-place"?

<sup>13</sup> The best parallel which came to my mind was *Quod licet Iovi non licet bovi*. CAQUOT/LEMAIRE, *Syria* 54 (1977) 207 thought already of a connection between *šltk* "your (oracular) consultation" and this gnomic line.

naanismen unterlaufenen aramäischer Dialekt"<sup>14</sup>. Others baptized the script "Gileadite" or Ammonite and thought the language to be a Southern Canaanite dialect<sup>15</sup>.

This symposium has enriched us, as I gathered from my notes with some more classifications. Firstly, some views diametrically opposed to one another: a Damascene Aramaic provenance of the scroll and so its language (Prof. LEMAIRE), whereas Prof. GREENFIELD does not find any conclusive evidence for an Aramaic classification. Mediating positions are taken up by Prof. McCARTER: archaic local dialect close to Ammonite and Moabite, Dr. DAVIES: a border-zone language, and yesterday Prof. WEIPPERT, I quote: "a peripheral language which is not yet Aramaic, but is about to become Aramaic".

I will not go over all the evidence again. I am glad that Prof. PUECH stressed the point once more that script and language are different, not immediately interrelated phenomena, so that the kind of script cannot have a definite say in the matter of the language of our text<sup>16</sup>. In the discussion the so-called *Nimrud*-ostrakon is repeatedly conjured up as a specimen of Ammonite script-ductus and language, but in a recent article B. BECKING proved, convincingly in my opinion, that whatever the script-tradition might be, its content is not necessarily a list of Ammonite names<sup>17</sup>. Three short inscriptions of phase M are according to Prof. PUECH "manifestement de langue et écriture araméennes". As far as I can see the linguistic identification Aramaic is only applicable to the inscription on the jar saying *zy šr*<sup>c</sup> "belonging to Sharû'a (?)"<sup>18</sup>. The few major inscriptions from the Ammonite area may show some similarities, even

<sup>14</sup> Cf. S.A. KAUFMANN, *BASOR* 239 (1980) 73a; McCARTER, *BASOR* 239 (1980) 50; H.P. MÜLLER, *ZAW* 94 (1982) 215.

<sup>15</sup> Cf. McCARTER, *BASOR* 239 (1980) 50a; HACKETT, *The Balaam Text*, 9-19, idem, *Orientalia* 53 (1984) 57ff; B. HALPERN, "Dialect Distribution in Canaan and the Deir Alla Inscription", in: D.M. GOLOMB (ed.) *Working with no data. Semitic and epigraphic studies presented to Thomas O. Lambdin*, Winnowa Lake 1987, pp. 137f.

<sup>16</sup> See also HACKETT, *Orientalia* 53 (1984) 60.

<sup>17</sup> Cf. B. BECKING, "Kann das Ostrakon ND 6231 von *Nimrud* für Ammonitisch gehalten werden?", *ZDPV* 104 (1988) 59-67.

<sup>18</sup> McCARTER's suggestion for *šr*<sup>c</sup> "gatekeeper" is not very convincing without parallels (*BASOR* 239 (1981) 50f). The religious function of the stone, which was probably a loomweight, seems to me very questionable (cf. HOFTIJZER, *ATDA*, p. 274f). A personal or divine name cannot be excluded, cf. the root *šR*<sup>c</sup> in Hebrew/Ugar. (KTU 1.19.I.14 "surge"?); *šārū*<sup>c</sup> "deformed" or something similar.



idiosyncrasies in their script compared to the Deir 'Alla script (*šadē, kaph, tēth*) but their language certainly reflects different dialects, a fact which becomes even more disturbing when the Deir 'Alla text proves to be a century older (ca. 800 BC) than is usually assumed.

As was remarked earlier in this symposium, the text on plaster is almost certainly a copy of a pre-existent *Sepher Balaam* a fact which infers that the written text could be considerably older than the inscription, not to mention the tradition beyond it. If our text was destroyed during the famous earthquake, which rocked both sides of the Jordan-valley in the days of Uzziah and Amos (Zach. 14:5, Amos 1:1)<sup>19</sup> and shows signs of wear and tear, a date around or even before 800 confirmed by C<sup>14</sup> datings, becomes almost inevitable<sup>20</sup>. It brings our text not only within the range of datings of the Amman Citadel text (9th–8th c.BC)<sup>21</sup>, but it becomes contemporary with the Mesha-inscriptions from Diban and Kerak (after 860 BC), the Kilamuwa-inscription from Zincirli (ca. 850); the Melqart-stela of Barhadad (between 850–810 BC<sup>22</sup>), the inscription of Zakkur of Hamath and Luash (somewhat after 800 BC)<sup>23</sup> and the Tell Fakhariyeh inscription (end 9th c.BC), i.e. the majority of them are datable, as far as I know within the range of fifty years and maybe less. And none of these inscriptions are written in the same vernacular.

It is certainly a pity that we do not have a comparable text from the kingdom of Judah or Israel, but even a superficial comparison with the Mesha-inscription shows that the language of the plaster text cannot be a Hebrew dialect, unless Moabite is classified as a kind of Hebrew as well<sup>24</sup>. The linguistic variety between the seven

<sup>19</sup> Cf. LEMAIRE, SEL 3 (1986) 91 n. 9; PUECH, *La Vie de la Parole*, p. 14, a point of view repeated during this symposium.

<sup>20</sup> Cf. IBRAHIM/VAN DER KOOIJ, *ADAJ* 30 (1986) 142; VAN DER KOOIJ, *Studies in the History and Archaeology of Jordan* 3 (1987) 109 and again the second part of his lecture today.

<sup>21</sup> Though this dating is only based on paleographical data, cf. F.M. CROSS, *BASOR* 193 (1969) 13–19; G. VAN DER KOOIJ, *Studies in the History and Archaeology of Jordan* 3 (1987) 109f.

<sup>22</sup> Cf. W.T. PITARD, "The Identity of Bir Hadad of the Melqart Stela", *BASOR* 272 (1988) 3–19.

<sup>23</sup> Cf. A.R. MILLARD, *PEQ* 111 (1978) 23.

<sup>24</sup> Cf. WESSELIUS, *BiOr* 44 (1987) cols. 591f. His linguistic assignment: Hebrew with some deviations from standard Biblical Hebrew is certainly not tenable in the light of all the linguistic evidence. S. SEGERT, "Die Sprache der Moabitischen

mentioned texts, which played their part in the discussion about the language of the Balaam-text is certainly bewildering. The variety in scripts and dialects in 9th century Canaan (in general the nations on both sides of the Jordan) reminds me of the situation of the Dutch dialects in the medieval Netherlands<sup>25</sup>. There was neither a standard-language, nor a standard-script. There were only local vernaculars and local scripts with their own idiosyncrasies and spelling conventions. These and other epigraphic finds (e.g. the ostraca of Samaria from the same period)<sup>26</sup> demonstrate that we are no longer able to conceive of a straight-line development for North West Semitic scripts and languages.

The picture of the language map is confusing as early as the Late Bronze period, in which two major alphabetic writing-systems prevailed each with their local variants. For example, the script of the archaic alphabetic corpus of Serabit el-Khadim shows clearly its own peculiarities and conventions compared to the variety of the proto-Canaanite linear scripts. The scarce linguistic information obtained from South Canaanite inscriptional material points also to notable phonological, morphological and lexical differences between, for instance, the texts of Serabit and comparable Ugaritic prose texts<sup>27</sup>. The elimination of alphabetic cuneiform and gradual standardization of linear scripts in the Iron I period does not necessarily imply standardization of orthography and language. VAN DER KOOIJ proved convincingly that adoption of the Egyptian way of writing with a pen-brush, also called broad-nib ink writing, represents the archimedean point in the development of early alphabetic scripts. Though it was used for more than one of the existing archaic scripts, basically one tradition survived and spread through the Levant and further<sup>28</sup>.

Inscription', *Archiv Orientalni* 29 (1961) 197-267; GIBSON, *Syrian Semitic Inscriptions I*, p. 72.

<sup>25</sup> Cf. J.M. VAN DER HORST/F.J. MARSCHALL, *Korte Geschiedenis van de Nederlandse Taal*, Amsterdam 1989, pp. 39ff.

<sup>26</sup> Cf. A.F. RAINEY, 'Towards a precise Date for the Samaria Ostraca', *BA-SOR* 272 (1988) 69-74 who dates them ca. 785-782 BC.

<sup>27</sup> Despite of ALBRIGHT's essays there is no conclusive evidence that the Serabit texts were written in a linear twenty-seven letter alphabet. Relative and demonstrative pronoun are formed with /z/ and not /d/. Attributive use of demonstrative e.g. *bmgd* z; lexical items e.g. *bšn* Ugar. *ptn*; *mhb* st.WHB not in Ugarit etc. See M. DIJKSTRA/I.D.B. BIGGS, *Corpus of Proto-Sinaitic Inscriptions*, (AOAT) forthcoming; provisionally M. DIJKSTRA, *Phoenix* 34,2 (1988) 39-53.

<sup>28</sup> Cf. G. VAN DER KOOIJ, *Early North-West Semitic Script Traditions. An Archaeologi-*



This revolutionary development in writing tells us however, next to nothing about the spread of North West Semitic dialects, nor does it imply that 10th century Phoenicia or Palestine were the only cultural cores of the area<sup>29</sup>. Why should we expect or try to find more order and pattern in a period, of which the history is dominated by local kingdoms and tribal confederations. Every new text from this period confronts us with variety and anomalous phenomena (for example, the archaic, or archaizing script of the Tell Fakhariyeh inscription). The situation does not seem to improve according to our standards when the Balaam-text belongs to the latter part of the 9th c. BC., that is still a hundred years before the beginning of the Neo-Assyrian domination, the period during which Aramaic developed into a kind of *lingua franca* or standard Aramaic to meet the administrative needs of the Neo-Assyrian empire in the West.

In our modern usage the terms Canaanite and Aramaic seem to have become mutually exclusive, whereas a rather great number of texts discovered show to a greater or lesser extent linguistic phenomena ascribed to both. A condition which induces some scholars to speak of mixed languages. I see little merit in such a statement, because in reality no other languages exist than mixed languages and dialects<sup>30</sup>. The problem seems not only to be a question of definition but also of the right nomenclature. Perhaps, we should admit with HOFIJZER the relative value of the distinction<sup>31</sup> and learn to avoid the denominator Aramaic for texts, composed in the mixed, local East-Canaanite dialects (roughly East of the Beqa-valley and the Jordan: Hamath, Aleppo, Deir 'Alla and Diban in the 9th and early 8th c. BC) before Aramaic emerged as *lingua franca*. Otherwise new classifications may compel us to identify (Old)Aramaic dialects *avant la lettre*<sup>32</sup>. Of the seven inscriptions

cal Study of the Linear Alphabetic Scripts up to c. 500 BC; *Ink & Argillary*, Leiden 1986; idem, *ATDA*, 31ff; idem, *Studies in the History and Archaeology of Jordan* 3 (1987) 107f.

<sup>29</sup> Cf. the remarks of E.A. KNAUF/C.J. LENZEN, "Edomite Copper Industry", *Studies in the History and Archaeology of Jordan* 3 (1987) 83.

<sup>30</sup> Cf. HALPERN, *Dialect Distribution*, p. 136.

<sup>31</sup> It might be noted again that HOFIJZER only choose to classify the Balaam-text as Aramaic after he expressed his doubt about the classical distinction between Aramaic and Canaanite, cf. HOFIJZER, *ATDA*, p. 300, see also *TUAT* II/1, p. 139; also HACKETT, *Orientalia* 53 (1984) 58.

<sup>32</sup> Cf. e.g. KAUFMAN, *BASOR* 239 (1980) 73a; idem, *Maarav* 3/2 (1982) 146 n. 22 who identifies the dialect of Deir 'Alla as peripheral Southern Aramaic com-

mentioned above the Tell Fakhariyeh bilingual, the Melqart Stela and the Zakkur-inscription are the earliest examples of evident Aramaic inscriptions<sup>33</sup>. When applying the classification Aramaic more appropriately for relevant texts as the bilingual of Tell Fakhariyeh, the Zakkur-inscription, the Sfire-treaties etc. from Aram Naharaim (ca. 800 BC onwards), we could continue to use Canaanite as the general denominator for the languages of the Levant (including the East-Canaanite dialects of the other side of the Jordan) up to the Neo-Assyrian period.

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pared to the central Aramaic dialects that served as basis of the better known Aramaic dialects of later periods!

<sup>33</sup> Cf. S.A. KAUFMAN, "Reflections on the Assyrian-Aramaic Bilingual from Tell Fakhariyeh", *Maarav* 3/2 (1982) 145ff. In my opinion the Melqart stela and the Zakkur-inscription take up a position between the Balaam-text and the Aramaic-Assyrian bilingual. They could also be classified among the East-Canaanite texts as classic examples of mixed dialect and style.



THREE SHORT COMMUNICATIONS AND AN ADDITIONAL  
NOTE BASED ON REMARKS MADE DURING THE SYMPOSIUM

THE FIRST OF THESE TWO VOLUMES IS A HISTORY OF THE  
CITY OF NEW YORK, FROM THE FIRST SETTLEMENT  
TO THE PRESENT TIME.



DEUX OBSERVATIONS À PROPOS DES RAPPORTS  
ENTRE LE TEXTE DE DEÏR 'ALLA (COMBINAISON I)  
ET LA BIBLE

J.-M. HUSSER

*La première observation* concerne un point d'histoire des traditions. La diversité des traditions bibliques relatives au personnage de Balaam est un fait bien connu de la critique. Quoi de commun entre le prophète exemplaire de fidélité à Yahvé que nous dépeint l'une des strates de la péricope des Nombres<sup>1</sup>, et le Balaam responsable de l'apostasie des fils d'Israël à Péor selon la tradition sacerdotale (Nb 31,8.16, Jos 13, 22)? entre le devin ridicule, moins visionnaire que son ânesse (Nb 22, 22-35), et le magicien redoutable mis en échec par Yahvé des récits deutéronomistes (Dt 23,5-6; Jos 24,9-10)? Il s'agit pourtant du même personnage, désormais historiquement bien attesté, et les accointances du récit de Nb 22-24 avec la tradition transjordanienne ne font plus de doute. L'importance de l'inscription de Deïr 'Alla ne saurait être surestimée pour l'étude de l'histoire des traditions. Si ces traditions remontent à un personnage historique, quelle qu'ait été l'ambiguïté de ses rapports avec l'une ou l'autre des tribus israélites, leurs divergences sont le fait d'interprétations et d'utilisations différentes d'un fonds commun. Permettez-moi de présenter ici une hypothèse rendant compte de l'histoire des traditions bibliques relatives à Balaam.

Même si l'on n'a pas encore épuisé toutes les directions de recherche sur ce sujet, nul ne peut nier aujourd'hui que la péricope des Nombres s'inspire pour une part de la tradition prophétique transjordanienne. De nombreuses affinités littéraires, tant dans la prose que dans les oracles, ont été relevées avec l'inscription de Deïr 'Alla – je n'y reviens pas. Le récit biblique a en outre retenu cette capacité du devin jordanien à entrer en contact avec le monde divin

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<sup>1</sup> C.-à-d. Nb 22-23\*, moins l'épisode de l'ânesse.

pendant la nuit et décrit la familiarité de ses dialogues nocturnes avec Dieu<sup>2</sup>.

L'intérêt de cette constatation réside précisément dans l'utilisation d'une tradition prophétique étrangère pour élaborer une histoire édifiante, prenant position sur la question des faux prophètes en Israël. Puisque le personnage de Balaam et ses oracles semblent bien connus en Israël, on comprend mal qu'on ait pu lui faire assumer le rôle exemplaire de fidélité à la parole divine si sa réputation était effectivement aussi sombre que le laisse entendre la tradition sacerdotale (Nb 31,8.16; Jos 13,22). Notre hypothèse est que la péricope des Nombres a utilisé, dans les étapes successives de sa rédaction, en plus de la tradition transjordanienne, une tradition proprement israélite relative à Balaam, à la fois littéraire et populaire, proche encore de la réalité historique (que nous ignorons), mais amorçant déjà une interprétation de l'activité du célèbre devin qui permettra l'utilisation qui en est faite en Nb 22-24.

Cette hypothèse développe et réajuste, grâce aux données présentement en notre possession, une proposition faite naguère par M. NOTH<sup>3</sup>. Après d'autres, celui-ci constatait le caractère adventice de la péricope des Nb parmi les traditions rassemblées autour du thème de l'"Hineinführung in das Kulturland", et avec lesquelles elle n'a rien à voir. Pour NOTH, il s'agit d'un cycle légendaire qui se développa autour du sanctuaire de Baal Péor, et qui n'aurait dû son insertion dans les récits de la conquête qu'au fait qu'il se serait trouvé associé à une antique tradition sur Balaq<sup>4</sup>. A partir de cette "Grundlage", le fonds proprement israélite se développa en deux directions. D'un côté la figure d'un Balaam hostile et dangereux, en relation avec Madiân et ses rois, tué par les Hébreux lors de l'expédition punitive contre les Madianites (Nb 31 & Jos 13): cette ligne de développement correspond au document J.

D'un autre côté, une tradition plus nuancée, transmise par E, où

<sup>2</sup> La reprise en Nb 22,9.20 (*wyb' 'lhm 'l bl'm lyh*) de la formule de DA I: 1 "et les dieux vinrent vers lui pendant la nuit" (*wy'tw. 'lwh. 'lhn. blyh*) n'est pas due au hasard, même si on la rencontre ailleurs dans la Bible (Gn 20,3 31,24).

<sup>3</sup> M. NOTH, *Überlieferungsgeschichte des Pentateuchs*, Stuttgart, 1948, 80-86.

<sup>4</sup> Primitivement indépendante, on trouverait trace de cette tradition sur Balaq en Jos 24,9-10, une fois supprimés les fragments 9b.10ab, considérés comme une notice dtr ajoutée postérieurement pour harmoniser ce passage avec Dt 23,5-6. Mais cette opinion est aujourd'hui abandonnée, Jos 24,9-10 est bien, en totalité, dtr.



Balaam apparaît aux côtés de Moab, et que NOTH voit évoluer en trois étapes: 1) la redoutable efficacité de la parole du sorcier constitue le matériau primitif, mais sa malédiction proférée contre Israël fut tournée par Dieu en bénédiction (cf Dt 23,5-6); 2) dans un second temps, Dieu interdit à Balaam de prononcer sa malédiction, et ce trait interviendrait comme un retard dramatique dans une composition littéraire; 3) enfin, la tradition transforme le magicien païen en un exemplaire homme de Dieu selon les normes israélites.

Poursuivant l'hypothèse de NOTH, G.W. COATS, dans son article intitulé "Balaam: Sinner or Saint?"<sup>5</sup>, attribue cette transformation du personnage qui, de l'incantateur à gages, devient un prophète yahviste type, à l'intégration de cette tradition attestée par Dt 23,5-6 et Jos 24,9-10 dans un récit légendaire dont le but est d'édifier l'auditeur. Mais la définition du genre littéraire de Nb 22-24 comme "légende" ne suffit pas à expliquer comment, d'un pécheur, on a pu faire un saint.

D'accord avec W. GROSS, H. ROUILLARD, K. SEYBOLD<sup>6</sup>, nous estimons que la théorie classique des sources appliquée à l'étude du Pentateuque ne peut rendre compte de l'élaboration de Nb 22-24<sup>7</sup>. Avec ces auteurs<sup>8</sup>, on peut dégager un récit primitif comprenant Nb 22,4c-21; 22,36-23,26; 24,11.25. L'ensemble ainsi circonscrit apparaît littérairement bien construit, avec une introduction (22,4c-6), puis deux parties principales, elles-mêmes articulées en deux volets (22,7-21 et 22,36-23,24), une conclusion (23,25-26 + 24,11.25).

Le corps du récit est organisé en un diptyque relatant d'une part les deux ambassades des émissaires de Moab, suivies chacune par un dialogue nocturne du devin avec son dieu, d'autre part, les deux

<sup>5</sup> G.W. COATS, Balaam: Sinner or Saint?, *BiR* 18, 1973, 21-29.

<sup>6</sup> W. GROSS, *Bileam. Literar- und formkritische Untersuchung der Prosa in Num 22-24*, München, 1974; H. ROUILLARD, *La péripécie de Balaam (Nb 22-24). La prose et les "oracles"*, Paris, 1983; K. SEYBOLD, in *BZ* 22, 1978, 144-145.

<sup>7</sup> Malgré la tentative de réhabilitation d'une "Quellenscheidung" due à L. SCHMIDT (Die alttestamentliche Bileamüberlieferung, *BZ* 23, 1979, 236-261) qui, abordant le texte par un autre biais que ses prédécesseurs (23,27-24,2), ne parvient pas cependant à renouveler la problématique.

<sup>8</sup> H. ROUILLARD, *Péripécie*, délimite ainsi ce qu'elle nomme le Niveau I du texte: 22,2-21; 22,36-23,26. GROSS, *Bileam*, qui se limite à l'étude du texte en prose, donne également comme unité littéraire primitive (Einheit I) (p. 147): 22,4c-6.7a\*c-21.36abc\*.37-41 23,1. 2abc\*.3.4a.5a\*b-7b.11-13b.14-18b.25 24,11.25.

oracles, précédés chacun par une scène sacrificielle. Cette structure symétrique est comme sous-tendue par le thème de l'opposition malédiction / bénédiction. Mais à travers ce thème structurel se trouve développé un thème théologique, constituant l'intention et l'argument principal du récit: l'allégeance inconditionnelle du prophète à la parole de Yahvé. La parole prononcée par Balaam sera celle que Yahvé "mettra dans sa bouche"<sup>9</sup>, et l'expression est employée avec assez d'insistance dans la péricope pour que l'on comprenne bien que l'auteur a voulu faire de ce devin étranger un véritable *nabi* de Yahvé<sup>10</sup>, je n'insiste pas sur ce point maintenant bien établi.

Mi 6,5 – dont B. RENAUD<sup>11</sup> situe la composition immédiatement après l'introduction du Dt – suit fidèlement la péricope des Nb:

Mon peuple, souviens-toi donc  
ce que tramait Balaq, roi de Moab,  
et ce que lui répondit Balaam, fils de Béor ...

La manière dont l'allusion est tournée suppose connu le récit de Nb 22-23. Au contraire, les deux notices de Dt 23,5-6 et Jos 24,9-10 s'en distinguent par une particularité significative: elles soulignent l'une et l'autre que Balaam a effectivement maudit Israël, mais que Yahvé lui-même tourna cette malédiction en bénédiction. Autrement dit, on retrouve le thème de l'opposition malédiction / bénédiction structurant également la péricope des Nb, mais plus du tout le thème théologique de la fidélité du prophète à la parole de Yahvé. Cette tradition dtr est donc, peut-être antérieure, en tout cas indépendante de Nb 22-24. Nous en voyons un indice supplémentaire dans le fait que Dt 23 et Jos 24 utilisent la racine *qll* (piel) pour "maudire", alors que Nb 22-24 n'emploie que *'rr* et *qbb*.

Cette observation permet d'apprécier comment l'auteur de la péricope des Nb composa son récit à partir de la tradition transjordanienne d'une part, dont il a repris nombre de traits relatifs au per-

<sup>9</sup> *hdb* 'šr yšm 'lhm/yhwš bpy: 22,38b 23,5.12.16a.

<sup>10</sup> Nb 22-24 fonctionne comme une véritable explicitation de l'idéal prophétique tel qu'on le trouve résumé en Dt 18,18b en deux expressions parallèles: "Je mettrai mes paroles dans sa bouche (*wntty dbry bpyw*) (A) / "et il leur dira tout ce que je lui ordonnerai" (*wdb* 'lyhm 't kl 'šr 'šwnw) (B). Les deux expressions reviennent dans la péricope de Bala'am comme un véritable leitmotiv, mais toujours séparément. Ainsi on rencontre la formule A en 22,38 23,5.12. 16, et la formule B en 22,8.35 23,3b.26.

<sup>11</sup> B. RENAUD, *La formation du livre de Michée*, Paris, 1977, 289-326.



sonnage de Balaam et d'expressions littéraire, et d'une tradition israélite d'autre part, dont Dt 23,5-6 et Jos 24,9-10 seraient des échos. Si nous nous risquons à définir en quoi consistait cette tradition israélite sur Balaam – indépendamment du problème que pose l'historicité de Balaam, roi de Moab – on peut supposer qu'elle véhiculait le souvenir que ce devin païen avait effectivement lancé des imprécations contre Israël. Apparemment, ces imprécations – malgré la redoutable réputation de Balaam – étaient demeurées sans effet, ce que, du côté israélite, on interpréta comme le fait d'une intervention de Yahvé qui changea cette malédiction en bénédiction.

Puisque la présence de ce devin aux frontières d'Israël est aujourd'hui archéologiquement prouvée, l'hypothèse qu'il ait pu, en des circonstances qui restent à établir, prononcer des imprécations contre tel ou tel groupe israélite nous paraît vraisemblable et rendre compte de l'image que l'on se fit de lui en-deça du Jourdain. Dans la péricope des Nb, il est même possible de voir une allusion à cet épisode dans le second oracle, et d'interpréter en ce sens Nb 23,23:

Il n'y a pas de présage qui vaille *contre* Jacob,  
ni de sort *contre* Israël<sup>12</sup>;  
il est dit à Jacob en temps voulu,  
à Israël, ce que El fait.

On remarque que ce verset contient précisément (23b $\beta$ ) l'une des nombreuses attestations du ND El dans les oracles bibliques de Balaam<sup>13</sup>, ainsi que la mention des oeuvres divines (*p<sup>c</sup>l 'l*) révélées par l'oracle, comme dans la première combinaison de Deir 'Alla (I:5)<sup>14</sup>. LEVINE n'a pas hésité à postuler "the derivation of the Balaam oracle from an El repertoire"<sup>15</sup>, et nous verrions dans ce verset le souvenir de la réaction israélite à ces imprécations lancées en vain contre le peuple de Yahvé.

De même, l'épisode de l'ânesse, dont l'originalité demeure si

<sup>12</sup> *ky l' nhs by<sup>c</sup>qb wl' qsm bysr'l*, où l'on peut comprendre le *b* dans un sens ad-  
versatif, "contre"; cf J. DE VAULX, *Les Nombres*, Paris, 1972, 280, et aussi H.  
ROUILLARD, *Péricope*, 301-309.

<sup>13</sup> Mis en parallèle à Jahvé en Nb 23,8, 'l est sans doute possible utilisé comme  
nom propre; il apparaît quatre fois dans les oracles du ch. 23 (contre deux fois  
Yahvé) (23,8.19.22.23) et trois fois dans ceux du ch. 24 (24,4.8.16).

<sup>14</sup> DA I:5 *wlkw . r'w . p<sup>c</sup>lt . 'lhn* : "venez voir les oeuvres des dieux".

<sup>15</sup> B.A. LEVINE, *The Balaam Inscription from Deir 'Alla, Historical Aspects*,  
in *Biblical Archaeology Today*, Proceedings of the International Congress on Biblical  
Archaeology (Jerusalem, 1984), Jerusalem, 1985, 354-365.

surprenante par rapport au reste de la péricope, et l'insertion si maladroite dans le cours du récit<sup>16</sup>, se comprendrait très bien comme une version populaire de cette tradition. Celle-ci pouvait facilement susciter un tel conte dont la verve ridiculise ce devin étranger réputé si puissant et rendu si faible devant Yahvé. Le fait que l'ange de Yahvé lui barre la route sous-entend qu'il était parti avec de mauvaises intentions, ce qui est en contradiction avec la version des faits restituée par Nb 22-24, mais correspondrait à ce que Dt 23,5-6 & Jos 24,9-10 laissent entendre. La fable de Balaam et son ânesse ignore visiblement le récit très théologique de Nb 22-23\*, mais est dans le droit fil de la tradition évoquée par Dt et Jos.

Ce Balaam payé pour maudire Israël, mais rendu inefficace par l'intervention de Yahvé, était suffisamment ambigu pour permettre deux développements inverses de la tradition: celui qui donna lieu au récit théologique de Nb 22-24 d'une part, celui qui renforça l'action négative du devin et que l'on saisit encore par bribes dans la tradition sacerdotale (Nb 31,8.16; Jos 13,22) d'autre part.

Si cette tradition israélite relative à Balaam - chaînon intermédiaire entre la tradition transjordanienne et la péricope des Nb - transmet le souvenir plus ou moins déformé d'un fait historique réel, on se demandera où et quand le situer. Faut-il le rattacher à la guerre entre Yoram et Mōsha de Moab (ca 845-840)? Nous laissons la question sans réponse, d'autant qu'il s'en ajouterait alors une autre: pourquoi l'épisode d'une escarmouche contre Moab fut-il transposé et intégré aux récits de la conquête?

*La seconde observation* porte sur une structure littéraire commune à l'inscription de Deir 'Alla et à la littérature prophétique biblique.

On a depuis longtemps remarqué que l'oracle transcrit par notre inscription est intégré à un récit racontant les circonstances dans lesquelles il fut délivré. Il y a un véritable souci de composition lit-

<sup>16</sup> H. ROUILLARD attribue la composition de cet épisode à une réaction défavorable au devin, résultat de débats sur la nature et la fonction du prophète (Dt 18,13-22) et pour contrer l'influence des oracles du Balaam de Deir 'Alla (cf *Péripcope*, p. 480). Mais on se demande alors pourquoi un récit composé postérieurement au Niveau I s'y intègre si mal. La critique classique attribue l'épisode à J depuis WELLHAUSEN; à la suite de H. GRESSMANN, *Mose und seine Zeit*, Göttingen, 1914, 326 s., on y voit l'utilisation par J d'une "Volkssage" tell qu'on en trouve ailleurs dans son oeuvre (Gn 3,1-5 32,24-32). L'origine populaire du récit ressort en outre du fait qu'on n'y trouve aucun des thèmes constituant la trame de la péripcope.



téraire, bien perceptible dans les premières lignes de la Combinaison I, et dont on peut énumérer les différents éléments.

Après la rubrique de la ligne 1, donnant le titre général, on observe quatre phases successives:

A - "Les dieux vinrent vers lui de nuit ..." (1 b-2).

B - "Et Balaam se leva de bon matin ..." (3-4a).

C - "Son peuple monta chez lui et ils dirent ..." (4).

D - "Alors il leur dit: "Asseyez-vous ..." (5 ss.).

Schématiquement, nous avons donc: A) une instruction divine adressée au prophète, B) une action symbolique accomplie par ce dernier devant témoins<sup>17</sup>, C) une question de la foule sur le sens de son attitude, D) un oracle.

On rencontre une situation exactement analogue avec l'une des grandes figures du prophétisme biblique, Ezéchiel.

Ez 24,15 La parole de Yahvé s'adressa à moi en ces termes:

16 "Fils d'homme, voici que je vais t'enlever brutalement la joie de tes yeux. Tu ne célèbreras pas le deuil, tu ne pleureras pas ..."

17 ...

18 Je parlai au peuple le matin; ma femme mourut le soir, et le lendemain matin, je fis selon ce qu'il m'avait or-

19 donné. Les gens me dirent: "Ne nous expliqueras-tu pas la signification pour nous de ce que tu fais?"

20 Alors je leur dis: "Il y a eu pour moi une parole de

21 Yahvé: Parle à la maison d'Israël: Ainsi parle le Seigneur Dieu: je vais profaner mon sanctuaire ...

Les actions symboliques sont fréquentes chez les prophètes, et particulièrement chez Ezéchiel. Ici, il est demandé au prophète de s'abstenir des pratiques du deuil après la mort de sa femme; à Deir 'Alla, au contraire, Balaam convoque des personnes pour jeûner et pleurer en leur présence deux jours durant. Cette attitude n'est pas la simple réaction d'effroi devant la gravité de la vision reçue, mais elle occupe, comme dans le cas d'Ezéchiel, une fonction essentielle dans la proclamation de l'oracle. Comme tous ces gestes prophétiques, elle signifie et actualise tout à la fois le message à transmettre<sup>18</sup>.

<sup>17</sup> Nous lisons bien, après la lacune de la ligne 3: *yzmn*, de la rac. *zmn* "inviter (à un repas)", sens attesté en araméen et en hébreu mishnique (*piel*), ce qui fait du jeûne de Balaam un geste prophétique.

<sup>18</sup> Cf J. LINDBLOM, *Prophecy in Ancient Israel*, Oxford, 1963, 171-172.

Mais en plus de cette fonction symbolique, l'attitude du prophète semble avoir également pour but de provoquer l'étonnement des témoins et de susciter leur question: "Que signifie ce que tu fais?" "Pourquoi jeûnes-tu? Pourquoi pleures-tu?" dit-on à Balaam. L'oracle intervient alors en réponse à cette question. On a là très certainement un schéma littéraire type servant de cadre à la transmission d'un oracle, et ce schéma comprend la séquence repérée au début de la Combinaison I:

- A) instruction divine,
- B) action symbolique,
- C) demande d'explication,
- D) exposé de l'oracle.

Cette observation confirme le caractère littérairement très élaboré du texte de Deïr 'Alla et peut, par comparaison avec la tradition biblique, aider à préciser sa fonction. D'autre part, malgré la relative rareté de ce schéma dans les textes prophétiques (on le retrouve intégralement en Ez 37,15 ss.), malgré aussi la distance chronologique séparant le texte de Deïr 'Alla et Ezéchiel, nous n'avons pas affaire à une pure forme littéraire; il n'est pas douteux qu'elle ait été l'expression d'un comportement social précis, inhérent à la fonction prophétique. Sur ce point – et bien curieusement – Ezéchiel semble très proche du comportement de Balaam.

Bala'am reçoit son message pendant la nuit et, dès le matin, convoque le peuple (ou les chefs du peuple)<sup>19</sup> pour commencer son "mime prophétique". Si nous prenons garde aux indications chronologiques du texte d'Ez cité précédemment, il semble bien qu'il, en fut de même pour lui: "La parole de Yahvé s'adressa à moi... Et je parlai au peuple le matin" (Ez 24,15.18). Puisqu'un même schéma prophétique paraît utilisé à Deïr 'Alla et en Ez 24, on peut supposer qu'Ezéchiel, lui aussi, entendait pendant la nuit ce qu'il annonçait le matin. Pareille façon de faire n'apparaît pas ailleurs chez les grands prophètes israélites; on l'attribuerait alors à l'une des caractéristiques d'Ezéchiel qui fut de renouer avec certaines pratiques du prophétisme archaïque. On le voit, par exemple, en train de consulter Yahvé au nom et en présence des anciens du peuple, assis autour de lui dans sa maison (Ez 8,1 ss.; 14,1 ss.; 20,1 ss.;

<sup>19</sup> Ligne 3: *yzmn . r[ʿy. ]qhl[. ]lwh*, "il convoqua les chefs de l'assemblée chez lui", selon une lecture proposée par E. PUECH, Le texte ammonite de Deïr 'Alla: Les admonitions de Balaam (première partie), in *La vie de la Parole de l'Ancien au Nouveau Testament*, Etudes offertes à P. Grelot, Paris, 1987, p. 19.



33,31 ss.), comme le faisait Elisée (2 R 6,32)<sup>20</sup>. Ces "audiences du matin" ont pu, à l'occasion du moins, être le lieu de la proclamation – à la fois gestuelle et verbale – d'une parole "vue" pendant la nuit.

Ces remarques ne prétendent nullement démontrer une dépendance quelconque entre le texte de Deïr 'Alla et Ezéchiel, mais seulement la permanence d'une forme littéraire et d'un comportement propres au prophétisme en-deçà et au-delà du Jourdain.

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<sup>20</sup> Cf W. ZIMMERLI, *Ezechiel*, BKAT XIII/1, Neukirchen, 1969, 108.209, qui évoque la possibilité d'une forme de clostration rituelle à laquelle aurait été astreint le prophète, au moins occasionnellement (cf Jr 36,5; Ne 6,10).

## REMARKS ON THE CLASSIFICATION OF THE NORTHWEST SEMITIC LANGUAGES\*

John HUEHNERGARD

The present communication, like several others offered at this symposium, concerns the dialect of the Deir 'Allā plaster text. It is part of a larger project on the classification of Central and Northwest Semitic, and is thus a report on work in progress.

There has been much debate at this symposium, and indeed since the appearance of the *editio princeps*,<sup>1</sup> about the proper classification of the Deir 'Allā plaster text; some writers have expressed the opinion that the text represents an essentially Aramaic dialect, others that it reflects instead a variety of Canaanite. Most of the discussion has been from a synchronic, purely descriptive point of view. In the present paper, I propose to take a diachronic perspective and to move the discussion into the larger theoretical framework of linguistic classification. I was happy to hear Prof. McCARTER in his presentation on the language of the text suggest that the Deir 'Allā dialect seemed to fall somewhere in the middle, *between* Canaanite and Aramaic. My goal here is to show, from a historical linguistic point of view, how such a situation could arise.

For some two decades now linguists have been re-evaluating traditional schemes of classification of the Semitic languages, and a consensus seems to be emerging of a new gross genetic classification, one that has had as its most articulate proponent Robert HETZRON.<sup>2</sup>

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\* I wish to thank the organizers of the Deir 'Allā symposium, Prof. Jacob HOFIJZER and Dr. Gerrit VAN DER KOOIJ, for the opportunity to present this communication at the symposium and for including it in this volume; the written version has been revised only slightly from what was presented orally. I am also grateful to Jonas C. GREENFIELD, Prof. HOFIJZER, Jo ANN HACKETT, André LEMAIRE, and Dennis PARDEE for their helpful comments on the oral presentation.

Note that angle brackets, < >, enclose graphemes.

<sup>1</sup> J. HOFIJZER and G. VAN DER KOOIJ, *The Aramaic Texts from Deir 'Allā* (Leiden: Brill, 1976).

<sup>2</sup> R. HETZRON, "La division des langues sémitiques," in A. CAQUOT and D. COHEN, eds. *Actes du premier Congrès international de linguistique sémitique et chamito-sémitique, Paris 16-19 juillet 1969* (The Hague/Paris: Mouton, 1974): 181-94; "Two Principles of Genetic Reconstruction," *Lingua* 38 (1976): 89-108; "Semitic Languages," in Bernard COMRIE, ed. *The World's Major Languages* (New York: Oxford, 1987): 654-63.



In HETZRON's scheme, as in many others, the Semitic languages are most fundamentally divided into an eastern and a western branch; the eastern branch contains only Akkadian and, we may now almost certainly add, Eblaite, while the western branch, which includes all the other languages, exhibits a feature that is innovative with regard to Proto-Semitic and Akkadian, namely, the use of the predicative verbal adjective, *qatvla*, as a perfective active verb. Within West Semitic we have again two branches: the conservative South Semitic, which includes Ethiopian Semitic, the Modern South Arabian languages and probably the Old South Arabian languages; and an innovative branch including Arabic, Canaanite, and Aramaic, which HETZRON labels Central Semitic. The innovative feature shared by this group of languages is that the form *yaqtulu*, which was originally simply the perfective form *yaqtul* marked for subordinate clauses (as in Akkadian), came to replace the earlier *yvqattul* as the main clause imperfective form.

Up to this point I and many others are in agreement with HETZRON's classification. With his internal subdivision of the branch he calls Central Semitic, however, we run into difficulty. Here HETZRON relies on the form of the feminine plural of the prefix-conjugation as his diagnostic feature: he suggests that Aramaic *yiq-tālān* preserves the early Semitic form; thus, in his view, Arabic *yaqtulna* and Hebrew *tiqtolnā*, both of which exhibit the ending *-nā*, reflect a shared innovation and constitute a separate innovative branch, Arabo-Canaanite, within Central Semitic. In a paper published in 1987, however, I argued that in the earliest dialects of Aramaic at our disposal, namely, the various Old Aramaic inscriptions, the second and third person plural feminine must likewise have ended not in *-ān* as in later Aramaic, but rather in *-nā* as in Hebrew and Arabic.<sup>3</sup> I attempted to show this both on formal historical linguistic grounds and on the basis of some hard evidence in the Fkhariyye form *pṣn* 'let them bake', where we should expect a ⟨Y⟩ to appear if the ending were *-ān* as in *\*liṣṣayān*. The formal difficulties involved in using *yaqtulnā* as a diagnostic feature were also noted the same year in an article by R.M. VOIGT, who would nevertheless still group Canaanite and Arabic together as a separate sub-branch.<sup>4</sup> In my opinion the form *yaqtulnā* or *taqtulnā* of

<sup>3</sup> "The Feminine Plural Jussive in Old Aramaic," *ZDMG* 137 (1987): 266-77.

<sup>4</sup> Rainer M. VOIGT, "The Classification of Central Semitic," *JSS* 32 (1987): 1-21.

Hebrew and Arabic, which is also found in Ugaritic, reflects instead the earliest Semitic situation, and so the similarity of the forms in those languages is the result of a shared retention and therefore not significant for classification.

I would propose a more traditional subdivision within HETZRON's Central Semitic group. The members of the conventional Northwest Semitic division—Aramaic, Canaanite, Ugaritic—all exhibit a few features in common that are not found in Arabic. At least one of these features must, in my view, be considered a shared innovation vis-à-vis a common Central Semitic and thus evidence of a genetic subgroup divorced from Arabic. This feature, already noted, for example, by H.L. GINSBERG in his 1970 article on the Northwest Semitic languages,<sup>5</sup> is the regular pluralization of monosyllabic triradical nouns—that is, *qatl*, *qill*, and *qutl* forms—by means of *a*-insertion, in addition to the usual external plural markers. It may be objected that this feature was inherited from Proto-Semitic and even from Proto-Afroasiatic, and is therefore, like shared retentions generally, not significant for classification.<sup>6</sup> It is certainly undeniable that *a*-insertion to form plurals is a trait going back at least to Proto-Semitic, for it is attested not only in the Northwest Semitic languages under investigation, but also in Arabic, in north Ethiopic, in the Modern South Arabian languages, and perhaps originally even in Akkadian.<sup>7</sup> What is unique to Ugaritic, Aramaic, and the Canaanite dialects, however—and this has not previously been given proper weight as a diagnostic feature—is the *distribution* of this feature, for only in those languages is *a*-insertion in the plural base both restricted to and obligatory in *qutl* nouns, and only in those languages is the addition of an external plural marker also mandatory (so that all such plurals are invariably doubly marked).<sup>8</sup> The presence of each of these factors in the languages in question

<sup>5</sup> H.L. GINSBERG, "The Northwest Semitic Languages" in *The World History of the Jewish People*, vol. 2: *Patriarchs*, ed. B. MAZAR (Givatayim: Jewish History Publications/Rutgers University, 1970): 102–24, esp. p. 102.

<sup>6</sup> Joseph H. GREENBERG, "Internal *a*-plurals in Afroasiatic (Hamito-Semitic)," in J. LUKAS, ed. *Afrikanistische Studien* (Berlin: Deutsche Akademie der Wissenschaften zu Berlin, Institut für Orientforschung, 1955): 198–204.

<sup>7</sup> See my "Three Notes on Akkadian Morphology," in "Working with No Data": *Semitic and Egyptian Studies Presented to Thomas O. Lambdin*, ed. D. GOLOMB (Winona Lake, Ind.: Eisenbrauns, 1987): 181–94, esp. pp. 183–88.

<sup>8</sup> For more detail concerning this feature the reader is directed to my forthcoming "Central Semitic and Northwest Semitic."



points clearly to an innovation in a common ancestor. Within HETZRON's larger Semitic scheme this subgroup ought properly to be labeled North Central (West) Semitic; we might also refer to it as the Syro-Palestinian (as opposed to the Arabian) branch of Central Semitic, but there seems little harm in retaining the time-honored Northwest Semitic, as long as it is borne in mind that its immediate ancestor is Central Semitic rather than Proto-Semitic.

The internal sub-classification of dialects within the Northwest (or North Central) Semitic branch has also continued to be a much-discussed topic. In my opinion, several dialects and sets of dialects, namely, the substratum Northwest Semitic dialects of the Palestinian Amarna letters, Hebrew, Phoenician, and Moabite,<sup>9</sup> all exhibit a significant number of shared innovations and thus also share a common ancestor and constitute a genetic subgrouping. The independent existence of this subgroup, which we may conveniently label Canaanite, must date at least to the fourteenth century since it includes Amarna evidence. The innovative features of Proto-Canaanite may be summarized as follows.

First, for the D and C suffix-conjugation forms we may confidently reconstruct Proto-Northwest Semitic *\*qattila* and *\*haqtila*, since these are of course the ancestors of the Aramaic forms, and since Ugaritic likewise probably had *qattila* for the D suffix-conjugation, as evidenced by a form in syllabic cuneiform;<sup>10</sup> these early forms became *\*qittila* and *\*hiqtila* in Hebrew, of course, but also, to judge from the evidence, in Phoenician and in at least one of the Amarna dialects, where we find the C form */hihbiʔe/* 'he hid' (*hi-ih-bi-e*, EA 256:7, from Pella).<sup>11</sup>

Second, the first person singular pronoun, originally *\*ʔanākū* as in Ugaritic, after becoming *\*ʔanōkū* with the unconditioned change of *\*ā > \*ō*—the so-called Canaanite shift, which is not in itself a significant feature—dissimilated to *ʔanōkī*. Also Proto-Canaanite, and

<sup>9</sup> Ammonite and Edomite are also usually included among the Canaanite languages, but since they offer no evidence for the particular diagnostic features noted in the following paragraphs, they may not, obviously, be labeled Canaanite on the basis of those features.

<sup>10</sup> Viz., *šal/ša-li-ma* for Ugar. */šallima/* 'has paid', in J. NOUGAYROL, et al., *Ugaritica 5* (Paris: Imprimerie Nationale, 1968): 187–89, text 96: *passim*.

<sup>11</sup> For a detailed discussion of the history of the D and C suffix-conjugation forms in Northwest Semitic, Canaanite, and Hebrew, see my "Historical Phonology and the Hebrew Piel," in Walter R. BODINE, ed., *Linguistics and Biblical Hebrew* (Winona Lake, Ind.: Eisenbrauns, forthcoming).

more important, was the concomitant change of the first person suffix-conjugation ending  $*-tū > -tī$ ; Aramaic and Ugaritic, of course, unfortunately offer no evidence for their early vocalization of this ending.

Third, the first person plural marker in Proto-Northwest Semitic was probably  $*-nū$  to mark the subject on the suffix-conjugation (*qatalnū*) but  $*-nā$  to mark both the direct object on verbs and the possessive on nouns. Proto-Canaanite saw the generalization of  $*-nū$  in all environments, whereas Proto-Aramaic leveled  $*-nā$  in all environments; again we have no evidence for Ugaritic.

While we therefore have evidence concerning only two of these features in each of Ugaritic and in Aramaic, nevertheless that is sufficient, in my view, to establish the existence of a Canaanite branch of Northwest Semitic distinct from Ugaritic and Aramaic already in the fourteenth century.<sup>12</sup> That Ugaritic and Aramaic constitute separate branches of Northwest Semitic is accepted by most scholars. Thus, it seems most reasonable to suggest that Ugaritic, Proto-Canaanite, and Proto-Aramaic are to be considered distinct and coordinate branches within Northwest Semitic.

To review, I would point to three features as characteristic of the newly emergent Canaanite dialect group: the change of  $*qattila$  and  $*haqtila$  to  $*qittila$  and  $*hiqtila$ ; the change of  $*anākū$  to  $*anōkī$  and the concomitant change of the first person singular suffix  $*-tū$  to  $*-tī$ ; and the generalization of the suffix  $*-nū$  for the first person plural. Another development, shared by many of the Canaanite dialects later, but not something we can register as a Proto-Canaanite feature, is the phonological realization of the emerging Central Semitic category of the definite article as a doubling of the initial consonant of a word, with a preposed  $*ha-$  when the form was phrase-initial.<sup>13</sup>

When we turn to examine the dialect of the plaster inscription

<sup>12</sup> Here I must disagree with S. KAUFMAN, for example, who has recently stated that "the division between Canaanite and Aramaic cannot be traced back any distance into the second millennium," in "The Classification of the North West Semitic Dialects of the Biblical Period and some Implications Thereof," in Moshe BAR-ASHER, ed., *Hebrew and Aramaic Panel Session, Proceedings of the Ninth World Congress of Jewish Studies, Jerusalem, August 4-12, 1985* (Jerusalem: Magnes, 1988): 41-57, esp. p. 42.

<sup>13</sup> See T.O. LAMBDIN, "The Junctural Origin of the West Semitic Definite Article," in H. GOEDICKE, ed., *Near Eastern Studies in Honor of William Foxwell Albright* (Baltimore: Johns Hopkins, 1971): 315-33.



found at Deir 'Allā we find no evidence for any of the features I have just cited as characteristic of Proto-Canaanite. Features that *are* cited as Canaanite by various scholars are, as far as I can tell, all retentions from the common fund inherited from Proto-Northwest Semitic; they are *not* part of the package of shared, specifically *Canaanite* innovations of the fourteenth century. We have therefore no firm linguistic grounds to call the Deir 'Allā dialect Canaanite. On the other hand, there is also not much evidence that would contradict the inclusion of Deir 'Allā in the Canaanite fold. The two items that are most frequently cited in this connection are the use of ⟨Q⟩ to write the Deir 'Allā reflex of Proto-Semitic \*ḏ and the writing of the third person masculine singular suffix on plural bases as ⟨W-H⟩. Concerning the ⟨Q⟩ I can only state emphatically that the use of an orthographic feature for linguistic classification is very risky; certainly the appearance of ⟨Q⟩ for etymological \*ḏ may in no way be cited as evidence that the dialect is Aramaic. After all, the orthography of the text is, everyone agrees, based on Aramaic precursors; therefore, if \*ḏ remained a distinctive consonant in this dialect, it is only reasonable to expect that the scribe would write it with the same character as was used in Aramaic texts.<sup>14</sup> At all events, the writing of \*ḏ with ⟨Q⟩ shows only that that phoneme had not merged with \*š; in this restricted sense, we may state that the Deir 'Allā dialect did not participate in one sound change characteristic of most Canaanite dialects, namely the merger of \*ḏ and \*š. But the significance of this unshared phonological feature for classification is marginal at best; after all, we assign only minor significance to the very same situation in Hebrew in its preservation of \*š as distinct from the merged reflex of \*š and \*ṯ, against the otherwise common Canaanite merger of those phonemes. Those who would point to the ⟨Q⟩ as significant for classification must also, on the same grounds, conclude that the

<sup>14</sup> See Jo Ann HACKETT, *The Balaam Text from Deir 'Allā* (Harvard Semitic Monographs 31; Chico, Calif.: Scholars, 1984): 111–13; eadem, "The Dialect of the Plaster Text from Tell Deir 'Allā," *Or.* 53 (1984): 57–65, esp. p. 61. This point was also stressed by P. McCARTER in his presentation at the symposium.

Note that the same argument may be made concerning the prefixed ⟨L⟩ for the negative \*lā- as evidence of an Aramaic dialect, as suggested by André LEMAIRE, "La langue de l'inscription sur plâtre de Deir 'Allā," *GLECS* 24–28 (1979–84): 317–40, esp. p. 325: the appearance of this feature shows not that the *dialect* was linguistically Aramaic, but only that the *orthography* was ultimately borrowed from Aramaic practice.

dialect of the Fkhariyye inscription is not Aramaic because its scribe wrote the presumably still-distinct reflex of \*θ with ⟨S⟩ rather than ⟨Š⟩.

The import of the writing ⟨W-H⟩ for the third person suffix is less clear. Certainly this writing is typical of Aramaic dialects. But a feature is of value for linguistic classification only if it is an innovative feature, not if it is one preserved from an earlier phase of dialect history. The problem with the ending ⟨W-H⟩ is that its origin is obscure; the traditional explanation, as often noted, is far from satisfactory. The most likely development is that proposed by W.R. GARR in his *Dialect Geography*, suggesting a simple early sound change common to most of the Northwest Semitic area, a change that cannot be regarded as taxonomically distinctive.<sup>15</sup> Thus we must hesitate to place much weight on the ending ⟨W-H⟩ for the classification of the Deir ʿAllā dialect.

We have seen, therefore, that there are no firm grounds to label the Deir ʿAllā dialect Canaanite, and only marginal grounds against such a label.

We may now examine features characteristic of Aramaic. We must begin by noting that many features typical of later Aramaic dialects may not be considered Proto-Aramaic developments, since they are not attested in all of the Old Aramaic texts at our disposal. I have already mentioned that the uniquely Aramaic form *yiqṭālān* does not yet exist as a jussive in several Old Aramaic inscriptions. The uniquely Aramaic development of a feminine plural nominal ending *-ān* is also not complete in the Sfire inscription, where we still find an example of the earlier Semitic *\*-āt*. The triumph of the form *miqṭal* as the G-stem infinitive is likewise still not complete at Sfire. The ending *\*-aʾ* as the phonological realization of definiteness is only just emerging in the early inscriptions, as shown in 1971 by LAMBDIN (see n. 13), and later corroborated in the Fkhariyye text. And the orthography of the early texts obviously shows that the common Aramaic set of consonant mergers reflects a later development. Since these features may, *a priori*, not be called Proto-Aramaic, I would suggest that they result from the spreading influence of one or two prestige dialects, probably of prominent urban centers, during the ninth and eighth centuries.

<sup>15</sup> W. Randall GARR, *Dialect Geography of Syria-Palestine, 1000-586 B.C.E.* (Philadelphia: University of Pennsylvania, 1985): 107-9.



There are, however, other innovative features that can be labeled Proto-Aramaic, that is, that can be considered innovations shared by all Aramaic dialects. One is the change of what was probably a vocalic \* $\eta$  to  $r$  in the words for 'son', 'daughter', and 'two'.<sup>16</sup> Another is the levelling of the ending \*- $\tilde{n}\tilde{a}$  for the first person plural, whereas Proto-Canaanite, as we saw above, levelled \*- $\tilde{n}\tilde{u}$ . In the morphology of the verb we may further cite the creation of a new Ct-stem \**hittaqtal* and the complete loss of the N-stem; the significance for classification of a shared loss is debated by linguists, but in view of the important morpho-lexical rearrangement in the verbal system that accompanied the loss of the N, i.e., the new morpho-lexical load that had to be carried by the  $t$  forms, I believe that we must attribute that loss to the period of a common ancestor.

Let us now return to the Deir 'Allā dialect. We noted earlier that there is no evidence for or against calling it a Canaanite dialect, with the minor exception of the non-merger of \* $\tilde{q}$  and \* $\tilde{s}$ . When we consider the dialect in terms of the *Proto-Aramaic* features just mentioned, we must note first that the appearance of *br* in Balaam's name is probably not relevant; names may not be used as linguistic data for the dialectology of the texts in which they appear,<sup>17</sup> and we may consider *br* in *bl<sup>c</sup>m brb<sup>c</sup>r* to be part of the character's name.<sup>18</sup> There is therefore overt evidence for only one of our *Proto-Aramaic* features, viz., unlike all Aramaic dialects, the Deir 'Allā dialect clearly has an N-conjugation. Thus, the Deir 'Allā dialect does not participate in one of the significant innovations according to which we identify Aramaic. By definition, therefore, the Deir 'Allā dialect may not be considered Aramaic. We may also not suggest, as has naïvely been done, that this dialect is Proto-Aramaic,

<sup>16</sup> See D. TESTEN, "The Significance of Aramaic  $r < *n$ ," *JNES* 44 (1985): 143–46. It should be noted that, as TESTEN observes, this change also occurred in the ancestor of the Modern South Arabian languages, a fact that must apparently be ascribed to coincidence.

<sup>17</sup> See my "Northwest Semitic Vocabulary in Akkadian Texts," *JAOS* 107 (1987): 713–725, esp. pp. 714–15.

<sup>18</sup> As Dennis PARDEE rightly pointed out to me, however, *br* in *bl<sup>c</sup>m brb<sup>c</sup>r* is, strictly speaking, a common noun linking to proper nouns. Nevertheless it is entirely possible that the whole chain *bl<sup>c</sup>m brb<sup>c</sup>r* used to refer to the character is external to the dialect in which the text was written; cf. *klmw br hy*[?] at the beginning of the Phoenician Kilamuwa text (*KAI* 24:1), and note that *br* is not written as a separate word in *brb<sup>c</sup>r* at Deir 'Allā. Certainty that the Deir 'Allā dialect had *br* as opposed to *bn* would require its occurrence in a fully contextual setting, such as 'he said to his son' or the like.

or "on the way to becoming Aramaic, but was not yet," or trying to be Aramaic.<sup>19</sup> That could only be true if the text were several centuries earlier than any Aramaic we have, which is clearly not the case. One has a curious picture of a dialect struggling desperately to catch up with its more progressive relatives; languages simply do not develop that way, and people do not speak proto-languages. In other words, by the period of the Deir 'Allā text, Aramaic texts and dialects have been attested for a century. Aramaic exists in the eighth century BCE, and the Deir 'Allā dialect either is or is not part of it; we may not posit the existence of Aramaic and "almost Aramaic" at the same point in time. Since the Deir 'Allā dialect does not manifest an important Proto-Aramaic development, we must conclude that it is not part of Aramaic.<sup>20</sup>

To recapitulate, we must, I believe, conclude that the dialect of the Deir 'Allā plaster text is not Aramaic and not demonstrably Canaanite. Here we run into what seems at first to be a dead end. To the best of my knowledge, previous writers, even those who recognize that the Deir 'Allā dialect seems to have some of both Canaanite and Aramaic, have assumed, tacitly or explicitly, that the dialect must be one or the other, though Prof. HOFTIJZER in the *editio princeps* (pp. 300-1) did question whether one *had* to choose between the two; most, however, insist that the dialect must be either Canaanite or Aramaic, usually with some degree of "mixing"; that is, it is "Aramaic with some Canaanite features," or "Canaanite with some Aramaic features." Even GARR, who at the end of his *Dialect Geography* situates all attested early-first-millennium dialects on a continuum, must conclude that the Deir 'Allā dialect is closer to Aramaic than it is to Canaanite,<sup>21</sup> as though those are the only choices available to us. In most of these discussions, the absence of a consistent *historical* linguistic perspective results in a certain fuzziness concerning the likely linguistic developments that could have produced an apparently unusual phenomenon like the dialect of

<sup>19</sup> Ernst Axel KNAUF, review of HACKETT (above, n. 14), *ZDPV* 101 (1985): 187-91.

<sup>20</sup> The suggestion of Professor LEMAIRE (see his contribution to this volume), that the text must be dated earlier than the inscription, cannot be countered. I find it difficult to accept, however, since it leaves us with no methodological control over the material, linguistically or otherwise. Furthermore, as I will propose presently, the suggestion is not, linguistically at least, necessary.

<sup>21</sup> P. 229 of the volume cited above in n. 15.



the Deir 'Allā text, which is neither Aramaic nor necessarily Canaanite. Some writers, to be sure, have suggested that the dialect represents a new language, proposing to call it variously Midianite, Gileadite, or the like. But these writers, too, generally describe their new language as closer to, or a dialect of, either Canaanite or Aramaic.

Nearly all who have considered this problem, I believe, make an assumption that is linguistically unnecessary and insupportable, namely, that after the second millennium, a Northwest Semitic language must be, or derive from, either Canaanite or Aramaic (so that, as noted above, Deir 'Allā is said to be either "Canaanite with some Aramaic features" or "Aramaic with some Canaanite features"). Likewise GARR lists Aramaic and Phoenician as the linguistic extremes of his dialect continuum; everything else must fit along a line *in between these two*. GARR's presentation of his linguistic features as a series of criss-crossing isoglosses in a tangle of mutually influencing dialects is both thorough and reasonable for a dialect geography; but, as noted earlier, that approach to the data by design shows only part—the bare surface—of the true linguistic reality.

Let us return to the mid-second millennium. There we find a cluster of Northwest Semitic dialects identifiable by the features discussed near the beginning of this paper. Around this time, in the northwestern part of the region covered by this cluster, a dialect that we will identify by its later texts as Ugaritic begins to be distinguished from the Northwest Semitic matrix by the innovation of a number of significant features in phonology and morphology. By about 1400 we may also isolate a sub-group we will call Canaanite, which has likewise separated itself from the rest of Northwest Semitic with the various significant innovations described earlier.

The question at this point is: what does the separation of Ugaritic and Canaanite leave? The usual answer to the question is, Aramaic, but this answer is not correct. Aramaic, like Ugaritic and Canaanite, is identifiable as an *innovative* dialect cluster, that is, as a group of dialects that share a common set of linguistic developments. It is simply not the case that once Canaanite and Ugaritic leave the Northwest Semitic fold the remaining speakers whose dialects were not affected by those developments *all* immediately took up the specifically Aramaic developments we find several centuries later. What was left after the specifically Canaanite dialectal de-

velopments, rather, was simply the remaining, still-developing Northwest Semitic matrix of dialects that were not affected by those developments. Some time later—by the ninth century at the latest, but otherwise impossible to determine in the absence of any earlier data—some of these remaining dialects in turn innovated the common set of features we consider to be typical of Aramaic. Some, but not all; that is, there must, *a priori*, have been speakers of Northwest Semitic dialects whose dialects were essentially unaffected either by the changes by which we characterize Canaanite or by those common to what we call Aramaic. In other words, there must have been Northwest Semitic dialects that were, by definition, neither Canaanite nor Aramaic. That we have little evidence for such dialects is interesting, but not surprising, since for the most part the texts we have come from major centers, which, as GARR has noted, are most susceptible to linguistic change. But that there were dialects unaffected by either Canaanite or Aramaic innovations is to be expected. In the plaster text from Deir 'Allā we have one such dialect. That, I believe, is why it seems to be neither fully Canaanite nor fully Aramaic in appearance: we have no evidence that it shares in *any* of the innovative changes that distinguish either Canaanite or Aramaic. That is also why the Deir 'Allā dialect seems so conservative: vis-à-vis the Canaanite and the Aramaic dialects, it *is*; the features it shares with Canaanite or with Aramaic are not the innovations, but features that are inherited from the common Northwest Semitic stock, features such as the following: the lack of a graphically explicit definite article; the relic consecutive prefix-conjugation for past tense; the preservation of the final *-t* in the third feminine singular of the suffix conjugation; the N-stem; forms like the second feminine singular suffix *-kī*, the infinitive *da<sup>c</sup>t* 'to know', and the imperative *likū* 'go'. This is not to suggest that the Deir 'Allā dialect is Proto-Northwest Semitic, without having undergone any development since the mid-second millennium. Rather, I would suggest, it was undoubtedly subject to areal phenomena like the loss of case-vowels and the subsequent change of final *\*-at* to *\*-ā* in feminine nouns. The dialect could also have undergone some innovative developments *not* found in *either* Canaanite or Aramaic, but these, as far as we can tell, are not evidenced in the small, unvocalized sample of the dialect at our disposal.

In this paper I have tried to show that in a discussion of linguistic classification, both the so-called "Stammbaum" theory or genetic



classification and wave-theory or dialect geography must be brought to bear on the evidence. When both the synchronic view and the historical development are considered, the picture emerges of a dialect that need not be classified as a form or sub-branch of either Aramaic or Canaanite, but rather as a representative, thus far unique, of another independent branch of the larger Northwest Semitic family.

## ASPECTS OF THE LITERARY STRUCTURE OF COMBINATION I

AL WOLTERS

The study of the Balaamite fragments found at Tell Deir 'Alla has hitherto been concerned mainly with matters of reconstruction, paleography and linguistics. One of the results of the initial round of studies of these fragments has been the recognition that they are the remnants of a literary, as distinct from a monumental, inscription. In fact they represent, in the words of A. LEMAIRE, "le premier exemple d'un texte littéraire en araméen ancien."<sup>1</sup> Since the first nine lines of Combination I can now be read as an almost continuous unit, we have a large enough passage of this remarkable ancient composition to explore the possibilities of a specifically literary analysis. It is with that exploration, limited to the first nine lines of Combination I, that the present paper deals.

It is true of course that any literary analysis will be significantly hampered by the remaining gaps in the text, and by the substantial disagreements which still exist among scholars about the interpretation of many details within the passage in question, but I hope to show not only that there is enough that is now clear to allow a preliminary literary analysis, but also that such an analysis can in turn help us to resolve some of the questions of detailed interpretation.

I shall take as my point of departure the overall arrangement and reconstruction of the text proposed by LEMAIRE in 1985,<sup>2</sup> although I shall differ from him in a few readings, and although I recognize that some of his restorations are quite speculative. What follows is my own presentation of the relevant text, together with my own proposed translation:

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<sup>1</sup> A. LEMAIRE, "L'inscription de Balaam trouvée à Deir 'Alla: épigraphie," in *Biblical Archaeology Today* (Jerusalem, 1985), p. 322.

<sup>2</sup> LEMAIRE, "L'inscription," p. 318. See also A. LEMAIRE, "Les inscriptions de Deir 'Alla et la littérature araméenne antique," in *Comptes Rendus de l'Académie des Inscriptions et Belles-Lettres* (1985), pp. 270-285, esp. 279-280, which is different on a few minor points.



## COMBINATION I, 1-9

1 <sup>1</sup> spr bl <sup>c</sup> m br b <sup>c</sup> r 2 <sup>2</sup> š. ḥzh. <sup>2</sup> lhn	TITLE	Inscription of Balaam son of Beor, the seer-man of the gods.
N. 3 h <sup>3</sup> wy <sup>3</sup> tw. <sup>2</sup> lwh. <sup>2</sup> lhn. blylh. 4 w[ymlw. <sup>2</sup> lw]h <sup>2</sup> .kml[y] <sup>2</sup> . <sup>2</sup> l. 5 wy <sup>2</sup> mrw. lb[ <sup>1</sup> c]m. br b <sup>c</sup> r kh. Q. 6 yp <sup>c</sup> [nhr] <sup>2</sup> . <sup>2</sup> hr <sup>2</sup> h. 7 <sup>2</sup> š l[htysrh. yp] <sup>c</sup> t	SCENE A	Behold, the gods came to him at night, and [spoke to] him according to these words, and they said to [Balaam] son of Beor thus: "The [Light] has shone its last; the Fire for [judgement] has shone."
N. 8 <sup>3</sup> wyqm. bl <sup>c</sup> m. mn. mhr 9 l.ymn. 10 l l h. 11 wlyk[h].ly <sup>2</sup> kl] 12 wbk <sup>4</sup> h.ybkh. 13 wy <sup>c</sup> l. <sup>c</sup> mh. <sup>2</sup> lwh. 14 wy[ <sup>2</sup> mrw.] lb <sup>1</sup> c <sup>c</sup> m.br b <sup>c</sup> r Q. 15 lm. tsm[.w]lm tbkh.	SCENE B	And Balaam arose in the morning, days, and cou[ld not eat], and he wept bitter tears. And his people came up to him and they [said] to Balaam son of Beor: "Why are you fasting and why are you weeping?"
N. 16 wy <sup>25</sup> mr.lhm. Q. 17 šbw. <sup>2</sup> hwkm. mh. šd[yn ]w 18 lkw. r <sup>2</sup> w. p <sup>c</sup> lt. <sup>2</sup> lhn.	SCENE C	And he said to them: "Return! I shall tell you what the <i>shaddayin</i> are ... Go on, consider the doings of the gods."
N. 19 <sup>2</sup> [h]n. <sup>2</sup> tyhdw. 20 <sup>6</sup> wšbw. šdyn. mw <sup>c</sup> d. 21 w <sup>2</sup> mrw. lš[m]š. Q. 22 tpry. skry. šmyn.b <sup>c</sup> bky. 23 šm.ḥšk. w <sup>2</sup> l. n <sup>7</sup> gh. 24 <sup>c</sup> tm. w <sup>2</sup> l. smr 25 ky. thby. ḥt.[b <sup>c</sup> ]b. ḥšk. 26 w <sup>2</sup> l. thgy. <sup>c</sup> d. <sup>c</sup> lm. 27 ky. ss <sup>c</sup> gr. ḥr <sup>8</sup> pt. 28 nšr. wq[ <sup>2</sup> ]. rḥmn. 29 y <sup>c</sup> nh. ḥ[sd. w]bny. nšš 30 wšdh. <sup>2</sup> prhy. <sup>2</sup> nph. drr. 31 nšrt. <sup>9</sup> ywn. wšpr	SCENE D	The gods have gathered together, and the <i>shaddayin</i> have met in assembly, and they have said to Sh[am]sh: "Sew up, bolt shut the sky with your cloud! Let darkness be there, and not brightness, gloom and not radiance; Yes, strike terror with the cloud of darkness, and do not remove it ever: hawk, swift, bat, eagle, and pelican, vultures, ostrich, stork, young of falcons, and owl, chicks of heron, dove, bird-of-prey, pigeon and sparrow.

My remarks on the literary aspects of this text will be organized under two headings, namely "colometric patterns" and "narrative architecture." For each of these categories I shall give a brief description and analysis, followed by a consideration of how they shed light on some of the remaining obscurities in the text.

It is clear from the layout of the text as I have presented it that I discern a colometric pattern in this passage of the inscription. In other words, it is my judgement (following a similar analysis by Victor SASSON<sup>3</sup>) that the text falls quite naturally into a series of dis-

<sup>3</sup> V. SASSON, "The Book of Oracular Visions of Balaam from Deir 'Alla,"

crete cola, which stand in various kinds of literary relation to each other.

The most obvious such relation is parallelism. We find this clearly illustrated, for example, in the bicolon which I have numbered 19 and 20, where the A-line ("the gods have gathered together") is clearly balanced by the B-line ("and the *shaddayin* have met in assembly"). We find a similar case in bicolon 13-14: "and his people came up to him/and they [said] to Balaam son of Beor." A special case is represented by the list of birds in cola 27-31, where the series of 15 ornithological terms falls readily into five lines of three terms each. We also note the clear case of internal parallelism in colon 15.

The recognition of parallelism as a literary feature of our text is a useful heuristic guideline in resolving a number of interpretative difficulties. In colon 2, for example, it tips the scales in favour of reading 𐤆𐤌 as a noun meaning "man" (so HOFTIJZER<sup>4</sup> and most others) against 𐤆𐤌 as Canaanite relative pronoun (so HACKETT<sup>5</sup>). Similarly, in colon 24, parallelism favours HOFTIJZER-VAN DER KOOIJ's reading 𐤌𐤌, understood with SASSON to mean "gloom",<sup>6</sup> over 𐤌𐤌 (so CAQUOT-LEMAIRE<sup>7</sup>) or 𐤌𐤌 (so MCCARTER<sup>8</sup> and HACKETT<sup>9</sup>). Once this has been established, the parallelism also justifies MCCARTER's bold assumption that the second obscure word in this colon (whether it is read as *skr* or *smr*) must mean something like "radiance."<sup>10</sup> The clear case of parallelism which we noted in bicolon 19-20 also has value for questions of exegetical detail. For one thing, it shows that 𐤆𐤌𐤌𐤌 and 𐤆𐤌𐤌 are synonymous terms, and since 𐤆𐤌 occurs in the Bible as a technical term for YHWH taking

*Ugarit-Forschungen* 17 (1986) 283-309, esp. 287-289. It should be added that my colometric analysis is quite different from SASSON's.

<sup>4</sup> J. HOFTIJZER and G. VAN DER KOOIJ, *Aramaic Texts from Deir 'Alla* (Leiden, 1976), p. 184.

<sup>5</sup> J.A. HACKETT, *The Balaam Text from Deir 'Alla* (Chico, CA, 1984), pp. 29, 31.

<sup>6</sup> V. SASSON, "Two Unrecognized Terms in the Plaster Texts from Deir 'Alla," *Palestine Exploration Quarterly* 117 (1985), pp. 102-103, and idem, "Oracular Visions," pp. 296-297.

<sup>7</sup> A. CAQUOT and A. LEMAIRE, "Les textes araméens de Deir 'Alla," *Syria* 54 (1977) 197.

<sup>8</sup> P. Kyle MCCARTER, "The Balaam Texts from Deir 'Alla: the First Combination," *Bulletin of the American Schools of Oriental Research* 239 (1980) 54.

<sup>9</sup> HACKETT, *Balaam Text*, p. 44.

<sup>10</sup> MCCARTER, "Balaam Texts," p. 54.



part in the assembly of the gods (so MULLEN<sup>11</sup>), both expressions in this context probably refer not to rebellion (*pace* SASSON<sup>12</sup>), but to legitimate deliberative assembly. For another thing, the bicolon in question shows that <sup>2</sup>lhn and šdyn are parallel terms, and may well indicate (*pace* SASSON<sup>13</sup>) that they are different designations for the same group of heavenly beings.

A particularly instructive example of the exegetical relevance of parallelism is the vexed question of the correct restoration of colon 4. It is commonly agreed, since CAQUOT-LEMAIRE's realignment of the fragments,<sup>14</sup> that there is a gap of 6 to 8 letters between the initial *w* and the sequence *h.km*. There is dispute, however, about how the gap should be filled, and how the letters after *h.km* should be construed. LEMAIRE and others read *w[ymlw. <sup>2</sup>lw]h kml[y]*, "and [they spoke] to him according to these words,"<sup>15</sup> whereas HACKETT, PUECH and others, making use of fragments Ve and XVc to fill the gap, read *w[yh]z. mħzh. kmš. <sup>2</sup>l*, "and he saw a vision like an oracle of El."<sup>16</sup> Without entering into the paleographic and morphological questions that are involved here, I would like to point out in this context that the solution proposed by LEMAIRE yields a clear parallel to colon 5, with *w[ymlw. <sup>2</sup>lw]h* balanced by *wy<sup>3</sup>mrw. lb[l]m.br.b<sup>6</sup>r*, and *kml[y]*<sup>2</sup> balanced by *kh*. Furthermore, this reading makes cola 3 and 4 stylistically analogous to cola 11 and 12, both involving the sequence "and they came . . . / and they said . . ." Consequently, since the alternative proposal of HACKETT and PUECH is stylistically awkward (making the grammatical subject of colon 4 different from that of 3 and 4) we conclude that literary considerations count against it and favour the reading of LEMAIRE, at least in its general thrust.

A final example of the heuristic value of parallelism is found in the bicolon 17–18, where there is a partial letter and a gap following *mħ.š*. According to VAN DER KOOIJ, the mutilated letter can be either a *dalet* or a *gimel*.<sup>17</sup> LEMAIRE reads <sup>2</sup>hwkm.mħ.šg[yh.lhyh] wlkw, "je

<sup>11</sup> E.T. MULLEN, *The Assembly of the Gods* (Chico, CA, 1980), pp. 230–31.

<sup>12</sup> See V. SASSON, "The Language of Rebellion in Psalm 2 and in the Plaster Texts from Deir 'Alla," *Andrews University Seminary Studies* 24 (1986) 147–154.

<sup>13</sup> SASSON, "Oracular Visions," pp. 306–307.

<sup>14</sup> CAQUOT-LEMAIRE, "Textes Araméens," pp. 193–194.

<sup>15</sup> LEMAIRE, "L'inscription," p. 318.

<sup>16</sup> HACKETT, *Balaam Text*, pp. 19, 33; E. PUECH, *Biblical Archaeology Today*, p. 356.

<sup>17</sup> HOFTIJZER-VAN DER KOOIJ, *Aramaic Texts*, p. 106.

vous montrerais combien gra[nd est le malheur] et venez ..."<sup>18</sup>. However, McCARTER and others read  $\text{h}^{\text{w}}\text{km.mh.šd[yn.p}^{\text{c}}\text{lw.}] \text{wlkw}$ , "I shall inform you what the Shaddayin have done. Now come ...". A glance at the next colon shows that McCARTER's proposal is stylistically preferable, since it makes  $\text{h}^{\text{w}}\text{lh}$  parallel to  $\text{šdyn}$  (as in 19–20), and  $\text{mh}$  plus verb parallel to  $\text{p}^{\text{c}}\text{lt}$ . Whether the verb is likely to have been  $\text{p}^{\text{c}}\text{lw}$  is doubtful, however. Perhaps SASSON is right in reading  $\text{zmmw}$ , "have conspired."<sup>19</sup> In any case, a verb is indicated, and in fact a trace of the verb is found in the  $w$  preceding  $\text{lkw}$ . This should not be taken as the copula, but as the ending of the missing verb,<sup>20</sup> so that  $\text{šbw}$  and  $\text{lkw}$  are also perfectly parallel.

Another literary figure which becomes clear if we recognize our text's colometric pattern is chiasmus. By reading  $\text{šd[yn]}$  in colon 17, we notice that the four cola 17–20 now evince the pattern  $\text{šdyn}$ ,  $\text{h}^{\text{w}}\text{lh}$ ,  $\text{h}^{\text{w}}\text{lh}$ ,  $\text{šdyn}$ . In fact these four lines together form a tightly constructed literary whole, pivoted around the repeated  $\text{h}^{\text{w}}\text{lh}$  at its centre.

There may be another example of chiasmus in the bicolon 6–7, if LEMAIRE's admittedly speculative restoration is right in reading  $\text{[yp]f}^{\text{t}}$  at the end of colon 7. The missing noun in colon 6, whether it is  $\text{nh}^{\text{r}}$ ,  $\text{lh}^{\text{b}}$  or something else<sup>21</sup>, is probably a poetic synonym for the sun, as is  $\text{š}$ , "fire(ball)," so that these lines contain the chiasmic series "shine, sun, sun, shine." On my reading then, this special bicolon, which is written in red ink in the inscription, would be the emphatically repeated announcement that the sun had "shone its last."

By way of conclusion to our discussion of the colometric pattern of the first nine lines of Combination I, we point out that this passage has many features in common with the poetry of the Bible and Ugarit. Whether or not it can be formally classified as poetry is probably a matter of definition. If colometric structure and regular parallelism are enough to define ancient Semitic poetry, then this part of the Balaamite inscription certainly qualifies. But if the use

<sup>18</sup> LEMAIRE, "L'inscription," p. 318.

<sup>19</sup> SASSON, "Oracular Visions," pp. 287–294.

<sup>20</sup> There is a space for three or four letters preceding the  $\text{waw}$ .

<sup>21</sup> LEMAIRE reads  $\text{nh}^{\text{r}}$  in "L'inscription" (1985), pp. 317, 318, but  $\text{lh}^{\text{b}}$  in "Les inscriptions", CRAIBL (also 1985), p. 279.



of the consecutive imperfect is a distinguishing mark of prose vis-à-vis poetry, then our text certainly does not qualify, since it is liberally interspersed with this verbal construction. Perhaps the Balaamite inscription is another example of what has been called "narrative poetry," which is well-attested in the Bible and other Northwest Semitic literature.<sup>22</sup> For our purposes it is sufficient to note that the colometric pattern and its associated literary features warrant the description of "the book of Balaam" as a fine example of ancient *belles lettres*.

We turn now to our second topic, which we have called "narrative architecture." It is important to bear in mind that our passage is in fact a narrative text, and not simply an oracle or prophecy without context. This is one of the many points of similarity between the Balaamite inscription and the story of Balaam in Num 22-24. If we analyze the structure of the narrative, it turns out to consist of four clearly delineated episodes or scenes, each beginning with a narrative section (N) and ending with a direct quotation (Q). If we include the title (the first two cola in our analysis) the surviving first part of Combination I thus has five separate components, corresponding to cola 1-2, 3-7, 8-15, 16-18 and 19-31ff. The last section is clearly the longest, and may have continued to the end of the work.

There are also five speakers or "voices" in the narrative. They are the narrator himself and the speakers of the four quotations: the <sup>3</sup>lhn (6-7), the people of Balaam (15), Balaam himself (17-21), and the šdyn (22-31 and beyond). If the <sup>3</sup>lhn are to be identified with the šdyn, the number of voices is reduced to four, but if we count the title as having its own "voice" (presumably that of the redactor or "publisher" as distinct from the narrator) we are back to five. The effect which this plurality of voices produces is one of great liveliness and movement in the narrative. SASSON is right when he states: "The First Combination presents a story which may be viewed as a dramatic piece."<sup>23</sup> The impression of liveliness and dramatic action is further enhanced by the fact that each voice speaks to its own audience. The narrator addresses an audience of his contemporaries, the <sup>3</sup>lhn address Balaam, Balaam's people ad-

<sup>22</sup> See J.C. DE MOOR, "Narrative Poetry in Canaan," *Ugarit-Forschungen* 20 (1988) 149-171, and the literature cited there.

<sup>23</sup> SASSON, "Oracular Visions," p. 285.

dress their prophet, Balaam himself answers his people, and the *šdyn* of colon 20ff. address the goddess Shamsh. As for the redactor, his audience was probably the coming generations of the Balaamite community.

An intriguing feature of the overall narrative architecture of the piece is that while a quotation is embedded in each narrative scene, Scene D in its entirety is embedded in the quotation of Scene C. This means that the speech of the *šdyn* (cola 22ff.) is a quotation within Balaam's narrative, which is in turn a quotation within the broader narrative of Scene C. This concentric structure has the effect of focussing all the attention on this speech of the *šdyn* to the goddess, and this speech is of course the burden of Balaam's oracle to his people. The literary structure here is a miniature analogue to that of Plato's *Symposium*, where the climactic speech of Diotima is related by Socrates, whose speech is in turn the last of a series of speeches embedded within the overall narrative of the dialogue. The technique involved is not just a matter of putting speeches within a narrative frame (as in the book of Job and many Platonic dialogues), but rather of repeating the whole framework-speech pattern *within* one of the primary speeches of the overall narrative. I am not sure whether this sophisticated narrative technique is found elsewhere in ancient Near Eastern literature. If it is, it might provide a valuable clue to the cultural milieu in which the Balaamite inscription found its home.

It is also noteworthy, in the light of this boxes-within-boxes literary structure, that the remarkable "bird passage" (the string of 15 birds' names in cola 27-31), is situated in the middle of the innermost box. It is a great pity that the extant inscription becomes very fragmentary after this bizarre ornithological episode. One wonders whether there is a return to the primary narrative level at the end of the piece, so that the bird passage is also pivotal in a chiasmic sense. In any case it seems clear that this unique passage, which constitutes a kind of literary *Fremdkörper* within the narrative, occupies a strategic position within the whole, and should probably be treated as a distinct literary unit.

This cursory examination of what I have called the narrative architecture of our text also sheds some useful light on a number of detailed questions of interpretation. Let me give three examples.

The word *šbw* in colon 17 has been taken by all students of the inscription as *šēbū*, "sit down," from the root *yšb*. It is of course also



possible to vocalize this verb as *šūbū*, "turn back," from the root *šwb* (later Aramaic *twb*), and we may well ask ourselves whether this interpretation is not more appropriate in the context. SASSON comments: "Since Balaam has a long and disheartening story to tell, he bids his audience to sit and listen,"<sup>24</sup> but this seems a rather trivial interpretation of the verb. Within the larger literary structure *šbw* is the first word of the quotation section of Scene C, which is in a sense the last scene, since it incorporates the following one. It therefore stands at a significant juncture in the narrative. Furthermore *šbw* is the first word spoken by Balaam himself after the buildup of the two previous scenes. After a period (probably lasting some days<sup>25</sup>) of fasting and bitter weeping, he breaks his silence and delivers himself of his prophetic oracle of doom. Is it likely that the first word he speaks to his people is an invitation to them to make themselves comfortable while he announces an impending disaster? To ask the question is to answer it. In short, the verb *yšb* seems singularly inappropriate as Balaam's climactic first word to his people.

On the other hand, the imperative of the verb *šwb* is an exhortation frequently uttered by the Hebrew prophets to their people when predicting God's judgement. For example, we read in 2 K 17:13:

The Lord warned Israel and Judah through all his prophets and seers: "Turn (*šūbū*) from your evil ways . . ."

It is striking that HOFIJZER in discussing *hzh* in I,1 refers to this biblical verse as "a clear parallel to our text",<sup>26</sup> but fails to note the parallel of *šbw* there with *šbw* here. There are also many biblical parallels of the imperative of *šwb* used absolutely, e.g. Jer 3:14: "Return (*šūbū*), faithless people, declares the Lord, for I am your husband" (see also Jer 3:12 and 3:22). It seems that the imperative of *šwb* can be a standard feature at the beginning of a prophetic oracle.<sup>27</sup> In all these cases the verb means as much as "repent", comparable to the *metanoie* of John the Baptizer and Jesus in the

<sup>24</sup> SASSON, "Oracular Visions," p. 294.

<sup>25</sup> This is the probable interpretation of the isolated word *ymn* preserved in colon 9.

<sup>26</sup> HOFIJZER-VAN DER KOOIJ, *Aramaic Texts*, p. 185.

<sup>27</sup> See W.L. HOLLADAY, *The Root Šūbh in the Old Testament* (Leiden, 1958), esp. pp. 137, 152.

New Testament.<sup>28</sup> I submit that this is by far the likelier meaning of *šbw* at this climactic point in the narrative architecture which is discernible in Combination I. Balaam exhorts his hearers to repent, to turn from their wicked ways, in order to ward off the divine judgement which he sees coming.

My second example concerns the verbs in the narrative component of Scene D (cola 19–21), where Balaam describes what happened in the assembly of the gods. In terms of overall literary design, this narrative section is structurally analogous to the corresponding narrative sections in the preceding three scenes, especially to that of Scene A (cola 3–5), which also consists of three parallel cola, as we saw earlier. There is one striking difference, however. All the verbs in the preceding narrative sections are examples of the consecutive imperfect, but all the verbs in this last narrative section are ordinary perfects. This feature has been noticed by some scholars,<sup>29</sup> but it continues to be puzzling.

My suggestion is that the use of the perfect here instead of the consecutive imperfect has to do with the fact that Scene D is embedded in Scene C, so that these verbs are part of *indirect discourse*. The use of the perfect here would then be similar to the use of the subjunctive under comparable circumstances in German.<sup>30</sup> To illustrate the point we might translate the relevant lines into German as follows:

die Götter *seien* zusammengekommen,  
 die Schaddajin *seien* im Rat aufgetreten,  
 die Götterversammlung *habe* zu Schamsch gesprochen.

The effect of this usage is to emphasize that Balaam does not tell this story as an eyewitness, but rather as reporter of what others have told him. Presumably he is alluding to the nocturnal visit of the *ʾlhn* recounted in Scene A. In characteristic prophetic manner he is in effect saying: "Thus say the *ʾlhn*." Balaam thus invokes no personal authority, but only the authority of his heavenly informants. We are suggesting, in other words, that by considering the unusual perfects in the last narrative section in the light of the overall

<sup>28</sup> See for example Mt 3:2 and 4:17.

<sup>29</sup> HACKETT, *Balaam Text*, p. 40, as well as H. WEIPPERT and M. WEIPPERT, "Die 'Bileam'—Inschrift von Tell Dêr 'Allā," *Zeitschrift des Deutschen Palästina-Vereins* 98 (1982), p. 88.

<sup>30</sup> H. LEDERER, *Reference Grammar of the German Language* (New York, 1969), pp. 125–129.



narrative architecture of their context, we can discern a subtle but significant feature of Balaam's role as subordinate spokesman of the gods.

Finally, I would like to make an exegetical point about the bird passage (cola 27-31). It was LEMAIRE who first demonstrated that this passage is simply a list of nouns referring to birds (or at least to winged creatures, since the bat is also included). He realized that words like *hṣpt* and *y<sup>c</sup>nh* were not verbal forms but also represented birds' names like the others in the context. The only difficulty which remained was the meaning of *ky* at the beginning of the series, which he translated "mais (à sa place?)," <sup>31</sup> but this seems rather forced.

My proposal, in the light of my earlier comments on narrative architecture, is to treat the five cola of the bird passage as a distinct block of material which does not itself exhibit a sentence structure. Accordingly, I propose to read *ky* in colon 27, not as a syntactic marker, but as another bird name.

There is in fact evidence for *ky* as a bird's name in the Hebrew Bible. G.R. DRIVER, in an article entitled "Job 39:27-28: the *Ky*-bird", (*PEQ* 104 [1972] 64-66), pointed out that the word *ky* in this passage should be taken as a noun designating some kind of vulture. For support he appealed to the LXX (*gyrs*) and its daughter versions, and to the Arabic cognate *kuy*, which is variously rendered "ibis," "bustard" or "pelican."<sup>32</sup> DRIVER was unaware that another ancient version, namely 11Q<sup>g</sup>Job, also supports an ornithological interpretation, since it renders *ky* in Job 39:28 as *ʿwz* "hawk". We are therefore fully justified in identifying *ky* in this Job passage as the name of a large predatory bird, possibly the hawk. There is every reason to believe that this is the same word as that which introduces the list of birds' names in the Balaamite inscription.

This still leaves the problem of how the bird passage is related to the cola which precede. It is possible that there is no grammatical connection at all, that the list of fifteen ornithological terms stands completely by itself, as a mysterious but unmistakable interruption of the narrative, like an inserted magical incantation with some occult meaning. There is also the possibility that the birds' names stand in apposition to the *ʿb hšk* which is threatened in colon 25.

<sup>31</sup> LEMAIRE, "L'inscription," p. 318.

<sup>32</sup> DRIVER, "The *Ky*-Bird," p. 65.

In that case  $\text{'}b$  should not be translated "cloud" (in any case, the common word for cloud in Canaanite and Aramaic is  $\text{'nn}$ ) but rather "swarm", closely related in sense to the root  $\text{'bb}$  or  $\text{'bh}$  meaning "be thick". In that case the birds constitute an apocalyptic swarm of winged creatures which will darken the sky. No doubt all of this is related to the ancient tradition that Balaam was involved in bird divination.<sup>33</sup> However, it is enough for our present purposes to note that *ky* is also a bird's name, and thus brings to completion the discrete literary unit of fifteen ornithological terms distributed over five cola.

With this example I conclude my discussion of aspects of the literary structure of Combination I. My purpose has been to show that the text under consideration, even in its present fragmentary condition, shows evidence of being a sophisticated piece of literary composition, and that recognition of its literary artistry can help us in solving a number of detailed questions of interpretation.

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<sup>33</sup> See the LXX in Num 23:23 (*oiōnismos*) and 24:1 (*oiōnois*), and Philo, *De Vita Mosis* I, 264 (*oiōnoskopia*).



## REFLEXIONS METHODOLOGIQUES SUR LE CLASSEMENT LINGUISTIQUE DE DAPT\*

Felice ISRAEL

§ 0.0. Parmi les différents problèmes que DAPT a posés, le problème relatif au classement linguistique n'a pas été jusqu'à présent résolu car il a été abordé, à notre avis, de façon incorrecte du point de vue méthodologique. Au cours des nombreuses analyses<sup>1</sup> des deux fragments, à l'analyse linguistique se sont superposées d'autres considérations de nature extralinguistique concernant l'archéologie, l'épigraphie, l'interprétation archéologique, historique, historico-littéraire et historico-religieuse. En outre, dans l'évaluation des faits linguistiques on a eu recours à des critères de logique binaire d'exclusion et d'inclusion qui, comme nous allons voir plus bas au § 1, ne tiennent pas compte de trois faits essentiels, soit: a) les faits linguistiques sémitiques ou bien sémitiques du nord-ouest communs ne présentent aucune utilité en vue d'un classement; dans certains cas, en effet ceux-ci peuvent être considérés comme des conservations; b) les faits orthographiques n'ont aucune valeur dans notre enquête; c) on peut appliquer une logique binaire d'exclusion-inclusion par exemple lors d'une confrontation entre l'hébreu biblique et les dialectes araméens littéraires, cela étant dû au fait que ces deux langues sont désormais le résultat d'un processus de standardisation; par contre, les textes épigraphiques reflètent des variantes dialectales locales et ceci vaut encore plus dans le cas de DAPT qui provient d'une zone limitée entre l'araméen et le cananéen<sup>2</sup>. Finalement, on doit remarquer que peu de chercheurs

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Un remerciement cordial à Mme. H. LOZACHMEUR de l'URA 1062 du CNRS pour l'aide généreuse dans la révision de mon texte français.

<sup>1</sup> Pour une bibliographie complète sur DAPT, voir A. LEMAIRE, *CRAIBL* 1985 pp. 270-85 et du même auteur dans *Deir Alla Symposium* pp. 55-57.

<sup>2</sup> M. WEIPPERT, *Deir Alla Symposium*: pp. 159ff.

ont explicitement choisi une perspective linguistique dans laquelle situer DAPT à l'intérieur du sémitique du N-O: c'est ce que G. GARBINI<sup>3</sup>, R.W. GARR<sup>4</sup> et J. HUEHNERGARD<sup>5</sup> ont fait de façon explicite.

§ 0.1. En considérant l'état des faits que nous venons de mentionner, nous présenterons nos réflexions dans l'ordre suivant: § I: examen de quelques faits linguistiques que d'autres collègues ont cités pour soutenir leurs thèses: pour chacun de ces faits nous montrerons pour quelle raison ils ne résolvent pas le problème. Au § II nous signalerons ici quelques-uns des classements qu'on a proposés et nous expliquerons rapidement pourquoi de telles définitions ne peuvent pas être acceptées. Dans le § III nous nous prononcerons sur la position du DAPT à l'intérieur du sémitique du N-O du premier millénaire.

§ I. *Quelques traits linguistiques erronément pris en considération*

- 1 Faits concernant l'orthographe: § 1.1 *l* proclitique de négation<sup>6</sup>; § 1.2 prén. suff. II p.s.f. *ky*.
- 2 Faits concernant la phonétique: § 2.1 graphie de *q* étymologique.
- 3 Faits concernant la morphologie: § 3.1: pluriel du masculin; § 3.2: prénoms suffixes: § 3.2.1: *-ky*; § 3.2.2: *-wh*; § 3.3: les thèmes verbaux; § 3.4: III p.s.f. et de l'accompli.
- 4 Faits concernant la syntaxe: § 4.1 le *waw* consécutif.
- 5 Faits concernant le lexique: § 5.1: *br*; § 5.2: *dbr*; § 5.3: *hđ*; § 5.4: *hzy*; § 5.5: *p<sup>c</sup>l*.

§ 1.1: S'il est vrai que cette graphie est attestée dans différentes inscriptions araméennes anciennes<sup>7</sup> pour exprimer la négation, cette graphie trouve son antécédent parfois dans l'orthographe ougaritique<sup>8</sup>; pour cette pratique orthographique nous suivons l'explication fournie par M. TSEVAT<sup>9</sup>.

<sup>3</sup> G. GARBINI, *Lingue semitiche*<sup>2</sup>: pp. 140-41.

<sup>4</sup> GARR, *Geography: passim* et pp. 229, 231.

<sup>5</sup> J. HUEHNERGARD, *Deir Alla Symposium*, pp. 282-293.

<sup>6</sup> Pour des références aux mots où aux faits linguistiques dans DAPT voir J.A. HACKETT 1984 (2), pp. 91-107, 127-35; *l* pourrait être aussi assévératif selon la suggestion de J.C. GREENFIELD, *Deir Alla Symposium*: p. 117.

<sup>7</sup> A. LEMAIRE, *Langue*, p. 325, pour une liste des attestations.

<sup>8</sup> St. SEGERT, *A Basic Grammar of the Ugaritic Language*: Berkeley-Los Angeles, 1985: § 65.2 pp. 99-100; UT: § 12.4 p. 108.

<sup>9</sup> M. TSEVAT, *A Chapter on Old West Semitic Orthography: Mélanges J. Bloch*, New York, 1960 pp. 82-91, en particulier pp. 85-86: pour la notation /a:/ avec (ʿ) dans l'araméen voir J. FRIEDRICH, *Zur Bezeichnung des langen ā in dem*



§ 1.2: S'il est vrai que cette notation est propre à l'araméen<sup>10</sup> on ne peut cependant pas oublier qu'elle reflète une prononciation /ki:/ correspondant à la forme reconstituée pour le protosémitique<sup>11</sup>; de plus, cette forme n'appartient pas d'une manière exclusive à l'araméen puisqu'on la retrouve également dans le néo-punique<sup>12</sup> aussi bien que dans l'hébreu biblique<sup>13</sup> dans des textes de tradition tant massorétique que qoumranienne<sup>14</sup>; on peut difficilement considérer certaines de ces formes comme des aramaïsmes.

§ 2.1 Exception faite pour quelques cas à propos desquels on peut s'interroger pour savoir si effectivement <q> rend *q* étymologique, par exemple *qb<sup>c</sup>n* «hyènes» ou «coupes»<sup>15</sup>. De plus, l'on doit considérer que la graphie du *q* étymologique avec <q> a amené bien des chercheurs à aligner la langue de DAPT sur le consonantisme de l'araméen ancien<sup>16</sup>; cependant, un examen même superficiel de la documentation du sémitique N-O nous permettra de montrer comment à l'intérieur de différentes documentations le phonème ne présente pas de rendement uniforme: par exemple, dans le yaoudien<sup>17</sup> près de *q* noté par <q> figurent les formes *šry* KAI 214: 30 et *šmrg*<sup>18</sup> KAI 215:16; dans l'araméen ancien se trouve la racine *mḥ*<sup>19</sup> Sefire A 42 (= KAI 222) et Zakkur A (= KAI 202): 15 – à confronter avec l'arabe *mḥd* et l'hébreu *mḥš*; et encore, dans

Schreibwesen des aramäischen: Or. 26 (1957), pp. 37–42. Cette graphie, habituelle dans l'arabe classique, est tardive dans cette langue voir W. DIEM, Untersuchungen zur früheren Geschichte der arabischen Orthographie? I, Die Schreibung der Vokale: Or. 48 (1979) pp. 207–57, et aussi dans la graphie est tardive dans le judeo-araméen, voir S. LANDAUER, Das Elif als mater lectionis im Jüdisch-aramäischen, *Mélanges A. Berliner*, Frankfurt a. M., 1903, pp. 215–26.

<sup>10</sup> A. LEMAIRE, *Langue*, p. 325 cf. SEGERT, *Altaramäische Grammatik*: § 5.1.3.3.3 pp. 171–72.

<sup>11</sup> GVG, I, § 105 et p. 309; MOSCATI, *Comparative Grammar*: § 13.23 p. 109.

<sup>12</sup> FRIEDRICH-RÖLLIG: § 112 p. 47; St. SEGERT, *A Grammar of Phoenician and Punic*, München 1976: § 51.221 p. 96.

<sup>13</sup> Pour une liste des formes on renvoie à Fr. BÖTTCHER, *Ausführliches Lehrbuch der hebräischen Sprache*, Leipzig, 1868: vol. II, § 871 p. 18.

<sup>14</sup> E.Y. KUTSCHER, *The Language and the Linguistic Background of the Isaiah Scroll* (1Q Is.a), Leiden, 1974: pp. 209 ss.

<sup>15</sup> M. WEINFELD, *Shnaton* 5–6 (1981–1982) commentaire à la p. 145 et traduction à la p. 146.

<sup>16</sup> A commencer par l'éditeur de DAPT pp. 283–84; A. LEMAIRE considère cet élément comme central, *Langue* pp. 324–325.

<sup>17</sup> P.E. DION, *La langue de Ya'oudi*, Ottawa 1984: § 10 pp. 96–97.

<sup>18</sup> Pour la graphie de *q* avec <g> l'on renvoie à l'étude de F. LEEHUIS, *An Early Witness for a Fronted /g/ in Aramaic? The case of Tell Fekheriye Inscription*, *Mélanges J.H. Hospers*, Groningen, 1984: pp. 133–42.

l'araméen ancien le NP du roi de Damas est rendu par *ra-ḥi-a-nu*<sup>19</sup> et *ra-qi-a-nu*<sup>20</sup>; dans l'hébreu biblique dans I Roi 6:34 près de la forme standardisée *ql<sup>c</sup>ym* coexiste la forme *ql<sup>c</sup>ym* du même, par exemple, les racines *rb<sup>c</sup>/rbš*.

§ 3.1 Si en DAPT le pluriel du masculin se termine par *-n* et non pas par *-m* comme dans l'hébreu biblique, dans le phénicien et dans l'ammonite<sup>21</sup> la terminaison *-n* appartient également aux dialectes cananéens tels que le moabite<sup>22</sup> et l'hébreu michnique<sup>23</sup>.

### § 3.2.1 Voir ci-dessus § 1.2

§ 3.2.2 La préposition *'wh*, ainsi que le fait que nous venons d'analyser au § 2.1, a été signalée par beaucoup de chercheurs comme les indices les plus sûrs en vue d'un classement de DAPT comme araméen; cependant, la forme du suffixe, qui déjà à l'intérieur de l'araméen ancien se prête difficilement à une explication<sup>24</sup>, peut être interprétée comme le résultat d'un phénomène phonétique. Le *waw* qui précède *h* peut être considérée comme un *glide* pour la reproduction du trilitérisme de la préposition: cette hypothèse nous paraît évidente si nous comparons les prépositions attestées dans l'hébreu biblique, *ʾl*, *ʿd*, *ʿl*, à leur forme en état d'annexion *ʾly*, *ʿdy*, *ʿly*, et aux graphies avec *ālif maqṣūra* de l'arabe *ʾly*, *ʿly*. Ensuite, si nous considérons d'autres langues sémitiques, nous pourrions observer, d'une part dans le phénicien et de l'autre dans le sudarabique que la préposition *ʿl* en phénicien est accompagnée

<sup>19</sup> ANET p. 283.

<sup>20</sup> Cité par GARR, *Geography*: p. 63 note 2: l'éditio princeps de ce texte cunéiforme nous a été inaccessible; cf. aussi le NP *hḏrḳy* dans le sceau VSA 55 dont la lecture correcte a été établie par A. LEMAIRE, *Sem.* 28 (1978) pp. 11-14; pour d'autres parallèles onomastiques voir M. MARAQTEN, *Die semitischen Personennamen in den alten und reichsaramäischen Inschriften aus Vorderasien*, Hildesheim, 1988 p. 155.

<sup>21</sup> K.P. JACKSON: *The Ammonite Language of the Iron Age*, Harvard Semitic Monographs 27, Chico California, 1983: p. 108.

<sup>22</sup> SEGERT, *Moabitische Inschrift*: § 4.331 p. 221, § 9.2 p. 260. Cette donnée est confirmée grâce à l'analogie par le mot *rhyn* du papyrus moabite dont l'édition par P. BORDREUIL-D. PARDEE est imminente. Je tiens à remercier ces deux amis et collègues pour m'avoir fait connaître cet important texte avant sa publication; pour l'instant, voir P. BORDREUIL, 20 ans d'épigraphie transjordanienne: *IVème Congrès sur l'Histoire et l'Archéologie de Jordanie*, Lyon 30 mai-4 juin 1989, sous presse.

<sup>23</sup> M.H. SEGAL, *A Grammar of Mishnaic Hebrew*, Oxford 1927: § 281 p. 126.

<sup>24</sup> DEGEN, *Grammatik*: p. 58. Pour la connexion du suffixe *-wh* avec la forme postérieure *-why* voir enfin J.W. WESSELIUS, *The Spelling of the Third Person Singular Suffixed Pronoun in Syriac*: *Bibl. Or.* 39 (1982) coll. 251-54.



par une forme variante  $^cl^{25}$  en cas d'annexion à un substantif aussi bien qu'à un prénom suffixe; dans le sudarabique épigraphique<sup>26</sup> la préposition  $^cl$  figure sous les formes  $^cl, b^cl, ^cly, ^clw, ^cln^{27}$  et en cas d'annexion d'un prénom suffixe, on retrouve la forme  $b^clwhw$ ; finalement, l'insertion de ce *glide* se trouve également en araméen ancien dans la forme d'annexion à un substantif:  $^clwy$  AP 5:6, 9 et aussi papyrus Amherst 63 col. VII: 17<sup>28</sup>; mais, en cas d'annexion d'un prénom suffixe: AP 5:11  $^clwyh$ . Cette graphie de la préposition  $^cl$  a été ensuite reprise par l'araméen babylonien<sup>29</sup> aussi bien que galiléen<sup>30</sup>, par le syriaque<sup>31</sup> et par le mandéen<sup>32</sup>. Notre hypothèse se confirme à plus forte raison dans la zone trans-jordanienne parce que dans le moabite<sup>33</sup>, le passage caractéristique de  $w > y$  dans les verbes de *tertia infirmae*<sup>34</sup> ne s'était pas encore complètement produit.

§ 3.3 Quant'à la formation des conjugaisons verbales dérivées, on distingue et des formes typiquement araméennes avec préfixe T et la forme typiquement cananéenne avec préfixe N. A notre avis, en vue d'un classement, il n'est pas correct de choisir l'une des deux formes au préjudice de l'autre et, par conséquent, de considérer DAPT comme araméen<sup>35</sup> ou bien cananéen<sup>36</sup>: en effet, la conjugaison

<sup>25</sup> FRIEDRICH-RÖLLIG: § 250 p. 125: pour l'inclusion de ce -t voir aussi P. SCHRÖDER, *Die Phönizische Sprache* ..., Halle 1869: § 120 pp. 212-13 et GVG, I, 253 b Anm. p. 498.

<sup>26</sup> A.F.L. BEESTON, *Sabaic Grammar*, Manchester 1984: § 34:10 p. 57: cf. *ibidem* § 34.9 p. 75 pour la préposition  $^cd$  et ses différentes formes  $^cd, ^cdy, ^cdn$ .

<sup>27</sup> A.F.L. BEESTON, *op.cit.* (note 26): § 33:3 pp. 53-54 avec renvoi à JAMME, *Marib* 643:29.

<sup>28</sup> Edition provisoire sous la direction de S.P. VLEEMING-J.W. WESSELIUS *JEOL* 28 (1983-1984), p. 135.

<sup>29</sup> C. LEVIAS, *A Grammar of Babylonian Aramaic*, New York 1930, p. 118.

<sup>30</sup> C. LEVIAS, *A Grammar of Galilean Aramaic*, Introduction by M. SOKOLOFF, New York 1986: § 120 p. 79.

<sup>31</sup> *Lexicon Syriacum*: p. 526 b.

<sup>32</sup> Th. NOELDEKE, *Mandaïsche Grammatik*, Halle, 1875: p. 194 note 2; voir aussi E.S. DROWER-R. MACUCH, *A Mandaic Dictionary*, Oxford, 1963 p. 194.

<sup>33</sup> Cf. les formes  $^cnuw$  l.6 e  $wy^cnuw$  l.5 pour lesquelles voir SEGERT, *Moabitische Inschrift*: § 4.5842 p. 227.

<sup>34</sup> MOSCATI, *Comparative Grammar*: § 16.121 p. 166.

<sup>35</sup> A. LEMAIRE, *Langue*, pp. 318-19 où l'on a la liste complète des auteurs du classement de DAPT comme araméen mais avec l'inclusion dans la liste aussi de savants qui n'ont pas procédé eux-mêmes à une analyse linguistique.

<sup>36</sup> La définition «canaanite» de V. SASSON *UF* 17 (1985) p. 285 et *AUSS* 24 (1986) p. 150 note 6 est à rejeter car elle est basée sur des considérations culturelles

faisant partie du protosémitique<sup>37</sup>, elle doit être considérée comme une conservation<sup>38</sup>; de plus, elle n'appartient pas d'une manière exclusive aux dialectes cananéens du moment qu'elle se présente en tant que forme résiduelle également dans l'araméen; pour ce faire, il faut rappeler les formes *nšh* AP 15:10<sup>39</sup> e *nhwy*<sup>3</sup> stèle de Xanthos 22<sup>40</sup>. Avant son attestation historique, l'araméen possédait également une conjugaison N et uniquement à l'époque historique, pour exprimer le passif-réflexif. Il a opté pour une formation avec préfixe T. En outre, si nous tenons compte de ce qu'on a annoncé dans le § 0.0 à savoir la validité d'une comparaison entre des faits attestés dans les langues littéraires et des faits attestés dans la documentation épigraphique, et si nous rappelons comment, par exemple, dans le phénicien<sup>41</sup> et dans le moabite<sup>42</sup> figure une conjugaison GT ou bien que dans l'inscription de Kilamuwa (= KAI 24): 10 ou est attestée une conjugaison causative réflexive avec infixé T dans la forme *yltnn*<sup>43</sup> mais que successivement dans le phénicien et dans l'hébreu il n'existe pas de traces, dans le système verbal, des conjugaisons à T infixé, nous devons conclure que la distribution des conjugaisons verbales dans un schéma fixe tel que nous le retrouverons dans le système des langues littéraires au cours de la deuxième partie du premier millénaire ne s'était pas encore complètement produite dans les premiers siècles du même millénaire.

et non pas linguistiques. La définition «south-canaanite», cf. HACKETT 1984 (1) p. 64 et 1984 (2) p. 123, n'est plus à retenir après la découverte du cananéen périphérique—ammonite, édomite, moabite—: les limites de cette définition ont été déjà signalées par Z.S. HARRIS, *Development of the Canaanite Dialects, An Investigation in Linguistic History*, New Haven 1939, p. 98 note 8.

<sup>37</sup> MOSCATI, *Comparative Grammar*: § 16.15 pp. 127–28. Pour un aperçu sur les conjugaisons verbales dans le sémitique voir M.H. GOSHEN-GOTTSTEIN, *The System of Verbal Stems in the Classical Semitic Languages: Conference on the Semitic Studies*, Jerusalem 1965, pp. 70–91.

<sup>38</sup> E.A. KNAUF, *ZDPV* 101 (1985) p. 190: GARR, *Geography* p. 215.

<sup>39</sup> Pour une hypothèse différente, cf. SEGERT, *Altaramäische Grammatik*, § 5.6.7.3.7. p. 256.

<sup>40</sup> Cfr. p. 154 de l'*editio princeps* par A. DUPONT-SOMMER dans H. METZGER-A. DUPONT-SOMMER-E. LAROCHE-M. MAYRHOFFER, *La stèle trilingue du Letoon, Fouilles de Xanthos VI*, Paris 1979: pp. 136–69. R. CONTINI, *OA* 20 (1981) p. 233, a défini le mot *nhwy* «un residuo lessicalizzato di questo tema verbale»—le nifal; pour une explication différente voir J. TEIXIDOR, *JNES* 37 (1978) p. 184 note 19.

<sup>41</sup> FRIEDRICH-RÖLLIG § 150 p. 69, inscription d'Ahirom (= KAI 1) l.2 *thtpk*, *thtps*.

<sup>42</sup> Stèle de Mesha (= KAI 181) ll.11,15 *lthm*, l.19 *bhlthmh*, l.32 *hlthm* cf. SEGERT, *Moabitische Inschrift*: § 4.563 p. 225.

<sup>43</sup> P. SWIGGERS, *Reflexive Verbal Pattern in North-Phoenician*: *ZDMG* 131 (1981), pp. 225–228.



§ 3.4 Si d'un côté la conservation de la désinence protosémitique – *at* pour la III p.s.f. de l'accompli distingue l'araméen dès ses attestations les plus anciennes<sup>44</sup> de l'hébreu et du phénicien<sup>45</sup>, de l'autre l'on doit cependant considérer que cette désinence protosémitique se maintient parfois même dans l'hébreu biblique<sup>46</sup> dans quelques formes qui peuvent être expliquées soit comme des formes poétiques<sup>47</sup>, par exemple <sup>3</sup>zlt Deut 32:36 soit, comme des formes déterminées par l'état de pause<sup>48</sup>.

§ 4.1 Dans l'évaluation du phénomène on doit tenir compte de deux faits, à savoir: a) le *waw* consécutif, ainsi que D. COHEN<sup>49</sup> l'a signalé, est fonctionnel à la narration; b) le phénomène est commun non seulement à l'intérieur du sémitique du N-O, son attestation se retrouvant dans l'hébreu aussi bien que dans l'araméen ancien<sup>50</sup>, dans le moabite<sup>51</sup> et peut-être dans le phénicien<sup>52</sup>, mais également dans la zone sémitique centrale entière<sup>53</sup>, parce qu'on le retrouve également en sudarabique épigraphique<sup>54</sup>. Dans ce contexte, il

<sup>44</sup> DEGEN, *Grammatik*: § 48 p. 64; SEGERT, *Altaramäische Grammatik* § 5.6.4.3.8 p. 247.

<sup>45</sup> FRIEDRICH-RÖLLIG: § 132 b p. 60.

<sup>46</sup> Pour une liste des formes cf. J. OLSHAUSEN, *Lehrbuch der hebräischen Sprache*, Braunschweig, 1861: § 226 pp. 448–449.

<sup>47</sup> KUTSCHER, *History*: § 55 p. 39. Le même auteur signale au § 212 pp. 127–28 des formes analogues dans l'hébreu michnique.

<sup>48</sup> BLH § 42 m p. 310.

<sup>49</sup> D. COHEN, *AEPHE* 1975–1976 IVième section pp. 241–247.

<sup>50</sup> Pour les attestations bien connues dans la stèle de Zakur (= KAI 202) et l'histoire de la recherche, cf. DEGEN, *Grammatik*, pp. 114–15 note 21 auquel il faut ajouter SEGERT, *Altaramäische Grammatik*: § 5.6.4.1.6 p. 246. § 6.5.3.2.1 p. 356–57 et § 6.6.3.3.2. p. 377.

<sup>51</sup> SEGERT, *Moabitische Inschrift*: § 9 p. 260 pour la liste des attestations.

<sup>52</sup> FRIEDRICH-RÖLLIG: § 266 p. 134 auquel il faut ajouter Fr. BRON, *Waw* conversif en phénico-punique: *GLECS* 17–23 (1979) pp. 607–10.

<sup>53</sup> Pour le concept du sémitique central, cf. Ch. RABIN, *The origin and Subdivision of Semitic*, *Mélanges G.R. Driver*, Oxford, 1963, pp. 104–115; G. GARBINI, *La configurazione dell'unità linguistica semitica: Le protolingue*, *Atti del IV Convegno internazionale dei Linguisti, Milano, 1963*, Milano, 1965: pp. 119–38, repris dans *Lingue Semitiche* 1 pp. 23–37, 2 pp. 23–42; cf. R.M. VOIGT, *The Classification of Central Semitic*: *JSS* 32 (1987) pp. 1–21, R. HETZRON, *Semitic Languages*, B. COMRIE, *The World's Major Languages*, New York, 1987, pp. 654–663, en particulier voir p. 656 pour la position de l'arabe.

<sup>54</sup> Pour l'attestation du phénomène en sudarabe cf. M. HÖFNER, *Altsudarabische Grammatik*, Leipzig 1943: § 61 pp. 75–76 et A.F.L. BEESTON, *A Descriptive Grammar of Epigraphic South Arabian*, London, 1962: § 52:10 p. 61; à ma connaissance, il existe une seule étude détaillée du phénomène, celle de B. GRUNTFEST, que je n'ai pu consulter que dans le résumé critique de J. RYCKMANS *Bibl. Or.* 24 (1970) pp. 272–73: à ce propos, cf. également DEGEN, *Grammatik*: pp. 3–4 note 2.

paraît évident que son attestation en DAPT n'est pas à retenir pour un éventuel classement.

§ 5.1 Abstraction faite de l'origine araméenne effective de Balaam<sup>55</sup>, l'occurrence du terme *br* dans l'inscription de Kilamuwa (= KAI 24) fait exclure le mot dans le but d'un classement.

§ 5.2 Il est vrai que le terme hébreu s'oppose au terme araméen, mais le substantif *db̄r* n'appartient exclusivement pas à l'hébreu du moment que dans les papyrus d'Eléphantine il contribue à la formation de la locution adverbiale *l̄db̄r*, «à propos de»<sup>56</sup>.

§ 5.3 Il est vrai que nous avons là la forme du numéral cardinal «un» en araméen: elle est cependant d'un côté le résultat d'une évolution phonétique et son emploi dans le but d'un classement s'avère donc problématique; de l'autre, la forme se retrouve en phénicien en tant que constituant de la préposition *lh̄d̄*<sup>57</sup>, «tout seul», attestée tant dans l'inscription de Karatepe (= KAI 26) II:5 qu'en Byblos 13:1<sup>58</sup>.

§ 5.4 Il continue d'être difficile de considérer le terme comme un aramaïsme, car la racine apparaît et dans l'inscription de Kilamuwa (KAI 24) aux lignes 9-13 et dans l'Ancien Testament<sup>59</sup> dans des passages où il n'est pas toujours possible de retrouver un aramaïsme.

§ 5.5 On sait très bien que la racine *p̄cl* est typique du phénicien<sup>60</sup> et de la poésie hébraïque<sup>61</sup>; on ne peut cependant pas oublier

<sup>55</sup> M. DELCOR, Bala<sup>c</sup>am Patôrâh «interprète de songes» au pays d'Ammon, d'après Num 22,5. Les témoignages épigraphiques parallèles: *Sem.* 32 (1982) pp. 89-91 et dans le même sens, F. VATTIONI, P<sup>2</sup> tôr (Num. 22:5 Deut 23:5): *AION* 30 (1980) pp. 465-71.

<sup>56</sup> DISO p. 55.

<sup>57</sup> Interprétation suggérée pour la première fois par G. LEVI DELLA VIDA, *RANL* 1949, p. 284 et reprise par H.L. GINSBERG, *JANES* 5 (1973), pp. 136-37.

<sup>58</sup> *editio princeps* J. STARCKY, *MUSJ* 45 (1969) pp. 257-73.

<sup>59</sup> Pour une analyse détaillée de l'emploi de la racine *h̄zy* dans l'hébreu biblique voir la monographie de H.F. FUHS, *Sehen und Schauen. Die Wurzel h̄zh im Alten Orient und im Alten Testament. Ein Beitrag zur semitischen Offenbarungsempfang*, Forschung zur Bibel 32 (1978).

<sup>60</sup> FRIEDRICH-RÖLLIG: § VII p.4.

<sup>61</sup> Pour une analyse détaillée des attestations dans l'hébreu biblique, cf.



que, bien que rarement, la racine est attestée également dans le syriaque<sup>62</sup>.

## § II Le classement de DAPT

Les dénominations qu'on a jusqu'ici proposées pour DAPT ont été les suivantes: ammonite<sup>63</sup>, galaadite<sup>64</sup>, madianite<sup>65</sup>, nordarabe<sup>66</sup> et hébraïque<sup>67</sup>, dénominations qu'il faut refuser, car, par exemple, pour l'ammonite, en ce qui concerne le pluriel du masculin<sup>68</sup>, nous connaissons une morphologie différente de DAPT et, en certains cas, cette même définition a été proposée selon un critère épigraphique<sup>69</sup> et non linguistique; si, d'un côté, les définitions galaadite ou madianite répondent à des critères géographiques, de l'autre, elles ne sont pas raisonnablement à retenir car on a trop peu de connaissances sur ces deux derniers dialectes<sup>70</sup>; le classement hébraïque a trouvé son fondement dans quelques correspondances formelles entre des textes bibliques et des passages de DAPT et surtout à la suite d'une intégration textuelle qui demeure en tout cas une hypothèse; la définition nordarabe doit être entendue selon la conception propre à G. GARBINI<sup>71</sup> sur les relations existants entre

P. HUMBERT, L'emploi du verbe *pā'al* et des dérivés substantifs en hébreu biblique: *ZAW* 65 (1953) pp. 35-44.

<sup>62</sup> *Lexicon Syriacum*: pp. 585 b-586 a.

<sup>63</sup> J.C. GREENFIELD, *JSS* 25 (1980) p. 251 emploie cette définition probablement pour des raisons géographiques: E. PUECH, *RB* 98 (1985), pp. 5-24, à la suite de F.M. CROSS, *AUSS* 13 (1975), p. 14-17, considère l'écriture de DAPT comme ammonite pour des raisons paléographiques; voir aussi E. PUECH, *Deir Alla Symposium*, pp. 221-238.

<sup>64</sup> J. NAVEH, *IEJ* 17 (1967) p. 256.

<sup>65</sup> A. ROFÉ cité par A. LEMAIRE, *Langue*, p. 338 note 24 et du même auteur dans A. BIRAN (éditeur), *Biblical Archeology Today, Proceedings of the International Congress of Biblical Archeology, Jerusalem April 1984*, Jerusalem 1985, pp. 365-366.

<sup>66</sup> G. GARBINI, *Henoch* 1 (1979), p. 170.

<sup>67</sup> J.W. WESSELIUS, *Bibl. Or.* 44 (1987), col. 591.

<sup>68</sup> Je suis en accord avec A. LEMAIRE, *Langue*, p. 324.

<sup>69</sup> E. PUECH, art. cité à la note 63.

<sup>70</sup> Sur le dialecte de Galaad en relation à Juges 12:6 cf. F. ISRAEL, Note Ammonite I, Gli arabismi nella documentazione onomastica ammonita, *SEL* 6 (1989) pp. 91-96, note 18 à p. 95. A la bibliographie citée dans notre article on ajoutera G.A. RENDSBURG, More on Hebrew šibbōlet: *JSS* 33 (1988) pp. 255-58; A.F.L. BEESTON, Šibbōlet: A Further Comment: *JSS* 33 (1988) pp. 259-61. Pour le dialecte madianite voir maintenant E.A. KNAUF, *Midian, Untersuchungen zur Geschichte Palästinas und Nordarabiens am Ende des 2. Jahrtausends v. Chr.*, Wiesbaden 1988: pp. 77-91.

<sup>71</sup> Pour les conceptions de cet Auteur sur l'histoire linguistique de l'Arabie avant l'Islam on renvoie à ses deux études *Sulle origini della lingua araba, Mélanges F. Gabrieli*, Roma 1964 pp. 123-134 (= *Lingue Semitiche* 1 pp. 82-96, 2 pp. 97-

le sémitique du N-O et le nordarabe; de toute façon, il faut remarquer que dans la langue de DAPT il manque des formations nominales typiquement nordarabe tels que des élatifs ou des pluriels brisés<sup>72</sup>: aussi doit-on refuser la définition en question.

Cette exclusion opérée, les trois choix possibles restants sont: dialecte cananéen<sup>73</sup>, dialecte araméen<sup>74</sup> ou indéfinition<sup>75</sup>, c'est cette dernière solution que nous avons adoptée nous-mêmes en définissant «langue de DAPT» la langue objet de notre étude. Nous sommes parvenus à cette position après les réflexions exposées au § I: en effet, l'examen des quelques traits choisis nous a montré qu'aucun des faits, objet de notre analyse, ne s'avère décisif dans un sens ou dans un autre. Cette situation n'est pas du tout inconnue dans l'étude des langues du sémitique du N-O: elle s'est déjà posée lors du classement du yaoudien<sup>76</sup> auquel quelques chercheurs<sup>77</sup> ont

117) et I Sabei del Nord come problema storico, *Mélanges F. Gabrieli*, Roma 1984 pp. 373-80. Pour la position de l'arabe dans le Sémitique, voir R. HETZRON, La division des langues sémitiques: *Actes du Premier Congrès International de Linguistique Sémitique et Chamito-sémitique*, Paris, 1969, Paris/The Hague, 1974, pp. 181-94; du même Auteur, voir l'article cité ci-dessus à la note 53.

<sup>72</sup> Pour l'ensemble des données linguistiques concernant l'arabe avant Alexandre on renvoie à R. ZADOK, *On West Semites in Babylonia during the Chaldean and Achaemenian Periods*, Jerusalem 1977: §§ 211-213 pp. 193-97 et *ZDMG* 131 (1981) pp. 42-84; il faut ajouter que des formations typiquement nordarabiques telles que l'élatif et le pluriel brisé paraissent déjà dans la première moitié du deuxième millénaire dans la langue d'Emar, dans l'attente de la grammaire de cette langue que D. ARNAUD nous a promise, cf. du même auteur *AEPHE*, Vème Section 94 (1985-1986) p. 268-69.

<sup>73</sup> HACKETT 1984 (1), (2).

<sup>74</sup> Cfr. A. LEMAIRE, *Langue*, pp. 318-19; l'on signale que S.A. KAUFMAN a entretemps changé d'avis, voir donc *The Classification of the Northwest Semitic Dialects of the Biblical Period and Some Implications thereof: Proceedings of the Ninth World Congress of Jewish Studies, Jerusalem 1985*, Jerusalem 1986, pp. 41-57.

<sup>75</sup> Pour cette position méthodologique nous nous sommes inspirés de la formulation proposée par H.J. POLOTSKY, *Semitics: World History of the Jewish People*, London, 1964, vol. I pp. 99-111, tout particulièrement p. 99 et p. 357 note 12. Je remercie mon collègue R. CONTINI, pour m'avoir indiqué l'existence de cette contribution. Nous signalons que la même position a été prise au début des études sur la langue de DAPT par G. RINALDI, *BeO* 20 (1978) qui définit la langue objet de notre étude comme «un dialetto prima ignoto».

<sup>76</sup> P.E. DION, *op. cit.* (cf. note 17) pp. 7-24; nous renvoyons à un de nos prochains articles où nous montrerons d'une manière détaillée comment plusieurs chercheurs ont procédé de la même façon dans le classement du Yaoudien aussi bien que de DAPT.

<sup>77</sup> G. GARBINI, art. cité (cf. note 66): p. 170; J.A. FITZMYER *CBQ* 40 (1978) p. 95; E.A. KNAUF *ZDPV* 101 (1985) pp. 190-191; H.P. MÜLLER *ZAW* 98 (1982) p. 216.



comparé la langue de DAPT. Cette comparaison est favorisée par le consonantisme et par le manque certain de la détermination; en outre, conformément aux critères que nous avons suivis dans l'attribution de l'ammonite au cananéen<sup>78</sup>, en DAPT il manque non seulement la détermination exprimée par l'état emphatique ou l'article préfixé, mais également la base pronominale *d* d'origine démonstrative employée en fonction relative – dont la présence dans les inscriptions DA 1818 et DA 2000 de la même phase M n'a aucune importance pour DAPT – et aussi les éléments *š*<sup>79</sup> ou *š*<sup>80</sup>. En conclusion, l'absence de ces deux faits distinctifs nous empêche de situer DAPT à l'intérieur de l'une des deux branches du sémitique du N-O au premier millénaire. Ainsi, l'impossibilité d'opérer un choix à l'aide de critères de logique binaire d'exclusion et inclusion nous a-t-elle amené à considérer dans la situation présente, l'indéfinition comme le choix le plus sage.

### § III La position de la langue de DAPT dans le contexte du Sémitique du N-O.

L'intégration de la langue de DAPT dans le contexte du sémitique du N-O s'est avérée problématique car, ainsi que nous l'avons vu au §§ I, II les critères de la logique binaire de l'exclusion et de l'inclusion paraissent impraticables; si l'on opère moyennant le critère de la géographie linguistique et en particulier du *continuum* comme l'a fait R.W. GARR<sup>81</sup> ou bien le critère du *Stammbaum* comme l'a fait au cours de ce symposium J. HUEHNERGARD<sup>82</sup>, nous nous re-

<sup>78</sup> Le critère d'exclusion que nous avons employé dans notre article *The Language of the Ammonites: OLP 10* (1979) pp. 143–159, en particulier pp. 148–49, a été appliqué également, et de façon indépendante, par G. GARBINI, art. cit. (cf. note 66) p. 169. Nous ne concordons ni avec G. GARBINI ni avec HACKETT 1984 (1) p. 65 et 1984 (2) pp. 31,35 en ce qui concerne l'interprétation de *š* en tant que pronom relatif. Le mot a été déjà correctement compris par l'éditeur de DAPT, J. HOFTIJZER p. 179 comme *'š* «homme». Ne figurant pas dans DAPT, le pronom relatif *zy* attesté dans DA 1818 et DA 2000 ne doit pas être considéré pour le classement linguistique comme l'a fait A. LEMAIRE, *Langue*: pp. 321–22.

<sup>79</sup> Sur le pronom relatif *š* cf. G. GARBINI, *Il pronome relativo š in fenicio e in ebraico: Mélanges M. Rodinson*, Paris 1986 pp. 185–189. B.A. LEVINE, *The pronoun š in Hebrew on the Light of ancient Epigraphy EI 18* (1985) pp. 247–252 (hébreu moderne).

<sup>80</sup> Attesté dans la stèle de Mesha (= KAI 181) l.29 et en édomite dans l'ostrakon de Khorbat Uzza l.4 (*editio princeps* Y. BEIT ARIEH, B. CRESSON, *TA 12* (1985) pp. 96–101).

<sup>81</sup> GARR, *Geography*: cf. les représentations graphiques à la p. 229 et à la p. 231.

<sup>82</sup> Deir Alla Symposium, pp. 282–293.

trouverons dans la même impossibilité de situer DAPT dans le cananéen et dans l'araméen; il faut aussi remarquer que, dans la représentation graphique proposée par ces deux collègues, on relève une position analogue de DAPT à celle du yaoudien.

La position analogue à celle du yaoudien nous amène à proposer pour DAPT une comparaison avec la langue de la poésie israélite ancienne<sup>83</sup> où l'on retrouve la rareté de la détermination et un lexique mélangé d'éléments hébreux aussi bien qu'araméens<sup>84</sup>.

Dans cette optique, la langue de DAPT doit être, à notre avis, considérée comme le plus ancien spécimen attesté par l'épigraphie de la langue poétique<sup>85</sup> des Sémites nord-occidentaux au cours du premier millénaire, présentant des analogies formelles, morphologiques et syntaxiques avec l'ougaritique pendant le deuxième millénaire et avec la poésie israélite ancienne de la Bible. Ce spécimen, si d'un côté il s'ajoute à des textes qui ont déjà faits objet d'études telles que les textes ougaritiques et bibliques, de l'autre il vient s'ajouter également à des documents araméens qui présentent le même type de langue et sur lesquels J.C. GREENFIELD<sup>86</sup> a tout récemment attiré l'attention des chercheurs, ainsi qu'au papyrus Amherst 63<sup>87</sup> dont le déchiffrement, encore en phase d'achèvement, laisse cependant entrevoir l'existence, même dans l'aire linguistique araméenne, de ce même type de langue.

Le grand mérite de DAPT consiste, à notre avis, non seulement à avoir fourni un important point de départ dans l'élucidation de cette langue poétique, mais également à avoir donné, sur le plan

<sup>83</sup> Pour une récente mise à jour des caractéristiques de la poésie israélite ancienne voir A. SAENZ BADILLOS, *Historia de la lengua hebrea*, Barcelona, 1988 pp. 65-70.

<sup>84</sup> G.R. DRIVER, *Hebrew Poetic Diction: VTS* 1 (1953) pp. 26-39.

<sup>85</sup> Nous avons déjà défini la langue de DAPT comme «archaïque et poétique» dans *Le Monde de la Bible*, 46 (1986) p. 44, mais cette définition a été antérieurement proposée par J. HOFTIJZER *editio princeps* p. 301 dans les termes suivants: «a poetic language used for curses, proverbs and the prophecy itself». Sur cet aspect littéraire voir aussi S. SEGERT, *WZKM* 72 (1980) p. 188 où le savant de Los Angeles parle d'«Aramaic poetry». Pour une division du texte de DAPT en paragraphes voir H.-M. WEIPPERT, *ZDPV* 98 (1982) pp. 77-103: cette division a été maintenue par M. WEIPPERT, *Deir Alla Symposium*, pp. 152-55.

<sup>86</sup> J.C. GREENFIELD, *Early Aramaic Poetry: JANES* 11 (1979) pp. 45-51; pour l'attestation du *parallelismus membrorum* dans la littérature araméenne ancienne voir G.E. WATSON, *The Ahiqar Sayings. Some Marginal Comments: Aula Orientalis* 2 (1984) pp. 253-61, en particulier pour la liste des mots parallèles pp. 259-61.

<sup>87</sup> Pour une bibliographie complète sur ce texte, voir S.P. VLEEMING-J.W. WESSELIUS, *Studies in Papyrus Amherst 63*, Amsterdam 1985 p. 97.



historico-linguistique, la confirmation, aussi au niveau lexical, de l'existence d'une phase linguistique, phase qui jusqu'à présent n'était concevable que du point de vue phonétique et morphologique<sup>88</sup>, et dans laquelle le cananéen et l'araméen étaient encore indifférenciés.

## ABBREVIATIONS

- ANET: J.B. PRITCHARD (éd), *Ancient Near Eastern Texts relating to the Old Testament*, Princeton 1969<sup>3</sup>.
- DEGEN, Grammatik: R. DEGEN, *Altaramäische Grammatik*, Wiesbaden 1969.
- Deir Alla Symposium: cette publication.
- FRIEDRICH-RÖLLIG: *Phönizisch-Punische Grammatik*, Roma 1970<sup>2</sup>.
- GARR, Geography: W.R. GARR, *Dialect Geography of Syria Palestine, 1000-586 B.C.E.*, Philadelphia 1985.
- GVG: K. BROCKELMANN, *Grundriss der vergleichenden Grammatik der semitischen Sprachen*, voll. I-III, Berlin 1903-1913.
- HACKETT 1984 (1): J.A. HACKETT, *The Dialect of the Plaster Text from Deir Alla: Or 53 (1934) pp. 57-65*. (2): J.A. HACKETT, *The Balaam Text from Deir Alla*, Harvard Semitic Monographs n. 31, Chico California 1984.
- KAI: H. DONNER - W. RÖLLIG, *Kanaanäische Und Aramäische Inschriften*, voll. I-III, Wiesbaden 1962-1964.
- KUTSCHER, History: E.Y. KUTSCHER, *A History of the Hebrew Language*, Jerusalem - Leiden 1982.
- LEMAIRE, Langue: A. LEMAIER, *La Langue de l'inscription sur plâtre de Deir Alla: GLECS 24-28 (1979-1984) pp. 317-40*.
- Lexicon Syriacum: C. BROCKELMANN, *Lexicon Syriacum*, Halle 1928<sup>2</sup>.
- Lingue Semitiche: G. GARBINI, *Le lingue semitiche, Studi di storia linguistica*, Napoli 1970, 1984<sup>2</sup>.
- MOSCATI, Comparative Grammar: S. MOSCATI (éd), *An introduction to the Comparative Grammar of the Semitic Languages, Phonology and Morphology*, Wiesbaden 1964.
- SEGERT, Altaramäische Grammatik: St. SEGERT, *Altaramäische Grammatik*, Leipzig 1975.
- SEGERT, Moabitische Inschrift: St. SEGERT, *Die Sprache der moabitischen Königsinschrift: Ar.Or. 29 (1961) pp. 197-267*.
- UT: C.H. GORDON, *Ugaritic Textbook, Grammar, Texts in Transliteration, Cuneiform selections, Glossary, Indices*, Rome 1965.
- VSA: F. VATTIONI: *I sigilli, le monete, gli avori aramaici: Augustinianum 11 (1971) pp. 47-87*.

<sup>88</sup> On fait allusion à l'article bien connu de S. MOSCATI, *Il semitico di Nord-Ouest, Mélanges G. Levi Della Vida*, Roma 1956, vol. II, pp. 202-21, en particulier pp. 218 et ss. Dans les études sur la langue de DAPT la thèse de S. MOSCATI a été évoquée explicitement pour la première fois par H.P. MÜLLER, toujours attentif dans ses analyses aux faits linguistiques, dans *ZAW 98 (1982) p. 216*. Un emploi cohérent de la même thèse au niveau lexical a été effectué par B. HALPERN, *Dialect Distribution in Canaan and the Deir Alla Inscription: Mélanges Th.O. Lambdin*, Winona Lake 1987 pp. 119-39, en particulier p. 136.

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# BIBLICAL QUOTATIONS

## *Genesis*

iii 1-5, 278  
vii 11, 123, 171  
vii 12, 124  
viii 2, 123  
ix 15, 171  
xvii 14, 122f.  
xx 3, 169, 274  
xx 9, 133  
xxi 16f., 169  
xxii 1, 148  
xxii 19, 176  
xxviii 11-18, 45  
xxxi 24, 169, 274  
xxxii 24-32, 278  
xxxvi 31, 32, 52  
xxxvii, 45  
xxxvii 24, 210  
xl-xli, 45  
xlix 1, 115

## *Exodus*

iii 1, 138  
iv 2, 138  
xvi 4, 124  
xxi 31, 32, 116  
xxiv 10, 65  
xxiv 11, 65, 214  
xxiv 15, 16, 124  
xl 34, 124

## *Leviticus*

xi 6, 203  
xi 13, 200  
xi 16, 130  
xi 18, 196  
xi 19, 172, 200, 201  
xi 22, 196, 197  
xiii 44, 65  
xvi 13, 124  
xxvi 15, 44, 123  
xxvii 32, 138

## *Numbers*

vi 8f., 69  
ix 15, 16, 124  
xvii 7, 124  
xxi 14, 166  
xxii-xxiv,  
45, 51, 84, 151, 175, 176, 273, 274, 275,  
276, 278, 299  
xxii 4-21, 275  
xxii 5, 51, 119  
xxii 7, 175  
xxii 8, 276  
xxii 9, 20, 176, 274  
xxii 22-35, 273  
xxii 35, 276  
xxii 36-xxiii 26, 275  
xxii 38, 276  
xxiii 3, 5, 276  
xxiii 7, 51  
xxiii 8, 277  
xxiii 12, 276  
xxiii 13, 176  
xxiii 16, 276  
xxiii 19, 277  
xxiii 21, 176  
xxiii 22, 277  
xxiii 23, 277, 304  
xxiii 26, 276  
xxiii 27-xxiv 2, 275  
xxiv 1, 304  
xxiv 3, 65  
xxiv 4, 51, 115, 175, 176, 277  
xxiv 6, 110  
xxiv 8, 277  
xxiv 11, 275  
xxiv 14, 207  
xxiv 16, 51, 65, 115, 175, 176, 277  
xxiv 17f., 176  
xxiv 17, 138  
xxiv 25, 275  
xxx 9, 123  
xxxi, 176  
xxxi 8, 273, 274, 278  
xxxi 16, 176, 273, 274, 278

*Deuteronomy*

- i 1,2, 166
- vi 9, 54
- vii 13, 136
- xi 17, 123
- xiv 7, 203
- xiv 11, 200
- xiv 15, 130
- xiv 17, 196
- xiv 18, 172, 200, 201
- xiv 20, 200
- xviii 10, 64
- xviii 13-22, 278
- xviii 14, 64
- xviii 18, 276
- xxiii 5-6, 273, 274, 275, 276, 277, 278
- xxiii 5, 51
- xxvii, 216
- xxvii 2-3, 54
- xxvii 4,8, 54
- xxviii 4,18,51, 136
- xxxii 8f., 179
- xxxii 11, 130
- xxxii 17, 170
- xxxii 36, 311
- xxxii 37, 110

*Joshua*

- vii 6-16, 45
- viii 32, 54
- xiii 22, 64, 175, 273, 274, 278
- xxiv 9-10, 273, 274, 275, 276, 277, 278

*Judges*

- vii 21, 131
- viii 6,15, 131
- xii 6, 162, 313
- xiv 3, 133
- xix 27, 176
- xx 19, 176
- xx 26, 45
- xxi 2, 45

*1 Samuel*

- i 7f., 169
- iii, 45
- iii 4,6, 148
- viii 13, 68
- xi 4f., 169
- xv 10-23, 45

- xv 16, 115
- xvii 4ff., 10, 25, 26, 36, 45, 131
- xix 17, 214
- xx 1, 133
- xxii 8, 133
- xxviii 5, 126
- xxix 1, 38

*2 Samuel*

- vii 4-17, 45
- xii 15-23, 168, 208
- xxi 21, 131
- xxii 29, 116
- xxiv 11, 176

*1 Kings*

- vi 34, 308
- viii 35, 123
- xiii 26, 65
- xviii 9, 133
- xix 15, 52
- xx 1-21, 38
- xx 26-30, 38
- xx 32, 39
- xx 34, 37, 39
- xxii, 45, 177

*2 Kings*

- v 6, 133
- vi 24-33, 38
- vi 27, 116
- vi 32, 281
- vii 6-7, 38
- vii 15, 39
- viii 7-15, 52
- viii 11f., 169
- viii 28-29, 37
- ix-x 28, 37
- x 32-33, 37
- xii 18-19, 38
- xiii 3,7, 37
- xiii 17, 38
- xiii 22-25, 38
- xiii 25, 39
- xiv 25, 39
- xv 25, 29, 40
- xvii 13, 205, 301
- xix 4, 16, 22, 23, 131
- xxii, 37, 215
- xxii 11-20, 45



*Isaiah*

- i 1, 166
- iii 1-5, 174
- v 20, 115
- vi, 45
- viii 1ff., 216
- viii 9, 186
- ix 1, 116
- ix 18, 127
- x 15, 174
- xi 1, 68
- xi 6-9, 132
- xiii 1, 115
- xiii 21, 130
- xiv, 72
- xiv 19, 68, 69, 70, 78, 79
- xiv 27, 215
- xix 3, 127
- xix 16, 126
- xxii 1, 16, 133
- xxiv 1-3, 174
- xxiv 18f., 171
- xxiv 19, 123
- xxv 5, 134
- xxvii 12, 162
- xxix 18f., 216
- xxx 8, 216
- xxxi 4, 129, 147
- xxxiv 5, 133
- xxxiv 13, 130
- xxxvii 4, 17, 23, 24, 131
- xxxviii 14, 197, 198, 202
- xl, 65
- xl 22, 62, 74
- xlili 18, 213
- xlili 19, 122
- xlili 20, 130, 132
- xliv 25f., 215
- xliv 6, 69, 79
- l 10, 116, 210
- liv 1, 11, 210
- lvii 16, 171, 209
- lxv 7, 131
- lxv 25, 132

*Jeremiah*

- i 1-3, 166
- i 11, 148
- iii 5, 171, 209
- iii 12f., 205
- iii 12, 171, 209, 301

- iii 14, 22, 205, 301
- iv 1f., 205
- vi 24, 213
- viii 7, 198
- x 2, 64
- x 11, 113
- xviii 11, 205
- xxiii 18, 22, 169
- xxv 5, 205
- xxx 5, 126
- xxxv 15, 205
- xxxvi 3, 205
- xxxvi 5, 281
- xxxvi 6f., 216
- xxxvi 7, 205
- xlvi 28, 138
- xliv 24, 213
- xliv 31, 123
- l 39, 130
- l 43, 213
- li 30ff., 123

*Ezekiel*

- viii 1ff., 280
- ix 9, 208
- xiv 1ff., 280
- xx 1ff., 280
- xxi 6-7, 208
- xxi 26, 175
- xxii 30, 208
- xxiv 1f., 216
- xxiv 15-21, 279
- xxiv 15, 18, 280
- xxiv 19, 133
- xxvi 17, 136
- xxx 18, 124
- xxxii 7-8, 45
- xxxii 7, 124
- xxxii 23, 24, 26, 32, 136
- xxxiii 31ff., 281
- xxxvii 15ff., 280
- xxxviii 9, 124
- xxxviii 11f., 123
- xxxviii 16, 124
- xxxix 4, 200

*Hosea*

- i 7, 166
- iv 7-8, 147
- iv 12-13, 147
- iv 12, 156

*Hosea*

- vi 8, 40
- xi 9, 210
- xii 12, 40

*Joel*

- i 1, 166
- i 4, 196, 197
- i 13ff., 208
- ii 2, 10, 45
- ii 15-27, 45
- ii 25, 196, 197

*Amos*

- i 1, 28, 35, 40, 166, 167, 237, 267
- i 2 - ii 6, 40
- i 2, 167
- i 3, 37, 40
- i 5, 40
- i 13, 37
- i 14, 40
- iv 11, 35
- v 18, 125, 171, 210, 211
- v 20, 45, 116, 125, 171
- vi 8-11, 35
- vi 13-14, 40, 179
- vi 13, 39
- vii 1-6, 189
- vii 2, 5, 40
- vii 8, 148, 208
- vii 9, 40
- vii 10-17, 164, 177, 216
- vii 10, 187
- vii 11, 40, 41
- vii 17, 41
- viii 2, 148
- viii 8, 35
- viii 13, 41
- ix 1, 35

*Obadiah*

- 1, 166

*Jonah*

- iii 6ff., 208

*Micah*

- i 1, 166
- i 8, 130
- i 16, 195
- ii 4, 156
- iii 6-7, 64
- iv 9, 133
- vi 5, 276
- vii 14, 138

*Nahum*

- i 1, 166
- iii 12f., 123

*Habakkuk*

- i 1, 65, 115, 166
- ii 18, 133
- ii 20, 216

*Zephaniah*

- i 1, 166
- i 15, 45
- ii 8, 10, 131

*Zechariah*

- i 4, 205
- x 11, 138
- xi 7, 10, 11, 14, 123
- xiii 5, 65
- xiv 4f., 237
- xiv 5, 35, 267

*Malachi*

- i 1, 166

*Psalms*

- ii 11, 126
- xviii 28, 116
- xxiii 1, 4, 138
- xxvi 4, 168
- xlvi 6, 9, 11, 131
- xliv 17, 131
- xlvi 9, 115
- lv 6, 126
- lv 13, 131
- lvii 4, 131



*Psalms*

- lx 4-5, 13  
 lx 6, 216  
 lx 8-9, 13f.  
 lx 8, 38  
 lxiii 3, 214  
 lxvi 5, 96, 115  
 lxxii 20, 165  
 lxxiii 2, 110  
 lxxiv 10, 131  
 lxxiv 12-17, 131  
 lxxiv 13, 123  
 lxxiv 18, 131  
 lxxviii 23ff., 171  
 lxxviii 23, 24, 124  
 lxxix 10, 12, 20ff., 39ff., 50, 52, 131  
 lxxxii 1, 115  
 lxxxiv 4, 199, 200, 211  
 lxxxix, 179  
 cii 9, 131  
 ciii 9, 171, 209  
 civ 2, 62, 74  
 civ 20, 115  
 cvi 37, 170  
 cviii 8, 38  
 cxvii 8, 124  
 cxix 20, 63  
 cxix 42, 130, 131  
 cxix 120, 125  
 cxix 126, 123

*Proverbs*

- i 1, 165  
 i 7, 158  
 v 19, 116  
 vii 18, 116  
 x 1, 165  
 xii 28, 129, 148  
 xiv 31, 131  
 xxii 17, 165  
 xxii 24, 168  
 xxiii 9, 158  
 xxiv 23, 165  
 xxv 1, 165  
 xxvi 2, 199, 200, 211  
 xxvi 7, 110  
 xxvii 11, 130  
 xxx 1, 165  
 xxx 17, 195  
 xxxi 1, 165

*Job*

- i 12, 172  
 ii 6, 172  
 iv 14, 126  
 iv 15, 126  
 v 7, 203  
 xii 25, 125, 210  
 xv 17, 115  
 xvi 12, 123  
 xix 26f., 214  
 xxi 32, 116  
 xxix 18, 130  
 xxx 3, 113  
 xxx 29, 130  
 xxxii 10, 115  
 xxxvi 2, 115  
 xxxviii 8, 10, 11, 123  
 xxxix 27-30, 195  
 xxxix 28, 303

*Song of Songs*

- i 1, 166  
 i 9, 265  
 iv 8, 168, 203

*Lamentations*

- ii 9, 123  
 ii 14, 65, 138  
 iii 2, 125  
 iii 31, 171, 209

*Ecclesiastes*

- i 1, 166  
 v 5, 214  
 vii 16f., 214  
 xii 4, 200

*Ezra*

- i 1, 166  
 ix 4ff., 208

*Nehemiah*

- i 1, 166  
 iii 18, 67  
 vi 3, 214  
 vi 10ff., 131  
 vi 10, 281  
 vi 13, 131

*1 Chronicles*

i 43, 52  
ii 14, 230  
v 11-17, 40  
v 26, 40  
xx 7, 131

*2 Chronicles*

vi 26, 123  
vii 13, 123  
xxxii 17, 131

*Ecclesiasticus*

xi 3, 172  
xliii 17, 203

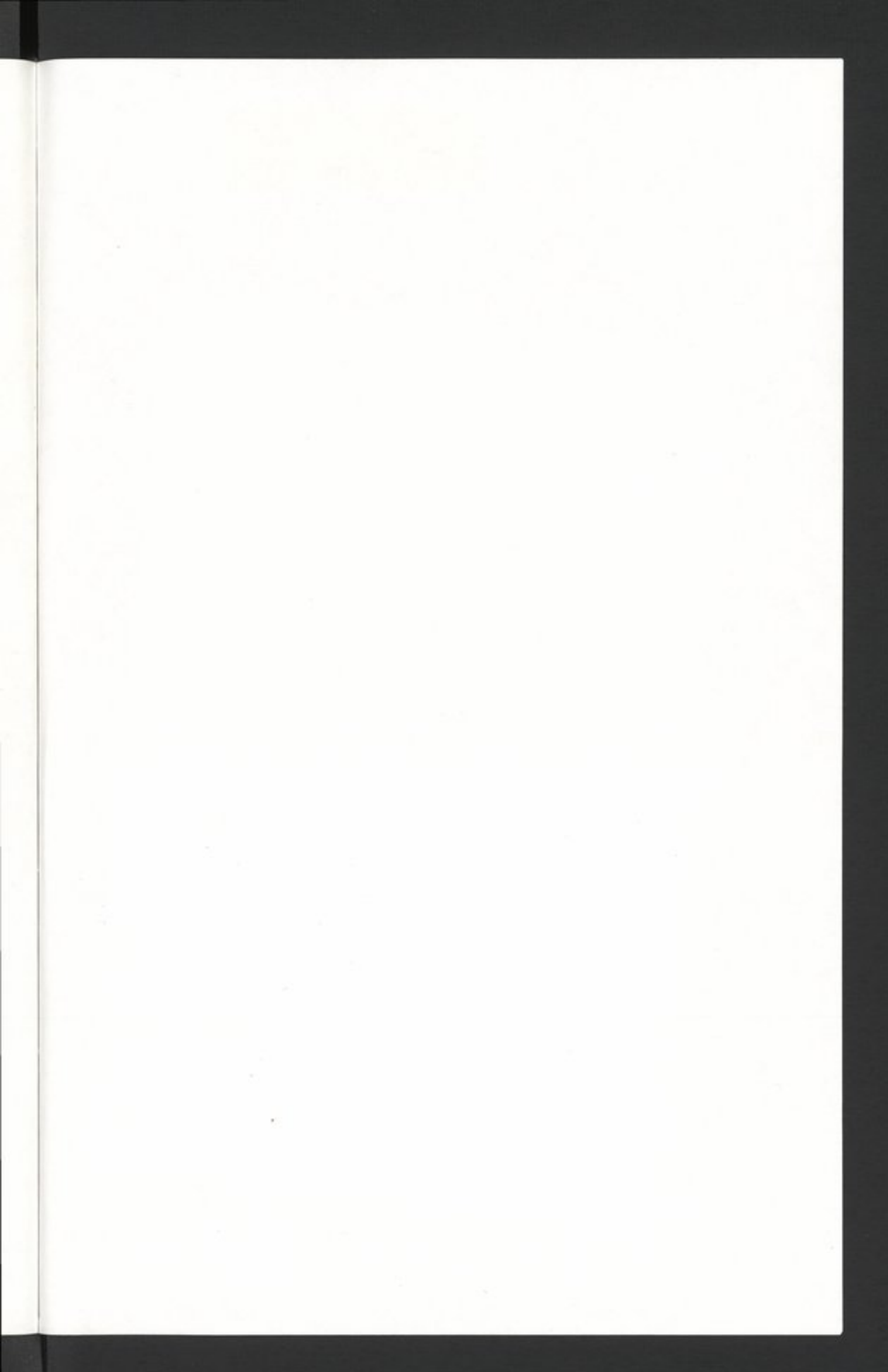
*Matthew*

iii 2, 302  
iv 17, 302

*2 Thessalonians*

ii 7, 187







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