BULLETIN OF THE INTERNATIONAL ORGANIZATION

FOR SEPTUAGINT AND COGNATE STUDIES

Volume 22   Fall, 1989

IN MEMORIAM JOSEPH ZIEGLER

Geboren am 15. März 1902 in Tauberrettersheim, zum Priester geweiht 1926, promoviert 1929 und habilitiert 1933 in Würzburg; nach einer Vertretung in Bamberg (1934/35) auf den alttestamentlichen Lehrstuhl im nahe Königberg gelegenen Braunsberg berufen und dort 1937-1944 tätig, nach der Flucht aus Braunsberg und einer zweijährigen Tätigkeit an der Philosophish-Theologischen Hochschule in Regensburg von 1948-1970 Inhaber des Lehrstuhls für Altes Testament und biblisch-orientalische Sprachen, 1961/62 Rektor, an der Universität Würzburg; gestorben daselbst am 1. Oktober 1988.

Ehrendoktor der Philosophischen Fakultät Würzburg und der Theologischen Fakultät Fribourg, Mitglied der Bayerischen Akademie der Wissenschaften in München und der Akademie der Wissenschaften in Göttingen, Träger des Bayerischen Verdienstordens, des Kulturpreises der Bayerischen Landesstiftung und der Medaille Bene merenti in Gold der Universität Würzburg.

Im Grund seines Herzens was er Geistlicher und Theologe. Das zeigen das Thema seiner Dissertation, mit der er 1929 bei seinem zeitlebens hochverehrten Lehrer Johannes Hehn promovierte: "Die Liebe Gottes bei den Propheten" und eine Abhandlung aus dem Jahre 1937, in der sich die theologische Intention mit dem Bereich verbindet, der der Inhalt seines Lebens werden sollte: "Dulcedo Dei, ein Beitrag zur Theologie der griechischen und lateinischen Bibel". Noch

im Jahre 1959 in Oxford, als ich mit ihm in der Bodleiana nach Psalterhandschriften mit hexaplarischen Noten suchte, sagte er, als sein Auge in einer Katene auf das Theologumenon von Zion als der mater ecclesiae fiel, wehmütig: "Das ist nicht für uns".

Die entschlossene Hinwendung zum Bereich der Textgeschichte und die Konzentration auf die Septuaginta haben zuerst vorwiegend innerkirchliche Gründe, die ihm als Schüler des 1925 indizierten Johannes Hehn einen "theologisch neutralen" Arbeitsbereich aufnötigten, und dürfen zuletzt insofern als eine "göttliche Fügung" bezeichnet werden, als er dadurch zum Hieronymus des 20. Jahrhunderts wurde (Gen 50:20). Der Anfang war bescheiden: Mit der Erklärung aus dem Ort seiner seelsorgerlichen Tätigkeit als Kaplan, Maidbronn, an den damaligen Leiter des Göttinger Septuaginta-Unternehmens Werner Kappler vom 4. Juli 1935, "dass ich grundsätzlich recht gerne bereit bin an der Göttinger Septuaginta mitzuarbeiten", übernahm er die Vollendung der von August Möhle begonnenen Edition des Buches Jesaia (1939). Mitte und Ende waren gewaltig: Noch in den Nöten der Kriegs- und unmittelbaren Nachkriegsjahre entstanden in kurzen Abständen die Editionen der übrigen Prophetenbücher: Dodekapropheton 1943, Ezechiel 1952, Daniel 1954 und Jeremias 1957, die anschliessenden Editionen der Libri sapientiales zunächst fast ohne Zäsur: Sapientia 1962, Sirach 1965, zuletzt nach krankheitsbedingtem Unterbruch 17 Jahre später die bereits einem leidenden Körper abgerungene Edition des Buches Hiob 1982 mit reichhaltiger Textgeschichte, "Beiträge zum griechischen Job" 1985, denen nur Unverstand das an einigen Stellen

erkennbare Nachlassen seiner alten Kraft als wissenschaftliche Schwäche vorzuwerfen vermag.

Die Mitte dieses Lebenswerks besteht in der Konzentration auf das von Paul Anton de Lagarde programmierte Prinzip der Rekonstruktion eines ursprünglichen Septuagintatextes durch Ausscheidung der Rezensionselemente. In dieser Hinsicht darf das Werk Joseph Zieglers, bei aller Verfeinerung der Methodik und der Einsicht in die handschriftliche Überlieferung, gegenüber seinem Vorgänger Alfred Rahlfs, in dessen Arbeiten das Grundschema der den drei Kirchenprovinzen zuzuordnenden drei christlichen Rezensionen "seine scharfen Kanten durch Anerkennung von Übergangsformen und Zwischenstufen verlor" (Walter Bauer), als ein erneuter und zuversichtlicherer Weg zu dieser von Hieronymus überlieferten textgeschichtlichen Position zurück bezeichnet werden. Der Weg führte hinsichtlich der origeneischen (palästinischen) und der lukianischen (antiochenischen) Rezension zum Ziel.

Das Werk als Ganzes, dessen in Einzelstudien ausgeweitete Fülle hier nur angedeutet werden kann, weist somit in drei Zielrichtungen: nach der Erkenntnis und Erkenntnismöglichkeit des ursprünglichen Textes (Beispiel: "Zur Textgestaltung der Ezechiel-Septuaginta" 1953), nach der Erkenntnis von Gewinnbarkeit und Charakter der Rezensionen und frühen Sekundärübersetzungen (Beispiele: "Hat Lukian den griechischen Sirach rezensiert?" 1959, "Beiträge zur koptischen Dodekapropheton-Übersetzung" 1944) und nach der Erkenntnis der inneren Struktur des auf diesem Wege gewonnenen Septuagintatextes als Ganzen (Beispiel: "Die Einheit der

Septuaginta zum Zwölfprophetenbuch" 1934/35). Die Septuagintaforschung hat auf diesen drei Wegen und auf dieser Grundlage weiterzuschreiten; der dritte führt wieder zum Ausgangspunkt dieses Lebenswerks zurück: zum theologischen Verständnis und zur Auslegung der Septuaginta als Dokument des Judentums und der frühchristlichen Kirche. Aber dieser Weg, der sein erstes Ziel in Isac Leo Seeligmanns Verwertung von Joseph Zieglers textgeschichtlicher Grundlegung zum Buch Jesaja fand ("The Septuagint Version of Isaiah" 1948), ist gegenwärtig eher verödet.

<div style="text-align:center">Robert Hanhart</div>

Programme

1: 00 - 3 : 00 Eugene Ulrich presiding

Albert Pietersma, University of Toronto, "P. Bodmer XXIV and the Text of the Greek Psalter"

Bernard A. Taylor, Binghamton, NY, "Lucian and the Old Greek in 1 Reigns: A Statistical Analysis"

Peter W. Flint, University of Notre Dame, "Exegesis Reconsidered: The Septuagint Translation of Isaiah 23: 1-14"

Julio Trebolle, Universidad Complutense, Madrid, " Textual Variants in Samuel-Kings"

The IOSCS also sponsored the following special session:

3: 45 - 5: 45 Eugene Ulrich presiding

John William Wevers, University of Toronto,   "Barthélemy and Proto-Septuagint Studies"

Robert A. Kraft, University of Pennsylvania, "The Edition of the Greek Minor Prophets Scroll (DJD VIII) and Barthélemy's Contribution to the History of the Biblical Text"

Leonard J. Greenspoon,   Clemson University and Annenberg Research Institute, "Recensions, Revision, Rabbinics: Early Developments in the Greek Traditions"

Business Meeting

The meeting was called to order by the President, Eugene Ulrich at 3 p.m.

1. Minutes of the 1987 meeting in Boston were read and approved.

2. The President reviewed plans for our 1989 meeting, in conjunction with the IOSOT, in Leuven, Belgium. The IOSOT meetings will convene on August 27 and last until September 1. We will meet on the preceding Friday and Saturday, August 25 and 26. Ulrich reported that he has received a number of proposals for individual papers. In addition, several ideas for panels are being explored. He also noted that the Executive Committee had decided that we should include our *Bulletin* in the combined publishers display at each annual convention He further requested that members keep the secretary-treasurer informed about changes (deletions, moves, etc.) in our membership list.

3. Mel Peters, *Bulletin* editor, was not present; Ulrich presented the report for him. Peters stated that there were two articles accepted for vol 21, but that the "Record of Work" needed expansion before the volume would be ready for publication. He also requested that the Committee consider recommending an increase in dues to cover increased costs in the production and distribution of the *Bulletin*.

4. In his report as editor of the SCS monograph series, Claude Cox noted that Gene Tucker of SBL had noted the generally good condition of Scholars Press. Our own series is doing very well. Recently accepted is a work by Robert Hiebert, "Textual Analysis of the So-called Syro-Hexaplaric Psalter." Ben Wright's manuscript on the Greek Text of Sirach is expected at the convention, and T. Muraoka has indicated he will soon submit the proceedings of a seminar in Australia. In addition, Barnabas Lindars has contacted Claude about publishing papers that are to be presented at a July, 1990 symposium at Manchester. Cox also reported on activity from the "Cognate Studies"side (with information provided by editor Bill Adler). Just published is John Kampen's *The Hasideans and the Origin of Pharisaism: A Study of 1 and 2 Maccabees*. Ted Bergen will soon be submitting a manuscript of Fifth Ezra, and Karl Gross has indicated that he will submit a grammatical study of Josephus' *Vita*. Bill has asked Amy-Jill Levine to edit a collection on women in Hellenistic Judaism, and she is doing so.

5. The Secretary-Treasurer reported that we have almost 370 individual and library memberships throughout the world. He seconded Peters' call for an increase in dues.

   The membership voted in favor of the Executive Committee's recommendation that dues be increased to $8, effective with the next volume of the *Bulletin*.

There being no further business, the meeting was adjourned at 3: 30.

Respectfully submitted
Leonard Greenspoon
Secretary-Treasurer

IOSCS TREASURER'S REPORT
July 1, 1987 - June 30, 1989

Initial Balance (6/30/87)............... ....$1158.52

Payments Received		+ $1847.95
7/17/87	37.00	
7/30 (int.)	19.35	
9/30 (int.)	13.13	
11/13	463.00	
12/30 (int.)	9.12	
12/31	240.00	
1/4/88	5.00	

[L. J. Greenspoon becomes treasurer,
with opening balance of 1496.59]

7/13/88	164.00	
8/3	80.00	
9/15	113.35	
9/21	91.00	
10/7	75.00	
10/18	35.00	
11/11	121.00	
11/15	62.50	
12/6	151.00	
12/20	55.50	
6/1/89	113.00	

Expenses			- $2778.97
7/9/87	(mailing)	48.87	
7/21	(supplies)	40.02	
9/01	(mailing)	230.17	
1/11/88	(mailing)	129.47	

[L.J. Greenspoon becomes treasurer]

4/26/88	(new checks)	11.42	
6/16	(printing)	866.25	
7/20	(supplies)	74.76	
8/08	(mailing)	202.79	
8/25	(mailing)	133.44	
8/26	(mailing)	14.88	
10/03	(mailing)	5.42	
11/03	(mailing)	21.48	
6/28/89	(printing)	1000.00	

Balance as of 6/30/89...$227.50

Leonard J. Greenspoon
IOSCS Treasurer
Clemson University

NEWS AND NOTES

Death of John Gammie
BIOSCS notes with sadness the death of one of its long-standing subscribers, John Gammie of Tulsa, Oklahoma. He died in December of 1989 following an illness.

New Books Received
Two recent books in the Series "Textos y Estudios «Cardinal Cisneros» de la Políglota Matritense" have been received. The first edited by Natalio Fernández Marcos and José Ramón Busto Saiz with the collaboration of María Victoria Spottorno and S. Peter Cowe deals with the Antiochean text of I and II Samuel and bears the title *El Texto Antioqueno de la Biblia Griega I. 1-2 Samuel.* Following the Introduction, it offers a new critical edition of the text of I and II Sam in Greek. The second book, *Glosas Marginales de Vetus Latina en las Biblias Vulgatas Españolas. 1-2 Samuel* was edited by Ciriaca Morano Rodríguez. Both appeared in 1989.

LXX Dissertation Abstract

The following abstract of a dissertation completed under the supervison of Professor Michael Fox at Wisconsin (Madison) was submitted by Ronald Lewis Troxel.

ESCHATOLOGY IN THE SEPTUAGINT OF ISAIAH

This study examines LXX-Isa's hopes for the future. It is more than a text-critical resource: it is an examination of the translation's theology. The study also considers how this translation reflects the understanding of Isaiah by the translator's religious community at Leontopolis, thereby revealing that community's understanding of its situation, and disclosing some of its hopes for the future.

The study asserts that LXX-Isa's hopes are best understood in the light of the book's nascence during the program of Hellenization imposed by Antiochus IV, the book's sharp contraposition between the pious (as Torah-observers) and the impious, and its conviction that the oppression of Judea by Antiochus IV was God's punishment of Israel for its neglect of the Torah.

The study examines, in turn, LXX-Isa's hopes for judgment of the impious, the deliverance of the pious and Israel, the role of Isaiah's servant-figure, the character of the new age for Israel, and the effects of the new age on the world. In considering each theme, the study discusses both passages that diverge from the LXX's *Vorlage* and ones that agree with it. For once it has been shown--through analysis of divergent renderings--that a theme is important to the translator, one can give equal weight to texts rendered more "literally" and ones rendered paraphrastically.

The final chapter asks whether LXX-Isa can be deemed eschatological. If we define eschatology as a vision of the in-breaking of a new age introduced by God, which definitively realizes human ideals, then LXX-Isa is eschatological, insofar as it envisions God's definitive establishment of Israel's deliverance and of universal piety.

Manchester LXX Symposium

The University of Manchester's Department of Biblical Criticism & Exegesis is sponsoring a Symposium on July 30-Aug 2, 1990 entitled: The Septuagint and its Relations to the Dead Sea Scrolls and Other Writings. The pricipal lecturers will be Professors Brock, Hanhart, Tov and Ulrich and several sessions will be devoted to shorter papers. Further inquiries may be directed to: Dr. George Brooke, Dept of Biblical Criticism & Exegesis, Faculty of Theology University of Manchester, Oxford Road, Manchester M13 9PL, England.

RECORD OF WORK

PUBLISHED OR IN PROGRESS

BOOKS AND ARTICLES:

Alba Cecilia, Amparo. *Biblia Babilónica. Jeremías.* Textos y Estudios «Cardinal Cisneros» 41. Madrid: Instituto de Filologia-CSIC, 1987.

Alexandre, Monique. *Le Commencement du Livre Genèse I-IV. La version grecque de la Septante et sa réception.* Paris: Beauchesne, 1988. [Reviewed by Alison Salvesen in *JSS* 40 (1989) 246-7]

Ammassari, A., ed. *Il Salterio latino di Pietro.* Rome: Città Nuova, 1987.

Brashear, W. "Potpourri [Samaritan and LXX papyri]." *Archiv für Papyrusforschung* 34 (1988) 5-13.

Brock, Sebastian. "The Septuagint." *Sourozh* 29 (1987) 32-44.

Brown, J. P. "The Septuagint as a Source of the Greek Loan-Words in the Targums." *Bib* 70 (1989) 194-216.

Catastini, A. (1) "4QSam[a]: I. Samuele il 'Nazireo'." *Hen* 9 (1987) 161-95. (2) II. Nahash il 'Serpente'." *Hen* 10 (1988) 17-49.

Clarke, E. G. ed. *Newsletter for Targumic and Cognate Studies 16:2* Toronto: U. of Toronto Dept of Near Eastern Studies, 1989.

Crown, Alan D. "The Morphology of Paper in Samaritan Manuscripts: A Diachronic Profile." *BJRL* 71 (1989) 71-93.

De Vries, Simon J. "The Three Comparisons in 1 Kings xxii 4b and its Parallel and 2 Kings iii 7b.": *VT* 39 (1989) 283-306.

de Robert, Philippe. "Aspects de l'exégèse samaritaine." *ETR* 62 (1987) 551-54.

Dirksen, P. B. and Mulder, M . J. *The Peshitta: Its Early Text and History. Papers Read at the Peshitta Symposium Held at Leiden 30-31 August 1985.* Monographs of the Peshitta Institute 4. Leiden: Brill 1988.

Díez-Macho, A., ed. *Biblia Polyglotta Matritensia IV: Targum Palaestinense in Pentateuchum 1: Genesis.* Madrid: C,S.I.C. , (1988).

Dubarle, A. M. "Les textes hébreux de Judith et les étapes de la formaton du livre." *Bib* 70 (1989) 255-66.

Greenspoon, Leonard. "Mission to Alexandria: Truth and Legend about the Creation of the Septuagint." *Bible Review* 5 (1989) 34-41.

Gross, H. *Tobit-Judith*. Die Neue Echter Bibel. Würzburg: Echter, 1987.

Haelewyck, J. C. "Le cantique 'De nocte'. Histoire du texte vieux latin d'Is. 26, 9b-20(21)." *R Bén* 99 (1989) 7-34.

Haendler, Von Gert. " Zur Arbeit an altlateinischen Bibelübersetzungen." *TLZ* 114 (1989) 1-12.

Haran, M. "The Two Text-Forms of Psalm 151." *JJS* 39 (1988) 171-82.

Le Déaut, Roger. "The Greek Bible: Hidden Treasure for Jews and Christians." In V. Mc Innis, ed., *Renewing the Judeo-Christian Wellsprings*. New York: Crossroad, 1987.

Levine, Etan. *The Aramaic Version of the Bible: Contents and Context.* BZAW 174. Berlin: De Gruyter, 1988. [Reviewed by: Rudolf Macuch in *TLZ* 114 (1989) 508-10].

Luke, K. "The Armenian Version of the Bible." *Bible Bhashyam* 13 (1987) 128-41, 291-301.

Martin, R. A., and Scorza, Sylvio. *Syntactical Concordance to the Correlated Greek and Hebrew Texts of Ruth: The Septuagint Series.* The Computer Bible 30. Wooster, OH: Biblical Research Associates,1988.

Müller, Mogens. "Hebrew sive graeca veritas. The Jewish Bible at the Time of the New Testament and the Christian Bible." *SJOT* 2 (1989) 55-71.

Neiderwimmer, K. "Ein Papyrusfragment mit Ps. 142 LXX." *Byzantinische Zeitschrift* 79 (1986) 265-7.

Nelson, Milward Douglas. *The Syriac Version of the Wisdom of Ben Sira Compared to the Greek and Hebrew Materials.* SBLDS 107. Atlanta: Scholars, 1988.

Oloffson, Staffan. "The Translation of Jer 2: 18 in the Septuagint: Methodological, Linguistic and Theological Aspects." *SJOT* 2 (1988) 169-200.

Owens, Robert. "The Early Syriac Text of Ben Sira in the Demonstrations of Aphrahat." *JSS* 34 (1989) 39-75.

Parente, F. "The Third Book of Maccabees as Ideological Document and Historical Source." *Hen* 10 (1988) 143-81.

Pérez Castro, F. ed., *El Códice de profetas de El Cairo: Vol. 6, Ezequiel.* Textos y Estudios «Cardinal Cisneros» 44 Madrid: Instituto de Filología, Departamento de Filología Bíblica y de Oriente Antiguo, 1988.

Puech, Émile. "Notes en marge de 11QPaléoLévitique. Le Fragment L, des fragments inédits et une jarre de la grotte 11." *RB* 96 (1989) 161-83.

Rehkopf, Friedrich. *Septuaginta-Vokabular*. Göttingen: Vandenhoeck & Ruprecht, 1989.

Ribera Florit, Josep. *El Targum de Isaías. La Versíon Aramea del Profeta Isaías.* Bibliotheca Midrásica 6. Valencia: Institución S. Jerónimo, 1988.

Robertson, Stuart D. Reports that he is working on a dissertaton under the direction of Professor Louis Feldman on " Josephus' Use of the Exodus Material in *The Antiquities of the Jews*"

Rothschild, Jean-Pierre, and Sixdenier, Guy Dominic, eds., *Études samaritaines. Pentateuch et Targum, exégèse et philologie, chroniques. Communications présentées a la table ronde internationale 'Les manuscrits samaritains. Problemes et méthodes' (Paris 7-9 octobre 1985).* Collection de la Revue des Études Juives 6. Louvain/Paris: Peeters, 1988.

Seitz, Christopher. "The Prophet Moses and the Canonical Shape of Jeremiah." *ZAW* 101 (1989) 3-27.

Skehan, Patrick W. and Di Lella, A. A. *The Wisdom of Ben Sira.* Anchor Bible 39 (New York: Doubleday, 1987). [Reviewed by: P. C. Beentjes in *JSJ* 19 (1988) 268-71; Robert R. Hann in *CBQ* 51 (1989) 350-51; M. Gilbert in *Bib* 70 (1989) 172-4.]

Tal, A. "The Samaritan Targumic Version of 'The Blessing of Moses' (Dt 33) According to an Unpublished Ancient Fragment." *Abr N* 14 (1986) 178-95.

Talshir, Z. "The Representation of the Divine Epithet tseva'ot in the Septuagint and the Accepted Division of the Books of Kingdoms." *JQR* 78 (1987) 57-75.

Taylor, Bernard. (1) An Analysis of Manuscripts b o c_2 e_2 in 1 Reigns." *HSM* (forthcoming). (2) "Lucian and the Old Greek in 1 Reigns: A Statistical Analysis." *Textus* (forthcoming). Review of: D. N. MILWARD, *The Syriac Version of the Wisdom of Ben Sira Compared to the Greek and Hebrew Materials.* SBLDS 107 in *JAOS* (forthcoming).

Tov, Emanuel. "Die Septuaginta in ihrem theologischen und traditionsgeschichtlichen Verhältnis zur hebräischen Bibel." In *Mitte der*

Schrift? Ein jüdisch-christliches Gespräch. Martin Klopfenstein, ed. Bern: Land, 1987.

Treu, K. "Christliche Papryi XIII." *Archiv für Papyrusforschung* 34 (1988) 67-78.

Troxel, Ronald Lewis. Completed a dissertation at the University of Wisconsin (Madison) under the supervision of Professor Michael V. Fox in June 1989 entitled "Eschatology in the Septuagint of Isaiah." [see abstract in News and Notes].

Ulrich, Eugene. "Daniel Manuscripts from Qumran. Part 2: Preliminary Editions of 4QDan^b and 4Q Dan^c." *BASOR* 274 (1989) 3-26.

Waltke, Bruce. "Aims of OT Textual Criticism." *WTJ* 51 (1989) 93-108.

REVIEWS :

AEJMELAEUS, A. and R. SOLLAMO eds. *Studien Zur Septuaginta-Syntax. Festschrift für Ilmari Soisalon-Soinenen.* (Helsinki: Academia Scientiarum Fennica, 1987). Reviewed by: Mario **Cimosa** in *Salesianum* 51 (1989) 346-7; Terrence E. **Fretheim** in *JBL* 108 (1989) 367.

DOMINIQUE BARTHÉLEMY, *Critique Textuelle de l'Ancien Testament, 2: Isaïe, Jéréremie, Lamentations. OBO* 50/2 (Fribourg: Editions Universitaires, Göttingen: Vandenhoeck and Ruprecht, 1986). Reviewed by: Paul E. **Dion** in *JBL* 107 (1988) 737-9.

DORIVAL, G. HARL, M. and MUNNICH, O. *La Bible grecque des Septante.* (Paris: Cerf/ C.N.R.S., 1988). Reviewed by: Gerard J. **Norton** in *RB* 96 (1989) 107-11.

ENERMALM-OGAWA, AGNETA. *Un language de prière juif en grec. Le témoignage des deux premiers livres des Maccabées,* Con B NT 17 (Stockholm: Almqvist & Wiksell, 1987). Reviewed by: D. A. **Bertrand** in *RHPR* 69 (1989) 215; Mario Cimosa in *Salesianum* 51 (1989) 354-5.

GELSTON, A. *The Peshitta of the Twelve Prophets.* Oxford: Clarendon Press, 1987. Reviewed by: M.P. **Weitzman**, in *JTS* 40 (1989) 162-65; M. D. Koster, in *JSS* 33 (1988) 281-85; Bruce **Chilton** in *JBL* 107 (1988) 744-6; M. J. **Mulder** in *NedTTs* 43 (1989) 143-4.

HARLÉ, PAUL, and PRALON, D., eds. *La Bible d'Alexandrie. 3. Le Lévitique. Traduction du texte grec de la Septante, Introduction et Notes.* (Paris: Éditions du Cerf, 1988). Reviewed by: A. **Marx** in *RHPR* 69 (1989) 205-6; A. **Hilhorst** in *JSJ* 20 (1989) 90-91.

JEANSONNE, SHARON PACE. *The Old Greek Translation of Daniel 7-12.* CBQMS 19 (Washington: Catholic Biblical Association, 1988). Reviewed by: H. D. F. **Sparks** in *JTS* 40 (1989) 533-5; J. **Lust** in *ETL* 65 (1989) 160-63.

KLEIN, MICHAEL L. *Genizah Manuscripts of Palestinian Targum to the Pentateuch,* 2 vols. (Cincinnati: Hebrew Union College Press, 1986). Reviewed by: R. **Vicent** in *Salesianum* 51 (1989) 155; Johan **Maier,** in *BZ* 32 (1988) 304-5; Bruce **Chilton** in *JBL* 107 (1988) 772-5; Sebastian **Brock** in *JSS* 33(1988) 279-81; Moses **Aberbach** in *CBQ* 51 (1989) 335-7.

LUST, J. ed. *Ezekiel and His Book.* BETL 74 (Leuven: Peeters, 1986). Reviewed by: Josef **Schreiner** in *BZ* 33 (1989) 136-37; Moisis **Rudolf** in *TRev* 84 (1988) 454-8.

SANDERSON, JUDITH. *An Exodus Scroll from Qumran: 4Q PaleoExod^m and the Samaritan Tradition.* Harvard Semitic Studies 30. (Atlanta : Scholars 1986). Reviewed by: Jean **Duhaime** in *CBQ* 51 (1989) 349-50.

SKEHAN, PATRICK W. and DI LELLA, A. A. The Wisdom of Ben Sira. Anchor Bible 39 (New York: Doubleday, 1987). Reviewed by: P. C. **Beentjes** in *JSJ* 19 (1988) 268-71; Robert R. **Hann** in *CBQ* 51 (1989) 350-51; M. **Gilbert** in *Bib* 70 (1989) 172-4.

STROTHMANN, W. *Kon kordanz sur syrischen Bibel. Der Pentateucch* vols. (Wiesbaden: Otto Harrowitz, 1986). Reviewed by: M. J. **Mulder** in *Bib Or* 45 (1988) 183-4;

STULMAN, LOUIS. *The Prose Sermons of the Book of Jeremiah.* SBLDS 83 (Atlanta: Scholars Press, 1986). Reviewed by Christopher R. **Seitz** in *JBL* 107 (1988) 520-21.

_____. *The Other Text of Jeremiah.* Landham MD: University Press of America, 1986). Reviewed by: F. **Raurell** in *Estudios Franciscanos* 90 (1989) 306-7.

TOV, EMANUEL. *A Computerized Data Base for Septuagint Studies: The Parallel Aligned Text of the Greek and Hebrew Bible.* (Stellenbosch: JNSL, 1986). Review by: D. R. **de Lacey** in *JTS* 40 (1989) 536-8; E. **Talstra** in *Ned T Ts* 43 (1989) 143-4.

WEVERS, JOHN WILLIAM. *Text History of the Greek Leviticus.* (Göttingen: Vandenhoeck & Ruprecht, 1986). Reviewed by: Byron **Shafer** in *CBQ* 51 (1989) 545-6; A. **Schenker** in *T Rev* 84 (1988) 366-7; Ralph W. Klein in *JBL* 107 (1988) 510-11; Anneli **Aejmelaeus** in *TLZ* 114 (1989) 17-19; Mario **Cimosa** in *Salesianum* 51 (1989) 364.

A CRITIQUE OF SVEN SODERLUND'S

THE GREEK TEXT OF JEREMIAH:

A REVISED HYPOTHESIS

J. Gerald Janzen, Christian Theological Seminary

The enigmatic sub-title notwithstanding, Soderlund in this book does not so much propose and defend a hypothesis of his own, as "seek to evaluate some of the major studies on [the Greek text of Jeremiah] that have appeared during the last 25 years," [1] The studies he evaluates pertain to three areas: (1) reconstruction of the earliest form of the Greek translation of Jeremiah (Joseph Ziegler); (2) resolution of the question concerning differences between the two halves of Greek Jeremiah (Emanuel Tov); and (3) relations between the Greek and the Hebrew text (the present writer).[2] Soderlund's evaluations of these three studies are made on the basis of his "in-depth analysis" of chapter 29 of Greek Jeremiah, "a passage of limited length in Jer-LXX that is tangent [3] in significant ways to the problem areas posed" (p. 2). However, "where

---

[1]Sven Soderlund, *The Greek Text of Jeremiah: A Revised Hypothesis.* JSOTS 47 (Sheffield: JSOT Press, 1985), pp. 1-2. Hereafter, page numbers in the text.

[2](1) Joseph Ziegler, *Septuaginta. Vetus Testamentum Graecum* : Vol 15, *Jeremias, Baruch, Threni, Epistula Ieremiae* (Göttingen: Vandenhoeck and Ruprecht, 1957, 2nd ed., 1976); (2) Emanuel Tov, *The Septuagint Translation of Jeremiah and Baruch : A Discussion of an Early Revision of the LXX of Jeremiah 29-52 and Baruch 1:1-3:8 HSM* 8, (Missoula: Scholars's Press, 1976); and (3) J. Gerald Janzen, *Studies in the Text of Jeremiah HSM* 6, (Cambridge: Harvard University Press, 1973). Hereafter, author and page number in the text.

[3]The word "tangent" is apt in an unintended sense. In spite of Soderlund's assurances, chapter 29 does not enable as "frontal" a consideration of Tov and myself as should be considered adequate. See below.

appropriate, Ch. 29 becomes the springboard for a discussion of issues that is comprehensive for the whole text of Jer." (p. 3).

In the greater part of the book (almost two-thirds, not counting the introduction), Soderlund offers first an exhaustively detailed "Inductive Study of the Manuscript Evidence for Jer-LXX CH. 29" (Chapter Two; 83 pp.), and then a "Critique of J. Ziegler's Critical Edition of Jer-LXX" (Chapter Three; 56 pp.). The first study is a monument to painstaking examination of individual manuscripts, and description, collation and grouping of the evidence into text-families. Assessment of this work must be left to those who habitually work with the data at similarly close range. The second study will be valuable to anyone who works with the Göttingen Septuagint. Soderlund gives Ziegler high marks for the reliability of the data in the apparatus, as well as for judiciousness in the reconstruction of the text itself. His major criticism has to do with Ziegler's approach to conjectural emendations in the reconstructed text, preferring that such emendations be confined to the apparatus or signalled by some typographical means (e.g., daggers) (p. 152), and holding that "the text would be improved if conjectural emendations were reduced even further" than Ziegler's already relatively conservative practice. This criticism underscores the fact that the Göttingen Septuagint is not a terminus but a milestone on the way to the original Greek text. To the present writer, who once spent two years in daily involvement with Ziegler's reconstructed text and critical apparatus, Soderlund's work here appears to be of very high quality. As one reviewer puts it, his assessment "elevate[s] the overall achievement of Ziegler to the category of a classic." [4]

If Ziegler emerges with flying colors, the same cannot be said for the two other works. In Soderlund's view, Tov has not succeeded in over-

---

[4]Leonard Greenspoon, *JBL* 107 (1988), 127.

turning Thackeray's two-translator theory, nor has the present writer established either that the short Greek text of Jeremiah always reflects a short Hebrew *Vorlage* or that where it does so the latter is superior to the longer Massoretic text.

As is well known to readers of this periodical, works on textual criticism arise out of protracted labor over multitudinous details, and understandably reviews often come as reports of their basic arguments and chief conclusions, with evaluations based on narrow probes into the underlying data, while thoroughgoing assessment is left to those whose own technical researches call for similar protracted study. Before his evaluations of the work of Tov and the present writer on Jeremiah are absorbed into the thinking of the wider scholarly public, those evaluations themselves call for assessment of intermediate length. In this article I shall offer such a critique, at times attempting also to advance the understanding of the textual loci in question.

I. *Soderlund on Tov on the Septuagint Translation of Jeremiah*

In the Greek text of Jeremiah, a decisive difference in vocabulary choice beginning at chapter 29 [5] has led to the designations *Jer a'* (chaps 1-28) and *Jer b'* (chaps 29-52). Thus, e.g., כה אמר is rendered τάδε λέγει 61x in Jer a' but only 3x in Jer b', while it is rendered οὕτως εἶπε not at all in Jer a' and 71x in Jer b'. In 1903 Thackeray accounted for this difference by positing two translators. Subsequently he applied his multiple translator theory to similarly striking disjunctions of usage in Ezekiel and in the Books of Reigns (Samuel-Kings). With the discovery of biblical

---

[5]i.e., at chap 29 of the Greek text. The different location and internal ordering of the Oracles against foreign nations in MT and G means that after 25:13 two systems of citation must be employed: primary reference to G will cite MT in parentheses; primary reference to MT will be by standard citation followed by (MT).

manuscripts in both Hebrew and Greek since the 1940's, others have tended to discern in some of these disjunctive usages the hand of subsequent revisers of an Old Greek text. Also Joseph Ziegler, who by and large assumed Thackery's explanation of Jeremiah, at one point questioned its correctness in the light of distinctive similarities between Jer a' and Jer b'. Tov has explored the range and the character of those similarities, and has argued that they can be accounted for only on the hypothesis of an original unified translation partially marked by later revision.

As Soderlund acknowledges, Tov's argument for an original unity of translation, from agreements between Jer a' and Jer b', methodologically emulates Thackeray's argument elsewhere for the translational unity of Jer b' and Baruch 1:1-3:18 (Bar a'). However, he asserts, Thackeray's own vacillations concerning the translational unity of Jer b' and Bar a' demonstrate the difficulty in principle with arguments from agreement. Moreover, in scrutinizing 12 of Tov's 45 agreements between Jer a' and Jer b', he finds them dubious or insignificant. Allowing that Tov "has effectively taken the available evidence [for the unity hypothesis] as far as it will go," he concludes that it does not go far enough to overthrow Thackeray's two-translator theory, so that Tov's reviser hypothesis remains unproven (p. 192).

In taking up the question of method, I place just the opposite construction on Thackeray's changing views as to the relation between Jer b' and Bar a'. Had his comments in both 1923 and 1928 agreed with his conclusions of 1903, one might take them as mere reiterations of his initial conclusions. But his words in 1923 show that he had returned to the question with sufficient care to reverse himself:

As regards the Greek and its relation to the translation of Jeremiah I have reluctantly abandoned my former view. . . . The similarity in Part I [of Baruch] to the style of the second translator of Jeremiah is due to a close imitation of his style, and is insufficient to prove that the translations were made by one and the same man." [6]

We should note the import of his words: First, the similarity is *insufficient* to prove identity of translator--that is, in principle similarity may be sufficient to enable identification of a single translator. Second, in this instance the similarity *is* sufficient to require explanation in terms other than coincidence. In 1928, upon yet further reassessment he found the "close imitation" theory inadequate:

> . . . there are *numerous parallels* with the LXX of Jeremiah, and here Baruch presents such *marked resemblances* to the style of the second of the two translators of that book as to suggest identity of hands, i.e. that the Confession in Baruch was already attached to the Hebrew Jeremiah when that work was translated into Greek. *Imitation will hardly account for all the phenomena* (see *J.T.S.*, IV, 261; the writer here reverts to his former view, which he was inclined to abandon, *Septuagint and Jewish Worship*, 87).[7]

This evidence of repeated study gives Thackeray's final verdict the character of a seasoned view, and affirms the validity in principle of arguments from agreement. It should be noted that in flirting briefly with the "imitation" theory Thackeray did not have to account for striking *differences* between Jer b' and

---

[6]H. St. John Thackeray, *The Septuagint and Jewish Worship* (London: The British Academy, 1923), p. 87.

[7]In Charles Gore, Henry Leighton Goudge and Alfred Guillaume, eds., *A New Commentary on Holy Scripture* (London: SPCK, 1928), Part II, p. 105 (italics added).

Bar a', such as differentiate Jer a' and b'. Had there been such differences, one suspects he would never have proposed imitation. In any case, pronounced similarities between Jer a' and b' cannot be attributed to *imitation* or (what amounts to the same thing) to the collaboration of two contemporaneous translators, unless one can provide a plausible *countervailing* motivation for the many differences.

A second comment on method, this time Soderlund's: in assessing Tov's "reviser" hypothesis, he confines himself to the 12 of Tov's 45 agreements between Jer a' and b' that fall in chapter 29. This sampling is inadequate. Since Tov's argument from agreements between Jer a' and b' encompasses only 45 similarities, requiring only 22 pages of discussion, would it have been too time- or space-consuming to re-examine all the evidence? Or would justice to Tov's argument not have been better served by attending at least to his strongest cases, instead of those that just happened to fall within chapter 29? As a case in point we may take the six Greek renditions to which Tov drew special attention and which, he maintained, "reflect an *unusual* [i.e., "incorrect"] *common understanding of a certain Hebrew word* and are therefore *of special importance* for establishing the common basis of Jer a' and b'." (italics added) Of these six renditions, only one occurs in chapter 29. I shall review the first five and then consider Soderlund's assessment of the sixth.

1. (Tov's #1, p. 24). (‏ה אדני‎) ‏אהה‎ -- ὁ ὤν Jer a' 3x, Jer b' 1x. The translator "probably derived ‏אהה‎ ('alas') from the root ‏היה‎ and . . . translated it in accordance with the LXX of Ex 3:14 ‏אהיה אשר אהיה‎ -- ἐγώ εἰμι ὁ ὤν." The distinctive agreement between Jer a' and b' is striking.

2. (Tov's # 14, p. 27). חצות חוץ ('street[s]', 'open spaces') -- ἔξωθεν, Jer a' 3x, Jer b' 6x (+Bar a' 1x). The data may be displayed as follows:

Incorrect renderings	Jeremiah a'	b' + Bar a'
in the Jeremianic cliché בערי יהודה ובחצות ירושלם	1	6
outside the cliché " " "	2	1
Correct renderings Standard to LXX)		
in the Jeremianic cliché (בערי יהודה ו)בחצות ירושלם	5	0

However we explain the occurrence of both correct and incorrect renderings of within Jer a', we would be hard pressed to argue that a different translator of Jer b' consistently imitated the incorrect renderings in Jer a' rather than one or other of the (more numerous) correct renderings. This datum alone deals a severe blow to "imitation" (or collaboration) theories, and strongly suggests a single translator.

3. (Tov's # 15, p. 28). תעלה -- ὠφέλεια Jer a' 1x, Jer b' 1x.

4. (Tov's # 27, p. 31). מסגר-- δεσμώτης. Other translators correctly construed the word actively (συγκλείων, συγκλείστης = מסגיר); but in Jer a' and b' (24:1, 36:2) the passive construal distorted its meaning (= מְסֻגָּר or מְסֻגַּר "prisoner").

5. (Tov's # 28, pp. 31-32). חול -- ἥκω. Jer a' 1x, Jer b' 1x.

6. (Tov's # 30, p. 32). הועיד-- ἀνθίστημι Jer a' 1x, Jer b' 1x (also Job 9:19, in similar context). Tov comments: "Because עמד was frequently rendered in the LXX with ανίστημι the translators of Jer and Job must have derived יועדני somehow from עמד or otherwise their *Vorlagen* differed from MT. The latter assumption may be supported by the rendition of Aq and Sym in 29:20(49:19): ὑφίστημι." Soderlund objects that: (1) The close

similarities generally in the Greek renderings of 27(50):44 and 29:20(49:19) suggest secondary harmonizing to produce the similar rendering of הועיד (2) Jer 2x and Job 9:19 contain the only occurrences of הועיד in the MT. (3) "The translation by ἀνθίστημι evidently derives from an association with the root עמד which on several occasions in the LXX was rendered by ἀνθίστημι " (166). His first point is pure conjecture. The appeal to a possible similar harmonization in ἐκδιώκω αὐτούς" assumes" (his word) gratuitously that the Greek text there has the same *Vorlage* as MT. He does not indicate how the second point bears on the issue. As for his third point, it is unlikely that the translators simply followed LXX precedent in rendering a form they thought was from עמד. The latter verb is rendered by ἀνθίστημι where its object is governed by a preposition (לפני בפני or נגד), never where the object is a suffix as in the three cases under discussion. More likely the general sense of הועיד was guessed from context. This suggests a single translator in Jeremiah. The rendering in Job may derive from there.

Of the six cases just examined, # 6 is perhaps the weakest datum for Tov's argument. But this points up the inadequacy--not to say unfairness--of Soderlund's method in confining himself to chapter 29 G and thereby ignoring ## 1-5. Let us turn now to weigh others of his specific assessments of Tov's data and arguments.

7. (Tov's #6, p. 25; Soderlund, pp. 155-56). הילל-- ἀλαλάξω Jer a' 1x, Jer b' 4x. Otherwise ὀλολύξω in LXX (+ 1x Jer b'). Soderlund questions "whether the equivalence ἀλαλάξω / הילל is to be regarded too exceptional for it to have originated independently in two different translators," especially since it is similar in both sound and meaning to ὀλολύξω. The question is rather why, given this two-fold similarity, we find this distinctive choice only in Jer a' and b'.

8. (Tov's # 19, p. 29; Soderlund, pp. 156-158).כל-- ἄπας. "Tov's statistics for saying " ἄπας occurs more often in Jer-LXX than in any other book of the LXX," and for saying, "the original translator of Jer (or the first scribe?) thus used ἄπας more than his fellow translators," are based on the entries in *Hatch-Redpath*. According to Soderlund, in the Rahlfs text ἄπας occurs in Jer a' 6x, in Jer b' 11x; in the Ziegler text, in Jer a' 3x, in Jer b' 6x. These 17 or 9 occurrences compare with 35 occurrences for *all the rest* of the LXX according to *Hatch-Redpath* (17x in Codex B). Soderlund is hardly correct in asserting that "the frequency of ἄπας in Jer is only *marginally* greater than in other books" (italics added), for 9 is one fourth of the 35 occurring *everywhere* else (one-half of the 17 in Codex B). He goes on to say, ". . . what may be of even more significance is the fact that no matter what set of calculations is used, Jer b' always has a higher frequency of occurrences than Jer a' (on the average, twice as high), an observation that could be taken as an argument for the *difference* rather than for the similarity between the two parts." The argument is doubly fallacious: (1) He takes a 2 to 1 difference in frequency to suggest different translators in Jer a' and b', but terms "marginal" a much higher difference between Jer and any other single book in the LXX. (2) The 2 to 1 difference is not "significant"; it is in ratio with totals for כל in MT: Jer a' ca. 191x; Jer b' ca. 284x.

9. (Tov's # 25, pp. 30-31; Soderlund, pp. 159-60).הלך-- οἴχομαι. Tov's statistics are Jer a' 3x, Jer b' 8x (including 29:8(49:7) where οἴχομαι = נסרה).[8] Soderlund writes, "the rate of occurrence of

---

[8]Soderlund's statistics, Jer a' 3x, Jer b' 6x, should be 3x and 7x, since he too later refers to 29:8(49:7). The text in dispute is 52:7, where the manuscript witness (Ziegler) is ἐπορεύθησαν] ωχοντο A-106' C'-613. Commenting on the C' group of texts, Soderlund elsewhere observes that "its erratic behaviour demands great care in separating the grain from the chaff." Nevertheless he points out that "[t]he early date of Codex Alexandrinus and composite nature of its text hold out the possibility that among its component parts is preserved much ancient material." (p. 92) Given (1) the massive frequency of πορεύομαι for הלך in the LXX generally (15 columns in *Hatch-Redpath*), including about 56x in Jer, (2) the rarity of οἴχομαι for הלך in LXX generally, (3) the frequency of the usage in Jer, and (4) the

---

οἴχομαι in Jer is admittedly high in comparison with the rest of the LXX . . . and could conceivably point to a special preference for this verb by the presumed single translator of Jer-OG," but, he cautions: (1) the equivalence οἴχομαι-- הלך can hardly be called rare since outside of Jer it occurs 3x in Gen, 2x in Job and 1x in 2 Chr. (2) "What is 'rare' indeed is the use of οἴχομαι for the niphal of סרה , but then this may not be overly significant either since the root סרה was rendered by a different Greek word on each of the 7 occasions where it occurred in the OT." (3) the proportion of 1:2 in Jer a' and b' (as with ἄπας) calls attention rather to the differences between the two parts. The third reservation is captious, as is the first (on both compare #8 above). In Genesis this Greek verb meaning "to go *out, to depart*," is used to render הלך with the same nuance. In Job the verb occurs 4x: for הלך (14:20, 19:10), for חלף(14:10), and for רדף (30:15, // עבר), in each instance with the special sense (suiting the Hebrew context) of "departing (this life)" or "going out of existence" (compare Hosea 10:14, οἴχομαι = שדד). Now, Soderlund finds no significance in the 'rare' use of οἴχομαι for נסרה at Jer 29:8(49:7). The question is rather why, given the variety of LXX treatments of סרה , it was translated with οἴχομαι just here in Jer with a nuance exploited also by the translators of Job and the Twelve. (Compare the agreement between Job and Jeremiah, above # 6; and Tov's comments on the affinities between Greek Jeremiah and Greek Minor Prophets, pp. 135-155.) Soderlund's attempts to minimize the significance of οἴχομαι in Jer are forced and tendentious.

---

the translation of הלך with πορεύομαι in // 2 Kgs 25:4, it is likely that at Jer 52:7 ωχοντο (C'-613) is original and the other witnesses reflect contamination from Kings. In that case Tov's figures are correct. Given the usage in Job, Hosea 10:14, and Jer 29:8(49:7), it may be that we should reconsider Jer 10:20 where for MT אהלי שדד the Greek evidence for the verb after ἡ σκηνή μου is ἐταλαιπώρησε Spohn] + ωχετο S*: + ωλεσεν 198; + (÷ O -Q-86) ωλετο rel. In view of Ziegler's identification of a few pre-hexaplaric readings attested only in S or S* (at times with one or two minuscules) (Ziegler, p. 49), it is conceivable that at 10:20 S* retains the peculiar nuance of οἴχομαι analogous to its use in 29:8(49:7). In that case both Jer a' and b' resemble Job and Hosea translators.

10. (Tov's # 31, pp. 32-33; Soderlund, pp. 160-161). ἐγχειρέω, "take a thing in hand, undertake, attempt, lay hands on, attack," Jer a' 2x, Jer b' 1x; ἐγχείρημα , Jer a' 1x, Jer b' 1x. Elsewhere in LXX ἐγχειρέω occurs only at 2 Chr 23:18. Soderlund's critique exemplifies his tortuous efforts to minimize Tov's data. He observes that ἐγχειρέω "is not an uncommon Greek verb (cf. LSJ) so that its appearance 3 times in Jer should perhaps not surprise us too much." The question is rather why, given its commonness in Greek, it does not turn up more often in LXX generally.[9] He explains the "strange equivalence" ἐγχειρέω-- הֹשִׂיא in 29:17(49:16) "by the confusion of the hiphil of נשׂא with the qal of נשׁא (similarly in 44[37]:9)." The bearing of this explanation on the significance of the translator's choice of ἐγχειρέω is quite the contrary of what he claims. He holds that " since the qal of נשׂא was translated in the LXX by some 90 different Greek words . . . , one has the right to expect almost anything as a translation of this verb." Rather, the remarkable thing is why, given this wide variety of approach to נשׂא, LXX never rendered it with this not uncommon word, while in Jeremiah it crops up no less than 5 times.

After these demurrals Soderlund admits, "However, it must be acknowledged that the unique use of the derivative noun ἐγχείρημα in the parallel passages of Jer-LXX 23:20 and 37(30):24 is noteworthy." Taken together with the verbal usage, "this phenomenon is striking." Yet he concludes, "How to account for the phenomenon is less clear." This is because he does not find enough instances of this type of agreement in the rest of the book. Therefore he is driven to "look to other explanations even for the appearances of ἐγχειρέω / ἐγχείρημα in the two halves of Jer. Sheer

coincidence, harmonization of parallel passages (though discounted by Tov, p. 33), cooperation and cross consultation among translators may be listed among such possibilities." At this point his intransigence becomes manifest. How many times will "sheer coincidence" account for Tov's 45 examples? His unargued dismissal of Tov's argument (from *evidence*) against the possibility of harmonization between 23:20 and 37(30):24 is disingenuous, and reverses the grounds on which he elsewhere argues for harmonization (above, # 6). His appeal to "cooperation" ignores the obstacle to such a theory in the many pronounced differences between Jer a' and b' which cannot have arisen between two translators working with any closeness of collaboration. Too often Soderlund strains at gnats in Tov's data, and then swallows camels in offering alternate explanations, or leaves the data unexplained.

11 (Tov's # 26, p. 31; Soderlund, pp. 162-164).יהוה צבאות-- κύριος παντοκράτωρ, Jer a' 7x, Jer b' 7x. Tov's argument is that "only in Jer and the M[inor] P[rophets] . . . is the Hebrew phrase rendered exclusively by κύριος παντοκράτωρ." Soderlund admits that this fact "may point to some interdependence among these books --including the two halves of Jeremiah-- but whether this interdependence is best understood in terms of a single translator is another question. Again Thackeray was aware of the common rendition of παντοκράτωρ running through Jer and MP . . . ." The appeal (here and elsewhere) to Thackeray as familiar with the data without resorting to a "reviser" theory overlooks that, whereas Thackeray studied and restudied the question of the significance of the agreements between Jer b' and Bar a'-- coming finally to the seasoned conclusion that the agreements required the hypothesis of a single translator--he only noted certain agreements between Jer a' and b', and then not nearly Tov's number (9 to Tov's 45!).[10]

---

[9]The sole occurrence outside of Jeremiah, in 2 Chron 23:18, may be explained by reference to the specifics of the Hebrew sentence:

וַיָּשֶׂם יהוידע פקדת בית יהוה בְּיַד הכהנים
καὶ ἐνεχείρησεν Ιωδαε ὁ ἱερεὺς τὰ ἔργα οἴκου κύριος διὰ χειρὸς ἱερέων

---

[10]Thackeray, *The Septuagint and Jewish Worship*, pp. 33-34.

To sum up: The strength of Tov's thesis lies in a core of very strong cases which Soderlund's assessment, confined to chapter 29, largely does not consider; in a second group of cases which, if not quite as strong, are numerous and characteristic enough to call for systematic explanation; and in a third group which could be accounted for in a variety of ways but which may well attest the single translator which the firmer examples point to. This variety of evidence is to be expected. The whole point of a hypothesis is to provide a reasonable account of individual phenomena whose significance in isolation is not patent. In the presence of a solid core and a plausible body of data which support a hypothesis, such individually mute phenomena by their very quantity become auxiliary evidence. In my view Soderlund's unaptly selected attempts to blunt the significance of Tov's data, while now and then plausible, are too often forced, beside the point, or *ad hoc*. On the other hand, it is the quantity as well as the quality of the cited agreements between Jer a' and Jer b' that vindicates Tov's use of the method Thackeray employed in identifying the translator of Jer b' with the translator of Bar a', and that argues for the soundness of his conclusions.

II. *Soderlund on Janzen on Greek and Hebrew Jeremiah*

In assessing Soderlund's evaluation of my study of the Greek and Hebrew texts of Jeremiah, I will first take up his critique of the general bases on which I proceeded, then examine a number of his specific discussions of readings in chapter 29.

1. *Justification for my study.* According to Soderlund (195-199), I reopened the question of the relation between Hebrew and Greek Jeremiah for three reasons: 1) the existence of improved Greek text critical data, 2) the discovery of new Hebrew MS evidence, and 3) the unsatisfactory character of

the present-day approach to the text of Jeremiah. This omits any reference to one of my justifications, the fact that "Current studies based on the biblical manuscripts from Qumran are presenting a new picture of the history of the biblical text in its broad outlines and are vindicating the method which seeks to use the Septuagint as a witness to a text tradition at times substantially divergent from M." (Janzen, p. 7). The result is to isolate my work from its context in current text-critical studies informed by fresh data which necessitate hypothesis-formation. Soderlund reports my second justification as it is stated in my introduction; but detailed study of the body of my work (including appendices) should have alerted him to the closely related fact that in chapter 52 Greek variants are frequently supported by the MT of the parallel passage in 2 Kings.

A related issue is the legitimacy of retroversion from the Greek text to a supposed underlying non-Massoretic Hebrew text. Soderlund asks "whether [very fragmentary] 4QJer[b] really does validate the process of retroversion on the scale that Janzen envisages" (p. 197), and exemplifies his concern with reference to my tables in Appendices A, B.2 and [B.4], where I present proper names (divine and human) always in Hebrew characters. A number of things may be said in response. Granted that retroversion must always proceed with caution, the issue is whether we can use versional evidence *at all* in the textual criticism of the Hebrew Bible. If we can--if indeed we must--the only way to use them is to posit their *Vorlagen*. Soderlund's comments about "scale" are methodological nonsense. If the use of the versions is legitimate *at all*, then it is necessary *at every point*. Ad hoc or even selective appeal to versional evidence is methodologically uncontrolled.[11]

---

[11]See, e.g., the remarks in passing of Thomas W. Overholt, "King Nebuchadnezzar in the Jeremiah Tradition," *CBQ* 30 (1968), p. 42 n 10. Emanuel Tov, who seconds Goshen-Gottstein's cautionary remarks about retroversion, illustrates its appropriateness throughout a whole chapter of Jeremiah: "Exegetical Notes on the Hebrew Vorlage of the LXX of Jeremiah 27 (34)," *ZAW* 91 (1979), pp. 73-93.

Soderlund cites my analysis of the proper names to exemplify the problems he has with my use of retroversion. Several of the issues that divide us come to focus here. The divergence between MT and G is so striking as to rule out case by case explanation (his preferred approach) and to necessitate explanation in terms of some hypothesis: abbreviation in translation, or a "short" *Vorlage;* if the latter, abbreviation in the*Vorlage* or development in MT. In the instance of the proper names the data to be accounted for are these: (a) Where chapter 52 is paralleled by the end of 2 Kings, Kings MT supports Jer 52 G against Jer 52 MT 10 of 12 times (see Janzen, p. 69, and reference to tables). This ratio of 5 to 1 is remarkable (especially in view of the possibility of harmonization between MT Kgs and MT Jer). (b) In the space of three short verses, at 43:3-9(MT), 4QJer[b] four times sides with G in omission of name elements and never with MT, twice in respect to (בן קרח) יוחנן, once in respect to גדליהו בן אחיקם(בן שפן), once in respect to נבוזראדן(רב טבחים) (c) The name of the Babylonian king is in the Hebrew Bible usually spelled Nebuchadnezzar but in Ezekiel and Jeremiah always Nebuchadrezzar--except in Jeremiah 27: 6 - 29: 3, where all 8 times it is spelled with "n." All 8x the name is missing in G, which in this section refers only to the "king of Babylon." (d) In several passages the repeated full name of MT produces an unwieldy and stylistically grotesque text difficult to attribute to a writer (even a Deuteronomistic writer!), but understandable as the result of transmissional or recensional development. We may illustrate from the incidence of יוחנן בן קרח in 40:8-43:5(MT) (see Janzen, p. 150). The full name occurs 4x in G, opening the first two episodes involving him (40:8, 40:13[MT]), and thereafter at key points in two subsequent episodes (41:11, 43:2[MT]); in the intervals G only contains the short name יוחנן (2x, 5x, 2x). MT everywhere contains the full name, except once where it contains the short name and G has a pronoun. The style in MT is grotesque, whereas G is

supported by 4QJer[b]. (e) In, e.g., Reigns(Samuel-Kings) a tendency toward the insertion of proper names is attested in the apparatus of the "Larger Cambridge Septuagint" of Brooke-McLean. Whether or not inner-Greek, the tendency is clear: copyists or revisers often fill out a more spare text, from pronoun to name, from first name to full name, from title to title plus name. The opposite tendency, to shortening or omission, is not nearly as noticeable. Soderlund's disregard of these specific considerations in favor of a general caveat (p. 197) leaves the data either to *ad hoc* judgment or to some implicit hypothesis undefended by rigorous textual analysis.

2. *Purpose and scope of my study.* Soderlund is critical of my limited focus on "zero" variants. (Where text-witness X contains a word or more not represented in text-witness Y, Y may be said to display a "zero" variant to X.) Granting my own acknowledgement of the need for detailed examination of the content variants and the transpositions in any full assessment of the character of the texts of MT and G, he wonders "whether sufficient notice has in fact been taken of this precaution throughout the book or whether the author has not indulged in more interpretation than is warranted by the self-imposed limits of the study." (p. 195) He concludes, "When *exclusive preoccupation* with 'omission' patterns *inhibits the perception* of *possible 'translation' patterns,* then we must ask whether this approach is not deficient in some way as well." (p. 199, italics added.) The inference as to preoccupation and perception is gratuitous, and the characterization is misleading. Either he was unaware of or he "forgot" my discussion (Janzen, 88-91) of the hypothesis of abridgement from translation technique as argued in pp. xxvi-xxxix of Giesebrecht's commentary on Jeremiah, and my methodological observation that I had carefully analyzed all of Giesebrecht's over two hundred examples, giving only sufficient examples to refute the characterization of G translation as "free," "careless of the meaning of the text," or "paraphrastic." Comparing the translator's apparent approach with that reflected in Greek Isaiah, I

concluded that while the translator's handling of Jer MT *prose* (apart from the zero variants) was not as literal as in some parts of the Pentateuch, it stands much closer to the latter sort of practice than to Isaiah G. My own unreported judgment is supported by that of Emanuel Tov: "Since the LXX's translation technique is relatively literal where the two texts overlap, it is unlikely that the LXX translator would have abridged his Hebrew *Vorlage*. "[12]

Ignoring these discussions, Soderlund raises the *possibility* of a translation *pattern* in the Greek text. Yet he nowhere offers a body of evidence or systematic argument for such a pattern, but leaves the suggestion to echo in the ear of his reader. To be sure, in his discussion of specific readings (in chapter 29, where the Greek translates poetry) he is at pains to claim translational freedom or oddness, but his comments are *ad hoc* and his evidence nugatory. One may compare his discussions of 29:10(49:9) at p. 137 and p. 222. In the first passage he judges that "there appears to be no reason why [καταλειμμα] could not have been used here to render עוללות," in a footnote adding that "[t]he most common Hebrew equivalence is, as might be expected, שארית ... or שאר" (p. 267n18) (One may put the matter more strongly: καταλειμμα is a good rendering of עוללות "gleaning," given that here as always the latter is "fig. of remnant" [BDB, 760a]. The semantic equivalence is reinforced by the fact that the immediately preceding Hebrew verb is שאר , rendered καταλειψουσι.) When he comes to study the Greek-Hebrew question, however, καταλειμμα becomes evidence of "free, quasi-paraphrastic" translation. This shift in interpretation is tendentious, and typical of his resort to *ad hoc* arguments concerning translation-character.[13]

---

[12]Emanuel Tov, "Some Aspects of the Textual and Literary History of the Book of Jeremiah," in P.-M. Bogaert, ed., *Le Livre de Jeremie. BETL* 54 (Leuven: University Press, 1981), p. 148. Elsewhere Tov expands this judgment: "The clue to problems [of a short text versus a long one] . . . lies in a correct understanding of the degree of the translators' faithfulness to their Vorlage. If a certain section was rendered in a free fashion, translational omissions and additions may be expected. On the other hand, if a certain translation unit was

Again under the rubric of "limitations of my study," Soderlund faults my confinement largely to text-critical issues. His case in point is the presence or absence of the phrase "my servant" with reference to Nebuchadrezzar in 25:9, 27:6, 43:10(MT). Observing that I proposed a purely textual solution to the problem-- מלך בבל עבדי arose secondarily at 27:6 by textual corruption from מלך בבל לעבדו involving haplography of ל and confusion between ו and י , and from there entered into the text at 25:9 and 43:10(MT) in accordance with the pronounced expansionist tendencies of MT-- he chides me for failing to take into account other factors such as "theological /philological aspects of the Hebrew, or translation *Tendenz* in the Greek," as "amply demonstrated by T. W. Overholt . . . ; contra Lemke, . . . Z. Zevit . . . and D.

---

rendered faithfully, such omissions and additions are not to be expected. Consequently, if a faithfully rendered translation unit is nevertheless shorter than the MT, its Vorlage was probably also shorter. The latter situation seems to apply to the book of Jer. With the exception of passages in which the translator encountered linguistic difficulties[], Jer was rendered quite faithfully[], and the prose sections of the translation may even be regarded as literal. We should thus not expect that the translator of Jer, who adhered in general to the Hebrew, shortened his Vorlage substantially." (*ZAW* 91 [1979], pp. 74-75) In his last sentence Tov argues against the notion that the generally faithful translator would have engaged at the same time in the sort of substantial shortening of his Vorlage that would account for the zero variants in Jer G.

[13]Soderlund considers οτι /אם "unusual." One may compare, at random, Job 9:19. Elsewhere he characterizes simple οψεις as a "misrendering" of the reflexive תתגודדי (p. 221) The translator may conscientiously have taken the Hebrew word as iterative (as indeed I would take it), describing the activity of the sword in cutting. Again, he takes επαρθητι as a "strange rendition" (reflecting translator's free attitude) for דמי in 29(47):6. (p. 221). Attention to απερριθη at v. 5 (for נרמתה read as נרמחה) and to 2QJer תתגודדי for MT תתגודדי suggests that G renders a text taken as ורמי (so also in Exod 7:20, Ezek 10:15). Yet again he holds that "in v. 7 the Greek phrase επι τους καταλοιπους επεγερθηναι *mysteriously* represents MT שם יעדה, suggesting again a very free use of ." (p. 285 n 33) His desire to characterize G a certain way inhibits the perception that (1) επεγερθηναι probably renders a text in which the rare Qal form of יעד is taken as some form of עור ; (2) שם is construed as a summary collective reference to the two previous prepositional phrases, and rendered accordingly. While τους καταλοιπους here is exegetical, the use is not "free" but is controlled by the larger context which calls, as Soderlund observes, for "frequent use of καταλοιποι."

Schmidt . . . ."   He goes on, "[t] he discovery of 4QJer[b] does not give us license to disregard such exegetical and contextual considerations; their merits must be evaluated whether a shorter Hebrew text exists or not." (p. 198). The work of Schmidt is not available to me; but let us take up the others referred to.

Zevit's article demonstrates what "my servant" might have meant in the time of Jeremiah, and that such a meaning would not have been at odds with Nebuchadrezzar as portrayed otherwise in Jeremiah. This demonstration removes objections--such as Lemke's--to the possibility of such an original reading. But when he goes on to say, "As for its exclusion in the LXX, it would seem that the technical import of the phrase מלך בבל עבדי נבוכדנאצר was no longer understood by the LXX translators and that it was purposely altered or omitted,"[14] his judgment is of little weight. In the first place, he overlooks that the G translator time and again struggled conscientiously to reproduce a text not fully understood (see my critique of Giesebrecht), and would have no difficulty in rendering the straightforward phrase in question, however he may have interpreted its precise import. In the second place, Zevit simply reflects (without systematic discussion) a then-current view that G frequently abridged. Zevit's claim that the "special meaning" of עבד יהוה in the exilic and post-exilic periods "undoubtedly influenced the LXX translators"[15] merely exemplifies the *ad hoc* conjectures by which the zero variants of G are often handled. Overholt's discussion, surveying, classifying and interpreting the presence of Nebuchadrezzar in the Jeremiah tradition, does not at all support the use to which Soderlund puts it. In fact, it is clear that Overholt is inclined to attribute the differences between Jer G and Jer MT to secondary recensional activity. ("There do seem to be

---

[14]Ziony Zevit, "The Use of עבד as a Diplomatic Term in Jeremiah," *JBL* 88 (1969), p. 77.

[15]Zevit, p. 77n21.

indications that the MT of 25:1-14 as a whole represents a later handling of the tradition common to both versions."[16]) His main concern is to argue (against Lemke) for the theological appropriateness of the title עבדי for Nebuchadrezzar. Nowhere does he so much as imply omission in G by translator's *Tendenz*. On the other hand, inasmuch as Overholt's construal of secondary recensional production of the title (by Jeremiah's disciples, p. 48; or even by Jeremiah himself, p. 44) omits to consider a purely textual origin-- by a common form of textual corruption--his discussion could also be faulted for one-sidedness. The justification of my approach is that discussion of the readings in question has revolved mainly around issues of translation-tendenz or theological appropriateness, whereas the possibility of a purely textual origin (and subsequent expansion) of the reading עבדי had not been thoroughly tested. Soderlund assumes that failure to discuss non-text-critical considerations indicates failure to ponder them. My silence in some instances indicates rather my (correct or incorrect) judgment that such considerations do not yield explanations at those points. My comments in other instances evidence my attention to such factors.

Generalizing approach. We come now to the charge that I succumbed to "the inherent attraction of broad generalizations that in one sweep can solve a multitude of textual conundrums" (p. 248), and Soderlund's preference for the "mediating" position in which "each reading has to be evaluated on its own merits, resulting sometimes in a preference for the MT, sometimes for the LXX." (p. 12) As a matter of record, it was after two full years of unremitting daily work on every divergence between MT and G (as well as on translation technique where a common *Vorlage* is apparent) in all 52 chapters of Jeremiah that I made generalizations judged to account most adequately for the various

---

[16]Thomas W. Overholt, "King Nebuchadnezzar in the Jeremiah Tradition," *CBQ* 30 (1968), p. 43; see also pp, 42, 43, 44, 48.

phenomena. That is the purpose of hypotheses. In writing up my researches I stated my hypotheses in the introduction as well as in the conclusion. That is not uncommon practice. To gather from this that I began with a hypothesis with which I hoped to sweep along all evidence, willy nilly, is naive or disingenuous. Given his own concentration on chapter 29G, he would have done well to leave the issue of "hasty generalization" quietly alone.

As for the "mediating" position which Soderlund prefers over one or other consistent hypothesis (abbreviation or expansion), the following remarks by Emanuel Tov are *a propos*:

> In text-critical studies, cases of a short versus a long text are normally evaluated individually on the basis of internal evidence. However, unlike other books of the LXX, the "omissions" of Jer do not occur occasionally; rather, they characterize the LXX of this book as a whole. For this reason they should be explained collectively. . . . Either we adopt the solution suggested above that the Hebrew text of the translator was shorter than the MT or we assume that the translator deliberately shortened the Hebrew Vorlage. In any event, the attempt should be made to explain individual "omissions" as much as possible according to one of the two possible hypotheses. . . . It would be methodologically unsound to invoke one principle in one passage and another one elsewhere. For since the same types of omissions recur throughout the book, they should be approached with the same method.[17]

These remarks come from one who has written frequently on both the Greek and the Hebrew text of Jeremiah.

---

[17]Tov, "Exegetical Notes . . . ," p. 76.

But do Tov's views have independent weight? Not in Soderlund's opinion. Observing that "Cross and Janzen have returned to . . . the 'expansion' theory," he continues:

> In this they are followed by E. Tov and Y.-J. Min who have sought to move beyond the work of Janzen by defining more precisely the recensional characteristics of the MT additions[]. But when it comes to documentation of the evidence that the LXX does in fact testify to a shorter Hebrew text, we are everywhere referred to Janzen's published monograph. (p. 13)

I have not had the opportunity to inspect the work of Y.-J. Min; but would Soderlund have us suppose that text critics at the Hebrew University like Tov, Talmon and Goshen-Gottstein would accept a thesis whose argumentation consisted merely in appeals "everywhere" to my work? I must assume that, however often Min may have referred to my work, the examiners were convinced that he came to the same or similar conclusions through his own analyses and independent judgments. Fortunately we need not guess at these matters in the case of Tov, for a close reading of his articles in Soderlund's bibliography makes it clear that Tov's views always are his own, based on his own work. In short, I may claim the general support of these two text-critics who have spent considerable time of their own on the problem throughout Jeremiah, over against Soderlund and his pilot study in chapter 29.

On tendencies toward expansion and contraction. Before we turn to Soderlund's examination of specific readings in Jeremiah 29 G, I must acknowledge the appropriateness of his critique of my evidence from the ancient world for secondary expansion versus secondary contraction of texts. I appealed to a general remark of S. N. Kramer about tendencies of oriental scribes to add rather than subtract; to W. F. Albright's reference to Egyptian mortuary texts; and so on. Acknowledging the operation of such an expansionist tendency, Soderlund rightly accuses me of not giving a complete

or balanced picture. In any case, my examples do not at all closely parallel the sort of phenomena we have in Jeremiah. His own counter-examples are equally irrelevant. When a Hittite version condenses Gilgamesh to about one-fifth the standard length, or the later Gilgamesh Epic at points is shorter than the earlier Atrahasis Epic, or a 7th century edition of the Myth of Nergal is longer than a 14th century edition which however "may represent an abbreviated local version" (p. 201), we are in the area of tradition-formation and re-formulation, a long way from textual criticism, and might as well invoke the relations between Psalm 105, Deuteronomy 26:5-11, and the Hexateuch, or abandon such broad-gauged analogs and get on with detailed analysis and hypothesis-formation in Jeremiah. In what follows I will take up specific analyses Soderlund makes of the readings in chapter 29. In passing I may reiterate that, apart from the extreme narrowness of his sample, the character of it was not at all well chosen. For one thing, he and his readers have no opportunity to test the translation-style of G dealing with prose texts. For another, chapter 29G is very thin in the sort of zero variants most characteristic of G. Nevertheless, let us proceed.

1. 29:1a(47:1) (Soderlund pp. 203-211). This variation unit concerns the short (G) and long (MT) superscriptions to the oracle against the Philistines. Soderlund reviews "the principal cases of divergent readings among the introductory formulas in Jer," namely at 2:1-2, 7:1-2a, 16:1, and 27(50):1. In each instance he poses *ad hoc* explanations in defence of MT, insisting that "conclusions . . . must be arrived at independently of any generalized theory regarding the relationship between the two texts." (p. 211) But the frequency and character of these variants demands systematic explanation. Since addition of framing and introductory materials is one characteristic feature of recensional purpose, Tov may be correct in so explaining MT. Soderlund indeed nods in this direction: ". . . the redactoral framework of the LXX *Vorlage* probably differed at various points from that

of MT," but he immediately cautions that "this cannot automatically be taken as a sure guide to a prior or superior structure of the prophet's oracles" (p. 211). In the absence of an established tendency of the translator to abbreviate, Tov's carefully argued and amply documented case for recensional development in MT[18] is preferable to the *ad hoc* supposition of repeated accidental omission in G *Vorlage* . But Tov's case has conveniently been swept aside by the false assertion that it is derivative of my work.

2. A propos of variation in formulas such as יהוה אמר כה , יהוה נאם, יהוה, אמר, Soderlund (pp. 212-214) notes especially the "disappearance act" (sic!) of the epithet צבאות -- 82x in MT (65x in a prophetic formula) and only 11x in G. Conceding the strong likelihood of a shorter *Vorlage* of G in such an instance, he speculates, "it is true that respect for the deity may have worked in favor of a longer form of the divine name rather than vice versa"--a motivation I did not propose. "But," he goes on to say, "even granting this point, one could not assume the same explanation for all other instances of a shorter text." I offered no such explanation. Soderlund's refusal to consider closely the general phenomenon of human name differences (see above) robs his discussion in this section of its point and force.

3. 29(47):4 (Soderlund, pp. 214-219). Soderlund's major point here is that when all variants (including "content variants") are taken into account, "it becomes clear that the LXX and MT give quite different interpretations of the passage." (p. 215) The difference is that in MT "the focus [of divine judgment] remains consistently on the Philistines" (p. 216), but in LXX "the prophetic judgment . . . shifts from the Philistines to Tyre and Sidon" (p. 216). This change of focus "can only be regarded as foreign to the structure

---

[18]Tov, "Some Aspects . . . ," pp. 145-167.

and as disturbing to the unity of the original composition" (p. 217). But the shift in focus to Tyre and Sidon occurs in MT, not in G! For in Soderlund's own translations, G reads "I will wipe out Tyre and Sidon and all the rest of their [i.e. the Philistines'] help," while MT reads "to cut off from Tyre and Sidon every remaining ally [i.e. the Philistines]." He goes on to say, "In the MT nothing is said concerning the destruction of Tyre and Sidon; these cities are introduced only by way of emphasizing the destruction of the Philistines." (p. 216). Just the opposite is the case. In G the consistent and unrelieved focus on Philistia's doom is intensified by the observation that Philistia's nearest potential allies, Tyre and Sidon, will be cut off--along with all other help--leaving Philistia naked and exposed before the coming doom. In MT it is Tyre and Sidon who are deprived of any and all allies, left alone to face the judgment on *them*.[19]

Of the "content" variant ולהכרית] והכרתי 2QJer = και αφανιω G, Soderlund argues that :

> the Qumran reading והכרתי completely breaks the parallelism of the phrase and introduces a clumsy change of subject which must be expressed in the form of an intrusive and quite inexplicable quote. . . . It would be quite unjustified to place the blame for such an awkward and meaningless interruption on the author of the original composition. Thus,while και αφανιω in this instance probably comes from a textual variant already present in the translator's *Vorlage,* it must be this *Vorlage* rather than the MT that is secondary. (p. 216)

Let us consider the textual evidence. Dated to the early first century C.E., "2QJer does contain some variants from MT, but on the whole it agrees more

---

[19]In Ezek 12:14, Isa 10:3, 20:6, 31:1-2, Jer 37:7 and Nah 3:9, judgments against a city or state include references to (the futility of) that power's allies.

with MT than with any other source."[20] That a manuscript in the tradition of MT agrees with G against MT gives strong grounds for its superiority. But what of the "awkward and meaningless interruption" in such a reading? Soderlund's objections are *ad hoc* and without weight. The structure of this part of the oracle in G + 2QJer is as follows:

הנה + figurative depiction of advancing armies (29[47]:2a).

Terrified reaction of the defenders (2:b).

Proximate source (מקל) of terror: the attackers graphically portrayed (3:a).

Further terrified reaction of the defenders (3:b).

Ultimate source (על) of terror: it is a destroying (שדד) "Day [of the LORD]" (4a*a*).

Direct speech from this ultimate source (4a*b*):

"I will cut off Tyre and Sidon, / and all the rest of their help."

Reiteration (כי) of intention of YHWH to destroy (שדד) the Philistines (4b).

That 4a*a* and 4b constitute an envelope is suggested by the reiteration of the verb שדד and by the distribution of the cliché יום יהוה)("The day that is coming to destroy" // "YHWH is destroying"). It would be rhetorically powerful for this envelope to contain direct speech against Philistia, in the form of an announcement that Philistia will face its foe unprotected by any allies, especially Tyre and Sidon who might be hoped to buffer an assault from the north. In sum, 2QJer and G offer an elegant poetic structure. MT levels through the preceding infinitive construction--itself grammatically and stylistically unexceptionable, but shifting the focus of the oracle, and

---

[20]Emanuel Tov, "Some Aspects . . . ," p. 146. At 44:2, 2QJer may agree at least in part with G in a text superior to MT. See Janzen, p. 57, # 163.

destroying the elegance of a text attested in two widely diverse textual traditions.

In 29(47):4, MT שארית אי כפתור // כי שדד יהוה את פלשתים.
G appears to reflect [את]שארית איים // כי שדד יהוה (a couplet of 5+5 syllables nicely balancing the short couplet in 4a*a*). Soderlund argues that G "would have little meaning" (p. 219). On the contrary: the Philistines of 4a*a* are poetically "seconded" in 4b as שארית איים just as in v. 5 Gaza and Ashkelon are poetically "seconded" by the phrase שארית עמקם.[21] The "admittedly speculative" proposal that "LXX translator was . . . troubled by the phrase אי כפתור in the context of this verse and decided to resolve the dilemma by simply omitting the כפתור and writing נסף instead," (219) disregards what a close comparison of G and MT in the *whole* of Jeremiah would show--that the translator strives mightily to represent the Hebrew text, even to producing strange results, but does not solve difficulties by omission. MT is easily explained as a gloss. To object that "nowhere else is כפתור mentioned in conjunction with coastlands or islands" (p. 218), and that other references to כפתור are "far-removed and peripheral" (p. 219) is to obscure what glosses are and how they may arise.

4. 29(47):5-6 (Soderlund, pp. 219-221). G's omission of הוי in v. 6 associates the line with what precedes rather than, as in MT, with what follows. This omission "is most naturally explained as a deliberate disregard of a word that did not fit the translator's (false) reading of the text" (p. 221). In fact a strong case can be made for the superiority of the Hebrew text behind G, in which vv. 5-7 is structured as follows:

Statement of fate of Gaza and Ashkelon (5a; 3 lines)

Direct appeal ("how long?") to the sword of the LORD to cease and be still (5b,6)

Response to the appeal ("How can it be quiet?") (7).

The direct appeal from those under attack (compare Hab 1:17), verbally accompanies their ritual baldness. Soderlund argues "That תתגודדי is to be associated with its *preceding* phrase . . . באה קרחה אל עזה is clear from a comparison of Deut 14:1, Jer 16:6 and Jer 48:37(MT) where in each case, as in Jer 47:5, baldness and acts of self-mutilation are juxtaposed as common features of pagan mourning" (p. 220). The complete data are these: Deut 14:1 refers only to *cutting* and *making bald* ; Jer 16:6 refers to *lamentation* and *cutting* and *making bald* ; Jer 48:37-38(MT) refers to *making bald* and *shaving* and *cutting* and *sackcloth* and *lamentation*. According to the more proximate parallels in Jeremiah, then, Jer 48:5-6(MT) may as readily refer to *making bald and lamenting* as to *making bald and cutting*. The cities' direct speech balances the LORD's direct speech in v. 4. That v. 5b*b* is part of that speech may be indicated by the way the appeal to the sword, "how long will you cut this way and that?" responds to the verb in God's: "I will cut off."[22] The secondary rise of MT הוי may have arisen to make explicit an implicit vocative, when the preceding line was construed with the first part of v. 5 under the influence of Jer 16:6, 48:37(MT).

5. 29:10(49:9) (Soderlund, p. 222). The analysis here has already been treated in part (# 2 and n. 13). It remains only to observe that לך] omit G, and G] + σοι look suspiciously related. G *Vorlage* may have suffered haplography by homoioarchton, לך--לא, With G + σοι the sort of minor

---

elaboration not unexpected in rendering succinct poetry. Or, G may have construed לך with what followed rather than what preceded. In any case, "free, quasi-paraphrastic" translation is not established.

6. In pp. 228-240 Soderlund takes up a number of variation units in clichés containing series of terms. Again his "main criticism" (p. 231) is that I have interpreted these phenomena *ex hypothesi,* whereas his detailed analyses vindicate MT or disprove any basis for preferring G. His resistance to the possibility of G's superiority is exemplified at 24:9, where I argue that לרעה is intrusive as foreign to the cliché in which it is embedded and arose in MT by conflation of a corruption of neighboring לזועה. Acknowledging the strength of my case, Soderlund yet cautions,

> This does not mean, however, that לזועה can never appear in series in the OT. A comprehensive review of the evidence should also note that לזעוה/לזועה does appear in series of two and three terms respectively in Ez 23:46 and 2 Chr 29:8 . . . , all of which are attested by the LXX, so that *in this sense* Jer 24:9 with two terms [רעה + זועה] is not anomalous. (232; italics added)

The strain in the argument is evident. Soderlund actually thinks that זועה in two *ad hoc* series *elsewhere* in the Hebrew Bible (Ezekiel and 2 Chronicles) softens the anomaly of רע intruding into the *Deuteronomistic* cliché ונתחים לזועה לכל ממלכות הארץ (as in 15:4, 29:18, 34:17[MT]; compare Deut 28:5 which Soderlund overlooks: לכל ממלכות הארץ יתנך יהוה . . . והיית לזעוה). How can one respond to such arguments? All too often he objects to strong proposals with such "possible" alternatives.

7. Soderlund's last specific analysis prior to his conclusions has to do with the question as to whether G intentionally omitted one or other occurrence of a number of MT parallel passages. Admitting that in my "detailed treatment" of the five variation units most commonly discussed "[t]he

arguments for a shorter *Vorlage* . . . are well marshalled," (245) he stoutly insists that "the significance of the shorter *Vorlage* at these places is . . . ambiguous;" and he repeats the charge that I *assume* as a *premise* that a shorter *Vorlage* thereby is superior, objecting that "the difficulty in accounting for the appearance of the MT passages as expansions from parallel passages is illustrated by Janzen's own attempts at such explanation." He offers as examples my discussions of 8:10b-12 (//6:13-15) and 23:7-8 (//16:14-15), observing that my proposals are "not without their touch of brilliance," but concluding that "when all is said and done they remain speculative theories beyond evidential control." (p. 246) It is not clear whether he means to imply that my theories do not control the data, or that there are no data to control my theories. The data are these, together with a summary of my explanation.

(1) *Of the two parallel passages, G attests only one.* The syntactical connection between 8:10 and 8:13 in G establishes the absence of 8:11-12 in G *Vorlage.* (2) In other parallel passages in Jeremiah, orthographic variants in MT are infrequent and random, but *the orthography of 6:13-15 is consistently fuller than that of 8:10b-12,* and *the latter passage stands much closer to the generally conservative orthography of Jer MT.* This suggests that 6:13-15 is intrusive from an orthographically fuller manuscript into the orthographically more conservative MT of Jer. That is, MT is a conflation of manuscript traditions with the passage in chapter 6 and in chapter 8. (3) *The text of 8:13a*--that is, the first line after the parallel passage--*is a crux interpretum* (see commentators *ad loc.*). *At the same time, G offers a content variant of the line that makes good sense.* Given the zero variant in G, 8:13a in MT may have suffered corruption after its original connection with the context was disrupted by a secondary insertion of 8:10-12.

(4) Recurring to datum 2 and the explanation proposed there, how might we account for the rise of two traditions with the passage at chapter 6 and

chapter 8? At this point another datum takes on interest: *6:13-15, which ends on the formula* אמר יהוה, *is immediately preceded by the formula* נאם יהוה, providing occasion for haplography by homoioteleuton which would necessitate restoration. (5) The question as to how the passage might erroneously have been restored at 8:10b-12 now brings another datum into the scope of our interest: *The lines in 6:12 are strongly evocative of the lines in 8:10a, through the common terms* שדות *and* נשים *and the common idea expressed through them,* providing conditions for mis-identification of the place of restoration.

The above summary indicates that my "theory" is an attempt to account (whether correctly or incorrectly) for a number and variety of data specific to the variation unit in question. His "refutation" makes no attempt to account for these data, but leaves them mute. I am unclear why he should consider his flat assertion to have controverted my argument.

Let us now return to Soderlund's final evaluation of this analysis. He ends by asserting that "Janzen's conclusions regarding the duplicate passage discussed are based on "the absence of clear evidence that G deleted purposely" and "the abundant evidence for the expansionist character of M ([Janzen] p. 96)" (p. 246). His assertion is erroneous and misleading. In the first place, as I have shown, my conclusions are based on an analysis of data which he ignores in his assertion that my conclusions are without evidential control. In the second place, my words here quoted do not refer, as he asserts, to "the duplicate passage discussed [8:10b-12] ," but come as part of my concluding remarks to the whole section on "The Second Occurrence of Doublets," pp. 91-96. It is concerning this whole multi-instanced phenomenon that I wrote, "In the absence of clear evidence that G deleted purposely, and in view of the abundant evidence for the expansionist character of M, we contend that the readings considered in this section represent further expansion from parallel contexts." (96) Soderlund comments, "we have been unable to endorse either of these conclusions on the scale suggested by Janzen's work." (p. 246) I should think not. The scope of his examination--two dozen or so readings, largely confined to chapter 29 and leaving out of account the vast majority of variation units as well as whole types of evidence (such as the relative incidence and character of conflations and haplographies in G and M)--does not provide him an adequate base for either endorsement or refutation.

In conclusion: To show that both Tov and myself are systematically wrong in our respective construals of the textual data would require a more thorough and methodologically disciplined demonstration than Soderlund presents. As it is, he has created an impression which allows reviewers like Greenspoon (who understandably has not taken the trouble to check closely behind Soderlund) to broadcast the verdict that Tov's thesis--like the vagaries of the stock market or the world of fashion (Greeenspoon's terms)-- is in for a time of waning popularity, and that mine was an idea whose time should never have come, but which, having come, "should come and go as swiftly as possible."[23] *Et tu, Soderlund.* As for Soderlund's work on the Greek manuscripts of Jeremiah 29, and Ziegler's critical text, this should remain as a monument to text-critical industry, exactitude, and finely nuanced judgment. The pity is that *The Greek Text of Jeremiah* should be so uneven in its two major parts. One might almost be tempted to posit two authors, or to wish that the second part could pass under the hands of a reviser. Perhaps that is the inadvertent promise in the book's otherwise enigmatic sub-title.

---

[23]Leonard Greenspoon, *JBL* 107 (1988), 127.

The Following Contributions are Invited:

1. Record of work published or in progress.
   (Please print or type in publishable format.)

2. Record of Septuagint and cognate theses
   and dissertations completed or in progress.

3. Reports significant for Septuagint and cognate
   studies. Items of newly discovered manuscripts
   or of original groundbreaking research will be
   given primary consideration. Reports should
   be brief and informative and may be written in
   English, French or German. Greek and Hebrew
   need not be transliterated.

4. Abstracts of Septuagint papers read before
   international, national, and regional academic
   meetings. Abstracts should be previously
   unpublished, not more than one page,
   double-spaced, including the time, place, and
   occasion of the presentation.

All materials should be in the hands of the editor
by June 1 to be included in that year's *Bulletin*.

BULLETIN SUBSCRIPTION / IOSCS MEMBERSHIP

Send name, address, and US $8 to:

Dr. Leonard Greenspoon, IOSCS Secretary-Treasurer

Dept. of Religion and Philosophy

Clemson University

Clemson, South Carolina 29634-1508 USA

In the Netherlands, send subscription price to:

Dr. Arie van der Kooij

Tortellaan 34, 3722 WD Bilthoven

Holland. Giro: 742325.