THE DAN DEBATE

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THE DAN DEBATE

THE TEL DAN INSCRIPTION IN RECENT RESEARCH

Hallvard Hagelia

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Contents

Preface

The original impetus for working with the Tel Dan inscription was my participation in a Norwegian translation project. Ancient texts with relevance for the study of the Old Testament needed to be translated into Norwegian. After having translated the Siloam inscription (Hagelia 2002), I began working on my own initiative on the Tel Dan inscription, which by then was a brand new find, not part of the original translation program. I very soon realized that a study of this inscription demanded a study of the debate following its publication.

For different reasons, the translation project was delayed and, ultimately, abandoned.

The discovery of the Tel Dan or 'House of David' inscription—it has been named differently—was a sensation. The text itself is of great importance, and a lot has been written on it. The actual debate is an important matter in itself for different reasons.

The present investigation refers to what are written in English, German and French. I cannot promise that it is all-inclusive, but I doubt that any substantial contribution or any significant—printed—opinion has escaped my attention. Most of the discussion has conducted in English. To my knowledge, nothing has so far been written on it in any of the Scandinavian languages, except for a short paragraph in Oredsson (2002: 34) and my own publications (e.g. 2004a).[1]

This investigation is a critical research history, intending to mirror the most important questions on the agenda regarding the inscription. The debate is followed historically, systematically and critically. My own critique and evaluations will follow currently and be summarized in some general deliberations and conclusions in the last chapter.

Writing history on so short a period as from the year of 1993 until today is somewhat risky, as we lack the historical long perspective. New discoveries might illuminate or complete the fragments concerned. Future scholarship might see clearer particular features not so easily visible within our short perspective. But we cannot wait for future distant perspectives. At

1. I have not made any investigation in other languages, except for casually observing some articles in Polish, Dutch and Italian, which I do not read confidently.These works are recorded in the Bibliography.

every stage of the process scholars have to carry on research with the perspectives available. Ours are the short ones.

Some readers would probably ask for a particular perspective on this research itself, a thesis. Is it possible to formulate and defend or refute a single thesis on this debate as a whole? The answer, perhaps, is: 'Yes, but not easily!' This debate has been too multifaceted to be put easily on one single formula. Some scholars have no other expressed agenda than presenting as objectively as possible the actual text in its historical, linguistic and religious setting. Of course, it is possible to trace subtle philosophical ideas or biases underlying their scholarship or setting the path for their investigation. Other scholars have a particular agenda or even outspoken ideological intention when lining up for start.

This monograph is a presentation of the most significant aspects of the debate. No single thesis is set forth, defended or refuted, for this work seeks to illuminate the different theses set forth, illuminated, supported or refuted by others who have written on the Tel Dan inscription.

This book is the second part a bigger project. The first part of it has been published by Uppsala University in 2006, under the title *The Tel Dan Inscription: A Critical Investigation of Recent Research on Its Palaeography and Philology*, in the series Acta Universitatis Upsaliensis, Studia Semitica Upsaliensia 72. That book is also distributed by Sheffield Phoenix Press.

Why all this work on a text comprising just 170 or so letters? Has the text not been discussed beyond measure already? The answer, quite simply, is that the Tel Dan inscription is an extremely important text, and that the extensive debate following its publication deserves a critical follow-up. The inscription comes from a period of time and a geographical area with sparse literary sources. It has implication for understanding the Aramaic language and palaeography, the history of the Arameans as well as the general history of the Levant at latter part of the ninth century BCE. It is also relevant for understanding the text of the Hebrew Bible, Middle Eastern archaeology and history of religion; it is something of a litmus test for different basic scholarly positions, not least the—somewhat over simplified—positions between the so-called 'maximalists' and 'minimalists', or 'revisionists' as some would prefer to call themselves. These labels are usually not used by the adherents themselves, but are often used by their opponents.

Professor Stig Norin, Uppsala, inspired me to continue with my investigation at an early stage of the process, when I was questioning whether it was a worthwhile enterprise. In Uppsala, where in 1994 I earned my doctoral degree (published in the same year) with the mentoring of Professor Magnus Ottosson and the late Docent August Ragnar Carlson, questions relating to archaeology have traditionally been given particular attention. I

should also express particular gratitude to Professor Emeritus John A. Emerton and Professor Graham I. Davies, both at Cambridge. Professor Emerton has spent valuable time with me discussing linguistic problems connected to the inscription, as well as the debate in general. Professor Emerton has himself been a diligent contributor to the linguistic part of the debate. With Professor Davies I discussed my project in general, as well as some particular aspects with the debate. Professor Davies, though he has not contributed directly (in writing) to the debate, has given me good guidance and feedback on earlier drafts of the present work. I also appreciate the support of my good friend of many years, Dr Jack Lundbom, for posing difficult questions related to my project, questions which forced me to give careful thought to my project in general and particular approaches in it.

Kristiansand, 11 May 2006

ABBREVIATIONS

AbrN	*Abr-Nahrain*
AHw	*W. von Soden, Akkadisches Handwörterbuch* (Wiesbaden: Harrassowitz, 1959–81)
ANET	James B. Pritchard (ed.), *Ancient Near Eastern Texts Relating to the Old Testament* (Princeton: Princeton University Press, 3rd edn, 1969)
AOAT	Alter Orient und Altes Testament
BA	*Biblical Archaeologist*
BAR	*Biblical Archaeology Review*
BASOR	*Bulletin of the American Schools of Oriental Research*
BI	*Biblical Interpretation*
BK	*Bibel und Kirche*
BN	*Biblische Notizen*
BR	*Bible Review*
CAD	Ignace I. Gelb *et al.* (eds.), *The Assyrian Dictionary of the Oriental Institute of the University of Chicago* (Chicago: Oriental Institute, 1964–)
CAL	*Comprehensive Aramaic Lexicon* (Web based, Hebrew Union College)
CIS	Copenhagen International Seminar
COS	W.W. Hallo and K. Lawson Younger, Jr (eds.), *Context of Scripture: Canonical Compositions from the Biblical World* (3 vols.; Leiden: E.J. Brill, 1997–2002)
CRBS	*Currents in Research: Biblical Studies*
DDD	K. van der Toorn, B. Becking and P.W. van der Horst, *Dictionary of Deities and Demons in the Bible* (Leiden: E.J. Brill; Grand Rapids: Eerdmans, 1999)
EI	*Eretz-Israel*
HAE	J. Renz and W. Röllig, *Handbuch der althebräischen Epigraphik* (4 vols.; Darmstadt: Wissenschaftliche Buchgesellschaft)
HALOT	L. Koehler, W. Baumgartner and J.J. Stamm, *The Hebrew and Aramaic Lexicon of the Old Testament* (trans. and ed. under the supervision of M.E.J. Richardson; 4 vols.; Leiden: E.J. Brill, 1994–99)
HSS	Harvard Semitic Studies
IDB	George Arthur Buttrick (ed.), *The Interpreter's Dictionary of the Bible* (4 vols.; Nashville: Abingdon Press, 1962)
JANES	*Journal of the Ancient Near Eastern Society*
JBL	*Journal of Biblical Literature*
IEJ	*Israel Exploration Journal*
JBL	*Journal of Biblical Literature*
JSS	*Journal of Semitic Studies*
JSOT	*Journal of the Study of the Old Testament*
JSOTSup	*Journal for the Study of the Old Testament*, Supplement Series
KAH	*Keilschriftexte aus Assur historischen Inhalts*
KAI	H. Donner and W. Röllig (eds.), *Kanaanäische und aramäische Inschriften* (3 vols.; Wiesbaden: Otto Harrassowitz, 1962–64)
NABU	*Nouvelles assyriologiques breves et utilitaires*
NEA	*Near Eastern Archaeology*
NedTT	*Nederlandse theologisch tijdschrift*
NRSV	New Revised Standard Version

OEANE	E.M. Meyers (ed.), *The Oxford Encyclopedia of Archaeology in the Near East* (New York: Oxford University Press, 1997)
OTS	Old Testament Studies
PEQ	*Palestine Exploration Quarterly*
RB	*Revue Biblique*
RBL	*Review of Biblical Literature*
RlA	*Reallexikon der Assyrologie*
SBL	*Society of Biblical Literature*
SEÅ	*Sven Exegetisk Årsbok*
SEL	*Studi Epigrafici e Linguistici*
SJOT	*Scandinavian Journal of the Old Testament*
TDOT	G.J. Botterweck and H. Ringgren, *Theological Dictionary of the Old Testament* (Grand Rapids: Eerdmans, 1974–)
THAT	Ernst Jenni and Claus Westermann (eds.), *Theologisches Handwörterbuch zum Alten Testament* (Munich: Chr. Kaiser, 1971–76)
TTK	*Tidsskrift for Teologi og Kirke*
TUAT	D. Conrad *et al.* (eds.), *Texte aus der Umwelt des Alten Testaments* (10 vols.; Gütersloh: Gütersloher Verlagshaus–Gerd Mohn, 1982–88)
TZ	*Theologische Zeitschrift*
UF	*Ugarit-Forschungen*
VT	*Vetus Testamentum*
VTSup	*Vetus Testamentum*, Supplements
WBG	*Wissenschaftliche Buchgesellschaft*
ZAH	*Zeitschrift für Althebräistik*
ZAW	*Zeitschrift für alttestamentliche Wissenschaft*

Chapter 1

INTRODUCTION

1. *The Discoveries*

Tel Dan, Tell el-Qadi in Arabic, is located under the southern slope of Mt Hermon, in Upper Galilee, not far from the headwaters of the River Jordan. The location is identified with biblical Dan, which, together with Beersheba, marks the traditional southernmost and northernmost points of Israel, as in the biblical phrase: 'from Dan to Beersheba'.[1]

Tel Dan has been extensively excavated, not least by Avraham Biran, the 'Grand Old Man' of Israelite archaeology,[2] who has worked at Tel Dan since the 1960s. Excavation of Tel Dan started in 1966 as an emergency project by the Israel Department of Antiquities, but soon developed into a fully fledged archaeological expedition. In 1974, it became the major archaeological project of the Nelson Glueck School of Biblical Archaeology of Hebrew Union College–Jewish Institute of Religion in Jerusalem.[3]

1.1. *Previous Discoveries at Tel Dan*

The inscription discussed in this monograph is not the first text to have been found at Tel Dan. However, none of the others has been anywhere near as important as this inscription.

A small inscription was found on the surface in 1965. It was an inscription written on the base of a pottery bowl, which read 'Of the butchers'.[4]

Already, in his first year of excavation at Tel Dan (1966), Biran found a small potsherd inscribed with the four letters לאמץ, 'belonging to Amoz'. This is the same name as the father of the prophet Isaiah,[5] who prophesied in the eighth century. The potsherd was also dated to the eighth century. Amoz

1. See Judg. 20.1 and 1 Sam. 3.20.
2. See *Celebrating Avraham: Avraham Biran, The Excavator of Dan at 90* (Shanks 1999).
3. See Biran and Naveh 1993: 81.
4. See *BAR* 20.2 (1994a): 28.
5. See 2 Kgs 19.2; 2 Chron. 26.22; Isa. 1.1.

was a fairly common name at that time, and the name is not necessarily to be connected to Isaiah's father.

A similar inscription—in Phoenician—was found on another potsherd in 1968: it read לבעלפלט, 'belonging to Baalpelet'. Since this name means something like 'may Baal rescue', the sherd probably originates from a jar owned by a non-Israelite. In 1988, yet another sherd with the letters ל and ט was found, two of the letters from the name בעלפלט. We do not know whether this was also from a jar 'belonging to Baalpelet', or who this Baalpelet actually was.

In 1976, a bilingual inscription—Greek and Aramaic—was found on an engraved stone with the text 'God who is in Dan'. On the basis of this text, the site was identified as biblical Dan. Uehlinger (1994) and Noll (1998) have tried to link this text to the Tel Dan stele.

In 1986, a jar handle with the inscription לעמדיו, 'belonging to Immadiyo', was discovered. It was found in a layer dated to the Assyrian conquest of northern Israel by Tiglath-pileser III in 733/732. The affixed element יו־, in the name עמדיו is a short form of יהוה, and the name is related to עמנו־אל, Immanuel (Isa. 7.14). עמדיו is a Yahwistic name, probably revealing his parents as devout Yahwists.

Another Yahwistic name was uncovered in 1988 on an eighth-century jar handle: it read זכריו, the personal name Zechariah, which can be translated 'Yahweh remembers' or 'may Yahweh remember'. The name is well known and occurs in several variants.

These were important discoveries, but they were, nevertheless, very brief inscriptions. No extensive text had been found at Tel Dan until this epoch-making discovery in the summer of 1993, and the two additional fragments in 1994.

1.2. *The Discovery of Fragment A*

In 1992, the Israel Government Tourist Corporation (IGTC) and the Antiquities Authority (AA) decided that Tel Dan 'was a site worthy of a major conservation and restoration project, so that…the site can be properly presented to visitors'.[6] Following this decision, preparations were made to remove debris from the eighth-century Assyrian destruction level outside the city-gate, following which a ninth-century gate was unexpectedly discovered. This gate represented an additional outer gate leading to the main city-gate complex. Other interesting finds of the same summer were a decorated capitol and five standing stones, presumably cultic stones or *mazzebot*. A low platform, 'probably either, for the city's ruler…or a pedestal for the statue of a deity'[7] had been discovered several years earlier. It had been concluded

6. Quoted from *BAR* 20.2 (1994a): 31.
7. See *BAR* 20.2 (1994a): 31.

from these discoveries that the small plaza between the outer and inner gates had a small 'gateway' sanctuary that could be considered a *bamah* of the kind found in 2 Kings 23.[8]

Between the previously known city gate and the one found in 1992, on the eastern side, there was an open plaza of around 475 square yards, three times as big as the city gate plaza. This was cleared in 1993 by Biran and his team. The famous stele was found on the eastern side of this plaza, apparently having been reused as building material in a wall. The archaeologists described the discovery as an unforgettable moment.[9]

The stone itself was actually discovered by the surveyor of the expedition, Gila Cook, who, in 1994, also found Fragment B2. Taking a closer look at the stone while still *in situ*, and helped by the direction of the early afternoon rays of the sun, which illuminated the engraved lines on the stone, they could see the contours of the written letters quite clearly. The stone was easily removed as only a small part of it was embedded in the ground. Turning the stone to face the sun, the letters became even more legible. The words, separated by dots, sprung to life.

The height of the fragment is 32 cm, with a maximum width of 22 cm. The stone was smashed in antiquity, and its original size is estimated to have been around one metre high and perhaps half a metre wide. The stele is made of the local basalt, its face is smooth, as is its right hand side, as seen by the reader. The letters were probably made with the aid of an iron chisel with a rounded edge.

The secondary usage of the fragment could be dated by means of the destruction level covering it. According to archaeological evidence, Biran and Naveh estimated that the gate complex was destroyed in the third quarter of the eighth century BCE, at the time of Tiglath-pileser III's conquest of northern Israel in 733–732. The stele would, accordingly, have been put into the wall some time before that date, 'although it is difficult to determine how much earlier' (1995: 86).

1.3. *The Discovery of Fragments B*

A year later, in June 1994, two more fragments were found at Tel Dan, identified by Biran and Naveh (1995) as being from the same stele. The first fragment (called Fragment B1) was discovered by area supervisor Malka Hershkowitz on June 20. It was found 13 metres to the south-west, in a wall built on the flagstone pavement of the square, 80 cm above the level of the pavement. This piece has a flat surface, 15 × 11 cm, with clearly written words, separated by dots, like Fragment A.

8. See *BAR* 20.2 (1994a): 33.
9. See *BAR* 20.2 (1994a): 33.

A few days later, on June 30, another small fragment (called Fragment B2) was found in a layer dated to the end of the ninth, or the beginning of the eighth century BCE. This fragment was also found by Gila Cook, who had found Fragment A, the previous year. This fragment was used as a pavement, and measured 9 × 6 cm. It contains four lines of clearly legible letters, separated by dots, like Fragments A and B1.

The pavement where the fragment was found was covered by debris dated to the Assyrian destruction. The pavement could not have been laid later than the Assyrian conquest, according to Biran and Naveh. Since the latest pottery collected from the layer of debris is from the end of the ninth or the beginning of the eighth century, the pavement could not have been laid before the beginning of the eighth century.

Fragments B1 and B2 are identified by Biran and Naveh as originally belonging to the same stele. This is a commonly held opinion today, which is supported by the fact that the fragments apparently fit together physically. But, as we will see, some scholars have doubts as to whether Fragment A and Fragments B1 and B2 belong to the same text. Even more scholars have doubts as to whether Biran and Naveh have added Fragments B1 and B2 correctly to Fragment A.

2. *Publication*

The first modest presentation of the find of the Tel Dan inscription occurred at the SBL International Meeting in Münster, Germany, in 1993, just a few days after its discovery. At that time Davies (1992) and Thompson (1992) had recently published their books on the possibility of writing a history of Israel. These books were a major focus of the conference. In 2001, Ehrlich published an article in the P.E. Dion Festschrift which retells vividly and personally the story of how the discovery of the Dan inscription reached the scholarly world outside of Israel:

> In a crowded plenary session, these two [Davies and Thompson] advocates of the so-called cautious or minimalist school of biblical interpretation were joined on the podium by Whitelam and Blenkinsopp. In the ensuing discussion, the position of the gentlemanly Blenkinsopp was, to a great extent, drowned by the strenuous arguments of his interlocutors. It appeared that we were witnesses to the birth of the new mainstream in biblical studies, one that denies the efficacy of employing the biblical texts in the reconstruction of a history of—for lack of a better term—'biblical Israel', which is considered to be an invention of the Persian, or preferably, the Hellenistic period.
>
> During a break in the Münster discussion, I encountered a friend who had attended a sparsely attended concurrent session. At that session, the Israeli archaeologist Barkay had mentioned in an aside that an inscription had just been found at Tel Dan, an inscription in which the 'House of David' was mentioned. In his opinion, this indicated that the historical memory of the

> biblical texts extended much further back in time than Davies, Thompson and Whitelam were willing to concede. My friend and I speculated about how this news would be greeted by the speakers at the plenary session, who continued to debate the issue of Israelite history without the benefit of the latest significant datum (Ehrlich 2001: 57-58).

This announcement poured petrol over the already over-heated debate on the issue of ancient Israelite history.

The find was followed up by big headlines in the *New York Times*[10] and *The Chronicler*[11] and other secular media, as well as initiating a lively debate among scholars on the Internet.[12] In Italy, the news of the inscription hit the media 'with great effect and sensational headlines', Garbini noted. On 7 and 8 August the news was in almost all the daily newspapers, while on the 23rd of the same month the *Corriere della Sera* published an interview with Avraham Biran.[13]

Biran himself presented the text at the SBL Annual Meeting in Washington in November 1993, and 'the 60 seminar participants gave the 85-year-old-archaeologist a standing ovation'.[14] Lemche and Thompson (1994: 4) called the happening a 'euphoria'. The text itself they characterized as 'immensely valuable in furthering our understanding of the biblical traditions' (1994: 8), but Thompson (1995a: 61) compared the 'circus atmosphere' at the Washington SBL Annual Meeting with the 'barnstorming that many audiences suffered' when the Ebla texts were found twenty years earlier, when the Patriarchs and Sodom and Gomorrah had, allegedly, been discovered. Thompson himself preferred his own 'more critical historical reading'.

3. *An Archaeological Sensation*

The importance of this text is characterized with grandiose words by scholars. Just a few examples: Halpern (1994: 63) wrote about the 'considerable enthusiasm' with which the discovery was met, 'particularly as a tonic against denials that there had been an Israelite state in the tenth century B.C.E.'.[15] Chapman (1993–94) called it 'one of the most important discoveries ever made in Levantine archaeology', and claimed that 'it will be at the

10. The news broke on 16 November 1993.
11. Summer 1993. *The Chronicler* is published by Hebrew Union College.
12. See Ben Zvi 1994: 26 n. 6.
13. See Garbini 1994.
14. *BAR* 20.2 (1994d): 47.
15. Halpern 1994: 63: 'Until the stela's discovery, the formation of a state of Israel could not be dated later than the mid-ninth century B.C.E., because Assyrian epigraphs of the 850s and 840s B.C.E. and the roughly contemporary Mesha stela mentioned kings of Israel, some (Ahab, Omri, Jehu, and, later, Joash) by name'.

centre of all discussion of the Israelite kingdoms for as long as the archaeology of this period and region are studied'. Lemaire (1994a: 31) claimed, on the basis of Fragment A, that it 'is indeed sensational and deserves all the publicity it has received', because it 'easily establishes the importance of Israel and Judah on the international scene at this time—no doubt to the chagrin of those modern scholars who maintain that nothing in the Bible before the Babylonian exile can lay claim to any historical accuracy'. According to Andersen (1998: 45), '[i]t would be impossible to exaggerate the importance of this discovery'. Paul E. Dion (1999: 154) claimed that the discovery of this fragmentary stele 'certainly will remain an important contribution to the historical study of the Levant during the Iron Age'. Finkelstein and Silberman (2001: 19) called it 'the single most significant inscription for historical validation… It provides an extra biblical anchor for the history of ancient Israel.' In their opinion, this artefact will 'change forever the nature of the debate' on the existence of David and Solomon (2001: 129-30). Carl Ehrlich (2001: 57) called it 'one of the most important epigraphic finds made in Israel in the nineties or in any decade'.

There has been general consensus that this is an extremely important textual find. However, opinions have differed as to how the inscription should be interpreted.

4. *A Controversial Text*

The Tel Dan inscription has proved to be a very controversial text. As we will see, while the swift publication of the *Editio Princeps* of Fragment A and B1 and B2 by Biran and Naveh (1993, 1995)—here referred to for convenience as 'the Editors'—has been commended by the majority, the work has been criticized by some scholars on the level of reconstruction and translation.

Several questions were opened up after the initial publication of the Tel Dan fragments:

The biggest headlines were not saved for the discovery of the stele itself, but were reserved for the phrases 'king of Israel' and 'House of David' that were soon understood to appear on it. In particular, a great deal of energy has been expended on the compound word ביתדוד, which, notably, is written as one word, not divided into two words with a word-dividing dot—בית · דוד.[16]

16. In this book the Aramaic text will be referred to in different ways. The inscription itself is written in Palaeo-Aramaic script, which, for convenience and technical reasons, will not be reproduced. Wherever authors to whom I refer quote the text in square script, I will follow their lead. Where they quote the text in transcription into the Latin alphabet, I will do the same.

Another main focus has been the interpretation of the word דוד. Should it be vocalized דָּוִד, as in the personal name David, or דּוֹד, as a genealogical term,[17] or as a term referring to a divinity?[18]

In non-biblical inscriptions, 'Israel' is mentioned for the first time on the Merneptah stele from Egypt, dated to around 1220 BCE.[19] But the Dan stele is, perhaps, the oldest non-biblical Semitic documentation of the name 'Israel'. And if the interpretation of ביתדוד as 'House of David' is correct, this is, perhaps, the first non-biblical reference to David and his dynasty;[20] certainly, it is 'the first time we have an inscription devoted exclusively to an Aramean–Israelite war' (Sasson 1995: 22).

However, at the same time, a series of other questions has been discussed, such as issues of grammar and linguistics, the history of the language, palaeography, archaeology and historiography, and so on. In particular, there has been an intense linguistic discussion on the question of the *waw consecutive*.[21]

An important meta-question has been the debate over so-called 'minimalism' or 'revisionism' vs. 'maximalism'. Or, as we will conclude, the debate on the Tel Dan inscription is rather a meta-debate on this debate, or a symptom of it.

The discussion surrounding the inscription has been a very intense debate. Müller (1995: 121) wrote laconically of 'ein lebhaftes Echo' ('a lively echo'),[22] while Knauf (1996: 9) described it as 'a small hurricane through the biblical foliage'. Those who already thought that King David really existed found his existence confirmed by this inscription, while those who do not believe in David's historical existence, Parker claimed, 'move heaven and earth, to get supposed evidence out of the world again'.[23]

Unfortunately, scholarship on these three scraps of inscription has sometimes radiated as much heat as light, with political and personal passions discernible in the language of the debaters. Couturier (2001: 73) has commented that 'It is rare that in so short a time such a discovery could arouse so intense a debate'.

Not all aspects of the debate have been equally just or fair. In some cases it has been less than flattering, even dirty. While preparing to write this

17. *HALAT*, 206B gives four alternative translations: '1. Geliebter, Liebhaber, 2 a) Vaterbruder, b) Vetter'.

18. On the deity דוד, see Barstad 1999: 259-62.

19. On the Merneptah stele, see, e.g., D.B. Redford in *ABD*: IV, 700-701 with bibliography. For the text, see *ANET*, 376a-78a, particularly p. 378a.

20. Cf. later, Lemaire's theory that he can see 'House of David' in the Mesha stone.

21. Cf. Hagelia 2006b: 136-56.

22. Non-English quotations will, in most cases, be translated into English, except for more extensive ones or ones where it has been deemed important to render them literally.

23. See Parker 1997: 156 n. 30.

introduction I deliberated over whether the less auspicious aspects of the debate should be referred to at all. On the one hand, such elements do not contribute very much to the academic discussion, being somewhat tabloid, while, on the other hand, it illustrates how important the different positions have been and still are to the debaters, showing the intensity of their engagement. This is a text with wide implications, and the expression of strong feelings has been an important aspect of the debate. I have, therefore, look at the darker side of the debate somewhat.

The discovery of fragments B1 and B2 in the summer of 1994 impacted significantly on the scholarly debate that had been initiated by the discovery of Fragment A the year before. It did not change the scholarly landscape completely, however. The interpretive problems remained basically the same, while, of course, important new text was added by the new fragments. The debate about the Tel Dan inscription did not start anew with the discovery of the new fragments.

The most important new aspect of the debate was whether the new fragments were really part of the original fragment found a year earlier.

The questions related to the term ביתדוד (*bytdwd*), were not directly influenced by the additional text. But new aspects were brought in with implications for the *bytdwd* question, as it became possible—at least for many scholars—to identify named kings of Israel and Judah. If the Editors are right in identifying the two kings as Jehoram of Israel and Ahaziah of Judah, then we had been brought forward an important step. This is the most important new historical contribution of the additional fragments.

The new fragments contributed further to illuminating linguistic questions already raised by Fragment A. This debate was intensified in the latter part of the 1990s and has continued into the new millennium with Muraoka and Emerton as two of the most high-profile contributors (cf. Hagelia 2006b: 140-43, 146-48).

Have the new fragments solved any problems raised by the first fragment? Only partially. Several problems are still unsolved and will continue to be discussed. Nevertheless, the fragments have contributed to achieving a broader consensus that *bytdwd* refers to ‘House of David’.

My own opinion is that the three fragments (A and B1 and B2) belong to the same stele (see later), and that it remains possible that more fragments could come to light as excavations continue. New fragments might solve some questions, but, obviously, raise others.

Lemaire (1998: 3) summarized that up until 1998 more than 60 articles had been written on the Dan stele. More have followed in the years since Lemaire’s writing. Na’aman (2000: 92) wrote of ‘a never-ending stream of articles dealing with a wide variety of its direct and indirect implications’. My bibliography illustrates the accumulation of that ‘never-ending stream’.

The year 2003 saw the publication of the first doctoral dissertation on the Dan inscription, by George Athas, which had been submitted to the University of Sydney in 1999.[24] To summarize with Athas: 'The interest that these fragments have generated demonstrates their value in the eyes of the scholarly community' (2003: 1).

In 1997, Parker said that it would be 'desirable, in the near future, to have a monograph that reviews all the evidence and all the arguments from a less engaged perspective' (1997: 157 n. 30). While Athas's work can rightly be considered such a monograph *on the stele*, it does not deal with 'all the evidence and all the arguments'. The present investigation (and Hagelia 2006b) sets out to be a similarly rigorous and detached treatment of the *discussion* surrounding the inscription.

5. *Methodology*

The present study treats almost all contributions that have been made to the scholarly discussion—significant and comprehensive ones, as well as less substantial ones. My bibliography comprises all publications that have come to my attention.[25] Some of the articles are very substantial and command heavy scholarly weight. Others are smaller remarks or standpoints with no thorough discussion attached. Some are important contributions on the Hebrew Bible and related scholarship in general that merely mention the Tel Dan inscription *en passant*. The substantial articles are, of course, the most important ones for the present study, and will duly receive the closest attention. At the same time, some of these smaller remarks have come from distinguished scholars, and are, therefore, of importance as expressions of opinion, even if they are non-argued statements.

My intention is to chronicle, as objectively as possible, how scholars have wrestled with this particular inscription, which is partly rather opaque, giving the highest priority to documenting and articulating the opinions, as expressed by the writers themselves. I do this often through the use of quotations, which are intended to present the opinions sympathetically and without seeking to debunk or otherwise filter the presented views. It is my hope that each author will be able to say that *his*[26] opinion has been reported adequately.

24. See my review of Athas's work in Hagelia 2004b.

25. Not all contributions mentioned in the bibliography are actually referred to in the text.

26. This has, indeed, been a 'male' discussion! Few, if any, female names have been observed among the contributors.

Having said this, it is my expressed intention to approach the debate critically. By this I do not mean that each contributor and contribution is *criticized*, but that the debate itself is critically edited and the main opinions are critically evaluated.

As a researcher, I am forced to engage what is actually written. Orally expressed opinions are not referred to here. The private opinions of this or that scholar are not accessible for criticism if they have not been published. Hopefully, this book will mirror representative opinions on the Dan stele and its text.

Since the year 2002, the publication of articles on the inscription has come to an almost complete halt, while the number of books commenting on it is increasing. The single most important book on the inscription is the published dissertation by Athas (2003), to which my discussion will return repeatedly. Accordingly, the present study concentrates primarily the formative years of the Tel Dan debate, between 1993 and 2003.

It is fair to say that the debate has now moved into another phase—one not so easy to track, since it continues mostly as scattered references in books and articles. Logistically, it is easier to trace what is written in scholarly journals than what appears in monographs and edited volumes. To be sure, when the Tel Dan inscription is mentioned, most often the primary focus is elsewhere, not on the inscription itself. Each book has its own agenda, and books tend to convey (one might even say rehash) previously argued opinions to illuminate their own aim or thesis. It would be a bold claim to say that nothing new has been said about the Tel Dan inscription outside of the 1993–2003 period that occupies the present study, yet it could be argued that scholarship has now accepted the view that not much more remains to be said.[27]

And so, while it has to be admitted that this book will not contribute new information about, or new insights into, the Tel Dan inscription, by tracing the debate critically, it will, hopefully, contribute to setting the debate in perspective.

27. Note that I offered a philological investigations of the inscription's Aramaic text as well as a critical investigation of the philological debate on the inscription in an earlier study (Hagelia 2006b).

Chapter 2

THE INSCRIPTION

The process of establishing the text of the Tel Dan inscription will not be pursued here as this job has already been done in one of my earlier studies (Hagelia 2006b), which has a synopsis of 33 different reconstructions of the text. In addition, the earlier work offers a text-critical analysis of the text and its palaeography, as well as a dating of the text, a grammatical analysis, a glossary with semantic analysis, a dialectographic analysis and a comparison between the content of this particular inscription and similar inscriptions from the Levant from the first half of the last millennium BCE.

On the back of that textual analysis I have established the following text as reasonable:

1. [ ]וגזר[ ]די ע[מר · א],
   …[sa]id (?) tr[eaty (?)] and he cut [ ] (syntax is unclear).

2. [ ]תלחמה · בא־[ ]בה[ ]יסק · אבי·[ ],
   [ ] my father went up…[fig]hting against/at…

3. וישכב · אבי · יהך · אל [אבהו]ה · ויעל · מלכ י[ש]/ראל
   and my father lay down, he went to his [ancestors], and the king of I[s]/rael.

4. ויעל · מלכ י[ש]/ראל · קדם · בארק · אבי [· ו]יהמלך · הדד[·]א[יתי
   and the king of I[s]/rael previously entered my father's land, [and] (the god) Hadad made [me] king.

5. אנה · ויהך · הדד · קדמי [· ו]אפק · מן · שבע[ת·---[(?)]
   …I, or: me. And Hadad went in front of me, [and] I departed from seven…(?)

6. י · מלכי · ואקתל · מל[כן · שׁב]ען · אסרי · א[לפי · ר]/כב
   …s of my kingdom. And I killed seven[ty] kin[gs] and captured th[ousands of cha]/riot(s).

7. [· אחאב] · בר · יהו]רם · איתּ · [קתלת · פרש · ואלפי · כב]/[ר
   …[cha]/riot(s), thousands of horsemen (or: horses). [And I killed Jeho]ram, son of [Ahab]"…

8. [מלך · ישראל · וקתל[ת · אית · אחז]יהו · בר[· יהורם · מל]
…king of Israel. And [I] killed [Ahaz]iah son of [Jehoram ki]/ng…

9. [מל]ך· ביתדוד · ואשם
…[ki]/ng of the House of David. And I placed (or: set, put…

10. [ ]ל · הם · ארק · ת/[א]
…their land (acc.) into…

11. [ ]ולה · אחרן,
…another…?

12. [ ]ראל ישׁ על ל[מ]/ך
…[ru]le(d) over Is[rael…]

13. [ ]על · מצר
…siege upon…

My earlier analysis (Hagelia 2006b) concludes that the text should be dated to the royal period of Hazael (the latter part of the ninth century BCE). As I see it, the Editors have done a good job in establishing the text, which is confirmed by a high degree of acceptance by other scholars. As long as the text is fragmented, absolute certainty can never be attained. The text presented above should be a reasonable and acceptable result.

Most scholars follow the Editors in placing the fragments side by side, and other solutions have not won general credibility. Some scholars are quite elaborate in their reconstruction of the text,[1] while others seem to deny themselves any scholarly imagination.[2] Good scholarship is recognized by qualified imagination based on already accepted knowledge.

From this platform, scholarship should reach out to formulate new theories and test them against new evidence. This is what characterizes the Editors and those who, with more or fewer adjustments, in general, follow them.

1. E.g. Puech 1994; Margalit 1994a; and, to some degree, Lipiński 1994.
2. E.g. Cryer 1994; Becking 1995, 1996; Knauf, de Pury and Römer 1994; Knauf 1996; Lemche 1998; Ehrlich 2001.

Chapter 3

THE FORGERY THEORY

The theory that the Tel Dan inscription is a fake has been discussed by Garbini, Lemche, Cryer and Gmirkin. There is no doubt that forgery in general is a major problem in the market of antiquities. Both Garbini and Gmirkin have devoted separate articles to the question of whether the inscription is a forgery. The other scholars just mentioned have discussed the question more briefly, but ended up affirming its authenticity. Let me, in particular, look at the arguments of Garbini.

1. *Garbini 1994*

Giovanni Garbini wrote his 1994 article on the basis of Fragment A alone, and his main agenda was to prove that the inscription is a forgery.[1]

The stele itself is characterized as 'of extreme interest, both for its antiquity and for its content'. In particular, Garbini, draws attention to the text's close relationship to the Mesha stone, while 'the most interesting datum' is the mention of the 'house of David' (*bytdwd*), which, Garbini notes, 'is in fact the first time in absolute that an extra-biblical source has even indirectly mentioned the great Hebrew ruler'. Garbini claims that 'it would be difficult to overstate the importance of the new document', but warns that such importance 'calls for a preliminary critical evaluation that examines every aspect of a text to become fundamental for the historical reconstruction of events in Israel'.

Garbini describes the stele's present physical condition, and estimates that it would, originally, have been around 1 metre high and 50 cm wide. He compares the conservation and legibility of the inscription with that of analogous epigraphs, notably the Mesha stone and those of Zakkur and Sefire,

1. An online translation of Garbini's article (available at http://www.geocities.com/Paris/LeftBank/5210/tel_dan.htm), and so it is not possible to give page numbers for the quotations that follow. It is to be noted that the English translation of the Italian original seems ambiguous in places, meaning that it is to be accepted with caution.

indicating how 'exceptionally fortunate (and therefore suspect)' one must consider the fate that befell the Tel Dan epigraph.

However, Garbini has his reservations, and claims that 'it should be said straight away, nevertheless, that the Tel Dan inscription causes notable perplexity'.

1.1. *Dating*

The epigraphic dating of the text is 'a grave problem' to Garbini. Against the Editors' suppositions, based on archaeological data and a comparison between the content of the text and the biblical text, he questioned the palaeographical data. Garbini opposed Naveh, who, on palaeographical grounds, dated the text to the ninth century. This Garbini interprets as an effort to harmonize with a dating around 875, one that, he argues, 'does not…correspond with the facts at hand'. Garbini criticizes Naveh for having 'basically kept to generalities, dedicating only a few lines to the subject of the script' itself, and compared the Dan text to the altar at Tell Halaf[2] and the inscriptions of Hazael. To Garbini that was sufficient evidence 'to exclude the first half of the ninth century immediately'. On the other hand, Garbini found strong affinities with the Zakkur inscription (first half of the eighth century) and with the Sefire inscription (around 754 BCE), and so concluded that 'there is no way to reconcile the dating of the epigraph…with the writing in which it has been drafted'. Garbini found a discrepancy of around a century in the palaeographical evidence—a phenomenon which led him to conclude that it is possible for an archaic script to turn up in a later document, but not possible that a later script should appear in an older document.

1.2. *Philology*

Garbini was even more perplexed by the Editors' philological analyses.

The *waw* consecutive narrative verbal forms *wyškb*, *wyhk*, *w'qtl* and *w'śm* are 'unusual' in Biblical Hebrew, Moabite (the Mesha stone) and the prophetic texts from Deir 'Alla, and one not found in Phoenician and Aramean, except for the Aramean inscription from Zakkur, which has three cases of *waw* consecutive with the imperfect. This phenomenon, he explains, is of a probable Canaanite origin. It 'needs to be underlined' that such forms in the Zakkur inscription are 'only found in one passage of a strictly religious "flavour", in which the sovereign records his prayer to Baal Shamim and the divinity's response'. Garbini therefore claims that 'the indiscriminate use of the *waw* consecutive in the Tel Dan inscription is thus a completely anomalous datum in the context of Aramaic epigraphy'.

2. With reference to Dankworth and Mueller 1988.

Another problem, 'an even more disquieting phenomenon', is that the verbal forms *ysq* (√ סלק, 'climb') and *yhk* (√ הוך, 'walk'), both imperfect, are used as perfects, 'though without the *waw* consecutive, just as in the Ugaritic, where nevertheless the phenomenon of the *waw* consecutive doesn't exist'.[3]

Yet another 'linguistic oddity' is the adverbial use of *qdm*, translated by the Editors as 'formerly'. Up to now, an adverb *qdm* is not known in Aramaic, which instead uses the plural noun *qdmn*, or written *plene*, *qdmyn*, or the singular noun *qdm* with the preposition in its adverbial function.[4] The text also uses *qdm* as a preposition—a feature 'typical of Aramaic', according to Garbini—in the phrase *wyhk hdd qdmy*, which the Editors translated 'and Hadad went before me'. To Garbini, however, this translation is unnatural, *qdm* being used in 'a doubly inappropriate manner'. He equates the Aramaic preposition *qdm* with the Phoenico-Hebrew *lpny*, 'in the presence of', as in two beings standing in front of each other. This is, allegedly, confirmed by 'all the Aramaic literature of the first millennium so far existent, as far as I know'. It should be noted, however, that *qdm* is not used of people standing one behind the other until medieval Jewish Aramaic.[5] 'Even more significant', however, is 'the anomaly' that Garbini found in the expression rendered 'Hadad walked in my presence'. In his opinion, such a 'presence' before a god, or a human being, even a king,[6] would involve religious disrespect, implying the inferiority of whomsoever is 'in the presence'.

Garbini noticed a difference between *qtl* and *hrg*. The former is used twice in the Tel Dan inscription, and is normal in Aramaic, while *hrg* is less frequent. On the other hand, *hrg* is used in ancient Aramaic when talking of killing in battles, or in conspiracies, while *qtl* carries the more general significance of killing.[7] Garbini's point is that, in the Dan text, *qtl* is used in a semantic position where *hrg* is expected.

Garbini found another oddity relating to what he calls the text's 'conceptual and ideological sphere'. On the basis of *b'rq 'by*, 'in the territory of my father' (line 5), *mlky*, 'my king' (line 6), and *'rq. hm*, 'their land' (line 10), he thinks 'the author of the inscription appears to be a ruler who has succeeded his dead father, but had to act as a king of an inferior level (if not of a pretender)'. In other words, the author was a vassal king. Yet the fact that he raised a victory stele, Garbini notes, 'reveals that he held himself as

3. The translated article seems incomplete and somewhat unclear at this point.

4. In Biblical Hebrew the adverb *qdm* is used in two post-exilic psalms (74.2 and 119.152) to mean 'long ago'.

5. With reference to Jastrow 1950: 1317ff.

6. With reference to Hoftijzer 1965: 251-52.

7. With reference to Sefire III.11, Nerab I.11 and the Yadic inscription of Panammuwa.

sovereign with full rights, which conflicts with the expression "my king"'. Garbini found it 'even stranger' that *'rq* is used of the land belonging to the king, his father, because 'land', as of a king's territory, is otherwise designated *gbl*. That term is used when a king talks about his land as his own possession, in contrast to 'land' as a god's property, which is designated *'rq*.[8]

1.3. *Comparison with Other Inscriptions*

In particular, Garbini compared the Tel Dan inscription with the Mesha inscription, which he calls 'the closest typological parallel we have to the Tel Dan stele'. Garbini found the content of the two inscriptions 'very similar', and argued that the Mesha stone 'has supplied the typological model for the epigraph fragment from Tel Dan', while the Zakkur inscription has 'furnished the author with the palaeography (not without echoes from the Sefire inscription), the Aramean terminology and, on the level of content, the image of the divinity who intervenes directly in the battle'. In his opinion, the Zakkur inscription 'was imitated down to the punctuation before the pronominal suffix *.hm*', which he found in the Dan text (line 10), as well as the Zakkur text (line 9).

These comparisons show that the Tel Dan fragment has been rather fortunate not only regarding its state of conservation and legibility, but also in the quantity of information that it communicates: a stroke of fortune which certainly does not strengthen its authenticity, in Garbini's opinion.

In general, Garbini felt discomforted by an Aramaic inscription dealing with the Hebrews rather than the Arameans. Hadad is mentioned only once, while the name 'Israel' is mentioned three times. In addition, the use of *bytdwd*, a term written as a single word, without a word divider, was usual in the Hebrew Bible, but at least odd for an Aramaic sovereign.

The richness of information supplied by the Tel Dan inscription was also viewed with suspicion by Garbini, and appears, in his opinion, rather anomalous in statistical comparison with other fragmentary historical inscriptions, such as, for example, the Mesha stele, the Bar Rakib stele from Zincirli.

1.4. *Conclusion*

On the basis of an accumulation of palaeographic data, the linguistic oddities and some ideological incongruities, Garbini has 'strong doubts as to the authenticity of the Tel Dan inscription', and concludes that the Dan inscription 'definitively presents a series of weighty anomalies that finish up indicating an unusual and improbable ancient Aramaic which is closer to biblical

8. His reference to the Zakkur B text (*KAI*: I, 202 B, lines 8-9) presupposes a reading other than *'rqy*, 'my land'. See Garbini 1994: n. 8. Garbini also refers, among other things, to the Mesha inscription (lines 5-6).

Hebrew and Jewish Aramaic than to the Aramaic of the first millennium BCE'. He does not question 'the good faith' of the Editors of the text, but maintains that 'it is not possible to rule out the possibility of tampering with the archaeological site with the purpose of causing the "finding" of that which was meant to be found. The impression that it is a forgery is reinforced by a series of considerations on the general content of the epigraph.'

The conclusion that the Dan text is a forgery, Garbini bases on internal evidence (palaeography, linguistics and conceptual level) as well as an explanation of how the alleged forgery was perpetrated (i.e. by imitation of the Zakkur inscription).

Garbini reminds his readers that such epigraphic forgeries are, in fact, not at all rare, and that they are 'all characterised by a precise ideological matrix', namely, the need for extra-biblical support for the events and people recorded in the Hebrew Bible. Garbini did not substantiate his allegations, but simply claimed that 'it's unlikely that it is by chance the production of epigraphic forgeries has intensified in inverse proportion to the progressive decline of Albrightian optimism regarding the confirmation that facts provided by "biblical archaeology" bring to the Bible'.

Garbini's final conclusion was that 'the possibility that the Tel Dan inscription is authentic seems to be so remote, but I would be quite happy to have my mind changed by arguments just as precise as those presented here'—a somewhat modified position, at least modest.

1.5. *Critical Objections*

Garbini's arguments were based exclusively on the evidence of Fragment A—the B fragments being undiscovered at the time of his writing. With the discovery of fragment B1 and B2, Garbini's proposals on dating were invalidated, and accordingly most scholars today date the text to the latter part of the ninth century.

Garbini's philological arguments might have more substance. The problem in deciding this, however, is that the extant evidence for the Aramaic from this period is so sparse that any arguments of the kind provided by Garbini are precarious and his philological speculations unpersuasive.

Garbini's arguments with reference to other inscriptions can also be refuted. That the Tel Dan inscription could have been 'copied' or reproduced from the Mesha and the Zakkur inscriptions, in some way, is illusionary, and is not confirmed by the addition of Fragments B. The argument that an Aramaic text would not describe inner Israelite relations is not trustworthy, since this is an Aramaic text describing a war between the Israelites (and the *bytdwd*) and the Arameans. Why should Arameans not tell that story? Equally untrustworthy is the argument that the inscription gives too much information.

Could the forgery perhaps have been produced in ancient times? Who would have done it? For what purpose? In my view, there is no good reason for producing such a forgery in ancient times.

What about his 'Albrightian' argument? Could the forgery have been produced more recently? The 'decline of the Albrightian optimism', as he calls it, has come about in the last part of the twentieth century. A forgery with the intention of slowing down, or stopping, this decline would, consequently, have originated just a few decades ago. Is it possible to create a forgery on a stone of such large dimensions, to secrete it under centuries of debris and fill, without evoking some suspicion from such an eminent archaeologist as Avraham Biran? Would it really be that easy to deceive such a trained archaeologist on his very life's project, Tel Dan? Garbini has not assessed these questions. The discovery of Fragments B1 and B2 completely crushes such an argument: Why would a forger spread out faked inscriptions across different locations?

2. *Cryer 1994 and 1996*

The question of the stele's authenticity was also discussed by Frederick Cryer (1994: 14-15) independently of Garbini. Cryer, who also wrote on the basis of Fragment A alone, in particular pointed out that:

1. there are reasons for uncertainty as far as the find context of the fragment is concerned;
2. the problem with medial vowel letters in both components (בית and דוד) of the word ביתדוד;
3. the lack of a word divider in the same word; and,
4. that the find was made at a very convenient time.[9]

In general, he claimed, on these four points:

> Given the many question marks that have been put by contemporary scholarship against the reliability of the Old Testament account of Israel's history, it is more worrisome than gratifying suddenly to be presented with an inscription that purports to set our minds at rest on at least some issues (1994: 15).

After referring to the main contributions written so far,[10] Cryer regretted that

9. Cf. the vigorous debate on the question of the possibility of writing a 'history of Israel' and the publications of Lemche, Thompson and Davies, in particular. As Cryer notes, 'In ancient historical research, as in the craft of the cloak-and-dagger spy, it is axiomatic that, if one finds information just at the moment one needs it, it is very likely to be false' (1994: 15).

10. Cryer referred to Puech 1994; Knauf, de Pury and Römer 1994; Kaswalder and Pazzini 1994; and Tropper 1993. The 1994 joint article by Kaswalder and Pazzini (in Italian) is not surveyed here.

> None of the scholars in question has confronted, or even posed, the question of the authenticity of the find. I submit that this failure alone bears witness to a certain lack of professional seriousness on the part of most of the scholars involved (1996: 8-9).[11]

However, in the end, Cryer, nevertheless, doubted that the inscription is a forgery. He found so many peculiarities in the text, that it is difficult to imagine that somebody would have made such a fake, and admitted that it would be difficult 'to hoodwink an epigrapher of Joseph Naveh's calibre' (1994: 15).[12] Cryer concluded with the prediction that 'the matter will definitely bear further looking into'.[13]

3. *Sasson 1995*

Victor Sasson (1995: 14 n. 10) discussed the Tel Dan inscription's authenticity briefly in a footnote to a wider study. Sasson admitted to having been a sceptic initially, 'but as my work on the text progressed, I became less sceptical'. His positive conclusion is based on the text's literary structure. The inscription, for Sasson, is not 'simply a pastiche, or an amalgam of bits of extant epigraphic finds'. 'A modern "scribe" would have needed an expert knowledge of NW Semitic epigraphy' and other questions related to the text to produce such a fake, he maintained. Sasson therefore concluded that it is 'hard to believe, that the text is a fake'. It is to be noted that Sasson wrote his 1995 comments on the basis of Fragment A alone.

4. *The* BAR *Symposium of 1997*

During the *BAR* Symposium held in 1997 (assuming that *BAR* 1997a accurately records the scholarly transactions), Niels Peter Lemche declared the Tel Dan stele to be a fraud, 'a playing card trick'. For Lemche, the fact that the inscription was not found on its original position but in a wall at the remains of a gate, left open the possibility that a fraudster could have placed it there. Lemche even claimed that 'all the pictures of it printed in the *Israel Exploration Journal* are fakes' (*BAR* 1997a: 37).

The participants of the Symposium agreed that forgeries occurred, particularly of bullae. But Dever and McCarter were, generally, not as sceptical as Lemche (and Thomas L. Thompson, who also participated in the debate).

11. Cryer conceded the possible exception of Tropper in this criticism of 'lack of *professional* seriousness', he was appropriately concerned to rectify two other glaring deficiencies in the treatment of this inscription' (1996: 9).

12. Ehrlich (2001: 58) writes of 'the eminent epigrapher Naveh'.

13. While Davies (along with Whitelam and Thompson—see Davies 2000: 121) does not see the inscription as a forgery, he later claimed that Cryer held this view.

McCarter claimed that 'the Tel Dan inscription is an extremely unlikely forgery…[since] it lacks the things a forgery would have…' (1997a: 38), and noted the discovery was witnessed by several spectators. 'I don't think there's much chance that the Tel Dan inscription is a forgery', McCarter opined.

This short summary illustrates the rather extreme situation, one which saw Levine and Tadmor, on behalf of the editorial board of *IEJ*, the journal in which the Editors published the inscription, come out with a clarification of how the editorial office publish their articles, claiming that 'recent charges regarding the authenticity of the inscriptions [Tel Dan and Ekron] are unsubstantiated. We deplore the recent series of unfounded allegations and call upon all Editors to insist, as we do, on proper standards of discourse by the members of the academic community whose words are cited and whose writings are published in the pages of their journals' (*BAR* 23.4 [1997a]). In particular, they rejected the allegation that the photographs had been fabricated, as Lemche had claimed.

David Ilan had been used by Lemche as the alleged source of the claim that the Dan inscription was a fraud. In response, Ilan pointed out (cf. *BAR* 1997b: 10) that he was not an eye witness to the find, and, secondly, that he had no reason to doubt that the stele was authentic, even if it was found in a secondary position (as fill in a wall). Ilan called Thompson's, Cryer's and Lemche's allegations 'cavalier', going on to say that 'It makes one wonder about the rest of their scholarship'.[14] Even later, Lemche (1998b: 39) did not consider it out of the question that Garbini (1994) was right, that the Dan inscription is a forgery, 'because some of the circumstances surrounding its discovery may speak against its being genuine' (Lemche 1998: 41).

Finally, William Dever (1999: 92a) alleged that Bob Becking (1997: 68) implies that the inscription is a fake. In my view, on reading Becking's article, this allegation is hardly substantiated.

5. *Gmirkin 2002*

Russel Gmirkin has questioned the authenticity of the inscription on the basis of a visual inspection of the broken edges of the fragments. Due to alleged traces on the edge of the fragments, Gmirkin argues that the fragments were inscribed after the rock was smashed. Gmirkin draws no final conclusion, but simply asks for a closer investigation of where letters end on the broken edge. His arguments are 'documented' by photographs.

14. For the *IEJ* Editors and Ilan, see *BAR* 23.6 (1997b), the column *Queries & Comments*.

I place the word 'documented' within quotation marks here because Gmirkin's photographs are so bad that they cannot be considered to document anything. A closer investigation of the broken edge should indeed be made—even if just for the purposes of eliminate Gmirkin's theory. I find Gmirkin's argument odd for a number of reasons: Is it reasonable to imagine that an inscription could have been made the way Gmirkin indicates? Why should someone have chiselled a text on a broken fragment in the way he intimates? Would not the smaller rock fragments have broken into further pieces while being inscribed? The inscription process would demand that the rock used be fairly substantial to withstand the physical stresses, and pounding with a hammer and chisel would surely have been problematic on smaller and lighter stone fragments such as B1 and B2. Furthermore, why would a scribe have written incomplete sentences on the individual rocks?

In the light of all this, it seems to me that the lines of text are best understood as being incomplete due to later (i.e. post-inscription) breaking.

George Athas (2003), who has offered by far the most in-depth epigraphic investigation of the stela, presents no evidence that could possibly support the claims made by Gmirkin and others that the Tel Dan inscription is inauthentic.

6. *Summary*

Sadly, forgeries are actually very common in the field of archaeology, and so the possibility of the Tel Dan inscription being the work of a modern forger should not be automatically excluded. However, as indicated above, the arguments of Garbini and Gmirkin are somewhat problematic, and have not, with the exception of Lemche, received popular support. Their arguments are not persuasive in my opinion. As far as I can see, the scholar who was closest to drawing a convincing negative estimation is Cryer—yet he, ultimately, came to no firm conclusion.

On the whole, the issue of the Tel Dan inscription's authenticity has not been widely discussed—and this, presumably, is because only a few scholars have found the question relevant. To be sure, with the exception of Gmirkin's (2002) treatment—it might be said that this aspect of the debate has died out.

My own conclusion is that the inscription is no forgery. In the words of Andersen (1998: 45), 'The authenticity of the inscription cannot be impeached'.

Chapter 4

THE RELATIONSHIP BETWEEN THE FRAGMENTS

The discovery of Fragments B1 and B2 raised questions relating to their internal relationship and their relationship to Fragment A. This is the most basic question raised by the new discovery. The three fragments were found in different places, not in their supposed original locations, though on the same archaeological site.

There is no doubt that B1 and B2 fit together physically. They have a common fracture surface.

But do B1 and B2 fit together with Fragment A? Many scholars have argued that there is no common fracture surface between these fragments, which would prove their mutual relationship. In *NEA* 64.3 (2001: front page and p. 147), however, a new photograph seems to document a common fracture surface between fragments A and B1 beneath the smooth top surface. As this photograph was published so late, being published in 2002, it does not feature in the debate surveyed here. If this evidence had been known, much of the discussion referred to below would scarcely have arisen.

One way to prove the interrelationship of the fragments is by geological analysis, seeking to establish whether the pieces are they from the same stone. There is no definite reason, in my view, to doubt this supposition, but that does not in itself prove that all three inscribed fragments are part of the same text.

Another way to decide on their relationship would be to analyse the palaeography; were they chiselled by the same writer? Cryer and Thompson, at least, are not sure of that. Cryer has claimed that the fragments do 'not form a document' (see below).

If geological examination were to prove that they are from the same stone, and palaeographical, syntactical and literary analysis were to indicate that they were written by the same writer, and that they were parts of the same text, how should the three pieces be joined together?

1. *Arguments against the Editors' Joining of the Fragments*

There are three main positions against the Editors' joining of the fragments: (1) the fragments do not belong to the same document, (2) Fragments B1/B2 should be placed above Fragment A, and (3) Fragment A should be placed above Fragments B1/B2.

1.1. *The Fragments Do Not Belong to the Same Document*

Frederick H. Cryer (1995b: 223) commented immediately on the new *editio princeps* from the Editors, confessing to have read the article 'with feelings that were quite kaleidoscopically mixed' (1995b: 223), announcing immediately that his remarks on the Editors' second article 'can not be other than critical'.

He did not accept the Editors' terminology, calling the fragments 'A', 'B1' and 'B2'. In Cryer's opinion the fragments do 'not form a document'. They are just 'Dan A' (or the *Bytdawd* inscription), 'Dan B1' and 'Dan B2'. He was prepared to accept joining B1 and B2 on '*physical* grounds' (Cryer used italics for emphasis), and proposed that they be called, for example, the 'King Hadad' inscription. But the writing of B1 and B2 is so disturbed by the break between them that it is not possible to see any sort of join on an epigraphic, or textual, basis, he claimed. There might, in fact, be epigraphic reasons to question whether B1 and B2 belong together, he argued.

Cryer maintained that a letter-by-letter epigraphic analysis of the original inscription by the Editors was lacking, and that 'This absent epigraphic description comes back to haunt Joseph Naveh in the present case'. Had he given such an analysis of both fragments (A and B1/B2), he 'would surely have noted that several of his characterizations are not congruent with one another' (1995b: 225). Cryer admits, however, that the fragments are written 'in essentially the same script', that they could be 'products of the same school', and even written by the same individual writer after an interval of some years.

Cryer referred to his colleague at the University of Copenhagen, Thomas L. Thompson,[1] who had concluded that the letters on Fragments B1 and B2 'differ in point of size' from the Fragment A letters, and that the lines in the fragments 'do not match up with their putative counterpart', as reconstructed by the Editors. Thompson saw 'insuperable problems in the amounts of space' assumed by the publishers for their reconstructions (Cryer 1995b: 226).

1. This information is probably based on a private discussion, for Cryer does not give any sources.

On the basis of this epigraphic analysis, Cryer concluded that it is 'a foregone conclusion that Dan A has nothing to do with Dan B (1 and 2)'.

Some of Cryer's conclusions seem to be premature, and in my view he has not done his job well enough.[2] There are still more epigraphic questions to assess before a conclusion can be reached. First, Cryer did not analyze all the letters in the fragments, individual variables within the respective fragments, or similarities between Fragment A and Fragments B. From that perspective, his analysis was one-sided. He seems to have based his epigraphic analysis on Ada Yardeni's drawing, not on the rock itself, and did not comment on problems related to epigraphic analysis of a copied text, compared to analyzing the original text. Yardeni's two copies were made with a time lag of around one year,[3] and her copy of the B-fragments is smoother than her copy of the A-fragment. Any investigation of the inscription on the basis of drawings is precarious, and as such analysis should be made on the rock itself, or at least on the basis of a photograph such as the one in *NEA* 64.3 (2001), which shows all three fragments in one picture, with all three fragments shown with the same graphic quality. Could not the alleged differences merely indicate some inaccuracy in the copies? Cryer did not engage with this question. Nevertheless, he did, himself, indicate that there could be inconsistencies in the shape of the letters within the same fragment. Could that not explain some of the alleged dissimilarities between the fragments? Furthermore, Cryer did not discuss at all whether the inconsistencies could be ascribed to general variety in the shape of the letters any hand written text would inevitably display. In addition, the rock's possibly grainy quality could have influenced the shape of the letters. No consideration was given to the problems of engraving a text in stone compared to writing with a pen. Bearing such questions in mind, Cryer's views are not all that convincing. His epigraphic analysis does not prove that Dan A is from a document other than Dan B1 and B2, as he calls them. His opinion has not gathered many disciples around it.

Thomas L. Thompson (1995b: 237) argued against combining the fragments from an archaeological point of view, and confesses that he was 'not comfortable with the alleged find spots'. The discovery of Fragment A was given 'two different descriptions' by the Editors, he claimed, which raised unanswered questions to him. How did it happen, and why were the fragments found in such different positions?

Thompson also questioned the joining of Fragments B1 and B2. On the one hand, he admitted that the joining of those fragments 'at first appears

2. Cf. Athas 2003: 94: 'Cryer's own examination of the letter types in Fragment A is often superficial, and his analysis of Fragment B is far from comprehensive'.

3. Rendered in Biran and Naveh 1993, 1995.

unexceptionable'. But 'one can not help but notice' that in the photographs 'not a single letter of either fragment is shared by both' fragments. Even if 'this is not much of an anomaly', as only six letters are involved, Thompson has problems with the joining of Fragment A to Fragment B. He claimed that 'The alignment of letters in A are [*sic*] noticeably consistent throughout the fragment. When the fragments are joined, this consistency deteriorates' (Thompson 1995b: 238). Thompson found differences in the horizontal alignment of the letters. The space between the lines and the average size of the letters are claimed to be different in the fragments. Therefore, he warned against accepting Biran and Naveh's join of B1 and B2 without caution.

Thompson had particular problems with joining fragments, noting that 'When it comes to the join of Fragments A and B…extreme scepticism is warranted' (Thompson 1995b: 239). He saw anomalies relating to 'the aesthetic perception of single composition', and the only physical evidence for this join is 'a notoriously undependable…join'. Thompson criticized, in detail, what he saw as 'a lack of correspondence' between the lines in Fragments A and B, concluding that 'the proposed reconstruction of our inscription is physically impossible'.

Cryer's and Thompson's articles were clearly co-ordinated. Though Cryer took more of a palaeographical approach than Thompson, the two were out to draw the same conclusion, to provide counter-arguments against the presentation of the Editors. Counter-arguments are of course warranted in scholarly debate, but they should be as substantial as the arguments they purport to refute. It is difficult to see how Cryer's and Thompson's arguments could refute those of the Editors in the case of the relationship between these fragments. They are not very convincing,[4] and we will see that they have not attracted many followers, although there are a few.

Bob Becking (1996: 21; cf. 1999) joined with Cryer (1995b) and Thompson (1995b) in making critical remarks on the Editors' interpretation. His article had a twofold aim: first, to show that the arguments for joining Fragment A to Fragment B 'are not convincing in view of the palaeographic and epigraphic data', and secondly, that Fragments B1 and B2, should be treated as a separate inscription in their own right.

It was a problem for Becking that the three fragments were found at various spots on the site, in locations 'which do not seem to be related stratigraphically' (1996: 22). As for the Editors' claim that the three pieces belong to the same stele, he maintained (1996: 22) that 'the distance between the pieces seems to be rather large for such a surmise. Their hypothesis could be made more plausible by palaeographic and epigraphic arguments'.[5] In his

4. Cf. the counter-arguments to Cryer discussed above.
5. A solution to this lack has also been requested by, e.g., Cryer.

own palaeographic analysis he claimed that 'the script of the three fragments is to be characterized as representative for the local variant of an Aramaic type of script which was slightly influenced by Phoenician script, which is known from documents dated in the ninth and eighth centuries BCE' (1996: 22). After analyzing the respective letters, his conclusion was that 'these differences make it very improbable that the three fragments were part of one inscription. They might have been written by the same person, though at different times' (cf. Cryer 1994: 6-9). He doubted whether it is epigraphically possible to reconstruct a fill text between Fragment A and Fragments B the way the Editors have done (1996: 23).

Becking added his own observations to those of Cryer (1995b) and Thompson (1995b). First, Becking did not see any significant difference in the size of the letters, but observed a difference in the width of the intervals between the lines (Becking 1996: 24), referring to Fig. 9 of the *editio princeps* (1995). He found that the average distance between the lines in Fragment A is 6.7% wider than in Fragments B, and claims: 'In my view this can only be explained by assuming that the fragments B1 and B2 have been part of an inscription different from Dan A' (Becking 1996: 22). On this basis, he called Fragments B1 and B2, as Cryer and Thompson did, Dan 1 and Dan 2, the latter a fragment of 'the second Dan inscription'.

Later, Becking (1999: 191) responded to Wesselius (1999) by saying that the latter's view 'is based on the assumption that the fragments Dan A and B1 + B2 should be construed as part of originally one inscription', a possibility which Becking still denied. He conceded that most scholars accept that Fragment A and Fragment B1 and B2 are part of one document, but this was still (cf. 1996) not self-evident to him. Becking listed four counter-arguments, namely: (1) the fragments were found in different places; (2) the script in Fragment A is, allegedly, not the same as that of B1 and B2; (3) there is not enough space between the fragments to accept the Editors' reconstruction; (4) the average interval between the lines in A and B1 and B2 are not identical. Therefore, Becking admonished 'more than great caution' (Becking 1999: 192), and counselled against accepting automatically that the fragments belong together. These arguments were next countered by Wesselius (2001; see below).

Becking concluded with Thompson (1995b) that the Editors' reconstruction is 'very improbable'.

Niels Peter Lemche referred to the Dan stele in his book *The Israelites in History and Tradition* (1998b). To him, 'it is still a matter of dispute… whether or not the fragments belong to one and the same fragment' (1998b: 39). Lemche argued that 'the fragments belong to two different inscriptions'. This is 'obvious' to him for two reasons: The lines in the two fragments do not match each other, and the style of the writing is allegedly different from

one fragment to the next. Lemche preferred to understand the fragments as either two different sections of the same inscription manufactured by two engravers, each responsible for his part, or as two separate, though related, inscriptions.

As for the irregularities in the lines, it is well documented and recognized that in ancient inscriptions lines could vary considerably. Arguing against joining the fragments on such a basis is not reasonable. Nor is it reasonable to argue against their join because of differences in the shape of the letters, as any hand-written text will inevitably have individual differences. This is also well documented, not least in one of my earlier studies (Hagelia 2006b).

1.2. *Fragment B Should be Placed Above Fragment A*

In two articles (2000 and 2001),[6] Gershon Galil expressed his opposition to 'the now almost universal opinion on how the two main fragments of this important stele relate to each other' (2000: 37). He listed scholars who have accepted the Editors' join,[7] scholars who have seen the fragments as being from different inscriptions,[8] and scholars who have refrained from accepting the join, without giving an alternative,[9] claiming: 'The attempt to join these two fragments (A; B) appears forced and very problematic'.

In his opinion, the B-fragments belong at the beginning of the stele, and should not be placed beside Fragment A, as the Editors and several other scholars have accepted. He argued seven points,[10] here rendered in short form:

1. the lines in Fragment A are not parallel to the lines in Fragment B;
2. there is no physical join between the fragments;
3. the completion of line 8 is impossible;
4. the completion of lines 2-3 is not reasonable;
5. the content of lines 6-7 is artificial and forced.

6. Galil (2000: 37-39) is primarily concerned (as the title indicated) 'The Boundaries of Aram-Damascus in the 9th–8th Centuries BCE', but has a paragraph on the Dan stele. Galil (2001) deals directly with (again, as the title indicates) 'A Re-Arrangement of the Fragments of the Tel Dan Inscription and the Relations between Israel and Aram'.

7. See Margalit 1994b; Na'aman 1995b (he simply presupposed the fragments belong together [p. 389]); Sasson 1995 (he actually treated only Fragment A); Kottsieper 1998; Noll 1998 (who simply stated that the fragments 'derive from a single large stone monument of uncertain original size' [p. 7]) and Dion 1999 (he, also, simply stated that the fragments belong together [p. 145]).

8. See Cryer 1995b: 224-27; Thompson 1995b: 237-39; Becking 1996: 22-24.

9. See Demsky 1995: 35 and Sasson 1996: 553 n. 16. Neither scholar had the opportunity to study the additional fragments.

10. See Galil 2001: 17. Galil (2000: 38) has six points, slightly different from 2001.

His sixth and seventh points are derived from his opinion that Hazael was not the originator of the text, so neither of these points is relevant to whether or not the fragments belong together.[11] On the other hand, the palaeographic argument of Cryer (1996: 17) that particular letters prove that the fragments are from different inscriptions, is, in Galil's opinion, 'groundless'.

Galil, as far as I can tell, has not mustered followers for his opinion that Fragments B should be put at the beginning of the stele, and indeed his opinion is scarcely acceptable given the documented join reported in *NEA* 64.3 (2001).

1.3. *Fragment A Should be Placed Above Fragment B*

George Athas proposed that Fragment B should be placed some 35 cm below Fragment A (2003: 189-91).[12] This argument is based upon his epigraphic analysis of the script and is as reliable as this type of analysis could be for the present case, that is, it is not all too reliable.[13]

2. *Arguments in Favour of the Editors' Join*

Most scholars have argued in favour of the Editors' joining of the fragments, though some have done so with some adjustments. This is the position defended by most scholars. It is not necessary to trace, or discuss, the particular opinion of every scholar who has defended this position. I will concentrate on their main arguments.

Shigeo Yamada (1995: 612) accepted that the three fragments belong to the same text, but suggested that the join is 'infirm'. 'Nevertheless', Yamada concludes, 'the proposed correspondence between the fragments, seems tenable'.

William M. Schniedewind (1996) accepted the joining of Fragments A and B as proposed by the Editors. While commending the Editors' work for being 'both prompt and remarkably precise', Schniedewind nevertheless saw that there was 'room for some new suggestions'. Schniedewind questioned specifically the size of the gap between them and the angle of the joint,[14] and explicitly argued against Thompson (1995b), claiming that Thompson's

11. See Galil 2001: 17.
12. See, in particular Athas's Fig. 5.4 (2003: 191)
13. Cf. the counter-arguments to Athas in Hagelia 2004b.
14. Schniedewind (1996: 77) argues: '[T]here is a distinct possibility that there was a larger gap between A and B1/B2 and the join between fragment A and B1/B2 must be considered a working hypothesis, a slight rotation of the fragments makes the lines match better and renders a more convincing join'. Cf. his drawing (Fig. 2), which is based on an electronic manipulation of the Editors' photographs, where Fragment B 1 has been rotated 2 degrees anti-clockwise, and B2 is rotated 1.5 degrees anti-clockwise.

'inflammatory remarks…about this joint are difficult to comprehend… [H]e exaggerates the "dissonance" and wrongly bases his perceptions on the published photographs as if they were an absolutely precise positioning of the fragments'.[15] The irregularities in the text are tolerable, in Schniedewind's opinion (1996: 87a). Schniedewind gave particular attention to the physical aspects of the fragments (1996: 78). Fragments B1 and B2 'make an obvious join' to him, while their relationship to Fragment A is less certain. Nevertheless, he notes that their archaeological context would, *a priori*, suggest that they should be related, an assumption he based on the fact that the fragments are of the same material, show similar palaeography and language, and 'may be related' in content, which 'should remove almost all doubt that the fragments are related', he maintains. Schniedewind accepts the Editors' joining of the fragments at line 5, which, to him, also 'forms a readable text with relatively minor difficulties'.

At first, Walter Dietrich in his book (1997a) did not accept the Editors' joining of the fragments. All the problems focused upon by Cryer and Thompson were recorded, and yet, to Dietrich, this particular question seemed not to be safely resolved. Since the fragments of the Tel Dan inscription were found at different times, Dietrich recommended caution with respect to coming to any decisions or conclusions.

In his later article (1997b), however, Dietrich changed his mind somewhat. Now he argued that the two fragments (B1 and B2) belong 'unquestionably' together and to Fragment A. Dietrich was not persuaded that the Editors' combination of the fragments is correct, though he clearly opposed Thompson's[16] questioning of whether they belong together at all. Furthermore, to Dietrich is seems 'almost desperate' (1997b: 30 n. 75) for Cryer to claim that the fragments 'have nothing to do with each other', while claiming that they could be 'products of the same school, or even of the same individual after some years interval'![17]

Ingo Kottsieper (1998: 476 n. 4) supported the joining of Fragment A and B recommended by Schniedewind. The objection from Thompson (1994: 238-39) that this joint is 'physically impossible' was roundly rejected. Cryer's objections (1995a) are also called 'completely groundless'. The sign variables indicated are, for Kottsieper, broadly within the limits of what would be expected from a hand-written stone inscription. Also, the fact that the fragments correspond in material and colour can only be explained by their belonging to the same rock. Kottsieper joined Dietrich (1997b: 30) in calling Cryer's and Becking's attempt to hold the fragments apart 'almost desperate'.

15. See Schniedewind 1996: 86b n. 5.
16. See Thompson 1995b: 236-40.
17. See Cryer 1995b: 225.

Jan-Wim Wesselius (1999: 165-65) argued that Fragment B1 and B2 'likely fit' together, that they belong with Fragment A, claiming that 'the placement of the fragments seems virtually assured'. With respect to the objections against the combining of Fragments A and B, he countered that 'none carries enough weight to challenge the Editors' proposal' (1999: 165). On the other hand, Wesselius conceded that we have 'a highly fragmented text', which makes any conclusion 'preliminary only', and that the discovery of additional fragments could alter our opinion of the text completely. With such modifications in mind, I will argue that we still cannot abstain from trying to interpret the text.

Jan-Wim Wesselius (2001: 88) admitted that Becking's objections (see above) were 'serious arguments', but argued 'that they only have relative value'. Wesselius turned around Becking's first objection, stating: '[I]f three fragments are found within such a relatively small distance of a few metres, it would be negligent not to try to join them together'. He asked whether Becking's second and fourth objections (2001: 88) were relevant at all. As for Becking's third objection, Wesselius argued that 'this can only be an argument against these restorations, not against the placement of the fragments'. Wesselius concluded that these arguments were, in general, not that decisive.

As for the joining of the lines between Fragment A and B, Wesselius found it to be 'impeccable' in four cases, 'likely' in three and in only one 'uncertain',[18] which, in his opinion, 'effectively constitutes a decisive argument for their belonging together in the way the Editors suppose' (Wesselius 2001: 89). The reasoning behind the Editor's placement 'is transparent and inspires some confidence'.

Between Becking and Wesselius, the position of the latter is clearly more in accordance with the widely accepted opinion on the relationship between the fragments that has emerged from the debate. Becking has placed himself mostly alongside the Copenhageners in relation to this question.

Nadav Na'aman (2000: 92) pointed out that while the inscribed surfaces of the fragments (A and B) cannot be joined, the broken edges can be brought together neatly (this is evident from the photograph, published in *NEA* 64.3 [2001]). This argument is significant, and has not been particularly challenged by other scholars. It is also in accordance with my own observation of the fragments, as they are displayed in the Israel Museum in Jerusalem.

18. For details, see Wesselius 2001: 88-89.

3. *Summary*

As we have seen, opinions differ between scholars as to whether and/or how the pieces fit, or do not fit, together. Some scholars have been vociferous about totally separating Fragment A from Fragments B. But most scholars realize that these three fragments belong together somehow, even if opinions differ on exactly how that relationship should look.

Galil produced a useful summary of the different positions (see above), before advocating his own theory that B1/B2 should be placed above Fragment A. In contrast, Athas (2003) places Fragment B far below Fragment A. Neither of these alternatives is particularly convincing, and most scholars place Fragment A side by side with Fragments B, basically the way the Editors have combined them.

Significantly, the publication of a new photograph in *NEA* 64.3 (2001) may prove decisively that the three fragments belong together and show how they fit.[19] This photograph has not featured in the debate summarized here. It is to be expected that this photograph—or, of course, the joined fragments themselves—will contribute decisively to the future debate on the relationship between the fragments. And yet, so much seems to be at stake for some scholars, that full consensus seems unlikely ever to be attained.

My own position is that the Editors have, in the main, correctly combined the fragments.

19. Cf. Na'aman's brief comment (noted above) on how the broken edges of the fragments can be brought together to form a neat join (2000: 92).

Chapter 5

THE ORIGINATOR

The identity of the originator of this inscription has been much debated. Several scholars have indicated that the inscription might have begun with the originator's self-presentation (cf. Dion 1999: 150), but the opening words are lost. The possible identities have been carefully summarized by Becking (1999: 188), who highlights the following candidates: Benhadad I;[1] Hadad-ezer, also called Benhadad II;[2] as well as Hazael.[3] In addition, Athas (2003) has suggested Benhadad III (or the II, as he calls him). All four suggested originators are Aramean kings. In contrast, Wesselius has defended the opinion that the originator was Jehu, the Israelite usurper, an argument I will trace in some detail in this chapter, along with Becking's objection to this argument.

I intentionally use the term 'originator' since the originator was not the person who actually engraved the text; this was done by a professional scribe on the orders of the king, or his court (cf. Emerton 2002: 484). We do not know whether the originator himself was literate.

1. *The Benhadad I Theory*

The Benhadad I (c. 880–870)[4] theory was defended by the Editors themselves in their publication on Fragment A (1993: 86, 95-96), based on 1 Kgs 15.20, where Benhadad is said to have attacked, among other places, Dan. After the discovery of Fragment B, the Editors changed their minds and argued for Hazael (see below). Emil Puech (1994: 233-41) adopted this theory on a similar basis.

1. Biran and Naveh (1993: 86), after the discovery of the additional fragments, they for Hazael as originator; see also Puech 1994: 233-41, and Knauf, de Pury and Römer 1994: 68-69.

2. See Dijkstra 1994: 12-14; Lipiński 1994: 83-86.

3. See Tropper 1993: 396-98; Biran and Naveh 1994: 18; Halpern 1994; Margalit 1994a; Yamada 1995: 612; Sasson 1996; Schniedewind 1996: 82-86; Dietrich 1997b: 31-32; Lemaire 1998; Na'aman 1999: 10-11.

4. The dates for the Aramean kings used here can be found in Anderson 1988: 269.

Knauf, de Pury and Römer (1994: 68-69) saw the inscription as the originator's celebration of a victory. His father had been dethroned by a king of the Omride dynasty. But then followed a political upheaval in favour of the Arameans, and the king of Israel was defeated by Benhadad (cf. 1 Kgs 15 and 22).

Notably, the three suggestions discussed here are based solely on Fragment A.

2. *The Benhadad II Theory*

According to Mindert Dijkstra (1994: 13), Hazael is not a good candidate for being the originator of an inscription such as the Ten Dan stele because he was a usurper. Benhadad III is also a consideration, but, in Dijkstra's opinion, the end of the ninth century, the date suggested for Benhadad III, would be too late. On the basis of 1 Kings 22, Dijkstra argued for Hadadezer, the son of Benhadad I, also called Benhadad II (870–842).

Eduard Lipiński (1994: 85) argued that the events reported in the inscription refer to the early period of the Omrides. He argued from 1 Kings 20, concluding that the king speaking in the text 'can safely be identified with Hadad-'idri', also called Hadadezer, or Benhadad II. This assumption was based on his interpretation of Fragment A. By 2000, he was of another opinion, and agreed with the Hazael theory (Lipiński 2000: 378).

Baruch Halpern (1994: 74a) concluded, after a thorough discussion of the matter: 'Our assumption, thus, should be that the stela fragment is the product of Benhadad II, in the late ninth century'. This position was also based solely upon Fragment A.

3. *The Hazael Theory*

The theory that Hazael was the originator of the text is the one advocated by the majority of scholars, who base their arguments both on Fragment A alone, as well as on Fragments A and B combined.

3.1. *Arguments Based on Fragment A Alone*

The first scholar to argue for Hazael (842–806 BCE) as the originator of the stele was Josef Tropper (1993: 396-98), who objected to the Editors' original Benhadad I theory. Actually, he seems to have misunderstood them (or was it just a misprint?), claiming that they supported Benhadad *II*, Bar-Hadad as he called him (Tropper 1993: 396), while the Editors themselves clearly said Benhadad *I* (Biran and Naveh 1993: 95). The Editors had discussed the Hazael theory, and rejected it, but Tropper found this theory 'plausibler' (1993: 397), because, after the death of Ahab, Israel experienced a short

period of instability, which was used by the Moabites to revolt against Israel. As a result of Assyrian pressure, the Arameans were not in a position to attack Israel immediately. Nevertheless, Hazael managed to establish power, and 'die Vormachtstellung von Damaskus war etwa zwei Jahrzehten lang gänzlich unangefochten' ('the predominance of Damascus went completely unchallenged for a couple of decades' [1993: 397]), until the Assyrians regained power over Syria at the end of the ninth century (around 803). On this basis, and on the basis of 2 Kgs 13.24-25, Tropper assumed that Hazael conquered Israel and Judah and commemorated his victory in the Dan inscription.

Baruch Margalit argued briefly along similar lines. Hazael was, allegedly, not of 'blue-blood' (1994a) in the line of succession, but could have been adopted by his predecessor (1994a: 20).[5] The stele was probably 'erected when Hazael was beginning to establish hegemony in Canaan and Syria, after having thwarted several Assyrian offensives', he argues.

3.2. *Arguments Based on Both Fragments A and B*

After the discovery of the additional fragments, Biran and Naveh (1995: 17) reassessed the question of the originator, and immediately concluded that 'the text now points to the time of Hazael, whose *coup d'état* preceded Jehu's revolt in 842 B.C.E. Moreover, it clearly indicates that the author of the stele was Hazael himself, although the name does not appear in the fragments found to date.'

For Shigeo Yamada (1995: 612), Hazael was simply an 'inevitable conclusion'. He saw a generally clear structure in the text, dividing it into two parts, one describing the days of his father and the other the days of Hazael himself, the main topic being a series of military conflicts between Aram-Damascus and Israel, with his military success sponsored by the god Hadad.

Victor Sasson (1995: 25) pointed out that the speaker of this text must have been 'a major historical figure' for five reasons:

1. he mentions his father several times;
2. he speaks of a war with two kings and their armies;
3. he claims the speedy aid of the god Hadad;
4. he claims massive destruction inflicted on the enemy;
5. he speaks of initiating an ensuing siege.

That is why 'the speaker must be considered to be a powerful king and more than an equal to the two kings to the south of the border' (1995: 25). He also underlined (1995: 13) the importance of having the text engraved on a basalt stone, and written by a professional and skilled scribe. To Sasson, that is

5. Hazael 'certainly did not murder' his father, Benhadad, also called Hadadezer; cf. 2 Kgs 8.

evidence that the inscription was meant for outdoor public display and to commemorate a far-reaching victory. All this indicates that the speaker was not a minor vassal, but an important historical figure. This thinking rules out the Editors' previous opinion that the originator was a vassal, or a minor dependent king.[6] He saw a 'good candidate, albeit not the only candidate' in Hazael, with Hazael's son, Benhadad (III), as 'a possible candidate' (1995: 28).

William Schniedewind (1996: 82-85; cf. Schniedewind and Zuckermann 2001), debated the problem at length in a discussion on the role of Jehu's revolt, concluding: 'In sum, this inscription should be attributed to Hazael'. This implies that he rejected, in advance, Wesselius's later argument (1999, 2001) that Jehu was the originator of the inscription.

Since Ingo Kottsieper (1998: 485) identified Jehoram of Israel and Ahaziah of Judah in lines 8 and 9, the conclusion was inescapable to him that the originator was Hazael of Damascus.

André Lemaire (1998: 11) compared the Dan stele with the Mesha stele, and called it, with Schniedewind, a 'typical…memorial stela intended as propaganda boasting of Hazael's victories against Israel and Judah'.

Paul E. Dion (1999: 151) based his identification of Hazael on the Editors' identification of Jehoram (line 7B). 'This remark is very important, in that it clearly points out Hazael, King of Damascus in the second half of the IXth century, as the author.'

Eduard Lipiński had, on the basis of Fragment A, previously (1994) dated the inscription early, identifying Hadadezer (Benhadad II) as its originator. But in 2000, he identified Hazael as the author of the text. Even if Hazael is called 'the son of a nobody' a usual designation for a usurper, in the Kurkh Monolith, 'there is nothing in the notice which signifies that Hazael murdered the king' (Lipiński 2000: 376; cf. 2 Kgs 8.7-15). Lipiński did not exclude the possibility that Hazael was, in fact, the son of Hadadezer himself, but by an inferior wife, or that he was not a legal heir to the throne.

Finkelstein and Silberman (2001: 129) claimed that 'there is hardly a question that it tells the story of the assault of Hazael, king of Damascus'. This is also seen as 'dramatic evidence of the fame of the Davidic dynasty less than a hundred years after the reign of David's son Solomon'.

William Schniedewind and Bruce Zuckermann (2001: 88) also argued that Hazael was 'the patron of the inscription', though their aim was to identify the father of Hazael.

6. Sasson 1995: 26: 'All in all, the original editors' theoretical discussion on this issue is highly convoluted, and the historical reconstruction is rather farfetched and flimsy'.

Few scholars have argued against the Hazael theory, except for Wesselius (1999, 2001), who argued for Jehu, in an attempt to harmonize the inscription with the biblical records, and Athas (2003: 258-59), who argued that the old arrangement of the inscription is 'now seen to be defunct'. The previous reconstructions of the names in lines 7 and 8 are invalid to him.

4. *The Benhadad III Theory*

There is some uncertainty surrounding the Aramean kings. Above, I have followed the chronology and naming of Anderson (1988: 269). There are, however, questions about the exact dates and even the names and sequencing of the Aramean kings named Benhadad. For instance, Miller and Hayes (1986: 251, 296) give no date for Benhadad I, or Benhadad II, who is called Hadadezer. The start of Hazael's reign is dated to 843 (the end date being uncertain), while Benhadad III is not assigned a date at all. This king is obviously the same Benhadad who Athas (2003: 263) calls Benhadad II, and whose death he dates to 'some time before 773 BCE' (2003: 264).[7] If Anderson's dating of Hazael's death to 806 is correct, then there is a length of time left for Benhadad III, c. 806–775.[8] The exact numbering and figures are not important here.

The first scholar to argue for Benhadad III as the originator was Galil (2001: 18), who saw Hazael's death in line 3, and interpreted the text as 'Benhadad's [Galil called him "Bar-Hadad"] admiration for his father Hazael', and called Hazael 'the founder of the dynasty and the greatest Aramean king of all times'.

Athas (2003: 259) discussed the Hazael theory while raising his objections to other scholars. He rejects the opinion of the Editors, because they, in his opinion, did not deal carefully enough with the author's problematic reference to his father. Their theory 'has been largely forced onto the evidence'. Margalit's view (2003: 260) is rejected because he built on the identification of the two named kings in lines 7-8. The way he dealt with Hazael's reference to his father is claimed to be 'quite inadequate', as is his punctuation of the Editors' reconstruction of the text. Due to such, allegedly, unfounded arguments, Margalit is charged with proceeding with inadequate methodology, which makes his conclusion 'decidedly backward'. Sasson is attacked for similar reasons (2003: 260-62; cf. also Yamada 1995; Na'aman 1995b). Lemaire's method (1998) is regarded by Athas (2003: 262) to be 'a more coherent path', when he refers to similar references to a 'father' in Egyptian and Assyrian sources. However, his view is, nevertheless, dismissed because

7. The question is discussed, in particular, in Athas 2003: 287-89.

8. Athas argues (2003: 264) that he had been on the throne 'for at least a few years before 796 BCE'.

his argument is based on the assumption that Hazael was the originator of the stele. In Athas's opinion, Hazael could not have been the originator because that does 'not comport with' the archaeological and epigraphic data, and therefore 'represents a backward methodology'. This is, allegedly, also the case with Lemaire's understanding of √מלך, which is also based on the assumption of Hazael as the author. Lemaire's view of this word is also dismissed for syntactical reasons (Athas 2003: 263). Schniedewind and Zuckerman's (2001) 'ingenious solution' that Hazael was the leader of a political group aligned to the worship of the deity El is rejected for 'three fundamental' reasons, the most important being that they build on the arrangement of the text now claimed to be defunct.

The point of departure for Athas was his archaeological, epigraphic and palaeographic dating of the text. He concluded that the stele stood on display for just a short period around 800, or early in the eighth century. 'The first few lines of Fragment A…prevent us from identifying Hazael as the author of the Tel Dan Inscription' (2003: 259), as he was called 'the son of a nobody', in Assyrian sources, and was the murderer of Benhadad II, according to 2 Kgs 8.15. This date seems, in his opinion, to be fixed before the historical questions related to the dating are discussed at length (2003: 255-65).[9]

For Athas, 'Bar Hadad II [III] is the only real candidate for the author of the Tel Dan Inscription' (2003: 265). In 2 Kgs 13.3, 24 he is called Benhadad (NRSV). Athas is the only scholar arguing for this king as the originator of the stele. Notably, Athas argues more *against* other theories than *for* his own theory,[10] which is mainly based on the assumption that he is the only candidate who fits Athas's interpretation of the archaeological, epigraphic and palaeographical data.

5. *The 'Don't Know Theory'*

After discussing the question of authorship, Ernst Axel Knauf (1996) reached a negative conclusion—he simply did not know who the originator of the text was. Hazael was dismissed because he was a usurper. It is seductive to him to see Jehoram of Israel and Ahaziah of Judah in the additional fragments. But since they were killed by Jehu, according to the biblical account, a serious problem arises: Jehu cannot be the author, either.

9. Athas 2003: 255: 'Since the inscription belongs to the latter period of the second construction phase at Dan's city gates, our author lived sometime c. 800 BCE'. After this claim, he discusses the historical questions.

10. Athas 2003: 263: 'Only one other king suffices for identification as the author of the Tel Dan Inscription, and that is Bar Hadad, Hazael's son and successor'.

Kenneth A. Kitchen (1997: 34) concluded that the speaker of the Tel Dan inscription was a regent in a country at war with a Hebrew kingdom on their northern flank. Since the language is Old Aramaic, 'as admitted by those best fitted to judge',[11] we should suppose he was a king of Aram-Damascus (cf. 2 Kgs 8.7–9.29; 10.32).

6. *The Jehu Theory*

The Jehu theory was proposed by Jan-Wim Wesselius (1999) and was immediately responded to by Bob Becking (1999). Becking commended Wesselius for an ingenious theory, but was fundamentally in disagreement with him. Becking's article provoked an extensive response from Wesselius (2001). There are several interesting aspects of this Wesselius–Becking controversy, though, interestingly, it has not generated any further spin-off in the scholarly debate. Wesselius did not win the battle—at least, nobody has come forward with a defence of his position. Be that as it may, the debate deserves some attention.

6.1. *Wesselius 1999*

Jan-Wim Wesselius related Fragment B to the killing of the kings Jehoram and Ahaziah. The problem with Wesselius's view is that, according to 2 Kgs 9.14-29, these kings were killed by Jehu, not by Hazael of Damascus. The presence in Israelite Dan of an inscription by an Aramean king, describing the killing of a king of Israel, can only be explained by assuming that the Arameans took over this location, used it as a sanctuary of their own, and placed a stele in it—a scenario which is 'by no means impossible' to imagine. In Wesselius's opinion, accepting that it was Jehu who killed the kings named in the stele is 'the only methodologically sound approach' (1999: 168). Until this possibility is refuted, we cannot seek to secure the identity of the originator as King Hazael of Damascus, he argued.

Initially, Wesselius discussed a number of considerations which must 'consciously or unconsciously have dissuaded many researchers from this assumption [that the originator of the Dan-text was Jehu himself]'. In his opinion, these objections are not decisive.

First, there is the problem of language. It would be expected that an Israelite would write in Hebrew, not Aramaic. This problem can be explained by reasoning that Jehu was probably subject to Damascus as a vassal.

11. Obviously said with reference to Cryer, Kitchen is opposed, in general, to the 'minimalists', and strongly so.

A second problem is the idea that an Aramean god had installed an Israelite king. Wesselius's explanation for this view is that, if there were ever an Israelite king who originally worshipped Hadad/Baal, it would have been Jehu, who declared his adherence to Baal very clearly in 2 Kgs 10.18. Jehu probably worshipped both Baal and Yahweh.

A third problem is that the murderer of a king should brag about the murder in an inscription. This objection is countered by reference to the other West Semitic royal inscriptions that talk about divine support in times of crisis.

If Wesselius is correct that the text originates from Jehu, it would mean that the text is directly related to the Bible and the history of Israel, which enhances its importance for biblical scholarship considerably. Wesselius relied, to a great extent, on linking the inscription to 2 Kings 10, without which, according to him, it would not contain much 'hard information'.

6.2. *Becking 1999*

In the same issue of *SJOT*, Bob Becking responded to Wesselius by asking in a heading: 'Did Jehu Write the Tel Dan Inscription?' The implied answer is: No! Becking had previously (1996: 22) dated the text, on palaeographic grounds, to the period between the ninth and the eighth centuries. In spite of his objections, he, nevertheless, calls Wesselius's proposal 'ingenious'. Becking, however, accuses Wesselius of not bringing in new evidence, or a new method. 'In fact', according to Becking, 'he has been rearranging existing evidence, applying about the same historical methods as other scholars have done before him by merging epigraphic and Biblical data' (1999: 191). The question is whether his rearrangement of the evidence 'is more convincing than the existing proposals'. Becking's critique of Wesselius's Jehu theory is a discussion of 'a set of propositions and implications'.

Becking focused on three points in Wesselius's article: (1) the claim that Jehu would have had a good relationship with Hazael, because he needed political back-up for his revolution and *coup d'état*; (2) how a Yahwist king could say that 'Hadad went before me'; and (3) how an Israelite king could write a stele in Aramaic, rather than Hebrew.

That Hazael would have supported Jehu's *coup d'état*[12] is called an 'assumption' which 'is difficult to assess' (1999: 195), because the Aramean aggression continued under Jehu's regime (2 Kgs 10.32-33). This makes an original coalition between Jehu and Hazael 'less probable'. According to Assyrian sources, Jehu paid tribute to Shalmaneser III.[13]

12. A theory Wesselius got from Schniedewind.
13. Cf. 1999: 197 n. 37 and *ANET*, 281a.

The argument that Jehu would have chosen to write in Aramaic because he wrote to those who had supported his *coup d'état*, Becking maintains, 'is as such plausible' (1999: 195), though this use of external sources does not confirm his claim.

Becking's main conclusion is that Jehu could not have been the originator of the inscription. He considered that some of Wesselius's theories were not impossible, as such, but nor did he find most of them convincing.

6.3. *Wesselius 2001*

Wesselius divided Becking's arguments against him into two groups: his use of epigraphy and his use of the Hebrew Bible as a historical source.

As for the first group, Wesselius claimed that it is up to Becking to prove that his interpretation is wrong. In view of the fragmentary state of the text, Wesselius once more underlined that 'no result can be deemed really certain', adding that even with Becking's detailed arguments, his conclusion would have been more or less the same: 'I feel in fact strengthened in my opinion', he claimed (2001: 91).

With reference to the second group, Wesselius entered into a comprehensive study of the Hebrew Bible and history (2001: 91-103), repeating his confidence ('in the footsteps of Lemaire'), that '2 Kings as it is now can be reconciled fairly easily with the remaining text of the Tel Dan inscription, and some striking agreements in terminology and ideas can be observed' (2001: 91), which to him was an additional argument in favour of seeing the text as a royal inscription by Jehu.

After a survey of the biblical material, Wesselius claimed (2001: 101), 'It remains highly remarkable, in any case, that at times a nearly verbal agreement between inscription and biblical text may be observed', which in his opinion was caused by literary dependence.

Wesselius accused Becking of having misunderstood his presentation of Jehu's religious adherence at the beginning of his reign. Here Wesselius consciously argued from silence, because 2 Kings 9 and 10.1-17 'are mostly silent about Jehu's public adherence to any god, while especially in his private conversations his zeal for YHWH is stressed'. At least 'the story does not exclude' this interpretation, he modifies.

Wesselius claimed (2001: 103) to have 'turned down most or all of Becking's objections against the interpretation of the Tel Dan inscription as a royal inscription of Jehu'. He ended up with answering Becking's question, 'Did Jehu write the Tel Dan Inscription?', in the affirmative, stating that Jehu 'is the "I" of this inscription'. As for the biblical account of what happened more than 2800 years ago on the road to Jezreel, it 'could be true or false or anything in between, but it is certainly a meaningful and intended text' to him (2001: 103).

7. *Athas 2003*

George Athas intervened in the debate between Wesselius and Becking. As for Wesselius's Jehu theory, Athas held that it flounders on numerous grounds (2003: 257). First, it fails to consider the archaeological landscape of Tel Dan, and 'is based solely on reconciling the text of the Tel Dan Inscription with the biblical narratives of Jehu', a thesis based on 'a number of dubious assertions'. Briefly, Wesselius's thesis 'rests on circumstantial evidence and too many uncertain possibilities', because, to Athas, it is conjecture that the Dan inscription mentions the slaying of two kings. Along with all the other reconstructions, Wesselius's thesis is claimed to be based on an incorrect arrangement of the inscription and it is rejected as 'impossible'.

8. *Critical Comments*

The strength of Wesselius's article is that he at least tried to eliminate the contradiction between the Tel Dan inscription and the Deuteronomistic Historian on the issue of who killed the kings, Jehu or Hazael. This is a new historical problem raised by the discovery of the Dan text. Wesselius took seriously that we have two accounts of the same event, one epigraphic and one biblical. Becking seems to agree that Wesselius had a coherent system of arguments, but he did not agree with him in identifying the originator of the inscription. Wesselius, on the other hand, is not persuaded by Becking's counter-arguments. On the contrary, he seems even more persuaded by his own arguments.

A main reason for this dispute is that Becking and Wesselius have different basic assumptions as to how to use the sources. As for the use of biblical sources, Becking accused Wesselius of operating on a fundamentalist basis, which Wesselius, with some reservation, understood, but still refuted. On the other hand, Becking defended positions closely related to what can be found in the 'minimalist' camp. Wesselius has greater confidence in Primary History as a possible historical source than Becking, who is more careful, or even reserved, with respect to Primary History. With such different points of departure, the two stories were predestined to arrive at very different conclusions.

Nevertheless, Wesselius has not gained any support for his argument that Jehu is the potentate behind the Dan text.

9. *Identification of the Originator's Father*

As we have seen, most scholars have argued for King Hazael of Damascus as the originator of the stele. Yet the identity of the originator's father has also been debated. In the inscription this individual is simply called 'my father'. The Editors did not identify him;[14] but several scholars have tried to.

Émil Puech (1994: 220, 221) argued that the originator's father was the Aramean King Benhadad II, and reconstructed Fragment A line 2 as אבי · הדד · [·]ר[ב], '[Ba]r Hadad, mon père'.

Victor Sasson (1995: 28) argued that the focus in the stele on the originator's father was necessitated by (1) a literary tradition or convention concerning public display, (2) evidence of a dynasty, and, most importantly, (3) the text's desire to establish the speaker's own historical claims over territories, which, in the past, had been the cause of battles or wars. The mention of the originator's father was merely paying lip-service, adding prestige and honour to the person concerned. Notably, Sasson did not attempt to establish the identity of the father mentioned in the inscription.

Shigeo Yamada (1995: 614) argued that the best candidate for the originator's father is Hazael's royal predecessor, identified in 2 Kgs 8.7-15 as 'Benhadad, king of Syria', that is, Benhadad II, a king who is usually identified with Adad-'Idri in Shalmanesar III's inscriptions.[15] The problem is that the usurper Hazael calls his predecessor 'father'. Yet, this can be explained: Hazael may have belonged to the royal family, but may not have had first claim on the throne—a reading made possible by the broad semantic meaning of the term אב, often translated 'father', in Semitic languages (cf. the Biran and Naveh 1995).

In Nadav Na'aman's opinion (1995b: 389), 'the [Dan] stela may safely be attributed to Hazael', whose origin is supposed to have been from the dynasty of Beth-rehob. This 'has an important implication' (1995b: 388) for the Tel Dan inscription. Dan is located at the southern end of the kingdom of Beth-rehob (Judg. 18.28), and is assumed to have been 'a natural target for an attack of a king of Beth-rehob' (1995b: 389). Hazael's father, and predecessor on the throne, is identified as Ba'asa (1995b: 389 and 393).

After some deliberation, Walter Dietrich (1997a: 140) concluded that Fragment B presents us with Jehoram of Israel (850–845), and Ahaziahu of Judah (845), who were both assassinated by Jehu in 845, a conclusion which is hard to evade. That the fragmentary names could refer to the somewhat earlier Ahaziah of Israel (851–850) and Jehoram of Judah (850–845) is refuted by the fact that neither is reported to have pursued any joint action

14. See Biran and Naveh 1995: 17-18.
15. Cf. *ANET*, 278b.

against Aram. That would contradict Fragment A, in which the order of succession is Judean to Israelite, while Fragment B has the opposite.

Ingo Kottsieper (1998: 485) claimed it to be indisputable that Hazael did not belong to the royal family. Kottsieper notes that Hazael's name is lacking in every inscription where it would be expected to be present were he a member of the royal family, while Shalmaneser calls him the 'son of a nobody'. Rather, Hazael held important positions in the circles around the royal family and was a confidant of the Aramean king (cf. 2 Kgs 8.7-15). Thus, he probably used the title 'father' in a symbolic way, since his predecessor was, probably, his patron. The identity of Hazael's predecessor is discussed in detail by Kottsieper (1998: 492-95). The problem is that 2 Kings 8 calls Hazael's predecessor Benhadad, while Shalmaneser III calls him Hadadezer, that is, Benhadad II. Kottsieper argued for using the name Shalmaneser used, Hadadezer, in his contemporary inscription.

William Schniedewind and Bruce Zuckerman (2001: 89) investigated the possibility of reconstructing the name of Hazael's father from the Tel Dan inscription itself. They rejected the reconstruction by the Editors and Puech (1994) in line 2, basing their own reading on a computer manipulation (Schniedewind and Zuckerman 2001: 89) which reconstructed · רקאל[ב]* אבי, '[Ba]raq'el, my father'—a well-known Semitic personal name meaning 'the lightning of El' (2001: 90).

Even though the originator of the text refers to his father three times, it is not possible to reconstruct his name with certainty. None of the proposals offered is indisputable. Even though there is a high degree of consensus that the originator was Hazael of Damascus, we are not sure who Hazael's father was. Noting the theories, I allow the identity of the originator's father remain an open question.

10. *General Summary*

The general summary and conclusion is, briefly, that most scholars who have attempted to identify the inscription's originator argue for King Hazael of Damascus, probably the most significant Aramean king of the era. The arguments for Hazael seem to be the ones with the most secure basis, given that they also fit the period to which the text is most reliably dated on other grounds, namely, the latter part of the ninth century.

Chapter 6

THE *BYTDWD* QUESTION

No other single part of the inscription has received so much attention as the term *bytdwd* in line 9. It therefore seems appropriate to dedicate an entire chapter to a discussion of the term. The term is interesting for different reasons—its language history (both components of the word are written *plene*), the non-use of word division, the history of religion, royal history, biblical history, history of Israel and the Arameans and so on—and in particular because this question constitutes the very watershed in the minimalist vs. maximalist controversy with respect to this inscription. In this chapter we will look exclusively at the reading and identification of *bytdwd*. Since this term is found in line 9 of Fragment A, the scholarly debate has not been significantly influenced by the discovery of Fragments B1 and B2.

1. *The Reading of* bytdwd

The problem with *bytdwd* is that it is written as one word, without a word divider, which are otherwise attested in the Dan inscription. The Editors rendered it as ביתדוד, in one word, but translated it 'House of David', as if it were written as two words. The fuss surrounding the word *bytdwd* has been remarkable. There has been an intense debate for[1] and against[2] reading

1. In favour of reading *bytdwd* as *byt dwd* were the Editors; Kallai 1993; Ben Zvi 1994 (hesitantly); Ha-Yehudi 1994; Lemaire 1994a, 1994b, 1994c; Millard 1994, 1997; Noll 1994, 1998, 2001; Puech 1994; Rainey 1994, 1995; Becking 1995; Demsky 1995; Freedman and Geoghegan 1995; Hoffmeier 1995; Müller 1995; Na'aman 1995a, 1995b, 1997, 1999; Rendsburg 1995; Sasson 1995; Zindler 1995; Knauf 1996; Schniedewind 1996; Dietrich 1997a, 1997b; Kitchen 1997; Knoppers 1997; Shanks 1997, 1999; Kottsieper 1998; Dion 1999; Wesselius 1999; Couturier 2001; Ehrlich 2001.

2. Objections to reading *bytdwd* as *byt dwd* have come from Cryer 1994, 1995a; Davies 1994a, 1994b, 1995; Knauf, de Pury and Römer 1994; Lemche and Thompson 1994. Ben Zvi (1994) expresses doubts about the two-word reading, while Lemche (1995a, 1998b), Thompson (1995a, 1995b, 1995c), Lehmann and Reichel (1995) and Athas (2003) should probably be classified in the same category, cf. his Bayt-Dawid solution (2003: 193).

bytdwd as *byt dwd*, but with a preponderance of 'yes'-voters opting for the idea of reading *bytdwd* as *byt dwd* and for translating it as 'House of David', or something similar. This statistical result is interesting, but the arguments are more important. Some contributors have simply made statements and not presented arguments to support their opinions, while others have argued more strongly and fully. Some have argued particularly forcefully, as if they had particular motives for arguing as they do.

We shall now follow the arguments for and against reading *bytdwd* as *byt dwd*, critically, concentrating on the most substantial contributions, and beginning with the arguments against reading *byt dwd*, as though it were divided.

1.1. *Arguments against reading* byt dwd

The Swiss scholars Knauf, de Pury and Römer (1994: 65-66) highlighted five arguments against reading *byt dwd* and translating it 'House of David'. The following is my translation of their points:

> Firstly, the geographical context in which the inscription was found implies that the battle was fought near Dan. Secondly, if *bytdwd* was intended to designate 'House of David', it would mean that the author of the inscription was able to impose a tribute on, or that the king was authorised to demand tribute from, an enemy. Thirdly, according to 2 Kgs 12.18-19, Hazael had conducted a campaign against Judah, but he did not refer to this event when he speaks in the 1st. person. And Hazael is not regarded as the author of this inscription. Fourthly, Naveh's reconstruction of [*ml*]*k* before bytdwd is called 'une sorte de monstrousité sémantique', because the phrase 'XY son of NN, founder of [dynasty]', is unknown from other Semitic inscriptions. Fifthly, it would be very surprising if the two phrases, *bytdwd* and *ysr'l* were to be used in parallel.

These scholars were mainly motivated by their historical understanding of *bytdwd* and the translation of the *dwd*-part when they argued for the unified term *bytdwd*.

Lemche and Thompson (1994: 9) based their arguments on the lack of word divider. Although they did not exclude the possibility that the text was written by 'a poor and inconsistent writer', it would have been, they claim, 'an extraordinary pity' that a scribal error would have affected this particular word! Perhaps the author 'did not know what the authors of biblical books knew: that a dynastic name of the kingdom of Judah needed to be divided in two words' (1994: 9-10), they speculate. However, this is unlikely, not least because *bytdwd* would have been just one of a large number of names of a single type: names referring to states.[3] All names of this type are divided by

3. References are made to Bīt Humriyya of Israel and the names of the Aramaic kingdoms Bīt Gusi and Bīt Adini.

dots. According to their conclusion, *bytdwd* should not be read as two words and translated 'House of David', it should rather be taken as a place name of the type *byt'l* (Bethel), which is always written as one word. The term *bytdwd* would then refer to a place near Dan (cf. below).[4]

The main objection of Philip R. Davies (1994a) was the interpretation of the undivided ביתדוד. When the Editors read it as two words and translated 'House of David', he called it 'an argument in reverse'. To read 'House of David', identifying it with the dynastic name of the kingdom of Judah, and paralleling it with *Bit Humri* read as 'House of Omri' and using it to designate the northern kingdom of Israel, is, frankly, 'speculation', he claimed. It is also 'speculation' and 'misleading' to stipulate that the *kaph* (ך) preceding ביתדוד should be read ך[מל], king, as 10-20 other letters 'might as plausibly be suggested'. After comparing this reading to Assyrian sources, Davies argues, that no inscription reads 'king of the House of Omri', nor is there any equivalent phrase in the Bible. This restoration 'is purely conjectural', in his opinion.

Cryer (1994, 1995a) argued from Semitic epigraphic parallels against reading *bytdwd* as *byt dwd*, but his arguments were primarily linked to how *bytdwd* should be understood, not so much to the question of word division itself.

For Ben Zvi (1994) the problem was that there is no epigraphic evidence for reading 'House of David' as *bytdwd*. Ben Zvi did, however, admit that the weight of the biblical evidence supports reading 'House of David'.

Athas (2003: 218-19) argued line 4A has the expression ·בארק [אלבין[תאל (as he reconstructs it), line 7A has אלפי · פרש and line 8A has מלך · ישראל—and line 10A even has הם · ארק. All this indicated to him that

> construct expressions are used to denote two or more concepts that are both individually elusive, yet connected genitivally in the given context. As a result, a word divider is used to demarcate the separate parts of a construct expression… Therefore, we must look to an interpretation that understands ביתדוד as one essential entity. Unfortunately, none of the possible interpretations is completely void of problems.

After a detailed study of the problem (2003: 219-26), Athas concluded that 'The most logical solution' is that the originator of the inscription was the ruler of a place called ביתדוד, and that Jerusalem 'is by far the most likely candidate for this place' (2003: 25). Athas, however, 'cannot stress enough' that ביתדוד should be taken as a reference to a Davidic dynasty. It may be a label with an etymology going back to a Davidic dynasty, but does not refer to the dynasty itself, he claimed.

4. Also, Lemche (1995a) and Thompson (1995a) have argued primarily on historical grounds against reading *bytdwd* as *byt dwd*, 'House of David', rather than from particular linguistic arguments. Thompson has also raised geographical objections.

In arguing this way, Athas indirectly confirms the existence of the historical David. After all, how could there be a Davidic dynasty, if there was no David?

1.2. *Arguments for Reading* byt dwd

In favour of reading *bytdwd* as if it should be *byt dwd* are, first and foremost, the Editors—although they, regrettably, give no real argument for it.

Lemaire (1994a, 1994b, 1994c) based his views on his finding of what he sees as a parallel in the Mesha stele, where he restored בת[ד]וד in line 31.[5] His main arguments are set out in the first of his 1994 articles, where he accepted the reconstruction and translation '[King of the] House of David' (line 9), adding that the text is authentic, 'no doubt to the chagrin of those modern scholars who maintain that nothing in the Bible before the Babylonian exile can lay claim to any historical accuracy' (1994a: 31). His other articles (1994b, 1994c) do not offer significant insights into this particular point.

Anson Rainey (1994) sided roundly with the Editors, and defended their interpretation of the undivided ביתדוד as 'House of David'. They 'cannot be blamed for assuming a modicum of basic knowledge on the part of their readers'. Rainey found support for his opinion in the formula BL'M. BRB'R, 'Baalam, son of Beor', from Deir 'Alla, which has a word divider between BL'M and BRB'R, but not between BR and B'R. Rainey also argued using the example of the personal name BRRKB, 'Bir-Rakib', from Zincirli, which also lacks a dividing mark between BR, 'son', and RKB, 'Rakib'.[6] In general, there is some inconsistency in the use and non-use of word-dividers. From this documentation, Rainey concluded that ביתדוד 'was

5. This reading of the Mesha text has been criticized by some and followed by others. It is followed by Halpern 1994: 63; 1995: 32b; Puech 1994: 227; Kitchen 1997: 35-36; Parker 1997: 46, 155 n. 13; Schniedewind 1997: 80b, 86a n. 2; Rainey 1998: 244; 2000: 117; 2002: 147b; Dion 1999; King and Stager 2000: 45a; Couturier 2001: 81; and Finkelstein and Silberman 2001: 129; cf. also Ben Zvi 1994: 29-32; Lemche and Thompson 1994: 13; Zindler 1995: 18. Davies (1994a: 54 n.) calls the proposal 'implausible'. Dietrich (1997b: 22 n. 37) seems to accept his theory, conditionally: 'Es scheint mir voreilig zu sein, wenn Lemaire…das von ihm entdeckte *btdwd* (evtl. auch *bt dwd*) sogleich auf das Davidshaus deutet. Ebensogut wäre denkbar, daß der Name der Dynastie hier—wie in der anschliessend zu besprechenden Stele von Tel Dan—das von ihre regierte Land, also Juda, meint'. Also, Gibson has found 'David' in line 12 of the Mesha text. See Gibson 1971: 71, referred to in Lemche and Thompson 1994: 10 n. 19. This interpretation is endorsed by A. Rainey (1998: 244, 247), an examination of the Beth Shean cylinder, the Ekron inscription and the Mesha stele. Thompson (1995a) turns down Lemaire's theory as 'very speculative' (1995a: 72). Smelik (1992) did not discuss the question.

6. See *KAI*: I, 216, 217, 218; cf. 221.

obviously recognized by the scribe of the Dan inscription as an important proper name. There is no reason whatever to doubt the correctness of the reading "House of David".'

Kurt Noll (1994) was not persuaded by Davies's arguments. On the question of the lack of word divider, Noll compared ביתדוד to BYTHRPD in the fourth Lachish letter,[7] which also has no word divider. The problem here, however, is that it is not evident to him whether BYTHRPD in this letter refers to a place or a political entity. Davies's argument against taking the final ־ך before ביתדוד in line 6 as part of ך[מל] has some weight, but that ביתדוד should be rendered as 'Bethdod' or 'Bethdaud' has, it seems to me, been plucked out of thin air, since no place with such a name is known. On the other hand, there are several examples of 'House of David' in the biblical texts. Davies contradicts himself, when he, on the one hand, claims that it is 'intrinsically more likely' to find place names without a word divider, and on the other hand, uses an example from the Mesha stone, where a place name is written with a word divider.

Gary A. Rendsburg (1995: 22) wrote a small but very significant article on the question of the undivided ביתדוד, focusing more in particular on the *bet-X-Formula*. In his opinion, ביתדוד lacks a word divider because it was read as 'one entity not requiring a word divider'. He also pointed out that words constructed around the X-בית formula were particularly frequent in Aramaic, 'more characteristic of Aramaic usage than they are of other Semitic languages'. No other corpus of ancient Northwest Semitic texts has such a concentration of X-בית examples. This is documented from proper names with several attestations in the Aramaic and Akkadian epigraphic corpus.[8] The instances of X-בית names in the Bible refer to Aramean entities.[9] Obviously, the Hebrew Bible uses the X-בית formula in contexts related solely to Israel and Judah, without any connection to Aramean matters, including the expression בית דוד, 'House of David'. Nevertheless, the Bible reflects Aramean terminology in reference to Aram (1995: 23). It is also noteworthy to Rendsburg that the Bible has the 'unique formulae' בית אפרים (Judg. 10.9) and בית ישׂשכר (1 Kgs 15.27), where we would have expected a reference to the tribes (1995: 23-24). 'These usages are the products of scribal schools with close ties to Aramaic practice'. Also, in cuneiform texts,

7. See *KAI*: I, 194 (with word divisions inserted) and *HAE* I/1: 421; III: 32 (without word dividers).

8. He relied, among other things, on *KAI*, *RlA*, *AHw*, *CAD* and a number of studies by A. Hurvitz.

9. Cf. בית חזאל (Amos 1.4), בית עדן (Amos 1.5), בית רחוב (Judg. 18.28; 2 Sam. 10.6) as well as אבל בית מעכה (2 Sam. 20.14-15; 1 Kgs 15.20; 2 Kgs 15.25), which are Israelite sites, but ones that had probably previously belonged to Aram (cf. 2 Sam. 10.6). For more references, see Rendsburg 1995: 23.

he found material which sheds light on 'the Aramaic nature of the X-בית' formula (1995: 24).[10] *Bit*-X names became more and more popular in Neo-Assyrian and Neo-Babylonian times (1995: 25), designating foreign, as well as domestic, cities and regions.[11] This also explains, in Rendsburg's opinion, another feature of Aramaic influence: common nouns of the X-בית type 'are more characteristic of Aramaic than they are of other Semitic languages'. Even though this 'cannot be defended with statistical data', it 'should be apparent to anyone with a broad-based familiarity with ancient Semitic languages' (1995: 25). In addition, there is the influence of Aramaic on languages to its west and east. Here, Rendsburg refers approvingly to two studies by Hurwitz (1992, 1993) on X-בית in Late Biblical Hebrew, accepting Hurwitz's conclusion that that Aramaic influence ultimately meant that the X-בית construction became the norm. Rendsburg concluded:

> In sum, X-בית common nouns are typical of Aramaic, and during the years in which Aramaic began to exert its influence to the west and east, nouns of this type entered lexica of Hebrew and Akkadian freely. The totality of the evidence demonstrates that X-בית was a strong characteristic of Aramaic phraseology. This fact explains why an Aramean scribe would use the expression ביתדוד for Judah, writing it as one lexeme not requiring a word divider (1995: 25).[12]

Rendsburg was supported, quite independently, by Na'aman (1995a: 20), who documented the 'typical phenomenon' of south Anatolian, Syrian and Palestinian kingdoms to be presented under a plurality of names. In particular, the eponymic/dynastic name '"Bīt-PN" is typical of many of the West Semitic kingdoms that emerged in the Fertile Crescent in the early first millennium BCE'.[13] Since our knowledge of names of kingdoms has depended mainly on Assyrian inscriptions, 'only one name is known' for such

10. Rendsburg (1995: 24) states: 'In Assyrian and Babylonian records Aramean states are repeatedly—[in a disproportionate manner]—referred to as Bit-X'. His examples are: Bit-Adini, Bit-Amukkani, Bit-Bahyani, Bit-Dakkuri, Bit-Garbia, Bit-Halupe, Bit-Sa'alla, Bit-Sillani, Bit-Sin, Bit-Yahiri, Bit-Yakini and Bit-Zamini; cf. Bit-Humri (House of Omri) and Bit-Ammana (House of Ammon), which could have reached Assyrian scribes through Aramean mediation. Cf. also Oppenheim 1977: 160. On Akkadian Bit + name of a person, cf. also van de Mieroop 2004: 164.

11. *RlA* has no fewer than 120 entries for Bit-X sites attested only in later periods of Mesopotamian history, Rendsburg (1995: 25) claims.

12. Thompson (1995b: 240) objected to Rendsburg's use of this material in relation to the *bytdwd* phrase, attacking his article as 'another curious case of such abandonment of sound method'. Thompson found no indication that *bytdwd* was used for Judah.

13. His examples were Bīt-Adini, Bīt-Agusi (Arpad), Bīt-Gabbari (Sam'al), Bīt-Hazaili (Damascus), Bīt-Ruhubi (Beth-rehob), Bīt-Humri (Israel) and Bīt-Ammana (Ammon).

peripheral West Semitic kingdoms as Judah, Ammon, Moab and Edom. Many of the West Semitic kingdoms in the Fertile Crescent, Na'aman notes, were called by *Bīt*-names.

William M. Schniedewind agreed that 'the most sensational aspect' of the Dan text has been the occurrence of ביתדוד and its reading as 'House of David'—a reading which, for Schniedewind, has been opposed by 'a small but vocal minority'. Schniedewind's own position is that the missing word divider presents no problem, and 'was irrelevant to the real issue of the debate…[which] concerns the underlying probability of a ninth-century inscription referring to David or, in this case, the "House of David"' (1996: 75b). He found no word divider in מלכי[ש]/ראל, 'king of Israel' (lines 3-4), either, which, to him, illustrated 'the fact that the missing word divider is irrelevant to the real issue of the debate'.[14]

The lack of a word divider was discussed by Hershel Shanks (1997)[15] with reference to an ostracon in the private collection of Shlomo Moussaieff in London. In a receipt for silver received in the temple, LBYTYHWH, 'for the house of YHWH', is written without a word divider. The text is not consistent in its use of word dividers. On the basis of its palaeography, this text is dated before the exile, and, so far, regarded as the oldest non-biblical evidence of the temple of Solomon. The origin of this ostracon is not known, but there seems to be general consensus that it is authentic.[16] In Shanks's opinion, the argument against reading 'House of David' is weakened by the testimony of the ostracon.

Francis I. Andersen (1998) focussed generally on the personal names known in Hebrew Bible times, discussing *bytdwd* and the reconstructed names in lines 7 and 8B. Andersen explained the historical development of the writing of the name of David, pointing out that '[t]he old spelling *dwd*, used in Hazael's monument, is the spelling used in the biblical books from before the exile, notably the book of Samuel. The later, longer spelling [*dwyd*], is used exclusively in Chronicles, and that spelling is the same as in the Dead Sea Scrolls' (1998: 45).[17] Andersen argued (1998: 44) that the

14. Schniedewind (1995: 75) states: 'In fact, the so-called missing word divider was as much a problem for the alternative reading. The real issue concerns the underlying probability of a ninth century inscription referring to David, or, in this case the "house of David"'.

15. *BAR* 23.6 (1997c): 32.

16. According to Shanks (1997: 31), both ostraca are to be 'awarded an "A" for authentic'. A. Lemaire, P. Kyle McCarter Jr and F. Moore Cross are said to have accepted their authenticity, which has also, allegedly, been confirmed by the laboratory Mikrofokus Oy in Helsinki. Shanks: 'Now, it seems, everyone is satisfied the inscriptions are authentic'.

17. Cf. Andersen's table on the spelling of 'David' (1998: 52).

practice of using dividing dots is in line with contemporary custom,[18] which differed from Hebrew in two ways: (1) a suffix could be separated from the name stem (line 10), and (2) a compound phrase could be presented as a single expression by omitting the dot, as in ביתדוד, as well as in Phoenician inscriptions, where the 'king of Byblos' is written מלכגבל.

Guy Couturier (2001) should be mentioned as a more recent contributor to the debate. With reference to the absence of the word divider in *bytdwd*, he traced, in detail, Phoenician, Ammonite, Hebrew and Aramaic epigraphic material. According to Couturier's analysis, the lack of a word divider was not unusual.

Rendsburg's 1995 contribution can be considered as the last major salvo in the debate about the lack of the diving dot in *bytdwd* (see, however, Shanks 1997 and Couturier 2001), and, aside from Athas's (2003) study, it may not be going too far to say that Rendsburg's work virtually brought the *bytdwd* debate to a close, at least with respect to arguments against reading *byt dwd*. As a result of Rendsburg's efforts, it seems that the reading of *bytdwd* as *byt dwd* is settled. Not many scholars have questioned whether *bytdwd* should be read as *byt dwd*. Inconsistency in word division is confirmed also in the Dan inscription itself, if the reading ארק · הם for ארקהם, 'their land', is acceptable (line 10). In my view, the reading of *bytdwd* as *byt dwd* should be accepted. But that does not necessarily mean it should be read as 'House of David'. As will be shown in the next section, this has long been a controversial translation, as controversial as reading *bytdwd* as *byt dwd*.

2. *How Should* bytdwd *be Understood?*

If *bytdwd* (ביתדוד) is read as *byt dwd* (בית דוד), the expected translation would be 'House of David'. This is the reading claimed by both the Editors and most of the scholars who read *bytdwd* as *byt dwd*. Nevertheless, however, there have been different opinions as to how this term should be understood.

Galil (2001: 16) classified the ways *bytdwd* has been understood into the following six groups:

1. the dynastic name of the kingdom of Judah,
2. a temple of the God of Israel,
3. the house of a local deity,
4. a toponym,
5. the name of a distinguished Israeli officer, and
6. an open question.

18. Presumably Andersen is referring to the *Aramaic* custom, though his wording is not explicit.

The alternatives are not exhausted by this list. None of these alternatives should be isolated from the others, but rather they should all be taken as an aid to seeing the *bytdwd* question from different angles. Galil's reeling off of who has chosen which alternative (2001: 16) is too simplistic, but is, nevertheless, useful for focusing the different angles. Some scholars have discussed several alternatives, and have not decided conclusively in favour of any particular solution. The debate is multifaceted, and the problems are knit together in a tight web. There is plenty of overlap between the different aspects of the debate, so the points should not be considered separate.

Before we look at the alternative solutions, we should look for comparisons with other texts.

2.1. *Dynastic Name of the Kingdom of Judah*

Walter Dietrich (1997b: 28) underlined explicitly the difference between kingdom and dynasty and argued in favour of *bytdwd* as the kingdom of Judah, not the dynasty of David. In this case, however, these questions are so intertwined that the difference is of a more theoretical nature than a real one. I am aware of the difference, but will not differentiate between them here. Also, differentiating between a kingdom and a geographical area is delicate, since, as a rule, a kingdom covers a geographical area. In this part of the chapter there will inevitably be overlapping between these different concepts.

Without discussing these problems in particular, the Editors (1993, 1995)[19] elected to understand *bytdwd* as the dynastic name borne by the kingdom of Judah.

Ehud Ben Zvi (1994) did not exclude the possibility that *bytdwd* could point to 'House of David' and carry the meaning 'kingdom of Judah'. 'It is true', Ben Zvi noted, 'that the weight of the biblical evidence supports the reading of bytdwd as "House of David"' (1994: 26). At the same time, Ben Zvi cautioned that 'categorical affirmations of such a reading are questionable and should be avoided' (1994: 29). Alternative readings 'do exist, cannot be ruled out, and should be kept in mind'. Ben Zvi pinned his confidence on the future discovery of additional fragments, speculating that 'perhaps they will shed more light on the meaning of the term *bytdwd* in this specific case' (1994: 29).

Mindert Dijkstra (1994: 10) immediately focused on the interpretation of *bytdwd*, regretting that 'before even a discussion could start about the nature and historical significance' of this expression, the interpretation 'the House

19. Biran and Naveh 1993: 93, on ביתדוד: '[This] is the dynastic name of the kingdom of Judah'; cf. Bīt Humri (Israel), Bīt Agusi (Arpad), Bīt Haza'ili (Aram-Damascus) and Bīt Adini.

of David' was again challenged.[20] If 'House of David' is correct, 'the stela only confirms the early existence of the dynastic name of the kingdom of Judah'. In its present condition, however, the stele 'hardly permits a historical conclusion about the nature of Judah's involvement in the events mentioned in the text', he argues.

Allan Millard (1994) argued that a 'House of David' existed and added—against Davies (1994a)—that 'the history of the ancient Near East shows it could have existed and that David's empire was not impossibly huge', as Davies had also claimed.

Hans-Peter Müller (1995: 127) found support for reading *dōd* as a throne name in several biblical references, but accepted as 'tatsächlich' ('a matter of fact') (1995: 136) that *bytdwd* refers to the kingdom of Judah.

Victor Sasson (1995: 22) conceded that he had 'no difficulty in seeing *bytdwd* as a possibly genuine Aramean reference to the kingdom of Judah'. His argument was historical on a more general level. At the outset, he accepted the Deuteronomistic Historian's general description of the age of David and Solomon as 'the golden age par excellence' (1995: 22), and argued that 'an Aramean pagan conqueror…would, conceivably, take the occasion to brag about the devastation he had inflicted on Judah…'[21] He saw in the inscription an Aramean conqueror bragging about the devastation he had inflicted upon Judah, and he employed the phrase *bytdwd* 'because it aggrandized the Aramean victory over a once powerful enemy'. In particular, he examined how this expression is used in the Bible, finding that the earliest occurrence referred to the civil war between Saul and David's family,[22] not to his political dynasty. The idea of 'House of David' is implicit in Yahweh's promise to David in 2 Sam. 7.1-16, which 'is primarily a political text that justifies the Davidic monarchy'. The earliest reference to 'House of David' as a political dynasty occurred in the aftermath of the Syro-Ephraimitic war (e.g. Isa. 7.3, 13), where reference is made to 'the legitimate dynastic leadership of the sons of David', in contrast to the apostate northern kingdom of Israel.

Ernst Axel Knauf (1996) commented, briefly, saying that even if *bytdwd* were to be understood as 'House of David'—an understanding 'was durchaus möglich ist' ('which is thoroughly possible')—it would only represent

20. Dijkstra (1994: 10) states: 'It is the old story. For lack of context every word and incomplete sentence can mean almost everything.'

21. His biblical references were Isa. 7.2, 13; 22.22; Zech. 12.7 and 2 Chron. 21.7; cf. also Ruth 4.22 and Isa. 11.1. He also refers (1995: 22 n. 23) to Lemaire (1994a) and his claim to have found *byt dwd* in the Mesha stele, which 'certainly deserves to be looked into carefully. [It] has all the evidence to support it… [But it] cannot really be compared with the emotional and intellectual impact that the Tell Dan inscription made.'

22. Cf. 1 Sam. 19.11; 20.16; 2 Sam. 3.1, 6.

evidence that the tradition of David, as founder of a dynasty, was known at the end of the ninth century. In Knauf's opinion, BYT in BYTDWD does, without a doubt, refer to a 'house', either a dynasty, or a palace or a temple. The problem, for Knauf, is DWD, which, he claimed, is a designation for a local god. This, therefore, implies that BYTDWD could be translated 'temple of Dod', or 'temple of the Beloved'. With the discovery of Fragment B, 'House of David' became a more plausible reading for Knauf. To him, this fits the position of David as described in 1 Sam. 30.26-31 and 2 Sam. 2.1-4. The use of the term 'Israel' for the northern state and 'House of David' for the southern state indicates to Knauf the transition from tribal state to area state.[23] The designation 'Judah' for the southern state is not confirmed until an Assyrian inscription from the year 727. Knauf concluded that the designation 'House of David' in line 9, is 'mit aller Vorsicht' ('with great care') acceptable. From this he derived that the development of Judah from tribal community, or tribal state, to 'modern' area state with a common ethnic identity was not yet completed. Knauf is, obviously, moving towards being more in favour of the interpretation 'House of David' since the discovery of Fragment B, which indicates the interpretation 'Dynasty of David'—but he does not say that explicitly.

William M. Schniedewind (1996) admitted that 'undoubtedly this reading [ביתדוד as "House of David"] has been the most controversial aspect of the inscription'. After discussing proposed alternative solutions, he concluded: 'None of these alternatives seems particularly compelling'. He endorsed the Editors, but admitted that the expression 'House of David' in this text is 'to some extent…surprising and unexpected'. In his opinion, Sasson (1995: 22) suggests a 'plausible explanation', which is 'probably not far from the mark' (Schniedewind 1996: 80b).

Walter Dietrich (1997a: 141) claimed that the author of the Tel Dan inscription called the northern kingdom by its political name, 'Israel', while the southern kingdom of Judah is referred to by its dynastic name, *bytdwd*, 'Haus Davids'. The inscription uses the Hebrew form *byt* for 'house', not the Aramaic *bt*, which he considers reflective of the current dialectical preference of people in this part of Palestine. They also named their land after their dynasty, whose eponym bore the name *dwd*, a name he connected to David, implicitly refuting the readings *daud*, *doud*, and so on. Dietrich elaborated on his arguments in a second article (1997b: 28), where he debated the *bytdwd* question more fully, explaining that the biblical language is not at all as unambiguous as it seems. That is also the case when בית is connected to דוד/דויד. In most cases it refers to the family of David, but not in the case of 2 Sam. 3.1-6, which talks about a long war between the

23. Cf. Dietrich 1997b.

'House of Saul' and the 'House of David'. This is somewhat anachronistic, according to Dietrich, since the author there is projecting back a turn of phrase from a later period, 'dabei aber die gemeinte Sache genau getroffen' ('but, in so doing, has hit the target exactly'). If *bytdwd* is understood as Judah, the problem is eliminated. That the kingdom of Israel is not designated by an equivalent to *byt š'wl*, 'House of Saul', posed no problem to him. It is surprising to Dietrich (1997b: 29) that the text does not use 'House of Omri',[24] which, he explains, in agreement with Knauf (1996: 10), could indicate a development in the transition from tribal state to a real state, a translation that came a hundred years earlier in the northern kingdom of Israel than in Judah.

Nadav Na'aman (1997: 47) simply claimed that 'the name Beth David for the kingdom of Judah fits perfectly into this ancient Near Eastern usage'.

Kenneth A. Kitchen (1997: 37-39) contains an extensive cartographic documentation of 15 *bayit*-names from the Semitic language area in the first millennium BCE, concluding that this 'is a dynastically orientated term for the kingdom of Judah, stemming from David as dynastic founder, as was Omri in Israel, Agusi in Arpad and a whole series more… Trying to evade the import of this evidence is simply a waste of time' (1997: 39).

Francis I. Andersen (1998: 43) simply claimed that the originator of the text was King Hazael of Damascus and that this monument 'identifies the kingdom of Judah as "the house of David". Hazael didn't invent that name. It can no longer be said that other people in that part of the world at that time, had never heard of David, and therefore what the Bible says about him is fiction' (1998: 45).

According to Jan-Wim Wesselius (1999: 183), 'nearly all the scholars who accepted the [king of the House of David] interpretation have assumed that this would need to be a geographical or ethnic designation, roughly equivalent to the kingdom of Judah in the Bible'. Yet it is not at all that simple, since a number of scholars exclude this possibility. As soon as we deny that the Dan text could have been written by an Aramean king, as Wesselius does, it is very likely that 'House of David' is, simply, the northern Israelite designation for Judah.[25] Even if the text does not talk in much detail about David's kingdom, he claims: 'David was apparently considered as the founder of the dynasty of the kings of Judah' (1999: 184 n. 31). It

24. Cf. Assyrian KUR (mat) bit-Humri, 'the land of Bit Humri', that is, 'the land of the House of Omri'. See Ahlström 1994: 573.

25. This he found corroborated in 1 Kgs 12.26-27; 14.7-8; 2 Kgs 17.21. Wesselius (1999: 184) states: 'It would seem that the use of this title, instead of "King of Judah", may have been caused by Jehu's evident desire to describe his killing as a major feat; he killed not only the monarch of the powerful kingdom of Israel, but also a king from the ancient dynasty of David.'

follows from Wesselius's argument that the text was written by Jehu, and that the 'House of David' could have been a northern designation for the kingdom of Judah,.

Israel Finkelstein and Neil Asher Silberman (2001: 129) found a historical timeframe for *bytdwd* ('House of David') in the ninth century, claiming:

> This is dramatic evidence of the fame of the Davidic dynasty less than a hundred years after the reign of David's son Solomon. The fact that Judah (or perhaps its capital Jerusalem) is referred to with only a mention of its ruling house is clear evidence that the reputation of David was not a literary invention of a much later period. Furthermore, the French scholar André Lemaire has recently suggested that a similar reference to the House of David can be found on the famous inscription of Mesha, King of Moab in the ninth century BCE, which was found in the nineteenth century east of the Dead Sea. Thus, the house of David was known throughout the region; this clearly validates the biblical description of a figure named David becoming the founder of the dynasty of Judahite kings in Jerusalem.

The position taken by most scholars is that *bytdwd* refers to the kingdom of Judah and/or the Davidic dynasty.[26]

2.2. *Arguments for Taking* bytdwd *as a Toponym*

Frederick H. Cryer (1994: 17) simply rendered ביתדוד as *Betdawd* instead of translating 'House of David', arguing that the reference is best taken as a geographical designation. Cryer treated ביתדוד from a historiographical angle, and did not try to identify its location. Cryer also desisted from identifying which word the letter *kaph* (כ) in front of ביתדוד could have been part of, suggesting, after some deliberations, that 'ביתדוד was the author's designation for a geographical unit which may have been equivalent to all or some part of the region we regard as Judah', and that the designation is somehow equivalent to the Assyrian designations *bit Humri* and *bit mitinti* and Hebrew toponyms like בית מעכה, ביתאל and בית שׁמשׁ.

Lemche and Thompson (1994: 13; cf. Thompson 1995a), understood *bytdwd* as a reference to a place near Dan. After dismissing the possibility of finding *dwd* or *bytdwd* in the Mesha stele (against Lemaire) and the possibility of reading [*ml*]*k bytdwd* in line 9 of the Tel Dan stele,[27] 'the possibility

26. Other contributors who seem to take this position, while not always explicitly arguing for it, include Kallai 1993; Halpern 1994; Lipiński 1994; Rainey 1994; Kottsieper 1998; Lemaire 1998; Noll 1998; Dion 1999; and Galil 2001.

27. First, Lemche and Thompson compare the alleged occurrences of 'David' in the Mesha stele, and find that 'David' in *dwdh* in the Mesha stele (line 12) 'is undoubtedly impossible' (1994: 11). Also, Lemaire's (1994a) attempt to read *bt dwd* in the Mesha stele (line 31) is rejected. Secondly, they test whether this understanding fits the Dan stele. In line 9, *bytdwd* is preceded by a …]*k* and a word divider. To suppose that this

remains', Lemche and Thompson claim, that *bytdwd* could be a place outside of Dan, a city in its vicinity, or a holy place at Dan. Therefore, they proposed that *bytdwd* could be directly compared to the *bt dwd* in Lemaire's reconstruction of the Mesha stele (line 31).

Lemche and Thomson were followed by Philip R. Davies (1994a), who had objected to reading *bytdwd* as *byt dwd*, and asked: Why can't it be a place name like Beth-*dod* (cf. Ash*dod*) or *Bethdaud*? Davies was surprised that the Editors had not discussed such questions at all, alleging, that 'House of David' was chosen primarily to establish a link to the Bible in the inscription. There are several other ways of reading דוד, Davies claimed, referring briefly to the phrase ארא‍ל דודה in the Mesha stele (line 12) and the noun *dawidum* in the Mari texts.

In a later study, Thomas L. Thompson (1995a: 61; cf. Lemche and Thompson 1994), elaborated further on the toponym question, arguing that *bytdwd* really is a place name, such as, for example, Bethel, not a personal name, because it is written as one word, and not *byt dwd* as in the Hebrew Bible. He translated *bytdwd* as 'temple of (the deity) DWD',[28] which he found support for in the Mesha stone's *dwdh*. The name *bytdwd* is there identified by Thompson as a type of geographical, or ethnic, toponym, 'which is very common in Palestine and has been found in extra-biblical and biblical texts' (1995a: 62).[29] And yet toponyms compounded with *byt* and personal names are not usual, the only known example beside *bytdwd* being *bit humri*. The name David, allegedly, 'originates as part of this place name', and the name *bytdwd* (Temple of *dwd*), like its Assyrian equivalents *Bit Humri* (House of Omri) and *Bit Amani* (House of Ammon),

> has its originating significance as a place name, with the implication that [David and Omri] were not names of historical persons, but of fictional characters, that originated as eponymic referents.

letter has to be the final *kaph* (ך) of √ *mlk* (מלך) is too simplistic to them—there are many other possibilities. The Editors supposed that the two kings of Israel and of the 'House of David' stand in parallel. 'However, this is far from the case' (1994: 12), Lemche and Thompson claim. They followed Knauf, de Pury and Römer, who argued that a compound concept, such as 'the king of the House of…', has, so far, not been attested in any Middle Eastern inscription, nor is it to be found in the Hebrew Bible with the meaning 'king of the dynasty of X'. This final …*k* could belong to an Aramaic verb such as *nsk*, which would be translated 'pour out a libation', and, which, in itself, is not an implausible alternative.

28. Thompson notes that this position was also defended by Ehud Ben Zvi in an e-mail correspondence.

29. The examples are Bit Humri, Bit Amani, Beth Israel, Beth Levi, and names constructed with divine names like Beth Shemesh and Betel—and *bytdwd*, in his opinion.

To prove that *bytdwd* cannot mean 'dynasty of David', Thompson referred to biblical texts,[30] before concluding that it would be 'very difficult to entertain "Davidic dynasty" as a translation of the *bytdwd* of our inscription' (1995a: 68). Even less does this prove the existence of an historical David, he claimed.[31]

Thompson concluded with three statements (1995a: 72). First, *bytdwd* seems to be a place name comparable to *Bit Humri* of Assyrian inscriptions, with *dwd* reflecting a divine name like *el* of Beth*el* or *šmš* of Beth *Shemesh*. Second, the David of the biblical stories is an etiological eponym, or 'eponymous founder', of the 'House of David' and the 'City of David', which are names from the Jerusalem traditions, drawing on a broad spectrum of terms related to patronage: 'City of…', 'house of…', 'Lord', 'King', 'Messiah', 'Father', 'Son', 'Servant', 'Retainer', and so on. Third, the toponym *bytdwd* 'does not refer to a historical David, but rather to the divine epithet, *dwd*, the historically known epithet of Yahweh'. David 'seems to be rather derivative of the familial associations implicit in the form of the place name *bytdwd* and its association with the monarchy in Jerusalem'.[32]

Thompson's standpoint differs from Lemche's in that he did not accept the translation 'House of David', but agreed with him (and with Cryer and Davies) in that he refuted the historicity of David, and preferred to take *bytdwd* as a toponym.

Frank R. Zindler (1995) in a letter to the Editor of *BAR*, took *bytdwd* as a toponym and not a dynastic name. 'Beth-Dod or Beth-Dawid—"Place of the Beloved"—would be a perfectly good name for a town' with an Adonis/Tammus cult, which is known in the north as '"the beloved" of Ishtar/Astarte'. Zindler also argued that, if Lemaire is right in reading *btdwd* in the Mesha text, it should be read as 'temple of Dod/Dawid'.

Guy Couturier (2001) dealt exclusively with trying to find the appropriate meaning of *bytdwd* and the reason for the lack of a word divider between *byt* and *dwd*. Couturier's article is an important milestone in the discussion of these questions. Couturier immediately announced in an English preface (2001: 72) that *bytdwd* 'is beyond doubt' a geographical name, which is widely attested in geographical names in Syro-Palestine, and that 'its usual context' is the first half of the first millennium BCE. Couturier translated *bytdwd* 'spontanément' as 'maison de David' (2001:73), a translation which to him was compelling ('s'imposer'). After extensive discussion of different aspects and uses of *bytdwd*, his conclusion was that it should be translated

30. 1 Kgs 12.16-19; 21.29; 24.9; 2 Kgs 10.11; 2 Chron. 36.16; Isa. 7.2, 13; Hos. 5.1; Amos 6.1.

31. This is argued for in particular, from 1 Sam. 20–26 (Thompson 1995a: 68-72).

32. Cf. Shanks 1997: 28-32; Thompson 1999: 203.

'House of David' and interpreted as a designation for the Kingdom of Judah. That 'House of David' is used for Judah does not surprise Couturier at all (2001: 82).

Couturier found it particularly interesting that the Assyrians designated the kingdoms of southern Syro-Palestine with *bît* composite names. The Hebrew Bible is taken as an important source for the study of geographical names with a *bêt*-element. Couturier counts a whole range of different names of this kind, either in combination with a divine name, with a personal name, with vegetables, with a geographical name, or with a garden, and so on. He referred to Judahite, Israelite, Moabite, Ammonite and Aramean geographical examples, both from biblical texts and extra-biblical sources, taking Josh. 13.13 as evidence for a mixed population in Upper Galilee on the border with the Arameans in the ninth century BCE.[33] In general, Couturier found geographical names with a *bêt*/*bît* element to be omnipresent and particularly frequent in Syria and Israel-Judah. In his opinion, *bytdwd* as a geographical name should be of no surprise to an historian or an epigrapher.

George Athas (2003: 271-81) discussed, extensively, 'The toponym "Bayt-Dawid"', which to him could be interpreted either as the name of a city (similar to 'Bethel'), or (as in the case of 'Beth-Rehob') as the name of a state (2003: 271). Since this geographical entity had a king, some sort of a state is assumed to be implied. For Athas, this could be either a state occupying a *region* known as Bayt-Dawid, or a *city* state centred on a city called Bayt-Dawid. Athas undertook a thorough analysis of the context of the term in order to identify what is actually meant by ביתדוד. Athas saw a close alliance between the king of Israel and the king of Bayt-Dawid. Not only, Athas noted, are the two monarchs mentioned successively in lines 8A and 9A, but also their lands are grouped together by the author of the text, which implies that the author of the text saw them as a single political unity, 'or, more likely', as a single terrain. Of these, the king of Israel is the more important to the ancient author. Athas did not imagine any other kings were mentioned on the stele, and concluded that 'Bayt-Dawid was an immediate neighbour to the kingdom of Israel' (2003: 272), probably with a common border. It should be looked for in the hill country south of Israel. Since no entity named Bayt-Dawid is known to us, it could either be another name for a previously known entity, or it could be a totally new entity altogether.

But what was Bayt-Dawid, actually? This question is discussed by Athas in relation to several named places in the south particularly, Beth Shemesh and Jerusalem. After a thorough discussion (2003: 272-75) based on

33. In particular, Couturier (2001: 78) referred to *Bêt Hazaël* in Amos 1.4 as a designation of Damascus. This designation is sometimes used by the Assyrians for Damascus, and could have been known by Amos. Ultimately, however, Couturier preferred to use it for the royal palace in Damascus.

archaeological as well as biblical data, Beth Shemesh is dismissed as a 'most unlikely' candidate for Bayt-Dawid. 'Jerusalem, however, fits the evidence remarkably well', Athas claimed (2003: 275), before he undertook a thorough discussion in favour of this city (2003: 275-81). The Jerusalem of around 800 BCE was 'confined to the narrow spur between the Qidron Valley and the Central (Tyropoean) Valley' (2003: 279), and was something like the capital of 'this poor insignificant and fractured region'. In an earlier period this city had been known by the name עיר דוד, 'City of David'. Since the noun עיר, 'city', is unattested in Aramaic, it is Athas's contention (2003: 280) that בית, 'house', is used as the Aramaic equivalent to the Hebrew עיר. For an interchange between these two terms in Hebrew, he referred to Josh. 19.41. In other words, Bayt-Dawid is a toponym for the city of Jerusalem, which, in reality, was a landed estate, or a city state (2003: 280), lending its name to the district immediately surrounding it, rather than to the entire region of Judah. The king of Bayt-Dawid referred to in the Dan inscription is identified as Joash ben-Ahaziah (2003: 281), who became king in 835 and was assassinated in 796 (2 Kgs 12.20).

2.3. *Arguments Against Taking* bytdwd *as a Toponym*

Allan Millard (1994) emphasized that Davies did a service by pointing out that ביתדוד could mean a variety of things other than 'House of David', and that 'Bet-dod' is another possibility. But Millard had two arguments against Davies. First, Millard noted that the ancient scribes were not consistent in their spelling of words or in their use of word dividers and gave some examples. Secondly, the final ת־ in בית is weakened in Aramaic when it was followed by a dental (*d* or *t*). This does not prove anything for or against reading ביתדוד as 'House of David'. Millard concluded that, even if the translation 'House of David' cannot be proved, Davies's proposal is no better than the consensus opinion.

Freedman and Geoghegan (1995) agreed with Davies (1994a) that the Editors should have raised 'the possibility of other readings' of *bytdwd*, noting that Biran 'gives no satisfactory alternatives and overstates their number and probability'. Freedman and Geoghegan also agreed with Davies that scholars should not be 'forcing archaeological finds into Biblical history'. At the same time, they accused Davies of being 'equally culpable by his opposite effort to suppress the significance of these finds in support of his theory that the Hebrew Bible contains little historically reliable information before the Exile' (1995:78). Davies is attacked for being inconsistent in his explanation of *bytdwd*. On the one hand, while it was important to Davies that this word is not divided into two, his presupposed used word division when reading 'house of' is perplexing. Moreover, Davies's explanation of *dwd* is considered as speculative as he thinks the Editors' explanation

is. That *bytdwd* could be a toponym is 'simply speculation', Freedman and Geoghegan argued. Their suspicion is that, even if *bytdwd* had been divided into two words, Davies would not have accepted the translation 'House of David', but would have explained it away as a toponym. Freedman and Geoghegan agreed with Rainey's charge (1994) that 'Davies doesn't seem to understand the use of word dividers'. Davies's toponym theory 'still doesn't answer the question of what bytdwd means'. In spite of his explanations, '"the House of David" is still the most likely interpretation, whether it is a dynastic name or a place-name'. No geographical location is attested with the name *bytdwd* 'in any text, tradition or inscription' (Freedman and Geoghegan 1995: 79). On the other hand, *byt-dwd* is used over 20 times in the Bible as a reference to David's dynasty, 'a fact that Davies neglects to mention'. Even if *bytdwd* were to be a toponym, that would not shake the translation 'House of David'. Freedman and Geoghegan's general conclusion is that Davies 'is not encouraging honest intellectual inquiry' (1995: 79).

Both Na'aman (1995a: 18)[34] and Schniedewind (1996: 80a) discussed proposed alternative solutions to the meaning of *bytdwd*, but did not accept the idea of taking it as a toponym or a geographical designation because no such place is known. The lack of evidence for a geographical location with this name is a decisive argument against this theory.

2.4. bytdwd *as the Temple of the God of Israel*

Several scholars have discussed whether *bytdwd* refers to the Temple of Jerusalem, implying that *dwd* could have been an epithet for Yahweh.[35]

In the event that *bytdwd* is not a toponym, Lemche and Thompson (1994; cf. Lemche 1995a) propose two remaining lines of investigation: examining, (1) what such a *dwd* might have been, and (2) the possibility that this *dwd* is none other than the David of the Hebrew Bible, 'in, however, a fabulous disguise'. It is not self-evident that *dwd* refers to the personal name of David. When vocalized דּוֹד, it could possibly refer to a 'beloved' person, perhaps a relative ('uncle'), and it could be used as a divine epithet (*HALAT*, 206-207). Lemche and Thompson did not go into the question of whether 'beloved' is an adequate interpretation in the case of *bytdwd*, specifically,[36] but stated

34. Na'aman (1995a: 18) states: 'among the many thousands of toponyms that appear in ancient Near Eastern texts and in the Bible, none has the element *dōd*. This indicates that *dōd* was (in the sense of "beloved, darling" or "paternal uncle") not a productive element in toponomy, and significantly weakens Davies's hypothesis of a toponym whose name has this element.'

35. That Davies (1994a) should be classified as taking this position, as Galil (2001: 160) contends, is a misunderstanding.

36. Lemche and Thompson refer generally to an old discussion inaugurated a hundred years ago by H. Winckler and later taken up by H. Gressmann and G. Ahlström. In this discussion, Lemche and Thompson (1994: 13) maintain that 'the weight of the

that 'the element *dwd* is not commonplace' (Lemche and Thompson 1994: 14). It is documented only twice in personal names from the Iron Age,[37] and there is no developed mythology for a divine *dwd* because Yahweh was 'the great Palestinian god of the Iron Age', dominating the theophoric names. At the same time, *dwd* 'is hardly the personal name of any god; it is rather an epithet, "the beloved"' (1994: 14).[38] Gods were, according to Lemche and Thompson, regularly defined by their function, not named,[39] and so it is difficult for them to follow the Editors in their assumption that *bytdwd* in the Hebrew Bible stories 'is so obviously to be understood as a reference to a Davidic dynasty' (1994: 14). The syllable *byt*, in this position, refers, rather, to 'the foundation of the patronage of Yahweh in Jerusalem represented by the temple (*byt*), and to the heroic David as the eponymic founder of Yahweh's patronage within Israel', which refers to 'the eternal centre of the *bêt hā'ᵉlāhîm* in Jerusalem' (1994: 14).

Niels Peter Lemche (1995a) deliberately avoided going into the *dwd*-discussion and the cultic context of the term *dāwīd*. However, on the basis of Müller (1995: 127), who argued that *dōd* refers, without question, to Yahweh, Lemche saw an opening for several interesting possibilities for new interpretations of *bytdwd*. If *dwd* refers to Yahweh as the 'beloved', Lemche maintained, then *bytdwd* could refer to the house of the 'beloved' Yahweh—and this would implicitly be an argument against taking *bytdwd* as evidence for the historical David. 'David' would then be a derivative of the nickname (*dwd*, beloved) of Yahweh, the *Schutzgott* of the dynasty (Lemche 1995a: 104). The story of David is historically unproven, he claimed, and should be taken as a historical reconstruction from a later period. Such a view itself opens up several possibilities for explaining why it was so important that Judah was ruled by a Davidic dynasty (1995a: 104). On this basis, Lemche argues that lines 8 and 9 in the Dan text are equally good evidence against the historicity of David as they are evidence for it.

Cryer (1995a) also responded to the Swiss scholars Knauf, de Pury and Römer, who had claimed that the fact that a phrase of the type 'king of the house of NN' is not attested in any Semitic language is the reason why the fragmentary expression *...k bytdwd* (line 9) could not refer to a king of a

arguments' is in favour of a god called *dwd* in ancient Palestine. They see possible references to this *dwd* in Amos 8.14; 2 Sam. 23.9, 24; Isa. 5.1; Song 6.2 and the city name Ashdod

37. Cf. Tigay 1986. They have many critical questions with respect to Tigay's position (Lemche and Thompson 1994: 14), which will not be referred to here.

38. Cf. Barstad 1999.

39. 'Only Yahweh may have had his own name', but not for a long time, since his name was substituted by other designations. The term '*dwd* is seen as a referent to Yahweh as "beloved", and *byt dwd* as a reference to his temple' (Lemche and Thompson 1994: 15).

'house of David'. For Cryer, 'a transformation of a lineage name into a place-name-plus-political-determinant', which is what he accuses Biran and Naveh of doing, would be, as Knauf, de Pury and Römer had claimed, 'une sorte de monstruosité sématique'. Cryer repeated a view he had espoused in a previous article (1994: 17) that parallelism with names like Bethel and Bethšemeš 'suggests the possibility that DWD is simply to be understood as some form of divine name or hypostatized epithet'.[40] Cryer seems to have gone a step further than his Swiss colleagues did, because they used this characterization to describe the phrase 'king of the house of NN', not explicitly the 'transformation' Cryer writes about.

2.5. bytdwd *as the House of a Local Deity*

The theory that *bytdwd* should be understood as reference to the house of a local deity was proposed by Knauf, de Pury and Römer (1994: 66-67) with reference to the Mesha stele (line 12) and in comparison with Amos 8.14.[41] This theory, however, has met with some opposition. Schniedewind (1996) discussed the proposed alternative solutions, but decided against reading it as a temple for a deity *Daud*—mainly because no deity by that name is known.

Kenneth A. Kitchen (1997) argued strongly that there is no basis at all for supposing the existence of a deity named *dwd*. There is no documentation whatsoever for such a divine name, he notes. No old king has come to our knowledge whose name contains a *dwd*-component. No *dwd*-temple has, so far, been found. There are no *dwd*-hymns, no sacrificial list for *dwd*, no *dwd*-rituals, no *dwd*-statues, no *dwd*-altars or any *dwd* cultic objects. The idea of a *dwd*-deity is, frankly, 'a modern invention by ingenious scholars from the last century'.

Walter Dietrich (1997b) also claimed that a god named Dōd probably never existed.[42]

In my view, the theory that *bytdwd* could refer to a temple, a Bet-Dōd, has lost its value. That is also the case with the analogies to the toponyms Bethel and Beth Shemesh, as claimed by the Copenhageners.[43] No problems are solved by going that way; the toponym theory is improbable. The most profound argument against this theory is that evidence for the existence of a deity, *dwd*, has not been found anywhere.

40. Cf. Lemche and Thompson 1994.

41. Cf. Knauf 1996: 10; Lehmann and Reichel 1995.

42. Barstad and Becking (1995) and Barstad (1999) confirm that there is no evidence of a deity named *dwd*.

43. See Thompson 1995a and Lemche and Thompson 1994. Against Lemche and Thompson, Dietrich argues that the toponym Ashdod is not a valid comparison for helping to solve the *dwd*-question, since it is derived from Akkadian *adadu*, 'measure', not from *dwd*; cf. Cross and Freedman 1963.

2.6. bytdwd *as an Officer's House*

Ehud Ben Zvi (1994) called for sobriety, and paid particular attention to the term *bytdwd*. The discovery of a text like this is not only exciting, Ben Zvi argued, it is also a significant and potentially important historiographic key. That is why it is so important to investigate all possible solutions to the problem of *bytdwd*. To Ben Zvi, 'the weight of the biblical evidence' supports reading *bytdwd* as 'House of David'.[44] The problem is that he could not see any epigraphic evidence for reading *bytdwd* this way. It would be possible, he argued, to see *bytdwd* as a reference to a temple of Yahweh, or to an officer's house at Dan, but he did not make a final decision for himself between these two possible solutions. As for the …*k* preceding *bytdwd*, Ben Zvi posits that 'reconstructions of this word can only be tentative, and be heavily dependent on an *already assumed* understanding of the meaning of *bytdwd*'.[45] He summed up by saying that the only other epigraphic instance of *dwd* relevant to the discussion is the Mesha text, which 'shares important traits with' the Dan text in that both point to the Northern Kingdom from an external perspective, and both describe a victory over it. The *dwd* of the Mesha text 'certainly does not mean "David, the son of Jesse"', but refers either to Yahweh, or a high ranking official. Neither of these two readings is contradicted by contextual clues in the Dan text, he claims. Dietrich (1997 b) explicitly objected to Ben Zvi's theory, claiming that 'die Idee von Ben Zvi…gehört ins Reich der Phantasie' (1994: 27 n. 53).

2.7. *An Open Question*

Among those leaving the identification of *bytdwd* open, Galil (2001: 16) has mentioned Barstad and Becking (1995) and Cryer (1994). What Barstad and Becking (1995: 10) actually said was that the solution proposed by Knauf, de Pury and Römer (1994) 'is far from convincing', but, on the other hand, that a reference to a Davidic dynasty 'provokes a comparable number of uncertainties'. Their suggestion, therefore, is 'to leave the lexeme under discussion untranslated for the time being'. As for Cryer, he did not actually say to leave the question open; rather, he argued explicitly for seeing in the term 'Betdawd', as he rendered it (1994: 16), 'the author's designation for a geographical unit which may have been equivalent to all or some part of the region we regard as Judah' (1994: 17).

2.8. *Does* bytdwd *Confirm the Historicity of King David?*

Cryer (1995a: 54) compared *bytdwd* to other Aramaic dialects, claiming that 'The most regrettable aspect' with the Editors is that they 'so singlemindedly' connected the Dan inscription to an historical David. In his opinion, it

44. Cf. 1 Kgs 12.19 and Isa. 7.2.
45. See Ben Zvi 1994: 28 n. 9.

would be appropriate to examine the Aramaic dialects for cognates to *bytdwd*, giving particular attention to two examples. First, Cryer looked to the Palmyran phrase *bt dwd*, which is translated 'house of cauldrons', an idiom for kitchen. Secondly, Cryer pointed to the 'fact' that the root *dwd* 'is seemingly attested' in Old Aramaic with the meaning 'friend',[46] which would make the translation of *bytdwd* as 'the house of the friends' 'a plausible reading'.

Niels Peter Lemche (1995a)[47] relied on his Copenhagen colleague, Cryer, for the interpretation of *bytdwd* and the lack of a word divider. Although Lemche did not follow Cryer in his refusal to read 'House of David' out of *bytdwd*, he did argue that Cryer's attempt to find another solution should be taken seriously. Even if *bytdwd* is accepted as a reference to 'House of David', Lemche argued, that is not evidence for the existence of David as a historical person. The existence of a Judahite kingdom, a 'House of David', is, allegedly, not confirmed by the mention of Israel in line 8 because lines 8 and 9 cannot be read as parallels. Lemche was not prepared to see that the author of the inscription had killed the king of Judah and the king of Israel, nor was he able to decide with any certainty whether *bytdwd* refers to a personal name or something else.

Knauf, de Pury and Römer (1994: 66) called the reconstruction [*ml*]*k bytdwd* an 'expression qui serait une sorte de monstruosité sémantique'. The problem for these three scholars, as we have seen, is the construct combination 'the king of Beth-PN'. Nadav Na'aman (1995a) documented several such constructions from both Assyrian sources and from the Bible, calling [*ml*]*k bytdwd* 'an excellent restoration', adding that *mlk* in the *status constructus* explains why *bytdwd* was written without a word divider; the phrase *mlk byt dwd* is a double *status constructus* ('king of the house of David'). 'To make the text clearer, the author wrote *bytdwd* as one word thereby avoiding the second construct state', Na'aman reasoned. That is why, he argues, the text has the similar formulae *mlk ysr'l* and *mlk bytdwd*.

Na'aman also protested against Knauf, de Pury and Römer's objection (1994: 66) that the two terms 'Israel' and 'Beth-David' do not pertain to the same category. According to Na'aman, 'one would expect the use of parallel names (Israel-Juda; Beth-Omri-Beth David' (1995a: 21). Na'aman maintains that 'The claim, however, is unfounded', arguing that the choice of names in the royal inscriptions is 'arbitrary', the scribes having selected names 'at will'. Secondly, a similar irregularity is found in the Aramaic and contemporary (ninth-century) list of kings who besieged Zakkur, where a king's names—tribal, dynastic, capital and geographico-political—are listed side by side. For Na'aman, 'Too little is known about the use of kingdom's

46. Cf. Lemche and Thompson 1994.
47. Cf. Lemche 1998b, which does not add much more to the present article.

names in Syria-Palestine in the ninth century BCE and one should avoid positing general rules about the assignments of names in local inscriptions' (1995a: 21).

William Schniedewind (1996: 80a) saw no reason to dismiss David, or the Davidic dynasty. The arguments set out by Cryer (1994) and Davies (1994) were both dismissed as not 'particularly compelling'.

Kenneth A. Kitchen (1997: 33) claimed, with a modicum of irony, that the phrase *bytdwd* clearly refers to the 'House of David', 'and not some speculation as "king of the House of *Dod", or "king of the House of *vessel(s)", or what not'. All these alternatives he called 'unlikelihoods', which 'can now be gently laid to rest, except (perhaps) in the murkier backwaters of anti-Davidic "scholarship"'.

Walter Dietrich (1997b) also saw no decisive linguistic problems in reading *byt dwd*. The lack of a word divider is no problem for Dietrich, who points to their being sufficient analogies to prove his point.[48] The reading [*ml*]*k bytdwd*, '[kin]g of the house of David', is not impossible, and no 'monstruosité sémantique', as claimed by his Swiss colleagues.[49] The term *byt* is written *plene* and looks like Hebrew. The usual Aramaic variant, *bt*, could be an accommodation to a South Palestinian pronunciation, he argued. In other words, Dietrich concluded, that there is no linguistic reason for denying a reference to King David or his 'House' in *bytdwd*.

As for the historical David, Dietrich concluded by claiming:

> the Tel Dan stele is the first extra-biblical mention of the name of David and a not unimportant support for the existence and significance of this ruler from the tenth century BCE (1997a: 141 [my translation]).

On the other hand, Finkelstein and Silberman (2001) claimed frankly that with such evidence as *bytdwd* at hand, 'the question we must, therefore, face is no longer one of David [*sic*] and Solomon's existence':

> The fact that Judah (or perhaps its capital, Jerusalem) is referred to with only a mention of its ruling house is clear evidence that the reputation of David was not a literary invention of a much later period… Thus, the house of David was known throughout the region; this clearly validates the biblical description of the figure of David becoming the founder of the dynasty of Judahite kings in Jerusalem.

George Athas (2003: 298-309) dedicated a separate chapter to 'Bayt-Dawid and the Quest for King David' in his important study. For Athas, as we have already seen, he identified 'Bayt-Dawid', as he reconstructed the name, as a toponym referring to Jerusalem. For Athas, the term should be taken as a political designation, not a dynastic name, like *Bît Humri*.

48. He referred particularly to Rendsburg 1995.
49. Cf. Knauf, de Pury and Römer 1994: 66.

Athas discusses Thompson (1995a) in some detail. In Athas's opinion, the nature of the patronage Thompson described is more complicated than Thompson assessed (Athas 2003: 300). When Thompson argued that the biblical character of David is the personification of Yahweh's patronage of Jerusalem's kings, 'this suggestion does not appreciate the complexity of patronage in antiquity', Athas claims, concluding that דוד in the label ביתדוד refers to the patriarch of a landed, aristocratic family in Jerusalem, and does not characterize 'the kings of Jerusalem as the subordinates in a relationship with their deity, but as the superiors in a relationship with their "subjects". The House of David is only seen to stand for Yahweh through the use of metonymy' (2003: 301).

To illuminate his position, Athas searched for evidence in certain key texts (2 Sam. 7.5-16; 1 Kgs 12.16-33; 14.7-11; Jer. 21.11-12; 2 Chron. 21.7; see 2003: 301-303), concluding that the texts demonstrate

> that any connection between Yahweh as patron deity and בית דוד (House of David) is purely metonymic. They are certainly not interchangeable labels and there is no reason to suggest the latter derived from the former. On the contrary, the evidence demonstrates that the term בית דוד specifies an aristocratic family as patrons of Jerusalem and the surrounding peoples. Thompson is close to the mark when he sees the term בית דוד representing a godfather, but the actual referent is the king of Jerusalem, not the deity Yahweh (2003: 303).

Is the Aramaic ביתדוד a reference to the Temple? Numerous toponyms with the ־בית element reflect a temple, but they are not exclusively connected with deities. The toponyms could be named after geographical features, Athas argues, but also after individual persons, of which he documented a number of cases (2003: 304). When it comes to the biblical figure of David 'we can be confident that the referent is not an entity in the author's own era, or a figure purely personifying Yahwistic patronage for the kings of Jerusalem… Rather, we can be confident that the referent is a historical personage and one who lived in the early Iron Age', Athas claimed (2003: 308). Yet, since Jerusalem 'was evidently a small feudal estate', David was not an imperial person, as traditionally imagined. Jerusalem, at the time it was conquered by David was actually, 'only a fortified compound with a small population' (2003: 305). For Athas, the Dan inscription does not prove, beyond doubt, the historical existence of David. The intention of Benhadad III was not to say anything about David, and so this naming 'is outside the inscription's scope' (2003: 308-309). Athas's conclusion is that while 'The Tel Dan inscription does not give us proof of an historical David…it may certainly be admitted as evidence' (2003: 309). Athas's final comment is the generalization that 'the inscription has increased the likelihood of a historical David' (2003: 317).

2.9. *Na'aman 1995a*

Nadav Na'aman (1995a)[50] is treated under a separate heading here because his article came just before the publication of Fragment B,[51] and became, somehow, a milestone in the debate, summarizing the arguments against, and in favour of the reading 'House of David', as well as and some of the other central arguments.[52] Na'aman examined critically some suggestions proposed by scholars for *bytdwd*, 'hoping to demonstrate that there is only one plausible solution for the controversial phrase' (1995a: 17). After having surveyed the work of scholars who adopted the translation of *bytdwd* as 'House of David',[53] Na'aman order the objections against this interpretation into four groups.

1. The suggestion set out by Knauf, de Pury and Römer (1994) that *dwd* could have been a local deity, and that *bytdwd* could refer to his sanctuary or a cultic object therein,[54] is rejected by Na'aman because 'no unequivocal reference to a deity named Dōd is known from the entire corpus of ancient Near Eastern texts or from the Bible' (1995a: 17), a view which is confirmed by Barstad and Becking (1995) and Barstad (1999).
2. Na'aman questioned the reading of *dwdh* in the Mesha stele as a title of Yahweh. For Na'aman, 'Honorary names and titles are well attested in different kinds of texts, but a victory stela of a foreign king is not the place to look for them' (1995a: 18).
3. Davies's suggestion (1994a) that *bytdwd* could be a place name that may be read as Beth-dōd, and, allegedly, could have been a place conquered by the author in the course of his campaign, is refuted by Na'aman because, 'among the thousands of toponyms that appear in ancient Near Eastern texts and in the Bible, none has the element dōd'. Another problem is that *dwd* is written *plene*, not as would be expected, defectively, *dd*. To Na'aman, the fact that *dwdh* (Mesha stele) and *dwd* (Dan stele) are written *plene*, 'seems to indicate that some other word than *dōd* (beloved) is intended here' (1995a: 19).

50. Na'aman 1997 and 1999 bring nothing new to the *bytdwd* question.

51. Na'aman's article was completed before the discovery of Fragments B1 and B2. Though the author was informed about the additional discoveries, 'which decisively corroborates the reading Beth-David' (see Na'aman 1995a: 22 [Addendum]), he decided to publish this article in its original form.

52. Dietrich (1997b: 25 n. 47) approved of this article calling it, 'die souveräne Replik von N Na'aman'.

53. Na'aman referred to Ahituv 1993; Lemaire 1994; Puech 1994; Margalit 1994; and Freedman and Geoghegan 1995.

54. Cf. Ben Zvi 1994: 27-29; Uehlinger 1994: 85-86.

4. Ben Zvi's suggestion (1994: 27), that *dwd* may refer to a person bearing the title *dwd* is refused since there is no evidence to support this opinion.

In short, the four alternative solutions offered for *bytdwd* 'are unconvincing or even erroneous' (1995a: 19). In particular, Na'aman notes, the eponymic/dynastic name '"Bīt-PN" is typical of many of the West Semitic kingdoms that emerged in the Fertile Crescent in the early first millennium BCE'. Since our knowledge of names of kingdoms depends mainly on Assyrian inscriptions, 'only one name is known' (Na'aman 1995a: 20) for new, peripheral West Semitic kingdoms, like Judah, Ammon, Moab and Edom. Many of the recently founded West Semitic kingdoms in the Fertile Crescent were called by *Bīt*-names. Therefore, according to Na'aman, 'one would expect that the Palestinian West-Semitic kingdoms were also called by such names'. In conclusion, Na'aman notes, 'The name Beth-David for the kingdom of Judah is exactly what one would expect in light of this usage of name attribution, and the long-standing tradition of David as founder of the dynasty' (1995a: 20).

3. *General Summary*

As documented above, the theories attached to the designation *bytdwd* are many and varied. This is the most controversial single question in the debate surrounding the Tel Dan inscription.

For some scholars a great deal is at stake at this point—and for different reasons.

To some, it is important to defend the opinion that the Hebrew Bible is a late, even a Hellenistic product, and that David is as historical as King Arthur and Gilgamesh. To them, a possible proof of the historical existence of King David would demand an extensive historical re-orientation—and perhaps loss of scholarly reputation.

To others, it may be equally as important to prove the historical existence of King David. For those scholars, the word *bytdwd* is *ein gefundenes Fressen*.

It is fair to have a theory and argue in support of, or against, it. But the arguments should be open. Hidden biases or agendas should not determine a conclusion, either for, or against, the historical David being behind the term *bytdwd*.

The discussion initiated by Lemche and Thompson (1994) on how to interpret the term *bytdwd* is essential as a supplement to the silence of the Editors on this term; the Copenhagen-based scholars asked questions that needed to be raised. However, whether these scholars have succeeded in

proving that *bytdwd* is a toponym, referring to a Yahwistic holy place at Jerusalem, or elsewhere, and disproving that the text refers to an historical Davidic dynasty, is another question. Their theory has never become the scholarly consensus.

Lemche's most original contribution to the debate is that he accepts the translation 'House of David' while maintaining that this does not say anything at all about the historicity of King David, or a Davidic kingdom. This argument has not won any supporters, as far as I can see.

Also, Cryer's investigations into other ways of translating *bytdwd* (1994, 1995a) are valuable as a supplement to the Editors' silence. Cryer is correct in his assertion that such translation work had to be done. Also, Cryer's reference to the Palmyran inscription was an important contribution to the debate, drawing attention to Ethiopic Enoch and its possible relationship to the Mesha inscription. Whether or not these contributions solve any problems in relation to the Dan inscription is another question, however. There is no scholarly consensus on Cryer's views.

The scholarly debate can be briefly summed up as follows: an important question has been whether *bytdwd* should be read as though divided into two words, to be read *byt dwd*. It is here that Rendsburg's (1995) arguments figure most significantly. As a result of his short but well-documented contribution, the arguments against reading *byt dwd* as two words have been significantly weakened. Scribal ambivalence towards word division is now well known. Today, reading *byt dwd* seems almost generally accepted, with only a few exceptions. Most scholars contributing to this debate have seen this reading as confirming the existence of David as an historical figure, while 'House of David' is understood as either a reference to the Davidic dynasty or, to the kingdom of Judah, parallel to the kingdom of Israel, which is also mentioned in the text. With the possible exception of the Mesha stele, this is the most ancient extra-biblical reference to King David. Having said this, the interpretation of *bytdwd* is still a matter of controversy, as this chapter has hopefully documented. This situation relates both to *byt* and *dwd* and to *bytdwd*, which can be interpreted in different ways, philologically.

As has been noted, while some scholars take *bytdwd* as a toponym, others take it as a geographical designation. In truth, the difference is not vast. Although no place with the name *bytdwd* is otherwise attested, taken as a designation for Judah, it is of course a geographical designation, as Na'aman has documented.

Gershon Galil (2001: 16) summarized five different interpretations of *bytdwd*, noting that it functions either as:

1. the dynastic name of the Kingdom of Judah;[55]
2. a temple of Israel's God, Yahweh, known as *dwd*;[56]
3. a 'house' of the god *dwd*, a local Israelite deity;[57]
4. a toponym, the name of a place conquered by the author of the stele;[58]
5. the name of the home of a distinguished Israelite official in the city of Dan.[59]

Galil's summary also refers to two additional aspects of this part of the debate (which one might choose to number consecutively from the points just listed). Galil refers:

6. to scholars who leave the issue open,[60] and
7. to the reconstruction of the word before *bytdwd*, which has been reconstructed as [*ml*]*k*, [*wyh*]*k*[61] and [*ns*]*k*.[62]

In summary, there is no general consensus on the interpretation of *bytdwd*. 'It is obvious that this conundrum has yet to be resolved', Athas (2003: 218) claims.

And yet some issues have been clarified: the word divider question seems to be resolved. That *bytdwd* could refer to a sanctuary or shrine for a deity named *dwd* has not been proved decisively and is refuted by most scholars. That *bytdwd* could be a toponym has not held water. The fact that *bytdwd* is mentioned along with 'King of Israel' indicates that it refers to a political entity, named after a person designated by the term *dwd*, which, actually, is the root of the personal name, David. Such arguments support the idea that *bytdwd* refers, in some way, to a person named *dwd*, 'David', who gives his name to a *byt*, 'house'. Who was this 'David' who gave his name to this 'house'? Archaeology knows no such 'David', but the Deuteronomistic Historian of the Bible knows a 'King David', the dynastic founder of Jerusalem, capital of Judah, twin state with Israel, a major figure in biblical historiography and poetry.

55. See, e.g., Biran and Naveh 1993: 96; Kallai 1993; Halpern 1994; Lipiński 1994: 95; Rainey 1994; Na'aman 1995a: 19-20; Sasson 1995: 22; Schniedewind 1996: 80-81; Kottsieper 1998: 484; Lemaire 1998; Noll 1998: 8; Dion 1999: 151-54.

56. See, e.g., Davies 1994a: 55.

57. See, e.g., Knauf, de Pury and Römer 1994: 65-66; Lehmann and Reichel 1995: 29-31; Knauf 1996: 10.

58. See, e.g., Davies 1994a, 1994b.

59. See, e.g., Ben Zvi 1994: 27-28, as a possibility.

60. See, e.g., Cryer 1994: 16-19; Barstad and Becking 1995: 10.

61. See Dion 1999: 148-49.

62. See, e.g., Dijkstra 1994: 12 n. 17; Knauf, de Pury and Römer 1994: 66; Uehlinger 1994: 86 n. 4.

After all this debate, it seems probable that *bytdwd* should be read as *byt dwd*, and that it should refer to 'House of David' as a dynastic designation with reference to the Southern Kingdom of Judah, though as a somewhat 'incongruent' correspondent to 'Israel'. That Judah is referred to as *bytdwd* is not all too remarkable, considering the prestige of the Davidic dynasty in Judah (cf. 'House of Omri' as a designation for the northern kingdom of Israel). This argument finds some support if we are correct in reading '[Jeho]ram son of [Ahab] and [Ahaz]iahu son of [Jehoram]' as a reference to the kings of Israel and Judah in Fragment B2. There might well be other solutions—as the present summary of the debate has shown—but no other proposal seems to me to be better than this one. According, this interpretation is followed by most scholars engaged in the debate.

Chapter 7

HISTORY OF RELIGION

The history of religion has not been one of the most important questions in the debate surrounding the Tel Dan inscription—it has only been touched upon when the terms *hdd* and *dwd* were debated. Yet, there are, nevertheless, enough indications to justify a discussion of the inscription's relationship to questions of a religious character. This investigation will inevitably and especially overlap with the chapter *The Interpretation of bytdwd*. The key terms for this investigation are גזר, הדד and ביתדוד, and, possibly, the idea of 'going to the ancestors'.

1. *Evidence from the Inscription*

The Aramean term גזר (line 1) is equivalent to the Hebrew כרת, which is part of the common biblical phrase כרת ברית, used for making a covenant or a treaty.[1] This connotation is important, but precarious. The semantic relationship between גזר and כרת is clear, but what is not clear is whether גזר refers to a ברית, or an Aramaic equivalent, for example, גזר עדות. A covenant, or treaty, could be of a religious or a profane character, as is well known from the Hebrew Bible, regulating a relationship between private individuals as well as between kings, king and people and king/people and God, Yahweh. What function we should ascribe to a possible גזר עדות in the case of the Dan inscription, is impossible to say, since the text evidently tells of both 'international' affairs and religious affairs.

As for הדד, this could be the name of an Aramean king as well as the name of the Aramean god, Hadad. In this inscription, Hadad is mentioned a couple of times. The originator claims that Hadad has made him king, and that Hadad went in front of him, apparently to war. In these cases, the natural understanding would be that it refers to the deity, Hadad, the Aramean equivalent of the Canaanite Baal. Hadad was the storm god who was

1. Cf., e.g., F. Stolz in *THAT* I: 858 and M. Weinfeld in *TDOT*: II, 253-79. See also Kutsch 1973.

particularly popular among the Arameans east of Jordan. Hadad was probably his proper name, while בעל (*ba'al*), 'lord', was his title.[2]

Another important religious aspect in the inscription is the presence of the theophoric elements found in the names of the two kings (lines 7B and 8B), elements which witness to the name of Yahweh. If this is a correct reconstruction, it brings an important tension into the text; the king of the Arameans proceeded in the name of Hadad, while the Israelite and Judean kings, supposedly, proceeded in the name of Yahweh (cf., e.g., Deut. 20.4 and 2 Chron. 20.17).

Whether the term ביתדוד refers to a deity, דוד, and his house, has been intensely debated, but it has not been possible to confirm, as we have seen in the chapter titled 'The *bytdwd* Question'.

The idea of 'going to the fathers' is a euphemism for dying that is well known from the Hebrew Bible. Yet, in itself, it does not necessarily have religious connotations beyond those related to death itself.

2. *The Debate*

In comparison with the debate surrounding the *bytdwd* question, the issue of the inscription's religious connotations has been far less extensive. At the same time, some other, significant, questions have been discussed.

E.A. Knauf, A. de Pury and T. Römer (1994: 65-69) discussed the *bytdwd* question at length, including questions related to the history of religion. One question is whether *bytdwd* could refer to some kind of building or cultic object. The word *bytdwd* is related to בית אלהים, 'house of god', in Judg. 17.5. If *bytdwd* designates a cult object venerated at Dan, the preceding…*k* would not be *wa-'akk*, but *wa-ássuk*, 'and I poured' or 'and I anointed' (1994: 67), a formula, they argued, very similar to *wayyassek* in Gen. 35.14. As a corollary, the object of the following verb, *śym*, could possibly be the inscription itself, or rather, the base of the stele.

The Swiss scholars were surprised that the Editors did not propose any alternative readings of *bytdwd*, since *dwd* could also be vocalized *dōd* and could, allegedly, designate a local god, a village patron.[3] As a particularly important item of evidence they referred to the bilingual Greek–Aramaic inscription found at Tel Dan bearing the inscription 'To the god who is in Dan'.[4]

2. On the god Hadad, see, e.g., W.A. Maier in *ABD*: III, 11, and J.C. Greenfield in *DDD*: 377-82.

3. Cf., among others, Röllig's comment on *dwdh* in the Mesha text (line 12). For Röllig, when the feminine suffix is removed, 'דוד muß dann eine Gottheit oder etwas Vergleichbares sein…' (*KAI*: II, 175). Cf. the possible reading דדן in Amos 8:14. As we will see, this opinion is questioned and scarcely acceptable; cf. Barstad 1999.

4. See the picture and bibliography in *ABD*: II, 17. Cf. Uehlinger 1994 and Noll 1998.

On the other hand, Mindert Dijkstra (1994: 10) opposed the alternative *BaytDōd**, and called it 'hardly convincing', because 'it shows clearly enough what limited information can be procured from such a damaged inscription'.

Christoph Uehlinger (1994: 85;[5] cf. Noll 1998) discussed the idea as to whether *bytdwd*, read as **baytDōd*, 'Haus des *dōd*', might possibly refer to a cultic statue in *Eine anthropomorphe Kultstatue des Gottes von Dan?*[6] His question, in the context of Knauf, de Pury and Römer's interpretation of Amos 8:14, was, 'ob sich das "Image" des Gottes von Dan in eine bestimmte Richtung konkretisiert läßt' ('whether the "image" of the god of Dan allowed itself to be made concrete in a specific way'). Two terracotta fragments and a number of faience figurine fragments had been found at Tel Dan, items which could have been part of an anthropomorphic cultic statue.[7] One issue is whether these finds could be linked. Such figurines are well attested in the biblical literature,[8] but, Uehlinger notes, are rarely unearthed at digs (1994: 88). After examining six concrete statue fragments (1994: 88-95) from Tel Dan, Uehlinger concluded that three of them constitute interesting evidence for the influence of an Egyptian cult in northern Palestine and the Phoenician area at the time of the 22nd Dynasty (1994: 95). One of the fragments Uehlinger examined, he thought might represent a king as a deity (1994: 96). This particular statue was bigger than the others. It was found with cultic objects with iconographic similarities to other deities. On the question of who this god of Dan was, Uehlinger took as a point of departure the fact that Tel Dan is situated at the source of the Jordan. Could this god of Dan have been a 'well god', *Quellgott*, who has made use of the regeneration symbolism of Bastet, Nefertiti and Osiris? To identify him by name, Uehlinger concluded is precarious on the basis of our present knowledge, even if an identification with the weather god of Mount Hermon, that is, Hadad, seemed to him to fit conveniently. Nor would Uehlinger speculate over any connection to the פסל of Micah (Judg. 17–18), or the Golden Calf of Jeroboam I.[9]

5. Uehlinger (1994) joined his fellow Swiss colleagues Knauf, de Pury and Römer (1994) in *BN* 72, and commented on whether *bytdwd* should be read as **baytDōd*, 'Haus des *dōd*', with reference to a cultic statue of a deity *Dod*.

6. Uehlinger 1994: 85-86: 'Trifft diese Deutung das Richtige…dann wäre hiermit der bislang älteste inschriftliche Beleg für das lokale Heiligtum von Dan…gewonnen'.

7. Tel Dan is highlighted (Uehlinger 1994: 87) as, so far, being the only Iron Age II place in Palestine, where (1) a *dōd*, or a *baytdōd*, could be documented, (2) a local אלהים is witnessed biblically (cf. Amos 8:14), and (3) a plethora of fragments of Iron Age cultic statues has been found.

8. See 1 Sam. 5.1-5 (Dagon in Gaza) and 2 Sam. 12.30 (Milkom in Rabbat-Ammon).

9. Cf. 1 Kgs 12.28-30; 2 Kgs 10.29; and 2 Chron. 13.8.

Uehlinger's article is, along with Noll's 1998 study, the most concrete attempt to associate a possible deity in the Dan inscription with a known figure from Dan. Yet Uehlinger was not able to make a definite identification, and, apart from Noll, no other scholar has tried to follow up his question related to this inscription.

Reinhard G. Lehmann and Marcus Reichel (1995) followed Knauf, de Pury and Römer in reading *bytdwd* as *BaytDōd*. For Lehmann and Reichel, since *bytdwd* has no word divider, a better solution would be to take *bytdwd* as a divine epithet, or a designation for a sanctuary, rather than as a dynastic name, finding support in their interpretation of Amos 8.14. They also found traces of a deity with the name of Ashima/Asima in the name of Ashima in 2 Kgs 17.30 and in Egyptian–Aramean personal names, and argued:

> [W]enn also Ashima, ein Gott von Dan und ein Dod von Beersheba gemeinsam in einen Text genannt werden, dann liegt es nahe, im letzten Wort von Zeile 9 des Tell Dan-Texts (*w'śm*) nicht eine Verbform, sondern ebenfalls die Gottheit Ashima zusammen mit Dod erwähnt zu finden (1995: 31) ('so if Ashima, a god of Dan, and a Dod of Beersheva are named together in a text, it is likely that the last word in line 9 of the Tel Dan inscription [*w'sim*], is not a verb form, but rather, also, the deity Ashima mentioned alongside Dod').

On this basis, and with support from Knauf, de Pury and Römer, Lehmann and Reichel reconstructed and translated line 9 as follows: '…und ich goß Libation] aus für/über BaytDod und Ashim…' ('and I poured out a libation] for/over BaytDod and Ashim'). For the identity of *'śm*, they relied on Silberman (1969), who identified the elements *'śm* and *hrm* in Egyptian–Aramean names as theophoric elements, and Wansbrough (1987: 103-16), who had found a similar situation in Ugaritic. Lehmann and Reichel drew parallels between *dwd* in the Mesha stele and the Dan inscription's *bytdwd* (1995: 31).[10] With hindsight, we can see that their theory has not taken hold.

Victor Sasson (1995: 18) saw, in the missing part of line 4, a possible prayer to Hadad by the text's originator, resulting in Hadad's alleged guidance and leadership. He saw the significance of Dan more in its 'cultic role as a major Israelite shrine' with its golden calf (2 Kgs 10.29) than as a military focal point (1995: 27). Placing an Aramean commemorative stele at Dan 'amounted to desecrating or negating the site, humiliating both the local deity and the king of Israel, by proclaiming that Hadad had crushed the enemies', he claimed.

10. Lehmann and Reichel 1995: 31: 'Schließlich ist sogar die Möglichkeit zu erwähnen, daß auch hier die Reihung *bytdwd. w'śm* als analoger Fall optionaler Nebeneinanderstellung von Götternamen mit und ohne w- wie Ugaritisch Ktr wHss bzw. Ktr Hss und also Ausdruck einer Entwicklung "from two distinct units via hendiadys to a single concept" [Wansbrough 1987: 110] zu sehen ist'.

In particular, Shigeo Yamada (1995) emphasized as 'a significant point in Hazael's claim' that his military success was sponsored by the god Hadad, 'in contrast to his predecessor's failure' (1995: 613). Here he found an apologetic aspect in the text.

Hans-Peter Müller (1995) presented some deliberations on the history of religion. Where line 5 refers to Hadad walking before the author of the text, Müller thought concretely of a stele with a symbol of a deity on it that was carried in front of the army (1995: 137). This image is analogous to what is known from Assyrian sources (and from the Hebrew Bible with reference to the Ark; cf. 1 Sam. 4.3-4 and 14.18). As for the rest of the sentence, he sees in *wyhk. hdd. qdmj* a parallel to the Hebrew participial phrase ויהוה הלֵך לפניהם,[11] 'Yahweh went in front of them'.

Hans M. Barstad and Bob Becking (1995; cf. Barstad 1999), in a joint study, discussed, in particular, the existence of a possible deity *dwd*, which had, allegedly, been identified by Knauf, de Pury and Römer (1994), Lemche and Thompson (1994), and Davies (1994b). Barstad and Becking's intention was to investigate the claim that a deity, Dod, was worshipped by the Aramean inhabitants of Dan in the ninth century. In their opinion, this was a theory that 'can not be proved or made plausible' (1995: 5).

Barstad and Becking began by reviewing the occurrences of דוד, 'beloved', 'love' or paternal 'uncle' in Hebrew and related languages. The etymology of דוד, Barstad and Becking noted, is made problematic by the connection to the personal name David. They also noted that attempts have been made to see the word as an epithet for Yahweh.[12] In Akkadian, *dadu*(*m*) is used of family members, kings and deities. In Mesopotamia, deities with names like *Dada*, *Dadu* and *Dadudu*, are known. Yet these names need not be connected to the kinship term **dad*, meaning 'paternal uncle', which is also used as the theophoric element in personal names (cf. Adad and Hadad). *Dadu*, as a theophoric element in personal names, 'is a case in point of the deification of a dead kin' (1995: 6), which is also to be seen in the use of father (אב) and brother (אח) as theophoric elements.

The question, for Barstad and Becking, is whether a deity, *Dōd*, was worshipped in ancient Palestine. Importantly, for them, the element *dd* occurs in the deity names *Dad* and *Dadat* in pre-Classical Arabic inscriptions from the mid-first millennium BCE, and in epigraphic Aramaic and Palmyrene onomastics, but not in Ugaritic, where meanwhile divine appellatives constructed from the root *ydd* are known, for instance, *mddb'l*, 'beloved of Baal'. The issue is whether this 'circumstantial, though ambiguous evidence

11. Exod. 13.21; cf. Num. 14.14; Deut. 1.30, 33; 20.4; 31.8; and Isa. 52.12. He also sees points of contact with Akkadian literature and the Zakkur and Mesha inscriptions.

12. See van Zijl 1960; Lemche 1995a.

[is] strong enough to allege the veneration of a deity Dōd in ancient Palestine' (1995: 6), and is illuminated with textual evidence from Palestine: the Mesha stele (line 12), Amos 8.14 and theophoric elements in personal names.

Barstad and Becking point out that in the Mesha inscription the phrase of interest is אראל דודה (line 12). They note that, in their commentary on the Mesha text, Donner and Röllig said that 'דוד muß dann eine Gottheit oder etwas Vergleichbares sein' ('dwd must, then, be a deity, or something similar', *KAI*: II, 175), a position that Barstad and Becking rejected as 'based purely on guesswork' (1995: 7). For Barstad and Becking, it is not clear what אראל refers to, but 'most probably [it] refers to a cultic item'. They concluded, with Jackson (1989: 96-130), that 'it is safe to say that an exact understanding of these words are [*sic*] still a mystery'. Barstad and Becking themselves added that the Mesha inscription does not offer clear evidence that a deity named Dōd was ever worshiped in Transjordan.[13]

As for Amos 8.14, the key point, for Barstad and Becking, is how to understand וחי דרך באר־שבע, a phrase which has frustrated translators from antiquity. Their conclusion was that 'we are in general hardly allowed to say anything very definite about the mysterious דרך of Am 8:14, the phrase in this text does not refer to the veneration of a deity Dōd' (1995: 7).

Next, Barstad and Becking investigated theophoric elements in personal names, immediately pointing out that biblical names composed with דוד 'are problematic' (1995: 9), and that they do not document any deity *dwd*.

Barstad and Becking note that Lemche and Thompson (1994) found a *dwd*-element in the name of the Philistine city Ashdod (אשדוד). However, since they did not provide a full analysis of the name אשדוד, Barstad and Becking concluded that 'their view is not open for debate since we do not know how to construe the name in its entirety', adding that 'even if dwd should appear in theophoric names which might be read as "Friend/Beloved of Yahweh", or "Yahweh is a friend", or anything similar, this does not imply that the word necessarily must function as a divine epithet' (1995: 9). According to Barstad and Becking, it is 'methodologically unsound' to classify all word elements appearing in theophoric names as epithets of deities, Barstad and Becking concluded (1995: 9). Names, they note, are constructed as sentences, and not all predicates are automatically epithets.

From this analysis of personal names Barstad and Becking concluded that 'even if the occurrence of דד/דוד in names appears to have been widespread in the ancient Near East, there is little evidence to support the existence of a deity Dod' (1995: 10), admitting that the noun דוד occurs as 'an epithet or a metaphor for God' in the 'Song of the Vineyard' (Isa. 5.1-7) and in the Israelite personal names דודי (1 Kgs 27.4) and דודו (Judg. 10.1; 2 Sam.

13. Smelik (1992: 65) translates אראל דודה as 'the fire-hearth of his *uncle*' (Smelik's italics).

23.9, 24; and 1 Chron. 11.12, 26). In the allegorical interpretation of the Song of Songs, דוד ('beloved'), is used for God and in later Christian tradition, for Christ. Nevertheless, the consensus today is that these texts are erotic poetry, and that דוד is used for the 'darling lover par excellence…and no mythology should be read into this text. The term does not refer to Yahweh or any other god'.[14]

Barstad and Becking ended their discussion by claiming: '[T]here is no evidence in Iron Age texts from Palestine and from the Old Testament supporting the existence or worship of a deity דוד' (1995: 10).

Yet, for all this, Barstad and Becking did not exclude the possibility that *dwd* 'may have been used as an appellative or epithet of deities in ancient Israel, but the evidence is far from conclusive' (1995: 10). Therefore, they find the interpretation of this point in the Dan text by Knauf, de Pury and Römer (1994) as 'far from convincing' (1995: 10). Barstad (1999) concluded similarly.

Barstad (1999; cf. Barstad and Becking 1995), examined Dod (דוד) lexically in *DDD*, claiming (1999: 260a) that, 'On the whole, the ancient Near Eastern material [on *dwd*] apparently raises more problems than it solves'. Furthermore:

> From the above we may conclude that even if the occurrence of *dwd*/*dd* in names appears to have been widespread in the ancient Near East, there is little evidence to support the existence of a deity Dod. Also, there is no evidence in the Hebrew Bible supporting the existence or worship of a deity *dwd*. The word *dwd* may have been used as an appellative or epithet of deities in ancient Israel, including Yahweh, but the evidence is far from conclusive (1999: 261 A–B).

This conclusion agrees with that of Barstad and Becking (1995).

In 1998, Kurt L. Noll published jis 'The God Who is among the Danites'. Along with Uehlinger (1994), Noll is the only scholar to have written specifically on the history of religion question in relation to the Dan inscription. The question has also been touched on by other contributors, but not as a theme in its own right, as is the case with Uehlinger and Noll.

Noll's point of departure was the discovery of an inscription at Tel Dan in 1976, an inscription bearing a dedication to 'the god who is among the Danites'.[15] Noll's main focus was not the phrase ביתדוד, though he found it necessary to 'clarify my position on certain basic issues' in his discussion on the question as to who the god at Dan actually was. Noll therefore had to make up his mind as to the meaning of ביתדוד. In a later study, Noll remarks laconically that 'this reading has not convinced many' (1998: 5).

14. This claim is levelled against Lemche and Thompson (1994) who refer to the allegorical tradition (that *dwd* was a deity) as an argument in their favour.

15. Biran 1976: 202-206.

Noll's intention was, based on Biran (1994b), to prove that the god of the Danites in the Pre-Assyrian Iron Age was Hadad, and to find, in the worship of Hadad, a state religion. After surveying the evidence, Noll concluded: 'It is only with the greatest difficulty' that the content of the Dan text can be related to 'the material cultural finds of the tell' (1998: 19). The question for Noll was: Which 'patron-god' was worshiped at Dan in Pre-Assyrian Iron Age? His argument was that 'this Iron Age city [Dan] was ruled by Aramean speaking dynastic kings who worshipped Hadad as their patron god. Although the city may have been captured by Israelites in a few periods, the likelihood is that relative continuity in Aramean rule and Hadad religion persevered at Tel Dan throughout the pre-Assyrian Iron Age' (1998: 23).

This key issue, Noll argued, is related to 1 Kings 15, a chapter which he assumed had preserved a reliable memory of an Aramean takeover of Dan early in the ninth century. For Noll, it is not unlikely that this would imply building activity in the area, not least that a royal memorial stele was erected. This was what Mesha and Zakkur did. 'The voice which speaks from the stone' (1998: 19) must have been Benhadad/Barhadad (1 Kgs 15) or his heir, Noll contended.

George Athas (2003: 268) claimed that the originator of the text, whom he identified as Benhadad II (III), mentions his deceased father because of 'a kind of ancestor cult enacted at various shrines belonging to the deity El-Baytel'. This is a claim without particular basis in the inscription itself, and is at best deducible from general practice in the Levant. When investigating the inscription's cultic implications (2003: 309-15), Athas found that 'it alerts to certain cultic developments' (2003: 309) occurring in Syria-Palestine at that time, which had to do with the nature of the cult of El and that of *mazzebot*, or sacred stones. As for the cult of El, he referred to his reconstruction and identification of the deity El-Baytel in line 4A (2003: 309), which to him appeared to be connected to the later deity Bethel, in which he saw an evolution of the more archaic deity, El. Further, he saw 'good reason to suggest a connection' between this deity and sacred stones, *mazzebot*, venerated at the cult place, since this was general practice at cult places, as is documented in the Bible as well as in other historical sources. Such *mazzebot* were actually found at Tel Dan.

3. *General Summary*

If גזר refers to a covenant, there may be religious overtones. But this has not been an important question in the debate.

The possible religious connotations in 'going to the fathers' has scarcely been discussed at all.

The attempts to identify any known cultic figure from Tel Dan with the Tel Dan inscription have failed, as have the attempts to find a deity, *dwd*.

Barstad and Becking have concluded, after a thorough investigation, that there is no support for seeing a deity in the term *dwd*. The fact is that there is no safe identification of a deity *dwd* in the ancient Middle East. Such an interpretation of *bytdwd* is an *argumentum ex silentio*, which is generally regarded as a precarious and flimsy way to argue. In conclusion, there is no decisive reason for seeing a deity *dwd* in *bytdwd*.

More promising are the attempts to find the deity Hadad in the text. In particular, Müller has underlined the though that the god Hadad went before the army as a parallel to the biblical idea that Yahweh marched before the Israelite army. The theological idea of a god marching before a people's army is well known from the Bible as well as from other sources. Whether this is the idea in the Dan inscription is still a question open to debate.

In his major study of the inscription, Athas refers in general to an El cult at Dan. But El is not actually mentioned in the text.

All in all, the conclusion has to be that the Tel Dan inscription reflects cultic or religious connotations, but it is not a religious text proper. This is a secular, military and historical text.

Chapter 8

The Inscription's Relationship to the Hebrew Bible

Is there any relationship between the Dan inscription and the Hebrew Bible? Should we make such a comparison at all? Some scholars are extremely reluctant to be drawn on such matters. Others are more confident, and some go rather a long way in relating the inscription to the Hebrew Bible. To this author, it seems evident that there are links, and that these links should be investigated. Yet it has to be said that there are different kinds of link, both indirect and direct. This has, actually, been a very important part of the debate, as we have already seen. But how are these links to be described or evaluated? First, we will look at the indirect links, in order to establish a general framework, and then we will look at the more direct links, before examining the debate in more detail.

1. *Indirect Links to the Hebrew Bible*

One indirect link is the geography. The stele was found at Tel Dan, an important site as far as links to biblical Israel are concerned. Beersheba marked the southern end of the country, while Dan marked the northern end of it, 'from Dan to Beersheba' (Judg. 20.1 and 1 Sam. 3.20). Dan was also the place where king Jeroboam I established a cult with a Golden Calf, in addition to the one at Bethel. Dan and Bethel marked the limits of the northern kingdom of Israel.

Another indirect link is the history Israel and Judah had in common with the Arameans. As neighbouring peoples, they had common borders and maintained different kinds of exchange. Dan was not far from Samaria and Damascus. There were ancient road links between the areas. Isaiah 8.23 mentions Gentiles living in Galilee; possibly some of these were Arameans. Although they fought several wars, particularly in the ninth and the early eighth century, these neighbouring lands also enjoyed commercial and cultural exchanges.

Another indirect connection was language; linguistically speaking, Aramaic is closely related to Hebrew, a language the Hebrews themselves later

adapted, and the language found its way into the biblical text in parts of the books of Daniel and Ezra. The palaeo-Hebrew alphabet, used in the inscription, was also used by the ancient Hebrews.

The inscription describes a period when the Arameans and Israel and Judah were at war, and the Arameans had the upper hand.

2. *Direct Links to the Hebrew Bible*

There are several direct links between the Tel Dan inscription and 2 Kings in terms of terminology and historical information.

The proper name הדד is mentioned twice (lines 4 and 6). The name Hadad occurs several times in the biblical texts as part of the personal name Benhadad. Hadad was the Aramean name of their national god, identified as Canaanite Ba'al. Benhadad, or, in Aramaic, Barhadad, 'son of Hadad', was the name of at least three Aramean kings. In the case of the Dan inscription, הדד refers to the god Hadad.

There is high degree of consensus among scholars that lines 7-8 refer to two named kings of Israel and Judah, respectively (as reconstructed) [יהו]רם בר[אהאב] and [אחז]יהו בר[יהורם], '[Jeho]ram son of [Ahab]' and '[Ahaz]iahu son of [Jehoram]', contemporary kings of Israel and Judah from around 845 BCE. This is the most securely datable reference in the inscription and it is therefore of a major importance of historical information.

The name of ישראל in line 8 refers to the kingdom of Israel, including its king, מלך ישראל. This reference is also of significant historical value, as it links, indisputably, the originator of the text with the kingdom of Israel. As this is an indisputable part of the text, this is, perhaps, the most important Bible-related part of it.

The much-debated expression ביתדוד (line 9), is, probably, to be read בית דוד, referring to the 'House of David', understood either as a designation for his dynasty, or as the dynastic name of the Davidic kingdom of Judah. Accordingly, this is also of great historical importance, even though the actual meaning of the expression is disputed.

In the combination of the names of the two kings and the mention of מלך ישראל and ביתדוד, we have four strong pieces of evidence directly related to 2 Kings. Four combined pieces of evidence say more than four individual bits of data; taken together, they send us a strong message about why the stele and its inscription were erected. Its originator is bragging that he has fought against, and conquered, two named kings of the kingdoms of Israel and Judah.

There might be a further linguistic link, amounting to an indirect relationship, in the number of 'seventy kings', if the Editors are right in their reconstruction (Biran and Naveh 1995: line 6). According to 2 Kgs 10.1,

King Ahab had 'seventy sons' in Samaria. In this phrase, we have the number 70, as well as the name of King Ahab and the mention of Samaria, the capital city of the northern kingdom.

The primary part of the Deuteronomistic History to be seen as the biblical counterpart of this inscription is 2 Kings 8. The two texts do not correspond to each other in all aspects; they are not direct variants of the same story, but they are variants in the sense that both tell of the struggle between the Arameans and Israel and Judah. 'The most plausible way of accounting for the differences between the two stories is to suppose that neither was written from a purely objective point of view, and that each left out events that were not to its credit'.[1]

3. *The Debate*

In the following discussion I will group the positions regarding the Tel Dan inscription into two divisions, employing the labels frequently used in this debate: 'minimalist' and 'maximalist'. This is, of course, very simplistic, and will probably be objected to by some. Not many scholars would classify themselves as real 'minimalists', even though they might agree with the 'minimalist' opinion on some issues. Perhaps even fewer would call themselves 'maximalists', as this designation could associate the position holder with a Biblicist, or even fundamentalist, perspective on the subject. Nevertheless, in spite of the difficulties, the designations are widely used. Even though the terms are somewhat of a broad-brush classification, they signify that there is a difference in attitude to the Bible as an historical source and the relationship of this inscription to the Bible. In the following I will not argue, in each case, why one scholar is classified as 'minimalist' while another is classified as 'maximalist'. Instead, I will note that 'minimalist' here refers to a person reluctant to use the Bible as an historical source, while 'maximalist' refers to a person who is more confident in using the Bible as an historical source, even if in a critical way.

3.1. *'Minimalist' Positions*

Niels Peter Lemche, Thomas L. Thompson and Friedrick H. Cryer, along with Philip R. Davies, who are gradually consolidating as a united front, sometimes referred to as the 'Copenhagen School', or the 'Copenhagen–Sheffield Axis', have presented opinions frequently referred to as 'minimalistic'. These scholars are regularly utterly sceptical of seeing any relationship between the Tel Dan inscription and the Bible and to reading any substantial historical information out of the Bible.

1. Emerton (2002: 491) on the relationship between the text of the Mesha stele and 2 Kgs 3 (cf. p. 490).

The second half of Niels Peter Lemche's study (1995a: 104-108) concerns the Hebrew Bible as an historical source. Initially, it is a discussion with Siegfried Herrmann (later, p. 107, also with Henning Graf Reventlow and Horst Sebass) about Hermann's critique of Lemche himself and of Frederick H. Cryer, arguing that Herrmann is critical of the epistemological basis for the paradigm shift in the history of Israel research, and secondly, that Herrmann had distorted the argument of his opponents. Lemche himself amplified his arguments with respect to two points (1995a: 105): the evidence from archaeology and the biblical testimony. It is not considered impossible *a priori* that these two categories of information could agree, Lemche argued. If they disagree, it is almost impossible to know which information is the more important. Yet, if the concern is from, for example, the time around 1200 BCE, Lemche would prefer to make use of documents from that period, rather than from the third century BCE (the period to which he dates much of the Hebrew Bible).[2] At the same time, contemporary sources could also be biased in favour of particular tendencies and ideologies. It is the task of the historian to penetrate these biases. As for the time around 850 BCE, Lemche argues that the situation is nearly opaque, as the biblical and Assyrian sources have no common denominator, leaving a problematic situation about which different historians have different interpretations.

Frederick H. Cryer (1996; cf. 1994, 1995a and 1995b) proposed that it should be axiomatic that the biblical text and archaeological finds are not compared (1996: 3). Historical sources should be evaluated independently from each other and then compared individually. Cryer accused the Editors of having 'grossly neglected' this principle.

In his later study, Niels Peter Lemche (1998b: 42) claimed that using the Bible to create a missing link between the 'Israel' of Merneptah and the Dan inscription is a 'false procedure', one that 'represents a premature blending of primary and secondary sources which may…prevent any type of evidence…from being properly utilized'.

Ernst Axel Knauf was positive, in principle, with respect to using the Hebrew Bible as an historical source (cf. how he uses the Bible in Knauf 2000, and especially 2001). Knauf, however, warns that this 'dialogue between archaeologists and biblicists is constantly endangered by a communicative breakdown… One reason for the mutual misunderstanding seems to be the fact that various people mean quite different things by the terms "Bible", "history", and "archaeology"' (2001: 262). This is the problem he discusses throughout this article. As for the historical value of

2. Lemche (1995a: 106): 'Doch darf hinzugefügt werden, daß es gewiß nicht sicher ist, daß eine Analyse, die auf Daten aus des 13. Jahrhunderts v. Chr. basiert, unbedingt die beste ist'.

the Tel Dan inscription, his articles generally mirror that he regards it to be an important historical source, but is somehow confused about its relation to the Bible.

3.2. *'Maximalist' Positions*

While Victor Sasson (1995: 29) used the biblical literature as an historical source, he pointed out that the book of Kings, which describes the period to which the inscription refers, 'does not offer much help' in clearing up the great historical problems. According to Sasson, the devastating war against Israel and Judah, described in the inscription, is not mentioned at all in the book of Kings. The book of Kings 'is not a modern history book', out to record historical, national and international events objectively, but rather a book that seeks to show the hand of God in certain historical events. And yet, in principle, he is not unwilling to use the Hebrew Bible as an historical source.

In contrast to the 'new archaeologist axiom' referred to by Cryer (1996), Schniedewind (1996: 82-86) made prolific use of the biblical traditions in relation to his discussion of the Tel Dan inscription, letting them speak for themselves in their context, and letting the inscription speak for itself, before he confronted the discrepancy between them on the question of who killed the Israelite and Judahite kings.

In contrast to Davies (1994a) and 'die sog. Kopenhagener Schule', Walter Dietrich (1997b) accepted that the Hebrew Bible is still useful as an historical source, and, therefore, recognized its importance in relating the Dan inscription to the Bible. In his opinion, the Tel Dan inscription should be read in the light of the Bible, signalling the importance of reading the Bible alongside the archaeological finds—when possible. According to Dietrich, 'Die Bibel kann immer noch als Geschichtsquelle dienen—vorausgesetzt, man geht ihr behutsam und selbstkritisch, methodisch kontrolliert und differenziert um' ('The Bible can still be used as an historical source—under the precondition that it is used with care and critically, and in a methodologically rigorous and differentiated way').[3]

If this is a reliable conclusion,

> dann ist die Stele von Tel Dan die erste außerbiblische Erwähnung des Namens Davids und damit eine nicht unwesentliche Stütze für die Existenz und die Bedeutung dieser Herrschaftspersönlichkeit des 10. vorchristliche Jahrhunderts ('then the Tel Dan stele is the oldest extrabiblical mention of the name of David. It is a not insignificant piece of evidence for the existence and importance of this ruling personality from the 10th century BCE') (1997a: 141).

3. Es spricht eigentlich nichts dagegen, umgekehrt aber sehr viel dafür, daß der Mann, den die Bibel ‚Dawid' nennt, eben der Dynastie- und Staatsgründer gewesen ist, auf den der Ausdrück *bytdwd* in der aramäischen Stele von Tel-Dan Bezug nimmt.

These are his last words on this matter.[4]

In his final *Ausblick*, Ingo Kottsieper (1998: 496-97) advised caution. The text is fragmentary and, Kottsieper noted, every reconstruction, including his own, should be used cautiously and recognized as preliminary. Nevertheless, in his opinion, the Dan text reinforces the value of the Hebrew Bible as a source for the exilic times, with the critical reservation that these texts are not primary sources.

Kurt L. Noll (1998; cf. 2001), underlined that we have two main sources of knowledge of Tel Dan in the pre-Assyrian Iron Age, namely, the Tel Dan inscription and the biblical text, which are, respectively, primary and secondary sources.

Paul E. Dion (1999: 154) concluded his article claiming that the Dan inscription 'seems to be in basic agreement with the biblical narrative of the events which attended the end of the dynasty of Omri in Israel'.

Previously, Jan-Wim Wesselius (1999, 2001) had argued eagerly for seeing Jehu as the originator of the Tel Dan inscription. This position builds on a basic confidence in the Hebrew Bible as a reliable source of historical knowledge. Wesselius's position was that the scant remains of the Tel Dan inscription can be reconciled, to a very great extent, with the biblical account of Jehu's revolt, even though he would not align himself with 'those who would try to prove the Bible true at all cost' (1999: 168 and 169).

Nadav Na'aman (2000) has high regard for the biblical traditions as possible historical sources, but not necessarily as better ones than contemporary sources. He, therefore, refused to harmonize epigraphic texts and biblical traditions when they disagree, as in the case of who killed the Judahite and Israelite kings, Jehu or Hazael. As Na'aman notes, the use of the prophetic stories as an historical source calls for critical analysis and great caution, especially when they conflict with the testimony of an external source, written not long after the events described [as in the case of the Dan inscription].'[5]

Bartusch's 2003 monograph, *Understanding Dan*, has just one single—and short!—reference to the Tel Dan stela (2003: 215 n. 350). He saw in this inscription a '*possible* link' (his emphasis) to 1 Kgs 15.20, King Benhadad's

4. Dietrich also wrote on 'Jehus Kampf gegen den Baal von Samaria' (2001), but merely touched on the Dan inscription there (p. 116 n. 6).

5. Na'aman 2000: 102: 'When a contemporary inscription conflicts with a biblical story that was handed down in oral tradition and recorded in writing many years after the events described, probably with significant changes, should we strive for simple, harmonising solutions?' His answer is obviously: 'No!' But: 'I do not maintain that these stories lack historical value. On the contrary, various prophetic stories about the northern kingdom evidently preserved elements of the ancient period' (2000: 103). Cf. Na'aman 1997.

conquering of the Israelite villages of 'Ijon, Dan, Abel-beth-Maachah, and all Chinnerot, with the land of Naphtali'.

George Athas (2003: 255) was clear that we must not 'dismiss the biblical evidence from having any value for historical reconstruction. Rather, we must recognize that the biblical texts are secondary sources and employ them with the appropriate contingent weight'. Athas used the Bible extensively himself, arguing that if the positions taken by Davies, Lemche and Thompson are followed, 'then the biblical account of how the toponym עיר דוד (City of David), and, therefore, its Aramaic equivalent ביתדוד (Bayt-Dawid) was derived, loses its value as a historical source' (2003: 299).

The inscription's relationship to the Bible is treated in a separate part of Athas's book (2003: 281-87). What he actually discusses is that it, allegedly, confirms the 'longheld suspicion about the biblical accounts of the wars between Israel and Aram-Damascus' (2003: 281), namely, that the account of Ahab's death in the battle against Benhadad (1 Kgs 22.29-37),[6] the story of Ahab's fall (and other narratives), have been misplaced. The contention is that these narratives do not refer to Ahab, but to the reigns of later Israelite monarchs. This he found to be confirmed by the Tel Dan inscription. The question is discussed in relation to 1 Kgs 22.1-40, 20.23-43 and 20.1-22 (Athas 2003: 283-87), an argument that will not be dealt with in detail here. What can be noted here, however, it that the notion that Jehu's coup had been backed by Hazael, as has been derived from previous interpretation of the inscription, is dismissed as making no sense in his interpretation and dating of the biblical text and the stele itself (2003: 282).

In general, Athas claims that 'the Tel Dan inscription demonstrates that there are definite historical kernels in the Bible that cannot readily be dismissed' (2003: 316-17).

4. *Conclusion*

My own position is to object to the stance of the 'minimalists'. Understanding the Bible too one-sidedly against a Persian or even Hellenistic perspective is not, in my view, acceptable.[7]

A more satisfactory approach to the archaeological and biblical evidence is to study them individually and independently, since such pieces of evidence are of different natures. Yet the sources should not be kept

6. He refers, mistakenly, to 2 Kgs 22.29-37, a misprint for 1 Kings (2003: 281). There is also a misprint in the footnotes, as there are two footnotes with the number 70; the last one should be numbered 71. The latter is the most important one, as it relates to the discussion of this misplacement.

7. Cf. Barstad 2008.

completely apart. After all, could there not possibly be a relationship between them? Could not impulses from one source raise new questions for the other source, and *vice versa*? Is such a use of sources, as Cryer, for instance, believes, really a mortal sin, methodologically speaking? Is that not the way scholars have always worked—and should work? The 'new archaeologist's' axiom should not be practised too rigidly.

The relationship between the Dan inscription and the Bible is disputed, and there are questions that have not been solved yet, or have been difficult to solve. Yet, despite objections, I would claim that, in this particular case, the archaeologists have found something that somehow matches what we read in the Bible. The common catchwords are 'Israel', 'Hadad', 'House of David' and probably—or possibly—the kings Ahaziah and Jehoram. Furthermore, there is a general historical relationship between the Bible and the inscription, since both refer to a common historical époque and geographical region.

In other words, there is a link between the content of the inscription and the text of the Bible. It would be a pity if such a connection were to be highjacked, or 'minimized'. Can the Bible illuminate the inscription? Can the inscription illuminate the Bible? It is a legitimate scholarly methodological tool to ask whether the Bible and the inscription might elucidate each other.

Chapter 9

The Inscription's Historiographical Significance

1. *Validity as an Historical Source*

The question of the Tel Dan inscription's validity as an historical source is, indirectly, well illustrated in a response from John A. Emerton (2002) to Thomas L. Thompson (2000).

Although Thompson was not dealing directly with the Tel Dan inscription in his 2000 article, he did briefly mention the '*Byt dwd*' issue.[1] Similarly, Emerton (2002: 321) did not refer to it in his response to Thompson. What is relevant to the present discussion is Thompson's general claim that 'No ancient text attempts to reconstruct the past as it happened'. While Thompson was making particular reference to the Deir 'Alla inscriptions and the Mesha stone, his claim and Emerton's response are highly relevant to the debate on the historical validity of the Dan inscription.

First, Thompson argued that the stories to which he referred are written in the first person, which, in his opinion, is evidence that the stories were written, fictitiously, long after the time of the rulers concerned. Emerton, in response, documented, with examples, his opinion on that theory, which was that 'the use of the first person is a frequent characteristic of genuine historical inscription' and that 'the first person is not evidence that the text in which it appears is fictitious and that it was written long after the time of the events that it records' (2002: 485).[2] The Tel Dan inscription is also written in the first person, in much the same way as the Mesha stone. Emerson's arguments are as valid for the Dan inscription as they are for the Mesha stone.

Secondly, Thompson argued from the role of the national god—in the case of the Mesha stone inscription, the god Chemosh—that the purpose of the stele was to honour Chemosh (2000: 323); the inscription is, therefore,

1. Thompson 2000: 325: '…the eponymic function of a truly historical *Byt dwd*'.
2. Cf. Lemaire 1998: 11: 'Even if the stele is propaganda, it is a small piece of Aramaic royal historiography which contains important historical information and interpretations about the kingdoms of Damascus, Israel and Judah in the second half of the ninth century'.

not historical. A great ruler from the past is epitomized 'by using the formulae and metaphors of the king as the faithful servant or son of god' in the same way as in the case of Hammurapi, implying that King Mesha of Moab would have described himself as a 'son of Chemosh'.[3] Emerton responded, with reference to textual criticism of the Mesha stele, that Thompson had misunderstood the text (2002: 485). The Tel Dan inscription has a similar reference to a venerable god, Hadad, who had, allegedly, made the text's originator, king. But that does not mean that the text was written for the sole intention of giving honour to Hadad, and without having any historical contribution to make.

Thirdly, Thompson commented, with respect to the Idrimi inscription, that this narrative 'presents us with an epitome of the king's reign. His destiny has been established by Adad: his enemies are defeated, his campaign completed' (2000: 324). From this, he drew conclusions about the Mesha stele, which also 'epitomize[s] the king's reign as having established lasting peace'. Both texts reflect the king's 'posthumous glory' (2000: 324). Also, here, Emerton responded with reference to a series of texts, which 'scarcely fit Thompson's pattern', concluding: 'Mention of a king's defeat of his enemies, his establishment of peace and prosperity, and his building works is not necessarily evidence for the fictitious character of an inscription' (2002: 486). Much the same could be said about the Tel Dan inscription. This text says nothing about building activities, fragmentary as it is,[4] though it does say something about the military achievements of its originator. This, however, does not necessarily invalidate it and make it fictitious.

Fourthly, Thompson showed that the inscription's style and form follows a well-known pattern in story telling (2000: 325). The Mesha inscription uses round numbers for the reign of kings, like many other historical sources do. Yet such numbers should not, solely for the reason that they are only approximations, 'be regarded as sufficient evidence that these numbers could not have been used in an inscription that was historical in character', Emerton objected (2000: 487). The Dan inscription also uses round numbers, such as 'seventy' and 'two thousand' or 'thousands', depending on how the text is reconstructed; they are not exact numbers. Be that as it may, we cannot, for that reason, dismiss the text as historically unreliable.

3. Cf. Cryer 1995b: 233: The 'god who goes before' (Dan A, line 5) somebody 'is a *topos* of ancient Near Eastern theology'.

4. Finkelstein and Silberman (2001: 205) argued that this was a commemorative stele, and they supposed it also mentioned Hazael's building activity, and that it could have been placed, either, at the city gate, or at an elaborately rebuilt cult place to Ashtar, 'probably rededicated to Aram's god Hadad'. Something similar was found at et-Tell, the Bethsaida of Roman/New Testament times, on the northern shore of the Sea of Galilee.

Fifthly, royal inscriptions regularly contain bloody descriptions of cruelty, such as 'becoming drunk on the blood' of the enemies (Thompson 2000: 325-26). This particular phrase is not in the Mesha inscription, or in the Dan inscription. Yet both texts do describe the cruelty of war, and should not be dismissed as unhistorical for that reason. Such descriptions are not fictitious narratives but express historical realities more or less literally. Each time has its own particular way of waging and memorializing cruel war.

Emerton concluded that he found Thompson's theory that the Mesha inscription is a posthumous fiction to be 'lacking in force' (2002: 488). Whether it is reliable in all its details is another question, one which does not shake its general historicity. The inscription is, of course, biased. It represents history as seen through the eyes of King Mesha of Moab. Something similar could be said about the Dan inscription.

Emerton (2002: 488-89) added another criticism of Thompson, which does not refer to his claim of the fictitiousness and posthumous nature of the Mesha stone. Thompson had claimed that 'The phrase, "Omri, king of Israel", eponym of the highland patronate, *Bit Humri*, belongs to a theological world of Narnia' (2000: 326). Omri was, allegedly, not a historical person, but an eponym, a personification of the state *Bit Humri*'s political power and the presence of its army in eastern Palestine (2000: 325). In reference to this, Emerton remarked, sarcastically: 'It seems that Thompson believes that the author of the Moabite Stone derived the name Omri from the Assyrian phrase Bit Humri', and that 'someone was able to read the Accadian in cuneiform and to recognize that the first letter represented a West-Semitic *'ayin*, and so infer that there was an Israelite king named Omri' (2002: 489). Emerton concluded that Thompson's theory is 'impossible' and 'improbable' (2002: 490).

Using arguments such as these, Emerton effectively countered Thompson's arguments against the historical reliability of the Moab stele—and, indirectly, commented on the question of the historical reliability of the Tel Dan inscription. The inscription should, in general, be taken as an historical source. Even biased texts could very well contain historically valid elements (cf. Emerton 2002: 491).

As an historical source, the Tel Dan inscription ranks side by side with previously known sources from the area, in that era, including the Mesha stone, the Panammuwa inscriptions, the Barrakib inscriptions and the Sefire stela.[5]

5. Cf., e.g., Na'aman 2000: 93; Athas 2003: 165-74.

2. *The Debate*

Questions related to historiography have become the most voluminous part of the debate, besides that of philology and the specific issues around the term *bytdwd*. Almost every scholar who has written on the Tel Dan inscription has had some historical argument to offer. In consequence, the volume of the debate on this issue has grown to an almost incalculable size. This situation, inevitably, has an impact on the presentation of the debate.

The historical debate relates to many questions. The debate on the identity of *bytdwd* has been investigated extensively in a separate chapter, but will also be given some attention here, within the context of historiography.

It would not be appropriate to give particular attention to the debate based on Fragment A before Fragment B was discovered. Even though not all scholars relate the fragments to each other in the same way as the Editors do, most scholars do see the fragments as part of the same inscription. Therefore, our main attention will be on the debate as it is based on both fragments, A and B (B1 and B2).

2.1. *The Editors*

The Editors, Abraham Biran and Joseph Naveh, set the agenda for the debate with their *editio princeps* to, respectively Fragment A (1993), and the united Fragments A and B (1995), which is why they deserve special investigation, before we examine the rest of the debate.

Since there was no king's name in Fragment A, the Editors claimed that 'one may theoretically attribute the Dan stele fragment to almost any king of Aram…who fought against Israel in the ninth century'. But they, nevertheless, raised some qualifications. As a 'son of nobody' (*ANET*: 280B), the usurper Hazael 'does not seem to be a good candidate', they argued. The speaker mentions his father no less than three times. Hazael was an officer, and had killed the king, Benhadad I.[6] The Editors were more inclined to see the speaker as a vassal of Hazael. Tentatively, they related the inscription to the battle described in 1 Kgs 15.16-22 || 2 Chron. 16.1-6, a campaign dated to around 885 BCE (1993: 96). If we read [ת]וקתל, 'and [I] killed…' (line 8), and if their understanding of the contextual relationship of this verb to 'king of Israel' and 'House of David' is correct, the speaker would be an enemy of Israel and Judah.

In reference to line 6, the Editors claimed that מלכי, 'my king', indicates that the speaker was a dependent of a king, and they assumed that the stele 'was erected by one of the commanders of the Damascene king, who might

6. Cf. 2 Kgs 8.7-17. In one of his inscriptions, Shalmaneser III boasts of having killed Hadadezer, *ANET*: 280B. Hadadezer is supposed to be the king's personal name, while Benhadad was his throne name.

have become governor of Dan and its vicinity'. The kings of Aram–Damascus ruled in the ninth century (cf. 1 Kgs 15.20). But since the text seems to indicate that the writer claimed the throne, they were 'inclined to assume' (1993: 96) that he was a subordinate to the king of Aram–Damascus. The small kingdoms of Maacah and Beth Rehob were judged to be adequate candidates. And yet, 'the nature of the biblical sources on the one hand and the fragmentary state of the Dan inscription on the other hand, do not allow us to draw definite conclusions' (1993: 98), is their final say. There may be other solutions, and they anticipate possible discovery of additional pieces of the stele, which might provide answers to the problems raised by the unearthing of the fragment.

After the discovery of Fragments B in 1994 some of their questions were, indeed, answered. At the same time, however, other questions were raised as well. In their discussion (1995: 17-18) of the situation after the discovery of the additional fragments, they said that 'quite a number of problems [still] remain unresolved'. They re-dated the text to the time of Hazael, claimed it was erected in the latter part of his reign, and that the author was Hazael himself, and they raised the text's status to 'royal inscription', a 'memorial stele describing Hazael's deeds'.

One problem is how a usurper like Hazael could mention his father no fewer than three times. That is remarkable, according to the Editors. As a usurper, it is assumed that he was not a member of the lineage of the Damascene kings, Benhadad I and II. Who his father was is obscure to them. Yet the Editors did not rule out the possibility that Hazael was a member of 'a secondary branch of the royal dynasty in Damascus'.[7] There is also what they called 'a serious contradiction' between the inscription and the biblical text as to who killed the kings. According to 2 Kgs 9.24, 27, Jehoram and Ahaziah were killed by Jehu. On the other hand, according to the stele (line 7) they were killed by Hazael himself. Was Jehu merely Hazael's agent?

2.2. *The General Debate*

When turning to history, Fredrick H. Cryer (1995b: 232) first deplored the fact that 'there is a number of scholars who can not resist the temptation to historify an unclear inscription in order to magnify its information potential'. Such scholars will 'no doubt' find 'King Hadad' in Fragment B (line 4), he reluctantly speculated. Yet Cryer himself saw quite a 'spectrum of Aramaic Hadads' from the Hadadezer of 2 Sam. 8.3-4 and 1 Kgs 11.23 to Benhadad III. This Hadad need not, necessarily, be an Aramaic figure, he could as well be the Assyrian Adad-nirari III, who died around 783. But Cryer saw no reason to choose any of these figures, since he interpreted this reference

7. See Biran and Naveh 1995: 18 n. 26.

to be to the god, Hadad. A reference to Hadad 'as *king* in a votive or commemorative inscription or orthostat can not, in the nature of things, provide us with a very useful date', he claimed. Cryer concluded his article claiming some agreement with the Editors on a very general level, but, in reality, disagreeing with them, profoundly, on the dating (1995b: 234).

Baruch Halpern (1994: 68A-74B) presented an extensive historical analysis, underlining that the Dan inscription 'invites us to sophisticate our historical paradigms…by questioning its accuracy…; its completeness; and its familiarity with archives, foreign sources, domestic annals, display inscriptions, and monuments' (1994: 63). 'Thus, the text can be best deployed, both against the revisionists and against the conservatives as a strong argument that our texts are neither entirely unreliable nor complete or reliable in details', he claimed.

Victor Sasson (1995: 27) thought that the precise historical context for the inscription was unclear, since it lacks personal names (except for the deity Hadad)—which were later found on Fragment B. Yet, for him, the 'overwhelming victory' described in the inscription presupposes a time of military weakness in Israel and Judah (1995: 28), one which disfavours the time of Omri and Ahab, and which also speaks against the Editors' original early dating. The historian's problem is that while the inscription deals with a major war, resulting in 'a devastating and most humiliating defeat' (1995: 29), the book of Kings does not mention it at all.[8] Sasson would place the events described in the inscription in the time of Jehoahaz of Israel (820–805) and Joash of Judah (841–805).[9]

The declared purpose of Shigeo Yamada (1995: 611) was 'to review the historical implications of the Dan inscription in combination with the other historical sources'. He was 'aware that our conclusions must remain hypothetical due to the defective state of the Aramaic inscription and the difficulties in evaluating accurately the biblical prophetic narratives'. But it is, nevertheless, Yamada argued, 'our duty to follow up every available clue in order to gain a reasonable picture of Aram–Israel relations', pointing out the 'rare situation' in the historiography of the ancient Near East, that historians now have an opportunity to examine an historical incident in the light of three distinct historical writings, namely (a) the biblical tradition, (b) Assyrian annals and (c) 'the commemorative inscription of a king of Aram'.

Keith W. Whitelam (1996) sided completely with the 'minimalists', aligned himself with the opinions of Davies and Thompson and argued against such scholars as Biran and Naveh, Rainey, Lemaire and Shanks.

8. But he saw a possible mirroring of the disaster in Psalms of national lament, such as Pss. 59, 74, 79 and 80.

9. According to Galil's chronology (1996: 72, Table 12).

Referring to the Editors' conclusion (1993: 98), Whitelam (1996: 166) pointed out, that, on the one hand, the biblical sources and, on the other hand, the fragmentary text from Dan 'do not allow us to draw a definite conclusion. There may be other possible scenarios…' On the interpretation of the Dan inscription, Whitelam claimed that, similar to the Merneptah stele, it contributes very little historical information (1996: 166-67).

William Schniedewind (1996) makes a contrast between what level of evidence would be acceptable to an historian as opposed to what a court would require and finds 'behind some recent writing on biblical history' the kind of attitude that can be read out of *Annales* school reasoning, which finds itself denying events had meaning and even that they happened at all. Schniedewind regretted what he called 'a general shift away from the natural sciences toward law as the standard for historical evidence', noting that, 'It is the jury and court system, not the scientific laboratory, that provides an analogy to the modern discipline of history' (1996: 76b). But the revisionist historians are not 'straightforward in laying out a guiding historiographic philosophy', he claimed (1996: 75b).[10]

Ernst Axel Knauf (1996) called the text an old Aramaic royal inscription,[11] but one of such a fragmentary condition that much is left to the interpreter's creative imagination. We are a bit better informed since the discovery of the additional fragments, but not much, he claimed. Knauf's 2001 article is of general importance as background for the historiographical debate related to the Dan inscription, in particular, because Knauf himself has been an ardent participant in the debate on the Tel Dan inscription. Knauf's 1996 article focused on the problematic dialogue between archaeology and biblical scholarship, on history as a text and context and as a four-dimensional construct.

In 1997, Hershel Shanks, the editor of the *Biblical Archaeology Review*, arranged, moderated and published, *in extensio* it seems, the debate between N.P. Lemche and T.L. Thompson, W. Dever and P. Kyle McCarter.[12] These four scholars have been central figures in the debate on the history of the ancient Middle East and Israel, archaeology and so on. The debate referred to, entitled 'Biblical Minimalists Meet their Challengers Face to Face', included comments on several topics. The symposium was not one of great scholarly importance, yet should not be totally passed over, since it received extensive attention. Shanks presents the debate in a very informal style, and

10. Behind this way of writing biblical history, Schniedewind (1996: 75b-76a) intuited the philosophy of the *Annales* school.

11. Knauf's article from 2000 can scarcely be read as anything other than a post-modern satire on his colleague historians' alleged fanciful imagination with respect to historical sources.

12. See *BAR* 23.4 (1997): 26-42, 66.

it is likely that he has transcribed tape recordings. There was nothing new in the debate itself, which was, mainly, a rather noisy repetition of old positions. The editor later admitted that '*BAR* has been criticized unusually heavily for printing the exchange' (*BAR* 1997b: 10).

Larry G. Herr joined in the Dan text debate in an extensive article on Iron Age II. Though Herr did not actually discuss the text, but called it 'the most famous inscription from an Israelite site' (1997: 140). Herr accepted the Editors' combination of the fragments, and claimed that it was 'most likely a victory stele set up in the ninth century to commemorate a triumph of Damascus over Israel under king Hazael'. He also accepted that it refers to the king of Israel and the House of David.

Francis I. Andersen (1998: 45) commented that '[t]he case of Hazael is the reverse of David'. For Andersen, their careers are similar in many ways, instancing their military prowess, their empire-building and the brevity of their reigns (1998: 45). While Hazael was an international figure, often dealing with the Assyrians, and, therefore mentioned several times in the Assyrian historical records, David and Solomon did not deal with the Assyrians, and are, therefore, not mentioned in their historical records. This silence proves nothing as to the historicity of David and Solomon, Andersen claimed. Andersen asked why we should not accept the biblical records about David and Solomon, when we do accept the biblical records about Hazael when they tie in with Assyrian records about him.

When Nadav Na'aman wrote (2000: 93) his intention was 'to discuss certain aspects of the inscription, some of which have not yet been dealt with by the research, and to offer the main points of [his] view on the messages of the inscription and on the problem of juxtaposing epigraphic inscriptions with the prophetic stories of the Bible' (2000: 93). The inscription is called 'the oldest royal inscription written in alphabetic script to have been found in the area of modern Israel'. Yet, Na'aman noted, several similar inscriptions have been found outside of Israel from around the same period, which he thinks 'calls for an explanation'.[13]

Na'aman stressed that there was no tradition for raising such monuments in the Syro-Palestine area; that was a tradition, he noted, which was previously known only from the Neo-Hittite inscriptions found in northern Syria and some Egyptian stela, of which one was the Shishak stele at Megiddo. But there are no similarities with the Egyptian or the Neo-Hittite steles. A royal stele of this genre is, instead, to be compared to the many victory steles

13. Na'aman referred to the Mesha stele, the inscription of Bar Hadad from north of Aleppo, the Zakkur inscription from Afis, the Kilammuwa inscription from Sam'al and the Hdys'y inscription from Tell Fahariyeh.

in Akkadian cuneiform script, erected by the Assyrians, who campaigned westwards from the beginning of the ninth century, and, in the reign of Ashurnasirpal II (883–859), who penetrated the areas west of the Euphrates.

Na'aman saw some similarities between the structure of the Assyrian inscriptions and some of the early alphabetical ones, such as, that they state the name of the writer at the beginning (which is lacking in the fragmented Tel Dan inscription), follow that with a description of wars and conquests, descriptions of construction projects, and end with curses against anyone who damages the inscription. The lack of uniformity and the diversity of the alphabetical inscriptions suggest that it was the actual concept of displaying such commemorations of triumphs and/or constructions that was first adopted—with the content of the inscriptions being dependent upon the creative talents of the scribes working in the diverse kingdoms in the region—and only later did a certain uniformity emerge (2000: 95).

In Na'aman's opinion, the use of alphabetical script in the royal courts in the Syro-Palestinian area probably began as early as the late tenth century.[14] According to Na'aman, comprehensive royal inscriptions began only in the latter part of the ninth century. Even though no such stele from the kings of Israel or Judah has been found so far, he argued that it is reasonable to suppose that those kings, too, wrote such inscriptions. 'The Aramaic inscription from Tel Dan is a fine example of the new fashion adopted by the West-Semitic rulers of the region' (2000: 96).

In conclusion, Na'aman claimed that historians have to rely more on primary than on secondary sources, implicitly, claiming, that for the latter part of the ninth century, the Dan inscription is a better historical source than 2 Kings.

2.3. *The Dating of the Inscription*

The debate around the dating of the text has already been touched on in several connections, but will be studied here more systematically. The dating of a text can be made on the basis of several aspects, such as archaeology, epigraphic material, palaeography, language and literary and historical content. Of not least importance is its comparison to related evidence. The epigraphic material, in this case, is basalt stone, which, in itself, is not datable.

As already noted the inscription was found at different locations within the Tel Dan precinct, in three different fragments. The archaeological details are explained by the Editors, who dated the (sparse) pottery assemblage from beneath Fragment A to 'nothing later than from the middle of the ninth century'. They added, 'It can be suggested, therefore, that the stele was

14. Cf. Na'aman 1996: 170-73.

smashed around that time and that, if so, the stele would have been erected during the first half of the ninth century B.C.E.' (1993: 86). As the latest possible date for the secondary use of Fragment A, they estimated the time of Tiglath-pileser III's conquest of northern Israel in 733/2, the date of the level of destruction covering the fragment. Pottery found adjacent to Fragments B1 and B2 was dated by the Editors to the end of the ninth century or the beginning of the eighth century (1995: 5). The dating of the sherds was decisive for their overall dating.

The palaeography has been discussed by some contributors. The Editors (1993: 95) dated the inscription palaeographically to the middle of the ninth century.

The language used in the inscription has been discussed thoroughly by several contributors, morphologically, phraseologically, semantically and syntactically. The Editors identified the language as Early Aramaic and dated it to the ninth century (1993: 87).

The text tells of an Aramaean attack against Dan, which the Editors, originally, linked to 1 Kgs 15.20, under the regime of King Asa of Judah (911–870) and Baasha of Israel (908–886).

Taking all these aspects into consideration, the Editors tentatively dated the inscription to the first half of the ninth century BCE (1993: 86, 98). On the basis of the additional fragments, the Editors later (1995: 17) changed their opinion, and dated the text to the time of Hazael, before the *coup d'état* by Jehu in 842.

Josef Tropper (1993: 396-98; cf. p. 401) discussed the Editors' dating, but ended up with a later alternative, around 835/30, which to him was more plausible according to his historical reconstruction. In his 1994–95 article, Tropper modified that somewhat in the light of the new fragments, but underlined that some of his fundamental theses, like the dating of the text to the reign of King Hazael of Damascus in the second half of the ninth century, had been confirmed (1994–95: 487).

Rupert L. Chapman III (1993–94: 28) discussed the Dan stele and the chronology of Levantine Iron Age stratigraphy. This was at a very early stage of the debate, the later direction of which he clearly foresaw. Chapman lined up some of the important questions that would recur in the later debate, but was reluctant to suggest a definite dating of the text.

Frederick H. Cryer (1994 and 1995a) and Niels Peter Lemche (1995a: 101), both working on Fragment A alone, dated the text to the eighth century. For Lemche (1998b: 41), the problem with a dating in the last half of the ninth century is that 'the lack of contemporary Aramaic evidence makes a dating precarious'. Cryer's dating of the inscription to the eighth century was good enough for him.

Baruch Margalit (1994a: 21) dated the text (Fragment A) to the early phase of Hazael's reign, the *terminus post quem* in 837, when he had established his hegemony in Canaan and Syria and had thwarted the Assyrian offensives, and the *terminus ante quem* in 805, the date of the Zakkur stele, which, in his opinion, quotes from the Dan inscription.[15]

Baruch Halpern (1994: 68-74) discussed the dating of Fragment A extensively. He was not able to date the text closely on a linguistic or palaeographical basis, but he requested the settling of two archaeological questions: the dating of pottery and the direct stratigraphic relationship between the wall, into which the fragment was integrated, and the inner gate structure (1994: 68b-69a). After an historical analysis, Halpern summarized (1994: 69b) his opinion as follows: the stele had been destroyed and reused in the century between 850 and 750 BCE, possibly somewhat earlier. But the Editors' early dating of Fragment A 'is simply not supported by the other evidences', he claimed. After lengthy argumentation, Halpern concluded, that the stele was the product of Benhadad II, and that it should be dated to the late ninth century (1994: 74a). As a corollary, its destruction and reuse is dated to the late ninth, or early eighth century.

Victor Sasson (1995), who wrote on the basis of Fragment A alone, dated the inscription to the latter part of the ninth century.

Hans-Peter Müller (1995: 133-37) discussed, in chronological order, on the basis of Fragment A alone, four possible historical contexts for the inscription: first, the transition between the tenth and the ninth century BCE (cf. the Editors); secondly, the time of Ahab, or the Omrides (cf. the Editors); thirdly, the time around the revolution of Jehu, and fourthly, the time before the *terminus post quem non* in the year 734, when Tiglath-pileser III conquered Israel. In his opinion, it was 'most probable' ('am ehesten wahrscheinlich', 1995: 136) that the inscription relates to 2 Kgs 10.32-33 and 12.17-19, that is, to the time after Jehu's revolt, the third alternative.

Ernst Axel Knauf (1996: 10) explicitly argued for a dating at the end of the ninth century. But what actually happened in Judah at that time is not revealed in the inscription, as he saw it (1996: 9).

William M. Schniedewind (1996) claimed from the outset of his study that the stele was erected by Hazael around the time of Jehu's revolt in 841. Schniedewind distanced himself from 'the rather curious proposal of Lemche and Thompson who date it to the late eighth or even early seventh century B.C.E.' (1996: 82b-83a).[16]

15. Margalit also argued that the Dan inscription may have a bearing on the date of the Deir 'Alla inscription, which, in his opinion, was '*composed*' (Margalit uses italics) before the Dan stele, and that Balaam was probably an older contemporary of Hazael.

16. Cf. Lemche and Thompson 1994 and Thompson 1995a.

Simon B. Parker (1997) identified the text as clearly reflecting a narrative account of the recovery of Aramean territory from Israel, dating from the second half of the ninth century.[17]

On historical grounds, Walter Dietrich (1997a) found a *terminus ad quem* in the eighth century, the campaign of Tiglath-pileser III into Syria, Israel and Judah in 733/732, when the stele was destroyed. The *terminus ante quem*, he set after the *Reichsteilung* (division of the monarchy) in the tenth century. However, Dietrich on palaeographic and epigraphic bases, he argued for a dating in the ninth century, giving three alternative dates for the text.

First, the Editors' original dating, early in the ninth century, Dietrich found to be in disharmony with the fact that the Bible records enmity between Israel and Judah at this time, while the stele reports enmity between both Israel and Judah and the Arameans. Secondly, the alternative date by Puech (1994), who linked the text to the Aramean wars of King Ahab (870–851), Dietrich dismissed as biased. Dietrich himself adhered to a third alternative, that the stele refers to the combat between Aram and Israel, which caused the revolt of Jehu in 845. The contemporary Aramean king was Hazael. Dietrich had questions as to how a usurper could talk of his predecessor as 'father', but explained it by the wide, and not just generic, use of the term 'father' in Semitic languages.

The text itself is almost uniformly dated to the ninth century, Dietrich claimed,[18] a period which, according to the biblical record, most of the Aram–Israel wars were waged.

Given what the sources in general say about this period, Kenneth A. Kitchen supposed that the Tel Dan inscription reflected conflicts no later than the year 841 BCE. On a palaeographic basis, Kitchen argued that the stele fits the period 850–800 BCE, and concluded: 'Hence, 841 BCE or later for its erection at Dan is unexceptionable' (1999: 35).

Implicitly, and because of the context of his translation, Kurt L. Noll dated the text to medio the ninth century. 'A date of c. 850 BCE plus-or-minus 50 years is probable' (1998: 7).[19]

17. Parker (1997) referred to the inscription briefly in the context of 'Studies on Narratives in Northwest Semitic Inscriptions and the Hebrew Bible', as the sub-title of his book says. 'Comparative studies' is the catchword for understanding Parker's presentation.

18. 'Als Rufer in der Wüste übten sich lediglich zwei Experten aus Kopenhagen', he remarked—somewhat acidly, referring to Cryer 1994 and Thompson 1995a.

19. Noll (2001: 184-85) offered another presentation of the inscription, where he paid particular attention to 'House of David'.

André Lemaire (1998) wrote on the Dan stele as royal historiography.[20] The discovery of Fragment B, in his opinion, confirmed his dating and interpretation of Fragment A. In general, he also agreed with the Editors in their reading and historical interpretation of Fragment B. He dated both the Dan and Mesha stele to the same period, the last quarter of the ninth century, and saw in Mesha an ally, or a vassal, of Hazael. That both of these texts use B(Y)T DWD reveals that this term was part of the diplomatic language of this period.

Nadav Na'aman argued that the respective Israelite and Judahite kings[21] were killed in 842 BCE, and that the text may indicate that this killing 'had taken place not many years before the stele was erected' (2000: 100). The conquest of Dan 'probably took place early in the Aramaic expansion in the reign of Hazael'. Na'aman, therefore, dated the text to the late 830s, 'not long after Shalmaneser abandoned the south Syro-Palestinian arena (…in 833 B.C.E.), and began to move into Anatolian lands west of the Taurus range'.

Israel Finkelstein and Neil Asher Silberman did not explicitly date the Dan text, but rather framed it to between 835 and 800, based, in general, on 'a re-examination of the archaeological evidence supported by new, more precise dating techniques' (2001: 202).

Gershon Galil (2001: 18) dated the text to Hazael's successor, his son Benhadad (III), whom he also regarded as its originator.

Galil's conclusion is roughly similar to the dating of George Athas (2003: 5-6) who regretted that the Editors' dating was arrived at on insufficient archaeological grounds, and gave great credit to the arguments of Halpern (1994). On the basis of archaeological analysis and the synchronism between the three fragments, Athas dated the stele to the early eighth century and concluded 'with some degree of certainty' (2003: 16) that all three fragments were broken at the same time. The recycling of Fragments A and B as building matter he dated to the periods of Jehoash and Jeroboam II (first half of eighth century), adding that 'this synchronic parallel between all the fragments strengthens the connection between them' (2003: 16). The question of dating is more explicitly discussed later in the book, in a separate chapter, which includes a specific time line for events dating from 799 to 791 BCE (2003: 295-98). His preferred date is 796 BCE (2003: 309).

In conclusion, the actual date of the stele has been much debated, but a date in the latter part of the ninth century has become conventional. Without adding anything to what has been argued previously, dating the text to the latter part of the ninth century seems reasonable.

20. Cf. Lemaire 1994a, 1994b, 1994c.
21. He accepted the Editors' reconstruction of the kings' names in lines 7-8.

2.4. *The General Historical Context*

The following picture has been accepted as a relatively safe one to describe the history of Israel and Aram in the ninth century. In the battle of Qarqar (853 BCE), a coalition of twelve kings of the seacoast, headed by Hadadezer of Damascus, Irhuleni of Hamath, and Ahab of Israel, fought against Shalmanasar III of Assyria, in his sixth year. In 841 BCE, Jehu paid tribute to Shalmanasar III, in his eighteenth year. These are relatively fixed historical abutments from which to bridge the history of Israel and Judah.[22]

According to Galil, the period between the death of King Ahab of Israel and the revolt of Jehu (c. 742) 'is one of the most difficult and complicated subjects in Biblical chronology. The Biblical data are contradictory and appear to be erroneous' (1996: 32). Galil found it impossible to reconcile all the biblical and external data, claiming that 'the majority can be confirmed, and they may possibly be based on a reliable tradition reflective of the reality' (1996: 33; see 33-43 for the broader discussion).

In general, the ninth century was a period in which Shalmaneser III (858–824), the great Assyrian king, expanded westwards and, in around the mid-ninth century, Israel and Judah joined forces to recapture territory in northern Transjordan claimed by both the Israelites and the Arameans, according to 1 Kgs 22.3 and 20.26-30 (cf. Margalit 1994: 20-21).

Baruch Margalit (1994: 21) saw two battles in the stele and the beginning of a third: a battle against Samaria,[23] where Hazael takes the credit for the king's death; a later battle against Judah (cf. 2 Kgs 12.18-19); and the siege against Samaria at the time of Jehu.

Shigeo Yamada (1995) concluded from the inscription (on the basis of Fragments A and B1/B2) that there were territorial conflicts between Ahab and Benhadad/Adad-'Idri until a bilateral treaty was entered into immediately before the battle of Qarqar. Such a coalition between Aram and Israel, he argued, is one of the preconditions for holding the Arameans on the northern flank in Hamath and preventing them from expanding further south. In this context, he doubted the historicity of Ahab's fatal battle against Aram at Ramoth-Gilead (1 Kgs 22) and Jehoram's war against Aram (2 Kgs 5–7). According to Yamada, 'The historical setting of these prophetic stories is probably inaccurate' (1995: 617). Other scholars have also made similar claims. This is, allegedly, the reason why the Tel Dan inscription does not say anything about Hazael's predecessor's victory over Ahab and Jehoram—a victory which the Bible records. Yamada reasoned that 'such a remarkable success by Hazael's predecessor would have been mentioned, had it actually

22. See Galil 1996: 156 and *ANET*: 276b-81b.

23. This accords well with the name of the two kings in Fragments B1/B2. But his reference to 2 Kgs 8.18-19 is not quite appropriate.

taken place' (1995: 618), and supposed that the hostility between Israel and Aram began after Hazael became king, sometime between 845–841.

The hostility between Aram and Israel was 'understandable' (1995: 618) to Yamada, since previously there had been peaceful relations with Benhadad/Adad-'Idri. The usurper, Hazael, probably broke the relationship, which he saw illustrated, in particular, by the war in Ramoth-Gilead.[24] Jehoram had refused to co-operate with Hazael in his anti-Assyrian military operations. This, Yamada argued, was the main reason for a battle between him and Hazael immediately before the Assyrian invasion in 841. Both the biblical tradition and the Dan text agree that Hazael was superior in his battle against Jehoram and Ahaziah. Jehu's *coup d'état* was facilitated because Jehoram and Ahaziah had been defeated by Hazael, and Hazael himself was busy on the northern flank against the Assyrians. Therefore, Jehu had two possibilities: either to re-establish the alliance with Aram against Assyria, or to isolate Hazael by allying himself with Assyria. Since the Assyrian sources do not mention any confrontation with Israel in this period, but relate that Jehu paid tribute,[25] he saw clear indication of the latter. This, as a matter of fact, implied submission to the Assyrians, who could expand forward as far Tyre and Sidon.[26] As the power of Shalmaneser III was weakened, Hazael gradually regained power and defeated both Israel and Judah and the Philistines.[27] He argued that lines 12-13 of the inscription reflect Hazael's later military expansions.

William M. Schniedewind (1996; cf. Schniedewind and Zuckerman 2001) claimed that the inscription necessitated a revision of the biblical description of Jehu's revolt and may also explain some enigmatic fragments of the biblical literature. Schniedewind discusses in depth the historical questions related to Jehu's revolt (1996: 82b-85b). The Deuteronomistic Historian of the book of Kings depicts Jehu as a liberator and a religious reformer, supported by the prophet Elisha, freeing Israel from the Phoenician queen Jezebel. This was, in reality, an affront to Ahab's Phoenician alliance, and may be a signal that Jehu sought new alliances, perhaps with Hazael (1996: 83b).[28] Schniedewind interpreted 1 Kgs 19.18-19 as a sign of 'such a collusion between Aram and Israel' (1996: 83b), and found similar traits in 2 Kgs 8.7-15 and 9.1-13. At this point Schniedewind did not accept the biblical account, which he argued is 'not well integrated into the Deuteronomistic

24. See 2 Kgs 8.28; 9.14-15 and the Dan text, lines 7b-10.
25. Cf. *ANET*: 280B and 281A.
26. Cf. *ANET*: 281A.
27. See 2 Kgs 10.32-33; 12.17-18; and 13.3.
28. Schniedewind would not refute the idea presented by Schneider (1996) that Jehu was a descendant of the Omride royal family. Assyrian sources call him 'son of Omri' (*ANET*: 280B and 281A); cf. 'son of a nobody', or 'commoner' (*ANET*: 280B).

History' (1996: 84a).[29] In Schniedewind's opinion, the alliance between Jehu and Hazael revived a previous alliance between Israel and Damascus that had been disrupted by Hazael's *coup d'état* in 845 BCE. This previous alliance Schniedewind found documented in the Kurkh monolith's report on the battle of Qarqar (1996: 85a). Such collusion with the Arameans would explain the 'unbridled hostility' toward Jehu in Hos. 1.4-5, where, he argued, 'the blood of Jezreel' refers to Jehu's bloody coup. For Schniedewind, 'The later author of Hosea saw Jehu's revolt as collusion between Israel and Aram that resulted in the slaughter of the king of Judah…' That Jehu's vassalage to Hazael caused the Arameans to claim areas formerly under Israelite control, he found indicated in 2 Kgs 10.32-33, as well as in 'some limited archaeological evidence' (1996: 85b; cf. Pitard 1987: 145-60).

Schniedewind concluded that 'this inscription should be attributed to Hazael', and disagreed with Halpern (1994: 74) that it would have been 'an emergency display inscription'. Instead, he saw it as 'a memorial stela much like the Mesha Stela' (Schniedewind 1994: 85b), which 'was intended as a propaganda boasting of Hazael's victories on the northern border of Israel'. The text directly and implicitly illustrates that the book of Kings 'is selective and political/ideological in nature', a 'redacted work that used earlier sources'. Schniedewind also concluded that 'this inscription should refocus our attention to the political dimensions of the "house of David"', and regretted that 'biblical scholars have tended to be overly enamoured with the theological idea of the "house of David" and have tended to read all references to David in prophetic literature as late or as reflecting eschatological and utopian ideals'. Schniedewind seems to think that the Dan text should modify such opinions, claiming: 'The Tel Dan inscription should remind us that the "House of David" was first a political designation and only much later did this political idea, by its association with the temple and priesthood, take on theological and ultimately eschatological dimensions' (1994: 86b).

As with the Mesha text, Parker (1997: 58) found references in the Tel Dan inscription to: (1) the previous occupation of the land by a hostile neighbour (against Israel); (2) the narrator's coming to the throne (in the new fragments) and the intervention of the national god (here Hadad) on the king's behalf; and (3) the narrator's victory over his enemies. Parker did not name the narrator, but says the narrator is 'either recounting one campaign against a larger array of diverse forces, presumably marshalled by Israel, or, summarizing several campaigns'. Parker argues that the narrator 'is operating on a larger scale than Mesha and so is able to spell out the character of

29. Schniedewind (1996: 84b) found a parallel to the discrepancy in the killing of Jehoram and Ahaziah in contemporary Assyrian records (*ANET*: 278B and 279B), where there are different opinions in earlier and more recent texts as to who killed Giammu of Balih, the inhabitants of his own area, or Shalmaneser III, himself.

the opposition—the number of the leaders and their chariot forces and the names and patronymics of the two particular enemies (according to the two new fragments as filled out by Biran and Naveh)' (1997: 58).

In a later article (2000) Parker discussed whether the authors of the book of Kings made use of royal inscriptions. The question is also discussed with relation to the Tel Dan inscription (2000: 366-67). Parker says: 'In sum, the evidence to date does not support claims that the authors of Kings used royal epigraphic monuments as sources for their history. This does not mean that we can be sure that they did not [use such monuments]…' (2000: 375).

Walter Dietrich wrote on the Tel Dan inscription in a book (1997a), as well as in a separate article (1997b).[30] In his book, Dietrich gave a relatively comprehensive and precise description of the problems connected with the Dan text, which he presented under the heading 'Scriptural Witnesses about the First Israelite Kings' (*Schriftliche Zeugnisse über die erste Könige Israels*, 1997a: 133). As 'Extra-Biblical Witnesses' (*Außerbiblische Zeugnisse*, 1997a: 134), Dietrich presented the Shishak stele and the Tel Dan stele, which he called 'Siegesstele' (1997a: 137), and he demonstrated how the Editors put the Tel Dan inscription into its historical context (1997a: 136).

Putting all the facts together, Dietrich argued that the following is decisive (1997a: 140-41): in the later part of the ninth century, an Aramean king knew two Israelite states, Israel and Judah, and the names of their respective kings. Dietrich referred to the northern kingdom by its political name, 'Israel', while the southern kingdom of Judah is named by its dynastic name, *bytdwd*, 'House of David'.[31] According to Dietrich:

> Demnach hätte ein gewisser David eine Dynastie begründet, die zur Zeit, da die Inschrift von Tel-Dan abgefaßt wurde, bereits ein auch im Ausland geläufiger, fest geprägter und sogar in der spezifisch lokalen Aussprache festgelegter Begriff geworden ist. Derlei aber geschieht nicht über Nacht, das braucht Generationen ('Accordingly, a certain David had founded a dynasty, which at the time when the Tel Dan inscription was authored, was already a well-known, distinguished and even an established concept in the specific local vernacular. Such does not happen overnight, it needs generations') (1997a: 141).

30. His book has a purpose different from that of his article. His book is a general presentation of the history of the early Israelite kingdom, where the Dan inscription is just one—but a very important—historical document. His article concentrates particularly on the *bytdwd*-problem. The book has a broad perspective and the article a narrow one. The two contributions were written around the same time for different audiences, but they communicate well together, and exhibit a complementary interrelationship.

31. For his discussion on this particular problem, see Dietrich 1997b.

In his article, Dietrich admitted that since these fragments were found at different times, many questions connected with them are unsolved, and that it is difficult, in general, to make connections between extra-biblical and biblical texts. We should, therefore, be careful not to derive historical conclusions that are too comprehensive (1997b: 31). That having been said, it should be evident that the author of the stele is King Hazael of Damascus. Dietrich is unsure whether the stele might have contained more names and historical information when it was intact (1997b: 32). Dietrich implicitly accepted that there had already been a Davidic dynasty for generations at the time this text was written.[32]

Ingo Kottsieper (1998: 483-95) offered an extensive historical commentary on the Tel Dan inscription, discussing the significance of the inscription and, in particular, the identity of the author. Kottsieper referred to the content of the inscription in a summary (1998: 484), interpreting the text as reflecting a reaction to an attempt by the Israelite king to break the treaty with the Aramean ruler. Kottsieper saw a reference to King Jehu's attempt to seize the throne in line 11.

While Kottsieper's 'detailed examination' (*Einzeluntersuchungen*) of the inscription is too extensive (1998: 485-95) to be traced in detail here, some of his 'details' (*Einzelheiten*)—in particular those relating to Hazael—deserve to be mentioned.

Since Kottsieper identified Jehoram of Israel and Ahaziah of Judah in lines 8 and 9, the inescapable conclusion was that the originator was Hazael of Damascus (1998: 485). This conclusion, however, raises two historical questions: Why, or how, would/could Hazael designate his predecessor 'father'? And how is this related to 2 Kings 9–11, the story of how the kings of Israel and Judah were killed by Jehu? In Kottsieper's opinion, Hazael was not a member of the royal family (1998: 485). The issue is therefore how he became king. According to 2 Kgs 8.15, Hazael's predecessor was assassinated. The fact that Hazael himself says nothing about that is natural, since he would not be expected to talk about killing the king. Kottsieper could not see that the throne exchange, as presented by Shalmaneser,[33] confirms his predecessor's violent death, and since 2 Kgs 8.15 is not an unbiased factual description, that leaves the Dan inscription as the more historically reliable: his predecessor was ill and died a natural death (line 3).

There is no source that explains who Hadadezer's successor was. Was there no dynastic rule? Did Hadadezer not have a capable son? Whatever the situation was, Hazael was obviously accepted in Damascus as ruler. With reference to details in his article, which will not be referred to here (1998:

32. Dietrich also wrote on 'Jehus Kampf gegen den Baal von Samaria', and only touched on the Dan inscription (2001: 116 n. 6).

33. Cf. *KAH*: I 30 I 25-27. Cf. the English translation in *ANET*.

492), Kottsieper argued for a covenant between Aram-Damascus from 853 BCE, built on a previous covenant with Hadadezer's predecessor, Bar-Hadad, cf. 1 Kgs 20.34. There is no direct indication of a covenant between Jehoram and Hadadezer, that is, Benhadad II, in the Dan text (cf. line 2), but the inscriptions of Shalmaneser could possibly be read as indicating such a covenant, at least until 848 BCE, which was possibly not broken until under Hazael. Kottsieper supposed (1998: 494) that Hadadezer continued the covenant until after the enthronement of Jehoram.

Kottsieper summarized that the Tel Dan inscription, along with the biblical accounts and the Assyrian text, had helped to develop a new picture of the relationship between Aram-Damascus and Israel (1998: 495), which he briefly sketched (1998: 495-96). His intention was to read the inscription in the context of the general political situation in the first half of the first millennium BCE, and to read it with due reference to relevant historical sources, the book of Kings included.

In his 1998 study, André Lemaire concluded that even though the Tel Dan inscription is propaganda, it is 'a small piece of Aramaic royal historiography which contains important historical information and interpretations about the kingdoms of Damascus, Israel and Judah in the second half of the ninth century' (1998: 11).

Niels Peter Lemche (1998b) argued that if the text is authentic,[34] belonging to a period around 900, 'it still includes a very important although confusing testimony to the presence of a state by the name of Israel in northern Palestine in the ninth or tenth century, ruled by kings' (1998b: 41). The stele's testimony of the existence of the state of Israel in this period 'is unequivocal', but the expression *bytdwd* 'has created many problems so far unsolved', he claimed.

Lemche argued that the Israelite king mentioned in line 8 of Fragment A will never be identified, unless additional comprehensive fragments are found. He concluded, after some deliberation, that 'any endeavour to choose between these options…is nothing other than free speculation' (1998b: 42).

After presenting his reconstruction and translation of lines 1-5 of the inscription, Nadav Na'aman (2000: 98-100) discussed its historical implications.

First, he pointed out that the Tel Dan inscription bears some resemblance to the opening passage of the Mesha inscription, noting that 'both refer to past events which served as pretext for present assaults and conquests' (2000: 98). The reference to Abel (full name: Abel-beth-maacah), which Na'aman found in line 2, 'is not accidental' (2000: 98). This city was an Aramaic enclave in northern Israel, one which had probably been attacked

34. Cf. Lemche 1998b: Chapter 3, 'The Forgery Theory'.

by an Israelite king, for which retaliation by the king of Aram would be expected. We must not forget that the Aramaic inscription was erected at Tel Dan, and the mention of Abel as the pretext for the Aramaic attack was probably meant to justify the conquest of its neighbour, Dan—just as the conquests by Omri, in the areas north of the Arnon River, were used as the pretext for Mesha's conquest of the land of Medeba.

Secondly, it is characteristic that both the Mesha and the Dan inscriptions date their offensives and conquests to the time of their fathers. The same feature is found in, for example, the contemporary inscription from Kilammuwa.

Thirdly, Na'aman pointed out the 'prominent feature of the emphasis on the legitimacy of the inscription's author' (2000: 99). The triple reference to 'my father' at the beginning of the inscription 'may be no accident'.

Despite the fact that the last part of the inscription is so fragmentary that we can only speculate about is content, Na'aman assumed that 'it recounted the conquest of Dan and the erection of the stele on the site', because this was usually made in the final portion of a victory inscription.

Kurt L. Noll (1998; cf. 2001), underlined that we now have two main sources of knowledge of Tel Dan in the Pre-Assyrian Iron Age, namely, the Tel Dan inscription and the biblical text, which are, respectively, primary and secondary sources. These sources give the impression that Dan was a non-Israelite city throughout most of this period, which is not gainsaid by material remains. This led Noll to speculate that perhaps Dan was under the supervision of Damascus during most of the period. Noll identified the two named kings in lines 7-8 as well as *bytdwd* in line 9 in the same way as the Editors in his 1998 article, while in his 2001 book the two kings have been erased from his reconstruction.

Eduard Lipiński (2000: 375-80) described the history of Aram, in general, in the last part of the ninth century. The stories in 1 Kings 20 and 22 are not ascribed to the time of King Ahab. The chronology of the Bible for this period is held to be 'extremely problematical'.[35] But thanks to Assyrian, Aramaic and Moabite sources 'a plausible reconstruction of events is feasible'.

Hazael's rise to power in Damascus around 843 changed the political situation in central and southern Syria radically. Hadadezer's (Benhadad II) anti-Assyrian alliance was dissolved, and the former allies of Damascus seem to have turned around and become its enemies. The Tel Dan stele indicates that Hazael waged war and marched against Israel, aiming at recovering the territory of Abel. Yet, 'its reason may have been deeper' (2000: 377), Lipiński argues, since Israel disappears in 842 from the list of the enemies of Assyria in Shalmaneser annals.

35. Cf. Galil 1996: 32.

Lipiński identified a battle between Aram and Israel in Ramoth Gilead in the year of 842, resulting in the killing of Jehoram of Israel and Ahaziah of Judah, who was his son and ally. 'This was one of the main battles in the history of the kingdom of Israel' (2000: 378), which is referred to in the Dan text and identified with the detailed account in 1 Kgs 22.3-37 and 2 Chron. 18.3-34. The Tel Dan stele is taken as the earliest account of this event, and the stele was probably erected 'shortly after the events'.

Lipiński saw Jehu's accession in the very fragmentary final lines (11-12), an event which, according to him, took place in the very same year of the battle. He found probable traces of Hazael's offensive in northern Israel and his seizing of towns and several named cites.

William M. Schniedewind and Bruce Zuckerman (2001: 91; cf. Schniedewind 1996), suggested as 'a tentative hypothesis' that their reading of line 2 reveals a succession of two kings with -*'el* theophoric names, Baraq'el and Hazael, who are part of a family, or clan, that acted to usurp a dynastic succession from a family with Hadad theophoric names. This political rivalry also mirrors a religious conflict within Aramean circles. To them, this accords with events in a number of small states where allegiance is focused on emerging national gods who are perceived as having a special relationship with the people who worship them.[36] 'A religious rivalry within the Aramean kingdom between adherents of El versus adherents of Hadad may reflect similar loyalties and tensions in Damascus as well', and is possibly reflected in the conflict between the usurper, Jehu, and the house of Omri. In this context, it is 'another interesting point' to them that Hazael had some sort of a relationship with an Israelite prophet anointed by Elisha.[37]

Finkelstein and Silbermann (2001) consider the inscription the most striking evidence for Hazael's offensive in Israel, linking the death of the kings Jehoram and Ahaziah with an Aramean victory, about which Hazael boasted in lines 7-8. This is regarded as the most important part of the inscription (2001: 129). The text is, in general, connected to contemporary destructions in Jezreel, Tel Rehov, Beth-shean, Tanaach and Megiddo, and constitutes evidence of a new discovery (2001: 202-203). This would be connected to Finkelstein's alternative ('low') chronology of Israelite archaeology and the alleged inability of conventional dating to identifying a mid-ninth-century destruction, which, for Finkelstein, was 'much less a period of Aramean occupation. But at Dan too, the alternative dating allows the identification of a destruction layer for the conquest of Hazael that is commemorated in the Dan stele' (2001: 203).

36. Cf. Chemosh in Moab, Milkom in Ammon, Qaus in Edom, Baal in Sidon and Yahweh in Israel.

37. See 1 Kgs 19.15-17; 2 Kgs 8–9.

The result of Hazael's war was devastating for Israel, as several cities were lost to the Arameans. Those cities were closer to Damascus than to Samaria and were situated in territories that Hazael claimed were originally Aramean. This is found to be confirmed in Hazael's comment: 'And my father lay down, he went to his [ancestors]. And the king of I[s]rael entered previously in my father's land.' In Finkelstein and Silbermann's opinion, Hazael was exaggerating, arguing that 'it is likely' that Hazael built a new city after his conquest, one that was actually 'an important link in a chain of Aramean cities and fortresses that guarded Aram-Damascus' south-eastern border against Israel' (2001: 205).

Finkelstein and Silbermann compare the finds at Tel Dan (and at Hazor) with a stronghold found at et-Tell, on the northern shore of the Sea of Galilee, which is identified with the Bethsaida of Roman times. In the ninth century, et-Tell was fortified with a massive stone wall surrounding the site and a huge city gate, similar to the one found at Dan. At the gate, a stele was found with a depiction of a horned deity on it, which they characterize as Aramean. For Finkelstein and Silbermann, 'its location, in front of the gate, offers the possibility that a similar stele may have been erected near the Dan gate, under the elaborate canopy' (2001: 204-205).

Finkelstein and Silbermann point out that Hazael's invasion of Israel in the mid-ninth century was followed up by prolonged occupation and the establishment of three fortresses at Dan, Hazor and et-Tell/Bethsaida, all of which displayed common characteristic Aramean features, and had a partly, or mostly, Aramean population. This theory is supported by the fact that, in almost every major Iron Age II site in the region, ostraca written in Aramaic have been found.

As Gershon Galil (2001) read the text, it originally began with the king's self-presentation, followed by an extensive historical introduction (2001: 18), which he reads out of Fragments B1/B2, including events from the days of Hazael, perhaps even his coronation. Hazael's death is found in line 3. Since the introduction of the Mesha stele comprises about a third of its content, a similar estimate can be made for the Dan stele, Galil argued.

For Galil, the main part of the text deals with the period of Hazael's son, Bar-Hadad (Benhadad III). The surviving words 'do not permit a reasonable reconstruction of the events. It is, obviously, a 'summary inscription'. The king of Aram took pride in his victories over many kings and their large armies' (2001: 18).

In line 4, Galil read the toponym Ubi or Upi. But whether *'by* is rendered as 'my father' or Ubi/Upi, 'we should assume that this document indicates an Israelite control over the region of Damascus during the period preceding the days of Bar-Hadad [Benhadad III], son of Hazael' (2001: 20). Galil concluded with a warning that care is needed when dealing with the inscription because the text is so incomplete.

In the last few lines of the text, Galil found the coronation of Joash of Israel (805 BCE).

Galil placed Fragment B above Fragment A, and added other opinions that deviate from those commonly held. In view of the relative consensus Galil was challenging, it remains to be seen whether his position will succeed in overturning the arguments he seeks to counter. The future is not too promising for that kind of proposal, except that Athas (2003) also dated the text to the period of Benhadad III, and that he agreed with Galil, against the Editors and the majority, in not placing Fragment B side by side with Fragment A.

George Athas (2003) has a separate chapter in which he reconstructs the events of the inscription (2003: 289-95). Actually, it is a reconstruction of the history of the era around 800 BCE. Athas sees the inscription as 'deliberate propaganda by Benhadad to promote his suzerainty over former Israelite territories in the face of his waning influence over these territories' (2003: 294).

2.5. *The Identity of the Assassinated Kings*

The most important new aspect added by the discovery of Fragments B1/B2 was the possible identification of the two kings of Israel and Judah, Jehoram and Ahaziah, whom the originator boasts of having killed. If this identification is valid, the text is firmly settled both geographically and chronologically. It is precisely this identification that has caused considerable discussion. A number of scholars have argued strongly against identifying Jehoram and Ahaziah.

It is to be noted that names were not identified in these lines by Cryer (1995b), Thompson (1995b), Knauf (1996), Millard (1997), Lemche (1998b), Ehrlich (2001), or Noll (2001).[38]

Frederick H. Cryer (1995b) disagreed completely with the Editors when they found Jehoram of Israel and Ahaziah of Judah in what he called 'Dan B'. Cryer was followed by Thomas L. Thompson (1995b: 240), who discussed the text's biblical linkage, claiming that finding Jehoram of Israel and Ahaziah of Judah in lines 7-8 (Fragment B) 'is historically irresponsible and has plagued the interpretation of these texts since Fragment A was first discovered'.

Also Niels Peter Lemche (1998b: 40) refuted the Editors' identification of Jehoram of Israel and Ahaziah of Judah in Fragments B1 and B2, which Lemche saw as 'based on their false joint of all the fragments', because 'we have no other sources telling us that Ahaziah of Judah was killed by an

38. Noll (1998: 9) claimed that reading the names of the two kings, as the Editors had, is 'certainly possible, but by no means 'unequivocal' as the Editors had claimed.

Aramean king'. According to 2 Kgs 11.21-29, the culprit of the killings was Jehu. Were the Editors' interpretation correct, Lemche argued, 'we would be in possession of a curious but important example of a contemporary text going directly against the evidence of the Old Testament'. Otherwise, Jehu should have been identified as the author of the text.[39] In Lemche's opinion such a theory 'would create more historical problems than it solves' (1998b: 41), as it would probably make Jehu an Aramean prince.

Some scholars followed the Editors only in part, accepting the Editors' identification of only one of the two kings, Jehoram or Ahaziah.

Paul E. Dion (1999: 152) identified Jehoram as one of the assassinated kings, but not Ahaziah (אחזיהו) as the other. This was, for Dion, because of the second name's 'solidarity with the questionable phrase "the king of the House of David"',[40] and because 'מלך is never followed by "the house of So-and-so"'. Where the other king is identified by the Editors, Dion found Jehu identified as the killer. Dion did concede, however, that these arguments are not strong enough to exclude the Editors' restoration.

Gershon Galil (2001: 18) presumed that the kings referred to in lines 7-8 were Jehoahaz of Israel and Jehoash of Judah, 'though it is unclear whether their personal names are mentioned in the inscription'. But he did not argue for this identification.

George Athas (2003: 241) had yet another solution, arguing that line 7 (B) mentions '[Jehoas, son of Ahaiah, son of Jeho]ram, son of [Jehoshaphat]', while line 8 (B) refers to '[Amaz]iah son of [Joash]'. This opinion is obviously influenced by his late dating of the text.

Other scholars have sided completely with the Editors, and have found both Jehoram and Ahaziah in lines 7-8. Key to this position has been the argument that Jehoram and Ahaziah were the only Judean and Israelite kings whose names end, respectively, with *resh* (ר) and *mem* (מ) and the theophoric element -*yahu*. With reference to 2 Kings 8–9 the editors concluded that 'the author of the stele was Hazael himself, although his name does not appear in the fragments found to date' (1995: 17; cf. 18). They were followed in this conclusion by Yamada (1995), Schniedewind (1996), Kitchen (1997), Parker (1997), Andersen (1998), Kottsieper (1998), Lemaire (1998), Wesselius (1999), Lipiński (2000) and Finkelstein and Silbermann (2001: 129).

Summing up, we find ten contributors following the Editors, seven abstaining from identifying any name, while Dion, Galil and Athas have other solutions, with Dion accepting the identification of Jehoram, and Noll admitting the possibility of reading the two kings' names just as the Editors'

39. This theory was later presented by Wesselius (1999 and 2001) and refuted by Becking (1999).

40. Cf. the *kaph* (כ/ך) preceding ביתדוד.

claim. Statistically, this gives a scant majority in favour of the Editors' conclusion. First and foremost, however, it illustrates that their reconstruction is possible, but not certain.

2.6. *Who Assassinated the Two Kings?*

The question of who assassinated the two kings is based on the assumption that the assassinated kings were Jehoram and Ahaziah. According to the biblical tradition they were killed by Jehu; according to the Tel Dan inscription, however, they were killed by the originator of the text himself. We have to choose between the two texts on this point (if, that is, the originator was not Jehu).

Shigeo Yamada (1995: 618–19) discussed the 'apparent contradiction' between 2 Kings 9 and the Tel Dan inscription (lines 7-8) concerning the circumstances around the assassination of the kings Jehoram and Ahaziah. In Yamada's opinion, it is 'perhaps not impossible' to imagine that the biblical text has tendentiously 'distorted the historical facts by presenting Jehu as the glorious killer by divine will'. Yet, for Yamada, 'a more likely solution can be proposed'. As he argued, a carefully protected king 'would have only rarely died in open battle', and 'it is a surprising or even unlikely possibility' that the two kings were beaten in the same battle. Yamada argued that the inscription exaggerates when it claims that its author killed both Jehoram and Ahaziah. Yamada observes that the verb *qtl* is connected to the execution of as many as 70 kings in line 6, which, in his opinion, is scarcely factual. He therefore concluded that 'it seems doubtful that *qtl* here signifies the actual killing of the kings'. It should rather be understood in the meaning 'to strike, defeat', Yamada argued, and referred to the parallel use of the Akkadian *dâku*, 'kill', and later Aramaic parallels (1995: 619-20). If this more enfeebled meaning of *qtl* is acceptable, two problems are probably solved: Both the unlikelihood that as many as 70 kings were 'killed', as well as the contradiction between the Tel Dan inscription and the biblical text concerning who killed Jehoram and Ahaziah.

This attempt to alleviate the contradiction between the Bible and the inscription as to who killed the kings of Israel and Judah, Jehu or Hazael, is perhaps the most significant feature of Yamada's article. Hazael's authorship is not questioned. Yamada's solution is to weaken the absolute meaning of the verb *qtl*, which, to him, solved both problems. Yamada's views have since been challenged by Na'aman (2000: 101).

Victor Sasson had previously (1995) defended Hazael as the author of the Tel Dan inscription. In another contribution, a year later, he elaborated on the issue, declaring (1996: 548) that it is not quite clear who Hazael actually was. Sasson argues that while the main biblical record of Hazael is found in 2 Kgs 8.7-15, these verses are not as clear as many scholars seem to presuppose (1996: 549).

Sasson identified three reasons that called into question the idea that Hazael was a murderer, or a usurper (1996: 548). First, Hazael was a person 'very close to the king'.[41] Secondly, 2 Kgs 8.15 does not actually say that Hazael killed Benhadad. Thirdly, the verbs used in 2 Kgs 8.15[42] 'could very well refer to the king himself', that he actually killed himself—accidentally or intentionally'. 'Grave personal forebodings about his recovery, or unbearable pain, might have led him to such a desperate act' (1996: 549). At best, v. 15 is ambiguous, Sasson argued. The reason may have been that the Hebrew historian 'might simply have had insufficient information available to him'. With reference to Shalmaneser III calling Hazael 'a commoner', Sasson was not sure 'whether the [Akkadian] phrase *mar la mammana*, "son of nobody", must necessarily refer to a usurper who carried no royal blood at all' (1996: 551). Furthermore, Sasson emphasized that 'neither the Assyrian records—nor the Hebrew ones for that matter—accuse him of regicide'.

The stories of Jehu and Hazael are termed by Sasson 'a tale of two fanatics' (1996: 551), which the biblical tradition views both 'as divinely appointed rulers' (1996: 552). The biblical tradition views both 'as divinely appointed rulers. One was to replace a physical king, the other a religiously/socially/politically decadent one. Both were in the scheme of things' (1996: 552).

Using the argument that Jehu and Hazael were allied, William Schniedewind (1996: 83b-85b) reduced the contradiction between 2 Kings and the Dan inscription as to who killed the kings. According to Schniedewind, Jehu actually did it, but in collusion with the Aramean king Hazael, who could, as a corollary, claim that he did it. 'Practically speaking, this could only have been accomplished with the tacit approval, if not the direct assistance, of the Aramean leader', he argued (1996: 85).

In the opinion of Kottsieper, it is unlikely that Hazael did this in his own disfavour ('ist es unwahrscheinlich, daß er [Hazael] diese Tat zu unrecht für sich reklamierte', 1998: 488). He argued for accepting the Tel Dan inscription as the preferable primary source: Hazael was their assassin, not Jehu. Yet, Kottsieper maintained, the biblical account should not be refuted, because Jehu obviously was an ally of Hazael,[43] and therefore could have been the actual executor of the killing—on Hazael's behalf. There could, Kottsieper argued, have been an historical kernel in the story of the anointing of Jehu. There was, according to 2 Kgs 8.7-15, contact between Elisha and an opposition faction in Damascus, which, obviously, made the contact

41. See 2 Kgs 8.7-9; cf. 1.1-4.

42. The verbs are ויקח, 'and he took', ויטבל, 'and he dipped', and ויפרשׂ, 'and he spread' (all of which are *wayyiqtol* forms).

43. Cf. 2 Kgs 9.14, where Jehu 'conspired' (ויתקשׁר) against Jehoram, which, to Kottsieper, indicates an alliance with another potentate, most likely Hazael.

between Hazael and Jehu possible. Jehu was, on the one hand, an agent of Hazael, and on the other hand, engaged by opposition groups to rebellion (1998: 491-92). This double position also explains why the alliance between Jehu and Hazael did not hold for long—Jehu had his own royal ambitions. For Kottsieper, this is what caused the military battle, which Hazael justifies in the Dan inscription.

André Lemaire (1998) argued that 2 Kings 9 is close to the events narrated, while the Tel Dan stele was probably engraved 20 to 30 years later. The inscription is supposed to recount Hazael's boast, implicitly saying that Hazael was responsible for the assassinations.[44]

Paul E. Dion (1999: 151) identified Jehoram as the killer, based on 2 Kgs 8.28-29 and the northern spelling of the name of Jehoram as יורם, not יהורם, while Jehu would be spelled יהו, without a final א (*'aleph*), as in Hebrew. Dion found this opinion confirmed by 1 Kgs 19.15-16 (cf. v. 17), which connects Jehu to Hazael via Elijah. Dion pondered whether they were in league (1999: 153), and found his theory reinforced by the events described in 2 Kings 8, which presupposed a need for intelligence by Hazael (cf. Schniedewind 1996: 85). He doubted whether Hazael really was a usurper on the throne of Damascus, as this notion is solely dependent on one single Assyrian source, which calls him a 'son of a nobody'. For Dion, that expression does not necessarily imply that he killed his predecessor; 2 Kgs 8.7-15 could be propaganda (cf. Lemaire 1991: 95-96).

According to Nadav Na'aman (2000) the discrepancy between the Dan inscription and 2 Kings 9–10 as to who killed the kings Jehoram of Israel and Ahaziah of Judah, Hazael or Jehu, should be seen in the context of the unstable and rapidly changing political situation in the Middle East between 850 and 840 BCE, a situation which he described in brief (2000: 100). The efforts at harmonization attempted by, among others, Yamada (1995), Schniedewind (1996) and Halpern (1996)[45] are refuted by Na'aman (2000: 101-102). In principle, he was not in favour of harmonizations between the Bible and extra-biblical sources. The friendly relations and co-operation between Jehoram of Israel and Ahaziah of Judah[46] are confirmed by Hazael in the Tel Dan inscription. Na'aman, therefore, maintained that 'the testimony of the Hazael inscription should be adopted as the point of departure for the historical discussion'. There is no question that the two narratives[47]

44. Here Lemaire relied on Halpern (1996) and Schniedewind (1996) who referred to similar ambiguity in the Assyrian sources. The Bible has a comparable ambiguity with respect to who killed Goliath, David (1 Sam. 17) or Elhanan (2 Sam. 21.19).

45. They argued that Jehu killed on behalf of his suzerain Hazael. Cf. Na'aman's counter-arguments (2000: 102). He wrote, erroneously, *ktl* for *qtl*, 'kill'.

46. See 2 Kgs 8.28-29; 9.14-28.

47. Cf. 1 Kgs 22.1-38 and 2 Kgs 9–10 and the studies of Lipiński (1977, 1978, 1979).

do not accurately reflect the chain of events which culminated in the death of the kings of Israel and Judah in battle. According to Na'aman, Hazael's contemporary inscription should be accorded primacy over the biblical prophetic narrative.[48] In spite of Hos. 1.4, which, for him, confirms Jehu's revolt, 'albeit very cautiously and tentatively, [there is a] possibility that the time lapse between the actual events and the writing of the prophetic story led to confusion between the death of the two kings in the battle against Hazael and the story of Jehu's rebellion' (2000: 104). At the time the prophetic story was written down, Na'aman argues 'the historical memory had grown blurred'.

Also Eduard Lipiński (2000) pointed out that there is a discrepancy between the Dan text and the biblical account as to who killed the two kings, Jehu or Hazael. The slaying of kings on the battlefield was not exceptional for Lipiński, but after discussing the biblical references, he concluded that the inscription 'very likely corresponds to the actual course of the battle, while the prophetic tradition adapts the facts to a theocentric vision of history' (2000: 380).

With reference to Hazael's booty inscriptions,[49] Gershon Galil (2001: 17) saw no reason to doubt the biblical source concerning the revolt of Jehu, and emphasized that there is 'neither reason nor logic in the claim that Hazael was responsible for the death of Ahaziah and Yehoram, who actually was murdered by Jehu'.

Finkelstein and Silberman (2001: 202) argue that 'is difficult to know for sure' whether it was Hazael or Jehu who killed Jehoram, noting that 'Hazael may have seen Jehu as his instrument, or perhaps memories of the two events became blurred together during the two hundred years that passed until the first compilation of the Deuteronomistic History'.

Since Athas (2003) offered a later dating of the text, he proposed an alternative solution for who killed the kings. Athas, whose reconstruction of lines 7 and 8 differed from the one proposed by the Editors, saw, just one king assassinated, Jehoahaz ben-Jehu, who died in 798 BCE (2003: 269).

If the contradiction between the Tel Dan inscription and the Deuteronomistic Historian as to who killed the two kings is at all possible to solve, the solution is probably to be found in a possible alliance between Hazael and Jehu.

48. In another study (1999: 11) Na'aman was even clearer: 'I believe that the contemporary inscription of Hazael should be given precedence over the late, highly literary, prophetic story of Jehu's rebellion'.

49. See Eph'al and Naveh 1989.

3. *General Summary*

An important debate on the significance of the Bible as an historical source has repeatedly been raised in the scholarly debate, in this case the Deuteronomistic, or Primary, history. Some scholars almost totally dismiss the Bible as too theologically biased to convey any historical information at all. This is the 'minimalist' position. Most scholars, however, take a more moderate attitude. Some of them have been accused by the 'minimalist' flank of 'fundamentalism'. In my view, this is an exaggeration. We have seen no contribution from any fundamentalist scholar in this debate. Contributors referring to the Bible as an historical source have used it critically, with higher or lower regard for its value as an historical source.

Some scholars have used palaeographic analysis for dating the text. That is particularly the case with Athas. Confidence in palaeographic analysis as chronological evidence vacillates, and the results, in the case of dating, vary.

As for the dating of the Tel Dan stele, proposals vary by around a century. Most scholars date the stele to the latter half of the ninth century. With a few exceptions, particularly from the Copenhagen scholars and Athas, there seems to be relatively high consensus about this dating.

Opinions differ as to whether lines 7-8 refer to named kings of Israel and Judah, but there seems to be a majority supporting that reconstruction of the text.

Most scholars attribute the origin of the Tel Dan stele to King Hazael of Damascus. But some have also argued for one of the Benhadads, while others are agnostic in this question. For instance, Knauf (1996: 10) claims that do not know at present who the author was.

Today, most scholars seem to agree that *bytdwd* refers to David in some way, either to the Davidic dynasty or as a designation for Judah, as a parallel to the kingdom of Israel, which is also mentioned in the text. Some would claim *bytdwd* is a toponym, a proposal which is energetically refuted by others. That *bytdwd* could refer to a deity is an idea with few adherents today, there being no evidence to support it. That *bytdwd* lacks a word divider is not seen as a significant problem in most recent treatments of the inscription, vacillation in the use of word dividers being well documented, possibly in the Dan text itself. Accordingly, using the lack of a word divider against reading *bytdwd* as 'House of David' should be regarded as an out-dated argument.

The identity of the originator's father is still unknown, but most scholars think they can identify two assassinated kings in lines 7-8.

Summing up these results, I conclude that the debate has brought to light as well as confirmed some concrete historical information. At the same time, it has also raised questions we have not yet been able to answer—and perhaps never will.

Although the debate will continue, and many problems are still left unsolved, we know a little bit more about the relations between Israel–Judah and Aram–Damascus since the discovery of the Tel Dan inscription.

In the opinion of this author, there is good reason to designate Judah according to the Davidic dynasty. The Deuteronomistic Historian had a dynastic concept related to David and his successors on the throne. What the Arameans knew, or thought, about this concept is obscure to us. Yet the Arameans could have observed that, by the time of the erection of the stele, there had been Davidic kings on the throne of Jerusalem since the time of David himself. There would have been more reason to call the Davidic kingdom *bytdwd* than to call the kingdom of Samaria *Bit Humri*, since the Omride dynasty had a shorter history and a weaker ideological, or theological, basis.

Chapter 10

General Deliberations

Soon after the Tel Dan inscription's discovery, Hans-Peter Müller summed up the main points of the debate as questions of philology, history and history of religion (1995: 121). These and other questions have formed the headlines in this investigation. In my general deliberations I will now draw out some general ideas and conclusions from the debate.

1. *The Validity of Archaeology for Biblical and Historical Studies*

The validity of archaeology for biblical and historical studies has not been a main theme in this debate, even though it was archaeologists who discovered the inscription and edited the text. Nevertheless, archaeology is basic to this debate, and the inscription has been extensively debated by professional field archaeologists as well as 'desk' archaeologists. Their mutual confidence in each other has not always been patent. Dever has charged the minimalists, in general, for being amateurs in archaeology.[1] The same charge has been levelled at Davies by Rainey (1994).

1.1. *What is Archaeology?*

Archaeology is a secular science investigating old, or ancient, cultures on the basis of items, texts, stratigraphy and other material remains. Rainey (2001: 149) defined archaeology—somewhat flippantly—as 'the science of digging a hole and the art of spinning a yarn from it'.[2]

Archaeology is not an exact science. Even though the finds are concrete, they have to be interpreted. Archaeology is an independent science, but can be related to different spheres, such as architecture, industry, pottery and so on, including biblical scholarship. That archaeology is not an exact science, but very much dependent on interpretation, is well illustrated in the debate we have followed here. Archaeological 'facts' are nothing more than the

1. Cf. Dever 2000a, 2000b.
2. For a more profound definition and description of archaeology, see, e.g., J.H.C. Laughlin, in *IDB* I: 232-47 (232).

artefacts themselves and the level of consensus arrived at based on their interpretation. The real 'facts' are really only the items and the texts found. Basic questions are still wide open for debate.[3]

1.2. *Is there Such a Thing as 'Biblical Archaeology'?*

'Biblical archaeology' is a traditional designation for archaeology with some relationship to the Bible. Many theologically conservative scholars have had great confidence in 'biblical archaeology'. Today, however, many archaeologists have abandoned the term 'biblical' archaeology in favour of, among other things, 'Syro-Palestinian' archaeology.[4] With regard to this question Dever (2000a: 95) claimed:

> 'Biblical archaeology' is long since dead; its obituary has been written; and few mourn its passing. The term 'biblical archaeology' itself is currently used by mainstream scholars only as a sort of popular shorthand for the dialogue between the two disciplines of Syro-Palestinian archaeology and biblical studies.[5]

There are several reasons for this shift in designation. One is that archaeology is not primarily occupied with elucidating the Bible. Archaeology, as a discipline, should not be seen as having a particular link to the Bible at all. It should not be limited to being an auxiliary science to biblical scholarship, or theology—or to any other profession. It is an independent science, and so it should be. The scope of archaeology is much broader than merely, for example, the biblical aspect of it. It investigates an area, or a period, through its own data, using the discipline's own methods. Whether or not these data elucidate, support or contradict the Bible is of secondary significance for the science of archaeology.

The emancipation of archaeology over against biblical scholarship influences how archaeologists relate to the Bible. The attitudes to the Bible as an historical source vary among archaeologists, as the discussion of the Dan text has clearly shown. Some are more confident in the Bible as a source of historical information, while others are less confident. Some scholars, for example, the minimalists, have appeared to be very critical of any reference to the Bible, while others have been more open in their attitude to the Bible as an historical source.

3. For a definition of archaeology and the usefulness of it in relation to the Bible, see, among others, Miller 1995: 255-59.

4. As a corollary, the journal *Biblical Archaeologist* has altered its name to *Near Eastern Archaeology* (from 1998). Nevertheless, there is still a journal named *Biblical Archaeological Review*. And Israel Finkelstein frankly defines himself as a 'biblical archaeologist' (*BAR* 2002: 49).

5. Cf. also Dever 2003. For a bibliography on this debate, see Skjeggestad 1992: 160 n. 3).

1.3. *How Communicative is Archaeology?*

For some scholars, archaeology is more or less mute, while this claim is countered by, among others, Knauf and Dever. Ernst Axel Knauf has argued that if archaeology is mute, the archaeologist has probably asked the wrong question. As Knauf puts it, 'Actually, archaeological evidence is no more silent than the Torah is to somebody who cannot read Hebrew' (1991: 41). Andersen (1998: 45) remarked, with reference to this debate and the Tel Dan inscription: 'Pots are mute; inscriptions are vocal'. The archaeologist William Dever argued acidly against the 'revisionists'[6] that archaeology is not mute, but some historians are deaf! Dever isolated 'two fundamental principles of modern archaeology—stratigraphy and comparative ceramic typology—[which] can easily be defended on empirical grounds that would be considered definitive in any of the "real" sciences' (2000a: 109).[7] Dever saw a problem with the dialogue between archaeology and biblical scholarship. Some scholars would, seemingly, prefer no dialogue at all between the two professions. Lemche and Thompson, who in 1994 wrote a joint article with the sub-title 'The Bible in the Light of Archaeology', were, to Dever, a prime example of this problem. In their opinion, 'a blatant disregard—maybe systemic—of such important historical maxims has become so apparent in a number of recent article on the bytdwd inscription that one must see this as a return to some of the worst abuses of the biblical archaeological movement of the 1930s-1960s' (Lemche and Thompson 1994:7).

To counter such attitudes, Dever wrote his book *What Did the Biblical Writers Know and When Did They Know It?*, with the sub-title: *What Archaeology Can Tell Us about the Reality of Ancient Israel*. The book is a scholarly, yet popular work, one which is in particular addressed to the 'revisionists' Davies, Thompson, Whitelam and Lemche and their allegations. Dever summarized (2001a: 44-52) their positions in three points:

1. The 'biblical' or 'ancient' Israel is fictitious; an 'historical' Israel did exist, but little can be said about it.
2. They fail to identify specifically what they mean by 'biblical' Israel.
3. Their approach is consistently 'minimalist'; there is little information to salvage about 'biblical' or 'ancient' Israel.

In Dever's opinion, this attitude is nihilistic (2001a: 51). His book is an attempt to gather archaeological finds to show that such knowledge could

6. Another designation for 'minimalists'. The 'revisionists' Dever referred to are Cryer, Davies, Lemche, Skjeggestad, Thompson, Whitelam, and so on, who are not archaeologists. This quotation comes immediately after a reference to Skjeggestad 1992.

7. In his opinion, it is a pity that archaeologists have failed to publish their data in an accessible form, and do, therefore, bear the responsibility for the absence of dialogue between archaeologists and biblical scholars.

not have been known to authors allegedly writing in Persian and Hellenistic times. For Dever, the intention of 'biblical archaeology' is not to 'prove the Bible', it is rather an attempt to gather facts from archaeology to show that a host of *realia* described in the Bible is not there by chance.

Dever claimed not to be a 'biblicist' or a 'fundamentalist', but, simply, an archaeologist, arguing against the 'revisionists' allegation that it is impossible to read historical information out of the Bible, for example, for the period of the Dan stele, that is, the ninth century. Dever clearly had a mission in writing his book, namely, to combat the 'revisionists'.

1.4. *Conclusion*

The Tel Dan stele is not a find from 'biblical archaeology'. The stele is an important artefact discovered by chance in a scientific excavation at Tel Dan. Archaeological work is digging soil to reveal ancient cultures. Archaeologists may have ideas about what they expect to find, but their programme is not about proving the Bible. In the case of the Tel Dan inscription, archaeology has, by chance, shed light on the relationship between Israel and Aram in a certain period that is also known to us from the Bible.

2. *Is It Possible to Reconstruct a History of Israel?*

Most of the contributors to this debate have in some way related the Dan stele to the history of ancient Israel and/or the general history of the ancient Middle East at the end of the ninth century BCE. This will not be recapitulated here. Here, I will limit myself to deliberating more generally on whether it is possible to reconstruct the history of Israel, Judah and Aram-Damascus for the period of interest, with particular attention to the Bible and the Tel Dan inscription as historical sources. I have quoted Gershon Galil (1996: 32) and others, scholars who have underlined that the period of the history of Israel with which we are concerned here is difficult to understand properly: 'The time frame of the short period from the death of King Ahab of Israel to the revolt of Jehu is one of the most difficult and complicated subjects in the Biblical chronology. The Biblical data are contradictory and appear to be erroneous' (1996: 32).

In general, the study of the history of ancient, or 'biblical', Israel is currently a discipline in crisis. There are different conceptions of 'history', or whether a 'history of Israel' in pre-exilic times can, actually, be written at all.[8] To oversimplify the situation somewhat: according to traditional

8. See, e.g., Edelman 1991 and Grabbe 1997. In the Nordic countries this question was discussed at a symposium at Gran, Hadeland (Norway), 15–17 August 1993. The papers presented at the symposium were published in *SJOT* 8.2 (1994).

presentations, it is possible,[9] but, according to the minimalists, or 'revisionists', it is impossible.[10]

Lemche (1995a: 108) claimed:

> Wir reden heute nicht mehr von einem Paradigmenwechsel, der nur einzelne Teilen der alttestamentliche wissenschaft erfaßt hätte, sondern von einem totalen Wandel, der sowohl mit Geschichte und Religionsgeschichte als auch mit Literaturgeschichte zu tun hat ('Today, we are no longer talking about a paradigm shift that only has to do with certain bits of Old Testament knowledge, but about a complete change that has to do with history, history of religions and even literary history').

This debate today influences most of the specialist literature on the Hebrew Bible, including the students' text-books. The tendency nowadays is a turn-away from the explicit 'history of Israel', replacing that either with an increasing attention to the narratives and the biblical 'story', or, on the other hand, a general history of the ancient Middle East, or a history of 'Palestine' (cf. Whitelam 1996).

2.1. *What are 'History' and 'Historiography'?*

What constitutes history and historiography is debated a lot nowadays. We cannot enter a particular epistemological discussion of such questions here. Traditional 'histories of Israel' have not been particularly occupied with epistemological questions as to what history, or historiography, is. The 'history of Israel' historiographers have not often participated in the general epistemological debate on the concepts of history or historiography.

Here 'historiography' is understood as the description of 'history', and 'history' is understood as a reconstruction on the basis of historical sources, describing not just what happened, but also why it happened in the particular historical context in which it did, and how that which happened, has been, and is, perceived. 'History' is not a static collection of historical data, but a dynamic process in a sequence of cause and effect. To write 'history' we need all possible and available historical sources, because all sources merely tell us a piece of reality. To complete the picture—or, we might say, 'movie', since historical reconstructions are, like motion pictures, made up of individual 'frames'—we need a series of data. A complete picture is more illusionary than a four-dimensional movie. To describe how 'history' really was will always be an unattainable goal. This, also, has to be in the historian's

9. See, for example, in the German Alt–Noth tradition (e.g. Noth 1950; Herrmann 1973), or in the American Albright tradition (e.g. Bright 1959; Anderson 1998).

10. See, e.g., Davies 1992, Whitelam 1995, Thompson 1992, 1999 and Lemche 1998. Cf. Lemche 1998a and 1998b.

mind when a 'history of Israel' is written. Such questions have also been discussed with relation to the Tel Dan inscription. The core of this debate is whether history writing should be called a scholarly art, rather than a science.

Bob Becking (1997) made two significant remarks with respect to this question. First, he noted that the historical events are not repeatable. It is impossible to experiment with history. Second, there is always a particular person involved in history writing, the historian. The historian is always a subjective interpreter of the data. Different historians interpret differently. Historian A may relate date 1 to date 2, while a historian B may relate the same event to date 3. 'This kind of small scale differences or preferences will lead to large scale differences when it comes to reconstruction' (1997: 68). Becking found the discussion on the Tel Dan inscription to be an excellent example of this problem. 'On a methodological level, it is here that the discussion between "maximalists" and "minimalists" finds its place.' This is aptly illustrated by the debate between Becking and Wesselius on the Jehu question.

Jan-Wim Wesselius (2001: 83) presented some important points of view on comparing historical data, points which should be noted briefly:

> There is a principle for interpreting difficult or fragmentary new epigraphic texts which is rarely stated explicitly, though not a few scholars unconsciously have recourse to it when elucidating such documents, namely that if an element in the text resembles a very common word, expression or episode, the very first option to be explored is that it is identical with it. Neglect of this principle can occasionally lead to what one could irreverently call a collective wild-goose chase after various other possibilities, not rarely by distinguished and highly capable scholars.

This is what he has seen in some of the debate on the Tel Dan inscription. As for the Hebrew Bible and history in general, which Wesselius discussed thoroughly (2001: 91-103), he concluded, with particular attention to the Jehu story:

> The biblical description of what happened on the road to Jizreel on a fateful day more than 2,800 years ago, still more than four centuries before it was made part of Primary History, can be true or false or anything in between, but it is certainly a meaningful and intended text. If, then, the account of events in the Tel Dan inscription can be reconciled with it, it certainly is very likely that both ultimately go back to comparable traditions, and it becomes rather likely, though by no means certain, that they reflect historical reality (2001: 103).

Wesselius's conclusion must be read with his theory that Jehu is the author of the text in mind.

A middle course between 'maximalism' and 'minimalism' is sought by Dever (2001a), who refused to follow the conservatives or the 'revisionists'. His book, *What Did the Biblical Writers Know and When Did They Know It?*, is primarily written to debunk the 'revisionists', who are labelled 'nihilist'[11] and 'historians' with no history.[12] For Dever, '…the revisionists do not intend merely to rewrite the history of ancient or biblical Israel; they propose rather to abolish it altogether' (2001a: 4). Dever's aim was to demonstrate that there are plenty of convergences between the biblical text and archaeological finds, and that these accumulated data, when taken together, have something important to say about the history of ancient Israel (2001a: Chapter 4).

Dever did not accept the Deuteronomist's story as history at face value. The biblical stories are, for Dever, theologically, and otherwise, biased. Yet, on the other hand, we should not undervalue the oral tradition as an historical source. With reference to Susan Niditch (1996), Dever claimed that 'the burden of proof in denying the role of the earlier oral tradition in biblical historiography must fall on the revisionists and other minimalists' (2001a: 280). The following conclusion from Niditch he calls 'must reading for anyone who wishes to confront issues of literature and history in the Bible' (2001a: 280):

> Recognition of Israelite attitudes to orality and literacy and of the complex interplay between the two forces us to question long-respected theories about the development of the Israelite literary traditions preserved in the Bible… Given this assessment of Israelite aesthetics and the importance placed on the ongoing oral-literate continuum, source-critical theories become suspect, as do other theories about the composition of the Hebrew Bible that are grounded in modern-style notions about Israelites' uses of reading and writing (Niditch 1996: 134).

As for the 'revisionists', Dever claimed that 'they do not understand that the late *editing* does not necessarily mean late *composition*, much less a late origin for the *tradition* as a whole' (2001a: 280).

What is left of the history of ancient Israel, as Dever sees it, is an historical 'core', which he presents in Chapter 6 of his book.[13] This is much more than the 'revisionists' accept,[14] but not as much as conservatives, the real 'maximalists', or 'fundamentalists', want to see. Yet it is more than

11. This is a frequently repeated contention in his book, cf. Dever's index.

12. See particularly Dever 2001a: Chapter 2, 'The Current School of Revisionists and their Nonhistories of Ancient Israel'; and 1999; and Whitelam 1996.

13. See particularly Dever 2001a: 267-71. The 'core' Dever finds is less than Whitelam (1996) finds, as Thompson (2001: 306) commented.

14. Though, Thompson (2001: 306-307) objects to Dever on this point.

sufficient to secure an historical place for David, Solomon and a 'House of David'. There is no reason, for instance, to dismiss the 'era of David' as being no more historical than many would regard the tales of King Arthur (as Thompson does). Despite many embellishments by the later Deuteronomistic redactors, the main elements of the story probably derive from ancient sources and depict actual conditions at the time. It is difficult, if not impossible, to explain how Jewish writers living in Palestine in the third and second centuries, when remnants of Israel had not experienced kingship for some three or four centuries, could have made up such complex stories out of thin air.[15]

Another middle course between the 'maximalist' and 'minimalist' positions is sought by Israel Finkelstein and Neil Asher Silberman (2001). These scholars had no particular 'mission', like Dever, to combat the minimalists. Finkelstein defines himself as a 'centrist' and 'biblical archaeologist', deploring the fact that he is attacked by both the minimalists and the maximalists (*BAR* 28.6 [2002]: 45, 49). Thompson commended Finkelstein and Silberman for making 'a great effort to avoid polemic' (2001: 316)—in contrast to Dever, who is rather critical of them: 'What we have in [this book] is an ideological manifesto, not judicious, well-balanced scholarship' (Dever 2001b: 60; see also *BAR* 27.2 [2001]: 60). And yet, like Thompson, Finkelstein and Silberman have, in general, found little historical evidence from pre-monarchical times.[16] Instead, they found an historical context for *bytdwd* in the ninth century.

In this context, Finkelstein and Silberman point out (2001: 129-30) that 'The question we must face is no longer one of David's and Solomon's mere existence'. For them, it really is possible to draw up some kind of a 'history of ancient Israel' for the time of David and Solomon and the divided monarchy. As for the details of it, there will always be debate. Historical information about the era is fragmented and disputed—and will forever be so. Yet, the era of the Tel Dan stele is not a complete historical vacuum.

2.2. *Conclusion*

These considerations illustrate the questions as to whether it is feasible to write a history of Israel, Judah and Aram-Damascus for the period we are studying here and indicate that it is not an impossibility. The Tel Dan

15. See Dever 2001a: 268-69.

16. See his review in *BAR* 27.2 (2001: 60, 62): 'What is now needed is a truly innovative and comprehensive history of ancient Israel, produced not by an idiosyncratic and doctrinaire archaeologist (Finkelstein) and a popular journalist and writer (Silberman), but by mainstream archaeological and biblical scholars. This book's heart is in the right place, but its head is hasty.' Is this a discrete reference to Dever's own 2001 book, one wonders?

inscription can be used together with the biblical tradition to illuminate this particular period in the life of Aram, Israel and Judah. Archaeology has provided us with artefacts and texts, such as the Dan inscription. Artefacts and texts are concrete evidence, but what archaeology finds is more or less accidental, and has to be interpreted. Artefacts have to be identified and their usage has to be decided upon. Texts have to be reconstructed, translated and interpreted. Such texts are not necessarily less biased than the biblical texts. Royal texts glorify the kings beyond historical accuracy. All this should warn the historian to be careful, as these are precarious sources. Nevertheless, it is possible to read something out of them.

When biblical tradition and archaeological finds are laid side by side, we have different kinds of evidence, ones which are often scarcely comparable. But there is at least something. It is the modern historian's concern to discern critically between the various sources and, out of them, to reconstruct what we call 'history'. We will never know for sure to what degree the pictures historiographers present are adequate, because every new find of historical significance will force us to change, or nuance, the picture we had previously. An historiographical picture of past times will inevitably be provisional—until more evidence is found. A complete picture of history will never be more than an unattainable goal—this is also true because historians continually ask new questions of previously known data.

This also pertains to the history of Israel, Judah and Aram Damascus in the period of the Tel Dan stele. We have a series of data, data which constitute the basis for writing a history of the period. These data are insufficient to write a history of the period as adequate as historiographers would like to write. The Tel Dan inscription has reminded us that there are always new aspects to discover. At the same time, something is known, and we should write the history as adequately as possible—from the limited sources available.

The biblical text is theologically biased, and this is because its intention is not to write history, in the modern sense of the word, but to use history to proclaim Yahweh's actions with respect to his people. The Deuteronomistic historians wrote a 'theological history', because they were more preachers than historians. This theological leaning decided what would be written and how it would be written. Important features of history were not recorded by these historians because they were not important to them, and some features were used in other ways than modern historians would have used them. This has deprived us of important aspects of history and has influenced our ability to write the history of these peoples. And yet, we are not left without any information at all, as the minimalists tend to opine.

On the other hand, the originator of this inscription was not himself objective either. Many details in the inscription are diffuse. The originator claims to have defeated the kings of Israel and the 'House of David', and a

number (70?) of other kings, for such and such a reason and with certain specific losses on the part of his enemies. Compared to what we read in 2 Kings and what we know in general from other sources, such information is not unreasonable (except for 70 kings!). However, whether all Hazael's details are in accordance with what actually happened, we will never know. He certainly had his particular agenda for writing what he did.

These are the uncertainties modern historians face when working with 2 Kings and the Tel Dan inscription. The difficulties are obvious, but the possibilities should not be dismissed.

3. *Epigraphic and Palaeographic Questions*

Palaeographic analysis was not undertaken by the Editors on Fragment A or on Fragments B1/B2, a situation which Cryer (1994: 3), in particular, regrets. That is not to say that palaeography has not been important in the debate, particularly as regards the dating of the inscription. Yet it is undeniably the case that palaeography has played a helping, supplementary role in the dating process. That changed with George Athas's (2003) study, which who used an epigraphic and palaeographic analysis of the stele as the main basis for dating the inscription to the early period of Benhadad III, just after 800 BCE.

3.1. *The Significance of Epigraphy*

Since epigraphy investigates the visible letters and text of an artefact, this is research basic to the understanding of a text. In the case of the Tel Dan inscription, the text is easily readable, except for in the fractures, where some of the broken letters are very difficult to identify, if identification is possible at all. Methodologically, the investigator has to make clear which broken forms actually count when identifying a letter and what letters they might be remnants of. These possibilities should then be checked against other identifiable letters from the same cluster of letters to identify a word, which should, next, be checked against the literary context. This can be an extremely difficult, but, nevertheless, absolutely necessary business. In the investigations presented here, different scholars have come up with different solutions. The task is to decide which solution is most plausible. Who is the best epigrapher will not be judged upon here. George Athas is the one who has written in most depth on the epigraphy of the stele. Weighty as his investigation is, however, we should not be immediately captivated by its comprehensiveness, because this is a scholarly profession and an artistic skill requiring experience.

In the case of the Tel Dan inscription, epigraphic analysis is particularly important since we have three fragments. It is the epigraphers business to relate the fragments to each other, to identify the fragmented letters in the

breakages, as well as to analyze, for example, how the text lines proceed from fragment to fragment. I have already discussed this issue elsewhere,[17] and have taken issue with Athas, in particular, regarding his placement of Fragments B1/B2 far below Fragment A.

3.2. *The Significance of Palaeography*

On the general significance of palaeography for the dating of a text, Millard (2000: 47) was rather pessimistic, since the results can be very diverse.[18] This is very well illustrated by how Davies and Rogerson have dated the Siloam inscription.[19] Palaeography is not an exact science, and its evidence should, therefore, be treated with caution. But we should not downplay its relative value. Even though it is not an exact, or undisputed, science, it is a science and it should be listened to.

This warning should be taken into account when reading Athas (2003) because he built much of his dating argumentation upon his palaeographic analysis. In general, Athas found that the inscription fits, palaeographically, into the latter part of the ninth century, or around the transition to the eighth century. This is, also, the impression given by a comparison with the tables in the *Handbuch der altehebräische Epigraphik*, which he probably does not know. However, to use palaeography to be more specific on the dating, as Athas did, is precarious.

In the case of the Dan inscription, palaeography—as well as epigraphy—is very important for deciding whether Fragment A and Fragments B belong to the same stele. As we have seen, some scholars doubt that they do, while the majority agrees they do really belong to the same stele—a conclusion very much based on a palaeographic analysis of the letters. The variety in the shape of the letters is reasonable since it is a human-made inscription.

Palaeography yields different results depending on the writing material and the method of writing. Idiosyncratic graphological features would probably vary more in writing executed with a pen than when chiselling. In the case of an inscription on stone such as the Tel Dan stele, the script is engraved with a chisel, possibly with a curved edge. Writing with a chisel on stone and writing with a pen on papyrus, leather or an ostracon are two very different ways of writing. An engraved text will not have the characteristic features a text hand-written with a pen has, unless the text was initially written by hand and then engraved accordingly. It would be more stylized

17. See Hagelia 2006a, 2006b.

18. Millard 2004: 47: '[T]he dates palaeographers assign to the ancient books and documents are very approximate, with a margin of 50 years and possibly more…experts can differ widely, sometimes more than a century… [D]ating by the style of handwriting is a matter of judgement and can only give an approximate answer.'

19. See Rogerson and Davies 1996.

according to contemporary conventions, and the individual letters would be influenced by possible irregularities due to harder or softer grain in the stone.

The contributors to the debate on the palaeography of the Tel Dan inscription have argued differently. Yet, apart from a few late daters, most scholars have dated the text, on palaeographic grounds, to the latter part of the ninth century.

3.3. *Conclusion*

Epigraphy and palaeography are generally important parts of textual analyses, particularly in the case of ancient and fragmented texts. They give some guidelines as to the dating of a text, providing dates that are not exact, but to within decades. While we should not draw too definite a conclusion on the basis of one single letter, or a few letters, certainty increases with the number of letters from the alphabet combined with the number of times each individual letter of the alphabet occurs, because, in a handmade inscription, there will always be individual features pertaining to each individual letter.

If we follow Millard, we would get a chronological context for dating the inscription of plus or minus half a century. This is confirmed when the letters in it are compared to the letter tables in *HAE*. If a text is devoid of historical information and its stratigraphic layer is unknown, epigraphy and palaeography would be the only historical guideline available. In such cases these disciplines are of particular importance, and could even be decisive for dating. In our case, the Tel Dan stele was found in secondary use, not in the stratum in which it would have been originally; accordingly, stratigraphical analysis merely indicates something of the stele's history after its removal from its original position. Yet the text contains significant historical information, which, for most scholars, indicates a dating in the latter half of the ninth century. In this case, palaeography has a role as a control in relation to the historical information, and leads us to ask: Does the writing here fit with the way we, otherwise, know people wrote in the latter part of the ninth century? As we have seen, most palaeographers think it does.

In the case of the Tel Dan inscription, its palaeography is of secondary importance for dating compared to its historical content, but it, nevertheless, provides an important supplementary contribution.

4. *Questions Relating to Theology and History of Religion*

The relationship between 'biblical theology' and 'religion' is an old problem, one which has been discussed, or 'solved', in different ways, at different times. It is beyond dispute that the theology of the biblical prophets and the actual religion of the people were different issues. This conflict was the

real cause for their existence as prophets. In the case of the Tel Dan inscription, there is no 'biblical theology', just 'religion', as it articulates Aramean religious beliefs. That is why we also operate with a 'history of religion' principle for this area, and era, of the ancient Middle East.

Questions relating to the history of religion, in the case of the Tel Dan inscription, primarily concern the interpretation of *dwd* in *bytdwd* and *hdd*, a debate which has been followed closely in this investigation. As for *dwd*, which some scholars have related to some deity, no such deity has been documented. Were there to be any religious content in it, it would be as an epithet for Yahweh (cf., e.g., Lemche 1995a: 103-104). In the case of *hdd*, however, the religious connotation is obvious, as this is the name of the Aramean god, Hadad. Hadad was the one who, allegedly, marched before the Aramean armies against Israel and Judah, making the war 'holy'. This matches with the concept of 'holy war' found, for example, in 2 Chron. 20:13-30. The Arameans and the Israelites had similar ideas on that concept; it was not just a war of people against people, or nation against nation, but war by a god against a god (cf. the arguments of the Assyrian commander in Isa. 36).

In conclusion, the Tel Dan stele is a royal victory stele whose inscription is not primarily a religious text. Its religiousness is limited to the author's honouring of Hadad for having made him king and for having led him in the war against his enemies. There are no explicit religious polemics in the text, unless a polemic against the God of Israel and the House of David is implied. And yet, in general, a stele with an inscription like this would be seen as having considerably more religious understatement in it than modern man would be inclined to imagine.

5. *Questions Related to Philology and Linguistics*

For various reasons, discussion of the philology and linguistics of the Tel Dan inscription is a very complicated matter.[20] One thing is the morphological classification of the individual words, which, in some cases, can be problematic. The problems are especially related to the use of the verbs. This has been a discussion more or less independent of the historical questions.

The first thing to do when an ancient text is found is to identify the language. In this case, the language is, with some reservations, identified as Aramaic. However, identification of the language itself is of subordinate significance in relation to understanding what the text actually says. Dating a text is important because it tells us from which historical period the text comes, and from which geographical area it originates, giving an historical

20. Cf. Hagelia 2006a, 2006b.

and geographical framework for studying the text itself, and for comparing it with other texts from the same era and area. Both the identification of the actual language and its dating are important for analysing the history of the language itself, but they are also of interest with respect to understanding the content of the text.

Morphology is a basic part of philology, with meaning being attached to the individual words, particularly the word roots. If we do not understand the words, we do not understand the text. In the Tel Dan inscription most individual words have been identified, even though there may be uncertainty as to the correct identification of some of them. It is also significant how the individual words are construed and how the word roots are used. This part of the investigation provides the derivative meanings of the individual words. In this case there is also some uncertainty here with some words.

The real meaning of a text is not arrived at until words are put together in the right syntactic order. This has been a difficult matter with the Tel Dan inscription for three reasons: (1) the text is fragmentary, (2) we do not always know how words should be related to each other, and (3) there is uncertainty as to the use of the verbs. This does not basically affect the general impression that the text concerns an instance of Aramean aggression against the kingdoms of Israel and Judah; a response, or retaliation, for a previous aggression from the King of Israel, with the outcome that these two kingdoms were defeated by the Aramean king, who was, probably, Hazael of Damascus.

In the linguistic debate on the Tel Dan inscription the use of the verb has proved to be the most serious problem. One problem is that grammatical terminology has not yet been settled among scholars. Prominent experts discuss terminology, disagreeing and misunderstanding each other on important matters. If this is a problem for the most distinguished scholars in the field, scholars of a secondary rank should be excused.

5.1. *The Study of the Language*

The study of the language itself is an important part of the investigation of the Dan inscription[21] since language is a basic part of culture. There is no history without culture; if there is no man, or people, there is no culture. Aramean culture was scarcely 'Aramean' without the Aramaic language. Accordingly, the study of a language is a study of a culture.

A text tells us more than the actual content of its message. The text has a language, and a language tells us something about how the users of that language think, what kind of ideas they have on issues of politics, religion, philosophy, family life and so on. This is the culture of the people using that language.

21. Cf. Hagelia 2006a, 2006b.

Languages have to be studied in relation to other languages, as cultures should be studied inter-culturally. That lead has been followed by several scholars in this case, and their conclusion has been almost unanimous: the language of the Dan inscription is a modified Early Aramaic, with features from other North West Semitic languages. Presumably, the people who used this modified language also had a culture modified by influence from their neighbours.

Our knowledge of the Northwest Semitic languages of this period is rather scanty (cf. Garr 1985). We need more texts to complete the puzzle. New texts will, obviously, provide us with new linguistic features, as we have seen with the Dan inscription. The study of a remote and long-extinct language like Early Aramaic demands not only knowledge and insight, but also some degree of inventiveness, creativity and independence, as new areas of information have to be analyzed. Victor Sasson, for instance, displays elements of such an attitude. Sasson has pointed out that, since our knowledge of Early Aramaic is fragmentary, we should be careful not to be too attached to school grammars. Such grammars are necessary, but not as reliable, in the case of languages of which we have scant knowledge, as they are in the case of familiar languages. With such a point of departure, Sasson has argued that the Aramaic *waw* consecutive was high language, used for solemn themes like war. While Sasson has met with objections to this theory, his willingness to test solutions, other than the ones previously accepted, when additional material of a grammatical feature formerly meagrely documented comes to light, is praiseworthy. In this field innovative scholarship is required.

6. *Minimalism vs. Maximalism*

A very important *meta*-aspect of the debate over the Dan inscription has been the question of 'maximalism' vs. 'minimalism'. Or, perhaps, we should turn the problem the other way around, and say that the Dan debate is the *meta*-debate and the maximalism–minimalism debate is the real issue.

It is a remarkable coincidence that the maximalism–minimalism debate arose at the beginning of the 1990s, almost simultaneously with the discovery of the Tel Dan inscription. The debate over these two themes, minimalism versus maximalism and the Dan inscription, have a mutual relationship—because the Tel Dan inscription is important for the historical questions at stake in the maximalism–minimalism debate, and because the inscription was a threat to the minimalist position. Becking (1997: 68) has claimed that 'on a methodological level, it is here[22] that the discussion between "maximalists" and "minimalists" finds its place'.

22. With reference to the interpretation of the Dan inscription.

6.1. *The* bytdwd *Debate as a Trigger*

The particular trigger causing this debate with respect to the Tel Dan inscription was the expression *bytdwd*, which has become something of a catchword in the debate over this text. If this phrase refers to the 'House of David', as most scholars nowadays argue, we probably have evidence for the dynasty of David in the ninth century. This flies in the face of the minimalists, who file David in the archives of eponymic and legendary kings.

We have seen that the lack of a word divider is no insuperable problem because of general inconsistency with the use of such dividers in antiquity. We have also seen documented that the *byt-X* formula was commonplace in contemporary inscriptions (cf., in particular, Rendsburg 1995). The scholarly mission is then to investigate *bytdwd* in its textual, as well as its epigraphic and historical, contexts.

This has been done by several scholars—with various results. But there is a clear tendency: scholars labelled 'minimalist', 'revisionist' or similar, tend to look for all possible ways to avoid translating *bytdwd* as 'House of David'. On the other hand, very few scholars outside of this camp have been so reluctant. Such an obvious tendency awakens the suspicion that this group of scholars has some kind of reason for their reluctance to translate 'House of David'. Of course, all possible translations ought to be investigated—in an unbiased way. There is no absolute proof that 'House of David', meaning 'Dynasty of David', or the 'kingdom of Judah', is the right translation. Yet, the indication is so strong that most scholars choose such a solution.

Along with the translation 'House of David', the possible reconstruction 'King of the House of David' is also open to discussion. 'King of the House of David' is rather concrete: it omits the actual name of the particular king, but does refer to a dynastic king. The simple phrase 'House of David' is a more open concept, one which could refer to a Davidic dynasty, but also to a Davidic kingdom, or the Kingdom of Judah, as a parallel to the Kingdom of Israel. In both cases, we have the reference to *dwd*, which most scholars agree refers to King David.

In this debate scholars from Copenhagen and Sheffield (especially Davies) have been the most staunch minimalists, or 'revisionists', the term they seem to prefer for themselves.[23] To them, the interpretation of the Tel Dan inscription is particularly important because it is a possible threat to their minimalist stance. It is in their interest to lower the dating and to explain away *bytdwd* as not referring to the historical David. Apart from a few other, not so vocal, scholars, defenders of a minimalist position have

23. According to *BAR* 29.2 (2003): 58, Diana Edelman from Sheffield claimed, at the *SBL* Annual Meeting in Toronto 2002: 'Stories about Sheffield as a bastion of minimalism are greatly exaggerated'. She even claimed King Saul to be 'an historical person and not merely a fictional character', according to *BAR* 29.2 (2003): 57.

been in a minority—and not all of them were even 'real' minimalists, either. There has been relative silence from their position on the *bytdwd* issue since the mid-1990s, with the exception of occasional remarks by Lemche (1998) and Davies (2000).

To call the debate between the minimalists and maximalists a fight over the authority, or inspiration of the Bible, is too narrow. None of the alleged maximalists referred to in this study would call themselves a fundamentalist, or argue from the idea that it is possible that the Bible is inspired. This is something else. On both sides of the front line we find historians, Bible scholars, archaeologists, epigraphers and linguists working with the same scholarly and historical-critical methods—yet coming to very different conclusions, indeed!

As far as the Tel Dan inscription is concerned, the minimalists do not seem to be winning the battle.[24] The Davidic Dynasty, and even the historical David, seems to have been confirmed, as have, possibly, the kings Jehoram of Israel and Ahaziah of Judah. A dating of the stele in the latter part of the ninth century seems to be in accordance with a general historical picture of the historical situation in Aram–Israel around that time.

Niels Peter Lemche (1995a: 108) has indicated the possibility for future dialogue between 'classical' scholarship and representatives of the new scholarship on the history of Israel. In his article, the labels 'minimalism' and 'maximalism' are not used. In Lemche's formulation it is absolutely necessary for a dialogue between the phalanxes that a basis for the discussion is agreed upon and that the agenda is appropriately defined. Self-evidently, there has to be a real will to a dialogue. Lemche's firm opinion is that the initiative for a dialogue should come from the 'classics', with everyone concerned recognizing that this concerns a total transformation of Hebrew Bible scholarship.

6.2. *Conclusion*

In spite of the views espoused by the minimalists, a conclusion seems inevitable: A century after King David lived there was a dynastic Davidic kingdom, known to an Aramean king, probably Hazael. This dynasty and its king ruled over a country mentioned together with Israel. The actual king's name is not mentioned in line 9, but he, possibly, appears in line 8: Ahaziah,

24. In spite of all their central positions, the scholars consulted by the *Biblical Archaeological Review* on their 25th anniversary (*BAR* 27.2 [2001]) agree that the radical minimalism referred to here will soon disappear. Their argument seemed to be that extreme opinions are not sustainable in the long run. Some of the minimalists' opinions will gain acceptance, but responsible scholarship will see through the emperor's new clothes, and see that they are, in terms of scholarship, naked. Importantly, *BAR* had questioned scholars who would not identify themselves as minimalists.

son of Jehoram.[25] The minimalist–maximalist controversy has not produced very much that is new. It has been more deconstructive than edifying. The possible gains have been that their often rather extreme opinions have, at least, been discussed—and often defeated.

So what? Has this simply been a struggle to defeat a scholarly adversary?

The fault of the minimalists has been that they have stripped the text down radically, reducing it to the naked 'facts'—a position which is right and important, in some ways, but which, on the other hand, deprives scholarship of imaginative investigation of the texts, tending to make things sterile and barren. Scholarship needs imaginative and innovative incentives, otherwise it risks being left behind in an eddy. This being the case, Puech is to be commended for his qualified fantasy—even though he seems to let his imagination run away with him. He was probably as well aware of his use of fantasy as the opposing minimalists were. Perhaps we should listen to Gottwald (2002), who took the spectator's position, commenting that the minimalist–maximalist dispute is more a question of teleologies than of historical facts. By 'teleological' he was referring to how 'we contemporary historians … shape our visions of Israelite history according to the various "end points" at which we stand in the long, ongoing history of Biblical interpretation.' It is a question of ideological agenda.

7. *The Sociology of the Debate*

Debates also have their sociology. Dever (2001a: 262-64) even debated the psychology of the debate in a sub-chapter titled 'Deconstructing Deconstructionism'. There is not only a general debate; there are also interactions between 'schools'. Participants in a debate identify themselves with teachers, colleagues, friends, politics, philosophies and so on. What has characterized the sociology of the Tel Dan text debate? Who participated in the debate? What were their interrelationships?

The contributions to this debate are of varying natures. Some contributions (mostly articles) are comprehensive and extensive, and George Athas has even written a book specifically on the Tel Dan inscription, debating a broad variety of aspects. Other treatments of the stele are short notes on a single topic, while still others are chapters or short sections in books. And yet, in their own way, they are contributions.

The different categories of publication should also be carefully noted. As a rule, I have not commented on whether a contribution is regarded as 'important', or 'less important'. 'Importance' is not decided solely by size, or comprehensiveness. A long and comprehensive article could refer simply

25. Cf. the reconstruction produced by Galil 2001: 19.

to everything available as a matter of course; it would be a good 'catalogue'—but not much more. Much of the present work will be understood as precisely this kind of 'catalogue'—and is partly intended to be so. Also, brief remarks can be very important, sometimes setting a new course for scholarship.

What counts in sociological charts are not only the individual and naked pieces of information, but the totality of the data, and the patterns it is possible to read from them. Questions to be asked here are: Who has participated in the debate? What do they represent? Where, and how, have they been published? Why have they been published? What has been their primary interest?

As for the origins of the contributions, there is a preponderance from Israel and the United States. Next come Canada, Denmark (Copenhagen), the Netherlands and the United Kingdom. The Dan inscription is, of course, very important to Israel, not least because it is an archaeological find with an epigraphic text directly related to the history of ancient Israel, written in a language related to Hebrew, and Israel has many famous archaeologists and epigraphers capable of a qualified opinion on this stele and its inscription.

Geographically, the scholars labelled minimalists are mainly confined to Copenhagen and Sheffield—at least as far as the debate surrounding the Tel Dan inscription is concerned. Some minimalist sympathy is, of course, to be found elsewhere, yet the main axis remains Copenhagen–Sheffield.

The minimalists named here are influential and loud-speaking. They sat as Editors of *SJOT* and *JSOT*, and were central in the formation of Sheffield Academic Press, a publishing house which had a significant publishing programme—not only of literature with minimalist leanings. Their books and articles received a lot of publicity and they lecture, frequently, worldwide. They belong to the Hebrew Bible scholarship's jet-set. The Copenhagen–Sheffield alliance is notable. The scholars involved are Thompson, Lemche, the late Cryer (Copenhagen), Davies and Whitelam (Sheffield), with the Copenhageners taking the most active part. Sheffield has contributed to the debate more indirectly through the journal *JSOT*, while *SJOT* has close links to Copenhagen. In contrast, nothing has been heard from the other Danish theological faculty in Århus—or from the two faculties in Sweden, in Uppsala and Lund. The only voice from Norway has been that of Hans M. Barstad of the University of Oslo (now in Edinburgh).

As for the publications, the activity has been most intense in the journals *BAR*, *BN*, *IEJ*, *JSOT*, *SJOT*, *UF* and *VT*, and remarkably low in *ZAH* and *ZAW*. *BAR* and *IEJ*, which both focus primarily on archaeology, have followed each other closely, with the editor of *BAR* writing several articles based on material published in *IEJ*. The editor of *BAR*, Hershel Shanks, is regarded, by many, as something of a 'missionary' or 'crusader', fighting,

among other things, the minimalist position. Shanks is also interested in the Dan text from a journalistic point of view—as news. *BN*, *JSOT*, *SJOT* and *UF* have published more general debate, with several substantial contributions. Studies on particular philological questions have appeared especially in *UF*, *VT*, *ZAH* and *ZAW*. Some important journals, such as, for instance, *JBL*, have been completely devoid of articles on the Dan text.

Behind such statistics it is possible to read alliances, different emphases and so on. Not too much should be read out of such statistics, however. In questions where much scholarly prestige is at stake, as is the case of the Tel Dan inscription, things are said not only in the actual texts, but also—and not least—between the lines. There are many ways of reading the contributions in such a debate.

As we have seen, several scholars have contributed to the debate on the Tel Dan inscription. Nevertheless, it is not as easy as could be expected to name those scholars who have set the tone of the debate. Indeed, while they have not necessarily set the tone simply by dint of being frequent contributors, the Editors, Abraham Biran and Joseph Naveh, should be explicitly mentioned. Due to the fact they were the Editors of the inscription, most scholars gravitate towards or at least feel obliged to engage with Biran and Naveh's solutions. Had another scholar been the editor, he would have set his own distinctive tone.[26] The Editors have been criticized on several points, not least by the minimalists, but they are both recognized as prominent scholars in their respective fields, as epigrapher (Naveh) and archaeologist (Biran), with the late Biran being hailed as the grand old man, Mr. Dan Archaeologist himself.

In their way, the minimalists have also set a tone. They have somehow been the 'heretics' in this debate and have received criticism. While it has been said that it is generally the heretics who have brought the world forward in knowledge and acknowledgment, this can scarcely be said of these 'heretics'. Their positions will probably not be the solutions followed by scholarly posterity. And yet their, at times, aggressive questions have raised several problems that needed to be discussed. They will probably not agree that they are the losers, but that is how it looks as I write. While I do not find myself in agreement with their positions, they have at least contributed to getting some questions, questions which needed to be investigated, explored.

26. The use of masculine language is intentional here. As I noted above, very few female scholars, if any, have engaged with the Tel Dan debate.

8. *How Important is the Tel Dan Inscription, Apart from in its Relationship to the Hebrew Bible?*

The current interest in the Dan inscription is nurtured by its close relationship to the Bible. Any text with such intertextual links would, inevitably, stir biblical scholarship—Jewish as well as Christian.

What of the inscription, if we had no Bible? The question is hypothetical and difficult to answer, not least because the biblical connotations are so embedded in our way of thinking about this part of the world at this time. Some considerations, however, do stand out.[27]

Had a text like this been found in Egypt, Turkey, Iraq or Iran, it would probably not have received much attention. These countries abound in monumental inscriptions, and so a text like the Tel Dan inscription would have been just another exemplar. In contrast, the archaeological excavations of Palestine have not offered many monumental inscriptions from that ancient time. The Mesha stone and the Deir ʻAlla inscription were the most famous examples before the Tel Dan inscription appeared. A monumental inscription from this area would automatically be met with wide scholarly attention. The discovery of the inscription caused Miller (1995: 257) to express the hope that 'sooner or later one of the Palestinian tells will produce archives'.

Apart from its connection to the Bible, the area where the Dan inscription was found does not have a history of the same worldwide interest as the ancient history of the Hittites, the Assyrians and the Babylonians, Persians and Egyptians, which were great empires in their own eras. There has probably been an underestimation of the importance of the role of the Arameans in world history, because they were located on the outskirts of the core areas of the greater kingdoms and the biblical world proper.

Without the Bible, we would not be able to identify the author of the inscription as Hazael. We would not have known much about the conflicts between the Arameans and Israel–Judah, or the *bytdwd*. The two kings in lines 7 and 8 would have been unknown to us, even if their names had been complete. Our only knowledge of Israel and Judah from the period would have been what we can read out of the historical sources found in museums worldwide, sources such as the Mesha stone, the Black Obelisk and some other inscriptions. Our knowledge of the ancient Hebrew language would have been limited to that which we find in ancient Hebrew inscriptions, as documented particularly in the *HAE* collection. As Miller rightly notes, 'were we entirely dependent on the archaeological evidence narrowly defined, we would not even know that ancient Israel existed' (Miller 1995: 257).[28]

27. Cf. Hagelia 2004b.

28. For a summary of the most important epigraphic discovery prior to the unearthing of the Tel Dan inscription, see Miller 1995: 251-55.

On the other hand, the Arameans were probably of greater importance than modern scholarship is aware. Admittedly, they never built an empire because they did not organize themselves that way—they were always subdued by neighbouring 'superpowers'. Instead of centralizing their power behind great kings, they existed as smaller city-states, of which Damascus was the most important. They rarely left great monumental relics such as those left by their great neighbours.

Yet the Arameans were not of lesser historical importance. This is not least documented by the importance of their language. Aramaic gradually developed to become a universal language. At an even later stage, it was the language of Jesus and the vernacular of the contemporary Jews; a language we know very well from these later stages, and which continues to exist, in various forms even today. Because the Tel Dan inscription is written in Early Aramaic and is well enough preserved to offer evidence of linguistic particularities, it is a very important piece of documentation of this language at a very early stage. This is not least documented in the debate surrounding its philology. The stela adds to the number of very early inscriptions in Northwest Semitic languages, and is a very important document for understanding the development of the languages in the area at this time.

As for its content, that would, of course, add important information about the general history of this era—for instance, about its military history and, to a lesser degree, its history of religion.

So, even if its importance would have been considerably reduced without the Bible, it would, nevertheless, still be an important historical document. Historians would still have to refer to it when writing the history of Middle East in ancient times. And yet, without the Bible, there would, in general, be less interest in the ancient history of the Middle East.

Chapter 11

Epilogue

A lot has been published on the Dan stele, as this investigation should clearly illustrate. Dietrich (1997b: 24) talked about 'an incomparable spring tide of publications'. The debate around this inscription has been comprehensive, intensive—some might say, toxic at times. Thompson (2001: 307 n. 4) wrote about 'personal hostilities'—with explicit reference to Dever. Numerous accusations have been thrown about in this, in some ways, merciless cat-and-dog-fight.[1]

The Tel Dan text is, to this writer, until the opposite can be proved, to be considered as three fragments of one and the same inscription. It is one of the most important texts ever found in Israel–Syria. For biblical scholars it ranks alongside the Mesha stele in significance. It is the most important text discovered since the Dead Sea Scrolls in the mid-twentieth century.

After Athas (2003), there was not much more to say, it seemed, except that his book received a devastating review from Victor Sasson (2005), who criticized him for arrogance (in his aspiration to have written a definitive study of the inscription), minimized reading (in adopting the methods of the deconstructionists), having a negative attitude to the Bible ('an unknown quantity at best and a pure fabrication at worst'), and for saying that *bytdwd* does not refer to 'House of David' but to Jerusalem itself, and because of his view on the absence of the *waw* consecutive. His book is 'generally disappointing, neither definitive, nor authoritative', according to Sasson (2005: 23). Furthermore, Sasson argues, the inscription itself does not deserve a book of 331 pages, and Athas's book is not worth its high price, while its content could have been reduced to an article or two (2005: 33). Sasson's review was countered by a response from Athas (2006) in which he claims that he does not hold a 'minimized reading' and actually argues against the 'minimizers', the inscription, for him, providing 'good evidence for the historicity of David' and suggesting 'the reliability of the biblical record', which 'we need to read…more carefully to avoid false expectations about what we are looking for in archaeology' (2006: 241). His opinion of the inscription is summarized in ten points (2006: 252-53), with the addition

1. Cf. Noll 1998: 12 n. 29.

of his reconstruction of the fragments (2006: 254-55). His article also includes another discussion of ביתדוד and ארק · הם (line 10A), which mainly defends his previous arguments (cf. 2006: 247-51).

Apart from the Athas–Sasson exchange, the debate has abated since 2003, having reached a kind of saturation point. On the one hand, some of the most crucial questions reached a degree of consensus. On the other hand, all possible arguments have been used and re-used over and over again. Monographs of different kinds now refer to the text with respect to its language, as giving information on 'House of David', on the kings of Israel and Judah, on Hazael, on wars between Aram Damascus and Israel–Judah and so on.

It seems that the debate surrounding minimalism vs. maximalism has also reached a kind of saturation point, though Dever (2000a and 2000b) and Thompson (2001) possibly testify against this, being among the last ones in the trenches. After all their publicity and publication, the minimalist camp seems not to increase in number, at least from the perspective of the debate on the Dan inscription. All through the debate we have heard the same few voices from Copenhagen and Sheffield. Will they not tire? Are they not being played with, like hooked fish, by the ever-present alleged maximalists? There was, of course, an important 'watershed' following the publication of the two additional fragments found in 1994. Some aspects of the debate prior to the second find are more or less redundant today.

While George Athas's dissertation, published in 2003, represents a fresh input into the discussion, and while it has seemingly been well received (his book came out in paperback in 2005), in the years following its publication it has not changed radically or reignited the debate. As I write this Epilogue (2009), there seems to be consensus, at least among some scholars, as to the interpretation of some matters related to this text. But, even if no general agreement has been attained, most questions raised by the inscription have, by now, been thoroughly discussed.

The debate is far from over, it has simply taken a nap, ready to be awakened by the smallest whisper of more, or similar, fragments, or further provocative arguments. There is a fair possibility that more fragments from the stele will be found. Tel Dan is a huge area to excavate, and excavations have been ongoing for several years. The late Avraham Biran led the excavations at Tel Dan from the 1960s until the 1990s, and excavations will still go on. Another fragment may be found today—or in ten years, raising new questions we can only imagine.

It is fair to say that we need more fragments—or the rest of the stone—to be able to move on substantially. If we were to be so lucky, several questions would find their answers, but several others would, without doubt, be raised. Historiography is, and always will be, a controversial business.

The last word has, obviously, not yet been written on Tel Dan and the Tel Dan inscription.

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INDEXES

INDEX OF REFERENCES

INDEX OF AUTHORS