"Offing the Establishment: DBAT 38 and the Politics of Radicalism"

Thomas L. Thompson – Copenhagen

In the Fall of 1990 and the summer of 1991, while still looking for my first secure academic post, I met two scholars whom I had often heard described by others as "radicals" in our field and whose writings I admired: Bernd Jørn Diebner and Niels Peter Lemche. At the time that I met them, both were solid members of the academic "establishment," and editors of new and innovative journals. Each held tenured posts at major universities. I was much impressed by the extent to which my work interrelated with theirs and by the common hopes we shared regarding the research directions of our field. In fact, it was this common ground of our research that led me to meet them both in the first place, as no common project had brought us together.

I was, I remember, appalled that in Diebner's own Heidelberg, this immensely creative and original thinker seemed to be ignored and marginalized among his colleagues. Although I was only there for a summer to do some private research supported by the *DAAD*, had been substantially absent from Old Testament studies for nearly a dozen years, and consequently, at this period of my career, was facing many of the anxieties of that hero of American folktale Rip Van Winkle, my Tübingen roots gave me immediate access to circles of discussion in which Diebner himself apparently played no role. This was particularly poignant as in the course of these visits I also came to discover the wonderful gadfly of a journal *DBAT*, the existence of which I had in my own isolation been entirely ignorant. The hours reading this journal's back issues allowed me to trace the ongoing development in Germany of many of the strains of research in which I had long ago participated in as a member of Kurt Galling's *Dyptichonkreis* during my Tübingen years, and especially from 1968 to 1975. This late reading of Diebner's journal reminded me of what I had of course already known: that my own work had not after all been going on just in Tübingen and later in the seemingly total isolation of my own head during the late 1970s and 1980s that I had sometimes imagined.

At this time, I also found that Lemche and Diebner shared with me both a private and a public disdain for professorially driven academic politics, and particularly that which carries such immense weight in the aspects of our field involving both publication and citation, and which directly impinges upon academic appointments and the vulnerability of newcomers and of those many scholars that live on the margins of our field. The passion with which we all engaged in this issue is linked with the fact that we were all products of the 1960s generation and its complex *Auseinandersetzung* with the "establishment." Both Diebner and I had had serious problems with academic power brokers early in our careers, and these conflicts had created patterns of response in both of our writings, reflected, for example, in excessively harsh language and at times in perhaps rash responses to both our critics and others we disagreed with. That we have occasionally been dealt with unfairly and disrespectfully is true, but it has been a long time that either of us have been able to claim innocence in scholarly debate. Nor is Lemche quick to shy from a debate when he has felt principle was involved.

Diebner might think of Lemche as the "establishment's radical," in contrast to himself; this is only marginally true. Diebner and I also have had considerable support from influential colleagues and friends, support which, in however quixotic ways, has effectively supported our survival in today's academy. The establishment has never been quite so monolithic as each of us has at times thought in our worst nightmares. The "establishment" provided me with academic appointments—albeit hardly continuous—research funds and access to influential avenues for the publication of my work—at least east of the Atlantic. Diebner has been provided for now many years with a tenured post at one of the most influential universities of our field, which post has given him the basis from which he could launch his own publishing ventures in security and independence. In this regard, both of us have only peccable credentials as "*Martyrer*" or even "radicals." Such dependencies need to be recognized and should not be allowed to be overshadowed by the distorting rhetoric of untrammeled independence that so easily becomes part of one's self-identity. In fact, we are all aware that secure university posts are essential to truly independent research. We are also aware that such security and independence for research is not provided on the basis of merit.

The very personal perspective of this preamble is offered in an effort to introduce an understanding of our scholarship that is not immediately apparent in our more formal perceptions of the development and progress of intellectual paradigms, nor in the related focus of our published *Forschungsberichte,*[1] where we too often concentrate on "origins" (the evidence for or access to which is most readily found in a linearly organized publication chronology) and on the surface arguments and conclusions of individual scholars in their articles and books. We do this in spite of our awareness that articles and books are quite typically published (and therefore dated) some two to three years after they had been first disseminated in lectures, seminars and discussions. Indeed, frequently enough they do not acquire a "canonical" form for as many as ten years. If one were to choose to go beyond the work of individual scholars and his or her copyrights, it is commonplace that seminal ideas are decades in their shaping, and as often as not, ultimately and truly dependent on unwitting teachers and colleagues, or even through the unwilling collaboration of our opponents in argument.

Along with both Diebner and Lemche, I held a perspective of our field that I believe anyone could describe as similar, and might well assume to have been related and interdependent. Yet I had not read much of either before 1988. Would I be justified in claiming that their work was dependent on me? Provably so. Not only had it been discussed at the IOSOT meeting in Uppsala in 1971 and at the Ecole Biblique that same year but both Lemche and Diebner had published significant reviews of my dissertation shortly after it appeared in the *BZAW* in 1974. The question, however may also be fairly raised: since, I had read neither of them, was my work dependent on theirs? Just as obviously so; though proof is more difficult. Lemche had finished his first major critique of Noth's understanding of the amphictyony already in 1968 which was published in 1972 and both of us were not only reading many of the same things but took part in the many lively discussions of Mendenhall's and Gottwald's hypothesis at the Edinburgh meeting of 1974. Similarly Diebner and I were each, independently of each other, working for more than a decade in the intensely interrelated worlds of Heidelberg and Tübingen respectively. A further question—one that relates to the purpose of this response and will I hope become clear—to what extent should I have responsibly cited them for all that I inadvertently learned from them, and does such

[1] My own very recent one included (Th.L. Thompson, *Early History of the Israelite People,* Leiden, 1992), pp. 1–170. Although, I already then felt it necessary to moderate—however inadequately (pp. 169f.)—the intrinsic distortions of this approach, the problems now seem systemic to any efforts of an individual to sketch a linear evolutionary understanding of the process of intellectual history that, is, in fact not linear at all in experience.

responsibility significantly change now that I know them and have precious little time to read anything anymore?

Returning to my metaphor of Rip Van Winkle, let us move ahead to 1993 when the "radicals" have taken the middle of the road, and when what was once new has begun to fray and is, indeed, "old hat." The ranks of the revolutionaries suddenly dwindle and those of the establishment as suddenly grow. What just twenty years ago was unacceptable now has acquired respectability. Appointments follow: I in Copenhagen, Knauf in Geneva, Niemann in Rostock, Hübner in Kiel, Davies in Sheffield and Niehr in Tübingen. Neither national nor confessional borders are barriers. But again, these changes are not merely about individuals. The geographical center of our field also has shifted decisively: from Europe to North America and Israel, but within Europe from Germany (and especially away from the old asparagus beds of Tübingen and Heidelberg) to the periphery, to Sheffield with literary and feminist criticism and the most productive publishing center of our field, to Amsterdam with its dominant role in hermeneutics, exegesis and theology, to Geneva with philology and linguistics and to Copenhagen with its focus on history and society. German—that once fabled first Semitic language—is today only one of many languages of our field where English commands all international discussion. Such changes—and my description here makes no effort to describe the whole by any means—also bring with them changes of perspective. It is from within this process of change and its on-going assessment that I would like to respond to the recent publication of Bernd Diebner.

In the December, 1994 publication of *DBAT* 28, Diebner, in a chain of articles and reviews, lays a charge of "nepotism" against the university of Copenhagen's theological faculty regarding its appointment in 1986 to a professorship of Old Testament exegesis. He buttresses this charge with the direct and indirect accusation that the current reputation of the then appointed professor, and many of the core conclusions of his work, reiterate unacknowledged conclusions that Diebner alleges he and others had reached 20–25 years ago. While the majority of his accusations are directed in an extremely personal attack on Lemche himself, the weight of the charges address the integrity of Copenhagen's appointment procedures.

Although I am marginally tarred with the sweep of Diebner's wide brush—and, using yet another metaphor, have been hit, so to speak, by the flying shards of his words—I feel that it is still appropriate that I, who hold the other Old Testament *professorat* in Copenhagen, offer a defense against Diebner's charges, especially as I was not involved in this 1986 decision, and as my position in Copenhagen today is entirely independent of Lemche. Diebner's minor insinuation that my appointment was the result of my friendship with Lemche can easily be dismissed as specious, if not pernicious, as at that time Lemche had met me only once. Our close cooperation, friendship and the interdependence of our work since then is no more inappropriate than my friendship with Diebner, a friendship that does not preclude criticism. It reflects nothing more than the close communication and cooperation, in which scholarship here in Copenhagen is normally pursued. In this response, I wish to deal both with the issue of whether the university has treated Diebner unjustly as he has claimed, as well as with the intrinsically related charge that the person who had been chosen at the time was obviously unfit. I feel that the charges are serious and require examination.[2]

The accusation that the "*Nielsen Schüler*" Lemche's appointment in 1986 had been inappropriate involves, as Diebner acknowledges, the context that Diebner himself was one of the applicants for this post. In Diebner's judgement, the choice of Lemche over himself was significantly determined by the fact that Lemche had once been a student of Eduard Nielsen who was the chair of the appointment committee. However, the entire range of the German concept "*Schüler,*" with its former implication of dependence, is inappropriate in the small world of Danish universities, where not only are "*Doktorväter/mutter*" purely administrative relationships and where the *Disputat* epitomizes a young scholar's independence,[3] but where teacher-student relationships are, of necessity, commonplace in appointments. As Lemche at the time of the appointment held a tenured post at the university of Århus, he was hardly a dependent of Nielsen's patronage. Lemche's scholarly

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[2] This response also indirectly responds to a further attack by Diebner on Lemche which is scheduled to appear in *DBAT* 29 in the form of a dedication to a caricature of a *Festschrift* for Lemche's 50th. birthday. This I have seen in proof form. Also promised is an annotated bibliography of Lemche, which I have not seen. I have decided to respond forthwith for two reasons. In my judgement, the *DBAT* 29 article does not add significantly to or alter either the nature or quality of Diebner's charges. Also in my judgement, justice demands that the seriousness of these charges be addressed openly and without delay.

[3] Under the old dissertation guidelines, such works were totally independent projects with no *Doktorvater,* no advice and no consultation possible.

independence of Nielsen is also apparent in his writings.[4] Moreover, Copenhagen's institute of biblical studies has an honorable record of obviously non-nepotistic appointments. In spite of the fact that we have only 4 biblical professorships, three foreigners have held such chairs in recent times, including Diebner's own Heidelberg colleague Gerd Theissen and the present writer. The specific charge of nepotism seems hardly warranted and is indeed insulting. Diebner has given us no specific reason to think otherwise. While Diebner makes selective statements that he claims are based on the clear text of the *acta* with which the grounds of this appointment were clarified. These statements of Diebner are made, however, independent of their context and to an audience that does not have access to these documents that they might judge the validity and truthfulness of Diebner's judgements.

Having said so much, I also would agree with Diebner that the faculty gave in this decision, and typically gives, considerable weight to major publications, as well as to the (sometimes concomitant) international "reputation" of the applicants. In my own judgement, this did play a role in the 1986 decision. Without suggesting that I would have made such an evaluation, and I certainly would not on principle, the use of this distinction, as one of the committee's means of coming to a decision, hardly seems inappropriate since the university as a whole rightfully encourages the appointment to professorships of well known and influential scholars. In itself, it is hardly possible to judge such a preference as wrong. Since the question of influence is one of the issues that Diebner presses most directly, I will return to this later. I only wish to argue here that this question, in my judgement, was a legitimate concern of the evaluation. While Diebner's comparison of himself with Albrecht Alt is somewhat ingenuous, the point made is very pertinent. Could the faculty, in weighting the importance of major books, ignore the perhaps greater seminal importance of scholarship among applicants who have chosen other forms of communicating their work. Such neglect would, I think, be a serious mistake. To avoid such arbitrariness, which an excessive concentration on specific formal characteristics of scholarly production might encourage, judgements concerning equivalencies should form a necessary part of any fair procedure.

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[4] Those unfamiliar with the Danish scene need only look at the very sharp exchange of opinion expressed in the recent Nielsen/Lemche quarrel over the question of hellenistic contexts: N.P. Lemche, "Det gamle Testamente som en hellenistisk bog," *DTT* 55 (1992, pp. 81–101; E. Nielsen, "En Hellenistisk bog?" *DTT* 56 (1993), pp. 161–174, and N.P. Lemche, "Det gamle Testamente, David og hellenismen," *DTT* 57 (1994), pp. 20–39. Such scholarly independence is commonplace in Denmark today.

In Copenhagen's appointment process, this aspect of the decision is specifically dealt with in the preamble of the written evaluation for each candidate. Prior to any comparative judgement between any given candidates, an evaluation is made as to each candidates qualification, with specifically graded judgements as to whether each candidate in turn is qualified for the post or not. While normal qualification is based on the completion of a *disputat,* other equivalencies are acceptable, such as the German habilitation, but also other publications that demonstrate a comparable competency and accomplishment. The possession of a PhD, for example, is usually not deemed sufficient for qualification,[5] but cumulative additional publications can be, and in this, the degree of influence, originality and importance of such play an understandable and significant role. The judgement here is made on the basis of each candidate's qualifications in comparison with a common standard of what the committee judges as required competence. It was in this context that Diebner's application for the 1986 Copenhagen *professorat* was in fact rejected, entirely independent of any comparison with Lemche and entirely apart from any "big-book" syndrome. While the faculty saw much of Diebner's work brilliant and immensely original and creative, they concluded that he had not then reached the level of accomplishment and publication equivalent to a Danish *disputat,*[6] which is the standard necessary for a professorial appointment here.

Only after candidates are judged "competent," are the applicants compared with each other and ranked on the basis of individual evaluations of their works submitted. Finally, justification for these judgements is specified and an argument is made for the choice of one candidate over another, before the whole is submitted to the faculty for decision. Diebner's assertion that the faculty preferred Lemche's larger contributions to Diebner's smaller but seminal works is simply false. The committee never made such a judgement as the members

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[5] I specify the normal evaluation of the PhD, as this was an issue in 1992 when I was appointed, when a similar equivalency examination needed to be undertaken. I, like Diebner, did not have either a Scandinavian *disputat* or a German *Habilitation.* This analogous situation I offer as at least *prima facie* evidence that the procedure of equivalency evaluation here in Copenhagen is not a procedure used to exclude candidates with irregular or foreign credentials. Rather, it facilitates such candidacies.

[6] In correspondence between Lemche and Diebner in January of 1985, Diebner makes it very clear that it had been his own decision not to habilitate in Germany. Personally I think this decision was a very unfortunate choice that has left him isolated from critical discussions with colleagues.

of this board never compared the work of these two scholars. Finally, Diebner had full rights in 1986 to protest this report of the committee and to appeal their evaluation. However, he did not; and so, we need to ask why he has decided to object now? To present himself as heroic victim?

Lemche (hardly a *professor maximus*, here, north of the German border), of course, can not be blamed for what Diebner sees the committee to have done. Nevertheless, a substantial part of Diebner's criticism is directed at Lemche himself, who is most specifically charged with withholding credit to Diebner for originating the central ideas of Lemche's own work. And so the question must also be asked: Have Diebner's accomplishments in our field been seriously ignored? I think so, and I also think that the field as a whole has been impoverished as a result. But is this neglect to be laid at the door of the *Kopenhagener*? Has Lemche—and for that matter have I—refused to afford Diebner the title of our mentor? Have we really been simply revisiting ideas long ago thought by him as he charges? Throughout Diebner's *DBAT* 28—even to the announcement of the title change of future publications of his journal—he is much concerned with the question of intellectual property.[7] Is his self-perception as progenitor of much that is new and only apparently new[8] among this new direction of research so patent that his fellow travellers might be reasonably accused of intellectual theft?

Immediately prior to the decision on the 1986 *professorat*, Diebner reviewed Lemche's book *Det Gamle Israel*.[9] In this review Diebner was critical but friendly and considered Lemche's book conservative but generally a positive step forward. Immediately following the appointment of Lemche as professor, however, Diebner published a review of Lemche's *Early*

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[7] Diebner, however, does not charge Lemche with offending against copyright. His charge is that of ethical, not legal, misbehavior.

[8] Here, the reader should be directed to E. Nielsen, *DTT* 1993.

[9] B.J. Diebner, "Traditionen über Israels Vorzeit, die keine Geschichtsquellen sind," *DBAT* 21 (1985) pp.246–251.

Israel.[10] It is in this review that we find the first clear signs of Diebner's charges against Lemche, not only regarding the charge of stealing his ideas but also the work and ideas of Lemche's colleague, Heike Friis.[11] This is important, as prior to the faculty's decision regarding the Copenhagen *professorat*, Diebner judged Lemche with only the normal failings of ordinary scholarship. Since this decision, however, he has consistently attacked Lemche's integrity, charging him with the theft of Friis' ideas, and also has implied that Lemche subverted Friis' academic career. Friis, however, was not a candidate for this Copenhagen post; Diebner was. I find the lack of specificity here in Diebner's charges a serious weakness in his argument, both in regard to alleged actions of Lemche as well as to what actually the ideas were that Lemche is alleged to have stolen.

It is significant that Diebner only first became aware of Heike Friis' work in 1983 and 1984, and he seems generally poorly informed of Scandinavian scholarship on these issues prior to this time. The briefest reviews of that scholarship is sufficient to dismiss the substance of Diebner's charges.

As Diebner has long been aware, Lemche wrote a Copenhagen *prisopgave* in 1968 on the same topic as had Friis in her gold medal effort. The specific topic had arisen from a course taught by John Strange. The origin of their topic was due to Strange's discussion and skepticism about Noth's ideas concerning the amphictyony and the deuteronomistic history which subsequently committed both these young scholars to submit this theory to critical examination.[12] The results common to both investigations were the rejection of Noth's

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[10] B.J. Diebner, "Es fragt sich, ob eine Landnahmetheorie erforderlich ist..." *DBAT* 22 (June, 1986), pp 215–222.

[11] A year earlier—simultaneous with his relatively positive review of Lemche's *Det gamle Israel*, Diebner had published a review of Friis' Copenhagen *prisopgave* of 1968 (H. Friis, *Forudsætninger i og uden for Israel for oprettelsen af Davids imperium*, prisopgave, Copenhagen, 1968, = *Die Bedingungen für die Errichtung des Davidischen Reiches in Israel und seiner Umwelt*, *BDBAT* 6, 1986), which he was preparing for publication: B.J. Diebner, "Die Abhandlung zeugt von grosser Reife," *DBAT* 21 (1985), pp. 243–246. He points out the pioneering importance of this work correctly, and in particular he stresses how it had preceded some well known studies such as that of John Van Seters (*Abraham in History and Tradition, 1975*).

[12] This is a most significant seminal influence on the work of both Friis and Lemche, which Diebner does not appear to have considered.

reconstruction of the period of the judges and, consequently, a challenge to the pre-exilic context of the foundations of the united monarchy: Friis more decisively, and Lemche, partially and with reservations. The texts of the *prisopgave* themselves, clearly demonstrate their independence and the integrity of each. Lemche published the first chapter of his 1968 work in 1972. In this work, Lemche clearly cites Friis' essay, and particularly credits her role in the critique of Noth's hypotheses (so, p. 100). Friis published the core of her conclusions in an article in 1975.[13] Unfortunately, neither of these publications received the attention they deserved outside of Scandinavia. Nevertheless it is pertinent to the question of Diebner's charges that already in 1977—long before Diebner was aware of the contributions of either Lemche or Friis in this new direction of scholarship—the Copenhageners clearly both showed themselves aware that a new paradigm had been cast for the field and that their work was very much part of that. Lemche did this in his lecture in Århus at the end of November in 1977 (unpublished) entitled: "*Israels oprindelse*,"[14] and Friis did so in her address to the Copenhagen *seniorseminar* of 1977, in a paper in its published form "Ein neues Paradigma für die Erforschung der Vorgeschichte Israels?"[15]

Lemche clearly credits Friis for her central role in the departure from Noth's hypothesis and cites both her *prisopgave* and her 1975 article. Friis, on the other hand, taking her starting point rather from a discussion in the British *Journal for the Study of the Old Testament* of 1977 on the patriarchal narratives, points to the present writer's dissertation and especially to the work of John Van Seters as clearly marking a shift of paradigms in the field.[16] Echoing the long expressed frustration of her teacher Eduard Nielsen,[17] Friis scolded German scholarship (and Diebner's *DBAT* in particular) for ignoring almost all of

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[13] H. Friis, "Eksilet og den israelitiske historieopfattelse," *DTT* 38 (1975), pp. 1–16.

[14] A similar perspective of both Friis' work and his own orientation towards sociological questions is clear in Lemche's *seniorseminar* paper in Copenhagen of March, 1979.

[15] *DBAT* 19 (1984), pp. 3-22.

[16] Th.L. Thompson, *The Historicity of the Patriarchal Narratives* (Berlin, 1974); J. Van Seters, *Abraham in History and Tradition* (Yale, 1975).

[17] See, for example, E. Nielsen, "Traditio-Historical Study of the Pentateuch since 1945 with special emphasis on Scandinavia," *Productions of Time*, ed. by B. Otzen and K. Jeppesen (Sheffield, 1984), pp. 11–29.

non-German and especially North American scholarship.[18] In fact, I believe Friis' article, here, expresses a clear understanding of what was happening in the 1970s that Diebner seems wholly unaware of.

Friis and Lemche were neither repeating each other's work nor were they dependent on each other. Friis was primarily interested in biblical texts and their development. She was not much involved in pursuing historical work independently of questions relating to the bible's context in history. Her questioning of Noth's hypothesis led to her (correct) dating of these traditions to the post-exilic period, which undermined the commonly assumed historicity of the Davidic United Monarchy. Lemche, on the other hand, questioned Noth's hypothesis from an extra-biblical perspective, which led him to challenge (correctly) the historical reality—from the perspective of a history of Palestine—of a period of the judges as well as the social-archaeological presuppositions of the monarchy's foundations. These were two (in terms of method) radically different directions of research that were complementary, but hardly collusive.

Diebner also wishes to accuse Lemche of stealing the ideas he and his Heidelberg colleague H. Schult published in a brief article in 1975.[19] Frankly, after reading almost everything Lemche wrote between 1968 and the present, I do not find that Lemche was ever interested in the theses that Diebner had sketched in 1975.[20] Friis' perceptions of 1977 are

[18] For example, she points out that Diebner refers both to Van Seter's work in *DBAT* 10 (1975) p.59 n4 and my dissertation in *idem,* p.60 n11, but thoroughly ignores the implications of either work, in spite of the fact that these works were close to the sympathies of the *DBAT* at this time.

[19] B.J. Diebner and H.Schult, "Thesen zu nachexilischen Entwürfen der frühen Geschichte Israels im Alten Testament", *DBAT* 28 (1994), pp. 41–46.

[20] This is completely apart from the assumption of Diebner that these scanty unsubstantiated notes for future work can be given the status of functional ideas or hypotheses. It must be recognized that in the mid-seventies, hundreds of comparable ideas were being jotted down for future work, but only a few of them were ever explored or followed up with clear analysis or systematic study. Diebner's own early ideas have rarely been worked out beyond their initial brief formulation. Exceptions to this judgement are his important articles: "die Götter des Vaters: eine Kritik der Vatergott—Hypothese Albrecht Alts," *DBAT* 9 (1975), pp. 21–51; "Neue Ansätze in der Pentateuch-Forschung," *DBAT* 13 (1978), pp. 2–13; B.J. Diebner and H. Schult, "Argumento e Silentio: das grosse Schweigen als Folge der Frühdatierung der alten Pentateuchquellen," *BDBAT* 1 (1975), pp. 24–35.

here very important. This shift of paradigms which has taken place over the last quarter century and which has affected our field took many directions. It has been a many headed monster, only three of which pertain directly to Diebner's attacks on Lemche: the historical critical perception of the chronology and composition of the bible (in which not only Friis but Diebner were intensely engaged), the extra-biblical historical claims of Noth's perception of the early history of Israel (which was Lemche's early focus of interest), and the broad-based effort to create a history of Palestine (and Israel) wholly independently of biblical narratives (which formed the focus of Lemche's later sociological interests). As Friis clearly pointed out, Diebner showed little interest in these latter issues, and generally ignored significant work on them. Lemche, on the other hand, has only been interested in the first topic as to notice, and infrequently cite, developing confirmations to his own work over the years. Conclusions drawn within these three directions have hardly corresponded with each other. Only since 1985—and indeed partly as a product of Lemche's *Early Israel* of 1985 and the English publication of his *Det gamle Israel* of 1988[21]—have these very different directions of research been joined in discussion and debate.

Not only do I doubt that Lemche has been dependent in significant ways on Diebner's or Friis' work, but it seems clear that he has been engaged in very different directions. He has been involved in scholarly discussions that are general to Europe outside of Germany. It is this discussion that has turned Old Testament scholarship away from the older paradigms and methods of Alt and Noth, and of tradition-historians generally. Lemche also has been a serious participant in the North American effort to separate the history of Palestine/Israel from biblical historiography—a tradition of scholarship of and in which Diebner is largely unaware and uninterested. It is in this context that Lemche's contributions of his 1985 *disputat* and subsequent research has been oriented. As the most pressing historical questions have been resolved, however, Lemche's interests in recent years have shifted to include questions of chronology and biblical composition as expressed in his articles on hellenistic contexts. However, his point of departure in these hellenism articles, is clearly not that of the German debate so central to both Diebner and Friis in the 1970s, but is rather related to questions of scribes, text creation and social context: interests which have always been a part of his perspective on the field.

[21] N.P. Lemche, *Ancient Israel* (Sheffield, 1988).

To buttress his charges, Diebner has republished in *DBAT* 28 the notes for an article from 1975 with which he chooses to prove his charges and his proprietary claims. I fear that Diebner's self-understanding of the independence of his scholarship has caused him to become blind to the nature of intellectual process and to an understanding of how ideas change and develop in a discipline such as ours. Diebner, who has viewed the academic world of our field for many years as Heidelberg's Jeremiah, seems to remain locked in his battle with "the establishment," in whose ranks he now sees Lemche (and me) standing.

That I have considerably neglected Diebner and his work is unquestionable. That I have misappropriated his ideas (as he implies in the headline of his review of my recent book in *DBAT* 28), however, is, equally unquestionably, false. I, as I mentioned above, was unaware of these ideas. I have no wish to mitigate the implications of this, nor to excuse my own obligations to know my field. Shortly before I sent my 1992 book to press, Diebner gave me a copy of the wonderfully original 1968 gold medal essay of Heike Friis. In my own opinion, the publication of this work in the late 1960s would have radically changed the direction of research then dominant outside of Denmark, and, I believe, would certainly have substantially pushed my 1974 book on the patriarchs leftward. However, it was not published, and it did not affect me, Diebner or the field elsewhere in any substantial way.[22] Within Denmark it did not simply remain on a dusty shelf of the royal library as Diebner claims, but was rather regularly used and cited throughout the seventies by Friis' colleagues (including Lemche)[23] and it has long been recognized for the pioneering contribution it certainly was. The world outside of Denmark neglected this work—as I had—primarily because most of us don't read Danish and it had neither been published fully nor translated. This is a quite common reason for good scholarship being neglected.

Even more reprehensible, perhaps, than my ignorance of my present colleagues' works, was my ignorance of Diebner's, as that was published and written in a language I have read

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[22]See *Early History*, pp. 89f.; also my more recent articles "Martin Noth and the History of Israel," and "William Dever and the Not So New Biblical Archaeology" (forthcoming).

[23] This judgement is based on a number of unpublished essays and lectures given by Lemche in Copenhagen and Århus from 1971 to 1982. Lemche cites Friis' work in his own *Prisopgave* (Copenhagen, 1968, pp. 202f.) and again in his *Israel i Dommertiden* (Copenhagen, 1972, p.100). See on this now, N.P. Lemche, 'Hvad er det vi har levet, og hvor går vi hen?' *Fra Dybet,* John Strange Festskrift, ed. by N.P. Lemche and M. Müller, *Forum for bibelsk eksegese* 5 (København, 1994), pp. 130–143, esp. pp. 130–133.

since the early 1960s. And here, a sensitive issue arises that—although only indirectly suggested by Diebner—is, I think, centrally involved in Diebner's complaint. While I was willing to revise the nearly complete manuscript of my 1992 study dealing with the history of scholarship in order to include a clear recognition of Friis' *prisopgave*, for I felt she had perceived already in the late 1960s much that the rest of us were then only beginning to grasp, I did not make the same kind of judgement about the contributions that Diebner had published in the *DBAT* throughout the mid and late 1970s. His work I judged more modestly: not so much pioneering and itself original as rather that of a brilliant and sharply critical commentator and fellow traveler. While he clearly recognized much that was new in the field, he did not himself establish arguments that changed the field. In his own words, he placed issues "on the table." That seemed to him sufficient. Throughout the 1960s, 1970s and early 1980s many were placing such ideas on tables.

In my book of 1992, I was trying to trace a methodological development that I saw within the field as a whole, and one that demanded a change of directions away from that of the past generation. Diebner's work, sparkling and editorial as it has always been, never convincingly established new ideas for any of us to follow. It did little more than assert what was part of a widespread conversation. As such, some of these ideas have been undifferentiated grist for the mills of others to grind. I did not recognize them—even from my position of hindsight of the early 1990s—as uniquely seminal ideas. I do not think I was wrong in this. This judgement is vulnerable. There were many ideas on the table—expressed by Diebner and others—that I have only begun to recognize during the past year or two, especially those surrounding the issues of a hellenistic chronology for *Tanak*. Here the republication of Diebner's and Schult's notes may help us better evaluate the complex interaction of ideas developing within a still rapidly changing field.

Diebner's and Schult's theses of 1975, relating to a post "pre-exilic"[24] ideology of what they saw then as a theocratic Israel and as relating to the notorious amphictyonic fiction, was undoubtedly fundamental in their development. Their other schematic statements relating to our field's historical perceptions of a united monarchy, to the evaluation of the Chronicler's work, to the metaphoric nature of the Exile and to the issues of chronology for the ideology

[24] Diebner's term is, here, unfortunately biblicistic, as the "exilic" literary matrix's historical referent of "exile" does not allow for any such historical period.

of biblical literature were already widely in discussion and would have remained there with or without this 1975 publication. Diebner and Schult introduced none of them *ex novo*,[25] howevermuch, their particular formulation unquestionably participated in what has turned out to be a significant movement within our field. By 1975, such a thesis nailed to the door of academia's cathedral, while bold and promising, was hardly unique. It reflected what was common property of many young academics. By 1975, the dye of the new paradigm had long been cast. What mattered in 1975 were not revolutionary manifestoes so much as broader based and substantiating debate, where these new insights could be established as aspects of a new understanding to be affirmed or to be rejected as inadequate. Not only had Van Seters published his thoroughly argued *Abraham in History and Tradition* in 1975, which was quickly followed by the studies of H.H. Schmid and Heidelberg's own Rendtdorff (and one must assume that Rendtdorff had hinted at such things to come in his lectures), and not only had Welten published his studies of Chronicles two years earlier, but Diebner and Schult's central thesis—that relating to the twelve tribes and the amphictyony—had itself been already substantially established by both Friis and Lemche in Copenhagen seven years earlier, had been explored by Fohrer in 1969 and had received further treatment in such works as those of Mayes and de Geus (who had nailed his own thesis to academy's door as early as 1965).[26] Moreover, the issue of the historicity of biblical literature (dealing with that of the patriarchs to that of the United Monarchy, Josiah, Ezra and Nehemiah) was, along with composition theory, among our field's most intensely debated topics in 1975, the direction of which had long been set by Van Seters' early articles in the late 1960s and by my dissertation of 1971. Furthermore, the historically more appropriate topics of social history and archaeology (intensely discussed throughout the 1960s) were beginning to take a direction (already at the Uppsala congress of 1971 and in very extensive discussions at the Edinburgh congress of 1974) thoroughly undermining any perceived history of "Israel," even that of

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[25] Lemche deals with many of the topics of Schult and Diebner's 1975 statement already in *Israel i Dommertiden* (Copenhagen, 1972); namely, Israel and Judah (pp. 99ff.); 12 tribes (p. 106), amphictyony (*passim*), Canaanites (at least, in Philistine dress: pp.92ff.) and 'monarchy' (pp. 88ff.). The many issues that Friis and Lemche raised in their 1968 essays could easily be misunderstood by an uninformed outsider as having provided an uncited foundation for Diebner and Schult's point of departure.

[26] On this, see my *Early History,* pp. 42f. n42.

Diebner's "post-exilic" one.[27]

Schult and Diebner do not cite any of this work, although they were certainly dependent on much of it. A considerable portion of this work was already available in partial or completed form, and all of it was very much a part of the intellectual currents in which those of us who were engaged in such research swam. These notes from the *DBAT* of 1975 were not, after all, available in either 1968 or 1971.[28] My remarks here should not be misunderstood. There is nothing wrong in Diebner's and Schult's neglect to give credit to this earlier work of others. It is quite understandable that they did not yet see such work as forming part of the agenda they personally were involved in setting.[29] Diebner and Schult simply, unknown to themselves, were reiterating, in abstract and summary form, the directions of this part of our field. One cites first of all that literature which is seminal to or which forms the matrix of one's own intellectual assay. In the world of new ideas, one leaves—and Diebner should have left—the work of the "objective" chronology of group perception to others. This work, after all, was not created by "giants" in the field. Established scholars like Rendtorff, Fohrer and Ahlström, or *prima donna*s of any kind, provided only rare voices to us who were only naive members of a growing chorus. We were all rather young scholars not yet completely aware of what was happening apart from our own narrowly defined circles and interests. That Diebner was unaware of what others were doing is a condition that must be called "human."

Which of these many directions does Diebner define as uniquely belonging to himself, the neglect of citation of which, would be doing him an injustice? My own entry into the hellenistic sweepstakes owes a debt to Wellhausen, whom I acknowledged in support of exactly this "thesis" already in my dissertation of 1971. However, I was not entirely aware

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[27] If seminal foundations of new ideas are to be stressed, one must point to M. Noth's terribly influential 1959 address to the IOSOT congress in Oxford and to J. Barr's *Semantics of Biblical Hebrew* of 1961.

[28] The years in which Friis, Lemche and I completed our first major contributions to the coming debates.

[29] In fact, in the mid-1970s, to my knowledge, only J.M. Miller and J.H. Hayes in the planning of their *Israelite and Judaean History,* finally published in 1977 and M. Tsevat in his Review of my *Historicity,* in *JBL* in 1975, might be accorded the kind of prescience Diebner seems to be claiming and demanding of others. Most of us had very little idea of where things were headed.

of the thesis' importance until Lemche pointed out the inconsistency of my thought some twenty years later when I was moving to support a Persian period chronology. And so, I credit Lemche with the instruction for which I was indebted, though not for the 2nd. cent. BCE–1st. cent. CE chronology with which I am now (in common with Diebner and Lemche) working. For that I am indebted to what I have learned from others, such as Mogens Müller, Frederick Cryer, Eugene Ulrich and Emmanuel Tov. However, of these four, only Cryer has formed a thesis anything close to the positions of Diebner, Lemche and myself. More than this, however, is involved; for there is no particular virtue in merely presenting a new idea that others have not worked with or that others are only beginning to explore. New theses have no particular virtue in themselves. In fact, we all know that they are usually a waste of time. Surely we have learned that much in the last century of biblical scholarship, a period in our field which has witnessed far more misdirections than carefully thought out hypotheses based on evidence. Putting on tables theses such as the present one under discussion, of a Hellenistic matrix for biblical composition, is only the first step to serious *Wissenschaft* in our field. In doing this, we have only begun our job. This hypothesis is hardly established, or even clearly formulated yet. The real work is in demonstrating that we have here a truly valid perspective for a better understanding of *Tanak*. We are yet a long way from showing this. At most, we have only a few hundred pages of suggestive notes. I think that we are in a better position today than when such ideas had been proposed a long time ago by such as C.C. Torrey and engaged by Wellhausen and many others. Nielsen is quite on the mark in his contribution to this debate, in pointing out that these ideas are not very new, but had been systematically proposed (and rejected) long ago, for example, by M. Vernes.[30] Neither Lemche, Diebner nor anyone has yet presented a commanding case. We have only been talking about tomorrow's work. While Vernes should have been cited by all of us, one hardly needs to give credit to more recent, unsubstantiated notes for tomorrow's *Wissenschaft.*

Kurt Galling, in the summer of 1968, gently scolded me in commenting on one of my early papers on the Nuzi tablets: These may or may not be ideas, he said, puffing on his cigar,—and as such they are quite sound for an American—but, you know, here in Tübingen we are expected to give reasons for what we think.

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[30] *Precis d'histoire Juive* (1889); cf. E. Nielsen, *DTT* 1993. p.169.