

This is the first book to use all the Aramaic Dead Sea scrolls to reconstruct original Aramaic sources from parts of Mark's Gospel. The scrolls have enabled the author to revolutionise the methodology of such work, and to reconstruct whole passages which he interprets in their original cultural context. The passages from which sources are reconstructed are Mark 9.11–13; 2.23–3.6; 10.35–45; and 14.12–26. A detailed discussion of each passage is offered, demonstrating that these sources are completely accurate accounts from the ministry of Jesus, from early sabbath disputes to his final Passover. An account of the translation process is given, showing how problems in Mark's text arose from the difficulty of translating some Aramaic expressions into Greek, including the notoriously difficult 'son of man'. A very early date for these sources is proposed, implying a date c. 40 CE for Mark's Gospel.

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SOCIETY FOR NEW TESTAMENT STUDIES

MONOGRAPH SERIES

General editor: Richard Bauckham

102

ARAMAIC SOURCES OF MARK'S GOSPEL

Aramaic Sources of Mark's Gospel

MAURICE CASEY



CAMBRIDGE
UNIVERSITY PRESS

PUBLISHED BY THE PRESS SYNDICATE OF THE UNIVERSITY OF CAMBRIDGE
The Pitt Building, Trumpington Street, Cambridge CB2 1RP, United Kingdom

CAMBRIDGE UNIVERSITY PRESS

The Edinburgh Building, Cambridge CB2 2RU, United Kingdom
40 West 20th Street, New York, NY 10011-4211, USA
10 Stamford Road, Oakleigh, Melbourne 3166, Australia

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First published 1998

Printed in the United Kingdom at the University Press, Cambridge

Typeset in Times 10/12pt [CE]

A catalogue record for this book is available from the British Library

Library of Congress Cataloguing in Publication data

Casey, Maurice.

Aramaic sources of Mark's Gospel / Maurice Casey.

p. cm. – (Society for New Testament Studies Monograph series 102)

Includes bibliographical references and index.

ISBN 0 521 63314 1 (hardback)

1. Bible. N.T. Mark – Sources.

2. Dead Sea scrolls – Relation to the New Testament.

3. Aramaic literature – Relation to the New Testament.

4. Jesus Christ – Language.

I. Title. II. Series: Monograph series (Society for New Testament Studies); 102.

BS2585.2.C35 1998

226.3'042-dc21 98-13839 CIP

ISBN 0 521 63314 1 hardback

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PREFACE

This book was written in 1994–6, when I held a British Academy Research Readership awarded for the purpose. I am extremely grateful to the Academy for this award, which enabled me to complete a major piece of research.

I am also grateful to all those who have discussed with me the problems of method and of detail which this work has entailed. I effectively began this research while reading for a doctorate at Durham University under Professor C. K. Barrett, whose extraordinary combination of learning and helpfulness with lack of bureaucracy or interference remains a model to which one can only aspire. I should particularly like to thank also the late Professor M. Black, Dr G. J. Brooke, Professor B. D. Chilton, Professor J. A. Fitzmyer, Professor R. Kearns, the late Professor B. Lindars, Professor M. Müller, and Professor M. Wilcox. I should also like to thank members of the Aramaic Background and Historical Jesus seminars at the Society for New Testament Studies, the Jesus seminar at meetings of British New Testament scholars, and an annual seminar on the use of the Old Testament in the New now generally held at Hawarden, for what I have learnt from them. I alone am responsible for what I have said.

I should also like to thank Professor A. C. Thiselton, Head of the Department of Theology at the University of Nottingham since 1991, for his impartial and unfailing support of work; my Nottingham colleague Dr R. H. Bell for many hours spent sorting out problems with the word processor on which this book was written; and the libraries of Durham University, St Andrews University, the School of Oriental and African Studies and the British Library for the facilities necessary for advanced scholarly work.

Finally, I should like to thank Professor R. Bauckham, Ms R. Parr and an anonymous Aramaist for their favourable comments and acceptance of this work for publication in very mildly revised form.

ABBREVIATIONS

Most abbreviations are standard. Those for biblical books follow the recommendations of Cambridge University Press; those for periodicals and series of monographs follow S. Schwertner, *International Glossary of Abbreviations for Theology and Related Subjects* (Berlin/New York, 1974); most others follow the recommendations for contributors to *Biblica*. Others are as follows:

- ABRL Anchor Bible Reference Library
ANRW H. Temporini and W. Haase (eds.), *Aufstieg und Niedergang der Römischen Welt*, many vols. (Berlin, 1972–)
BN *Biblische Notizen*
IJSL *International Journal for the Sociology of Language*
JSP *Journal for the Study of the Pseudepigrapha*
JSS.S Journal of Semitic Studies, Supplements
MPIL Monographs of the Peshitta Institute, Leiden
TWNT G. Kittel and G. Friedrich (eds.), *Theologisches Wörterbuch zum Neuen Testament*, 10 vols. (Stuttgart, 1933–79)

1

THE STATE OF PLAY

The Gospel of Mark is written in Greek, though Jesus spoke Aramaic. Moreover, Jesus' ministry was exercised among Jews, whereas, by the time Mark's Gospel was written, many of Jesus' followers were Gentiles, and this Gospel shows traces of Gentile self-identification. It follows that the change in language from Aramaic to Greek was part of a cultural shift from a Jewish to a Gentile environment. If therefore we wish to recover the Jesus of history, we must see whether we can reconstruct his sayings, and the earliest accounts of his doings, in their original Aramaic. This should help us to understand him within his own cultural background.

For this purpose, we must establish a clear methodology, not least because some people are still repeating every mistake with which the history of scholarship is littered. I therefore begin with a critical *Forschungsberichte*. This is not a comprehensive catalogue of previous work, but a selective discussion of what advances have been made, what significant mistakes have been made, and the reasons for both of these.

The early fathers give us very little reliable information about the transmission of Jesus' words in Aramaic before the writing of the Gospels. Eusebius has the apostles speak ἡ Σύρων φωνή (*Dem. Ev.* III.4.44; 7.10), his name for the Aramaic dialects contemporary with him, but he gives us no significant help in getting behind the Gospel traditions. He quotes Papias early in the second century, Ματθαῖος μὲν οὖν Ἑβραϊδὶ διαλέκτῳ τὰ λόγια συνετάξατο, ἡρμήνευσεν δ' αὐτὰ ὡς ἦν δυνατὸς ἕκαστος (*HE* III.39.16). This is not true of Matthew's Gospel as a whole, but it may reflect the transmission of Gospel traditions in Aramaic.

There are plausible reports of lost Gospels written in a Hebrew language, probably Aramaic rather than Hebrew. Jerome, understandably stuck on ἐπιούσιον in the Lord's prayer, looked in a

Gospel called 'according to the Hebrews', and found 'maar', with the sense 'crastinum', 'to-morrow's', and hence a future reference.¹ This is very likely to be right, a preservation of the Lord's prayer from the Aramaic-speaking church.² **מחר** really does mean 'tomorrow's', and the reference is likely to have been eschatological. Those Gospels which survive, however, all of them in the dialects of Aramaic generally known as Syriac, are translations from our present Greek Gospels *into* Aramaic. The process of translating the Greek Gospels into Aramaic is significantly different from trying to reconstruct original sources. Nowhere is this better illustrated than with the term 'son of man'. This was originally the Aramaic **בר (א)נשא**, a normal term for 'man'. By the processes of translation and Christological development, this became a Christological title in Greek, ὁ υἱὸς τοῦ ἀνθρώπου.³ Since it had become a Christological title, it could not be translated *into* Syriac with **בר (א)נשא**. Hence Tatian produced the expression **ברה ראנשא**, and later translators produced also **ברה דגברא** and **ברה דברנשא**. These expressions naturally lent themselves to interpretation remote from the original **בר (א)נשא**. Philoxenus of Mabbug commented:

עלהדא לם אתקרי ברה דאנשא, מטל דהוא ברה דאנשא
חדתא דקמם עבר פוקדנא.

'For this reason, then, he was called "the (lit. his) son of (the) man", because he became the (lit. "his") son of the new man who preceded the transgression of the commandment.'⁴ Here the term has been interpreted as 'the son of *the* man', and the man in question has been identified as Adam, so that in effect the term is held to mean 'son of Adam'. This is quite remote from the meaning of the original **בר (א)נשא**. Once **ברה דאנשא** was established as the term which Jesus used to refer to himself, Syriac fathers could use **בר (א)נשא** of him in its original sense, apparently unaware that he had done so, and in ways remote from his view of himself. An anonymous poem on faith has this:

¹ D. Hurst and M. Adriaen (eds.), *S. Hieronymi Presbyteri Opera. Pars I, 7. Commentariorum in Matheum Libri IV* (CCSL LXXVII. Turnholti, 1969), ad loc.

² J. Jeremias, *New Testament Theology*, vol. I (London, 1971), pp. 196, 199–201; see p. 51 below.

³ See pp. 111–21, 130–2 below.

⁴ J. W. Watt (ed.), *Philoxenus of Mabbug: Fragments of the Commentary on Matthew and Luke* (CSCO 392, SS 171. Leuven, 1978), frag. 23.

לא הוא לבר אנשא בתולתא טעינא הות

It was not (a/the) son of man that the virgin was carrying.⁵

What this means is that Mary gave birth to Jesus as both God and man, not only to a man as a normal human mother does. Thus the Syriac versions and fathers alike, though helpful in some matters if used carefully, are no simple guide to what was said and meant by Aramaic sources of the Gospels.

The next significant development took place as independent scholarship emerged slowly from the Reformation and the Enlightenment. A few learned men noticed Semitic features in the Greek of the Gospels, and sought to explain them with reference to the actual Semitic terms which lay behind them. As scripturally orientated scholars, however, they tended to resort to Hebrew rather than Aramaic, because their primary resource was the Old Testament. Sometimes, this did not matter in itself. For example, in 1557 Theodore Beza commented on the idiomatic use of *πρόσωπον* at Matt. 16.3, 'Hebraicè פנים'.⁶ This points to a correct understanding of this idiomatic usage.

Such an approach, however, will inevitably come to grief when Aramaic and Hebrew are seriously different. The term 'son of man' is again the best example of this. Commenting on Matt. 12.8 in 1641, Grotius gave several reasons why *ὁ υἱὸς τοῦ ἀνθρώπου* could not be a simple reference to Christ, including that 'בן אדם' [*filium hominis*] meant 'hominem quemvis', 'any man'.⁷ This comes close to a reason why *ὁ υἱὸς τοῦ ἀνθρώπου* could not be a Christological title on the lips of Jesus, but it leaves insoluble problems behind it. If we know only this, we cannot explain why Jesus used the Aramaic term בר (א)נשא (א), or how it came to be transmuted into a Christological title. The Bible-centred nature of this limitation is especially obvious in Grotius, for he could read Aramaic and Syriac.

During this period, scholars also edited texts and wrote works of reference. The first edition of the Syriac New Testament caused a great stir in 1555, on account of its claim to be written in the

⁵ S. P. Brock, 'An Anonymous Madrasa on Faith', *OrChr* 64, 1980, 48–64, p. 50, stanza 4, line 1.

⁶ T. Beza, *Novum d n Jesu Christi testamentum* (Geneva, 1557), ad loc. I had access to *JESU CHRISTI D. N. Novum Testamentum* (Geneva, 1565).

⁷ H. Grotius, *Annotationes in libros evangeliorum* (Amsterdam/Paris, 1641), ad loc. I had access to this as *Annotationes in quatuor Evangelia & Acta Apostolorum in H. Grotii Opera Omnia Theologica* (Amsterdam, 1679), book II, vol. I.

language of Jesus.⁸ In 1596 the Maronite George Michael Amira made a similar claim in the introduction to his Syriac grammar, giving this as a reason for its usefulness. He proceeded to illustrate this, commenting for example on *ταλιθα κουμι* (Mk 5.41): *ܬܠܝܬܐ ܟܘܡܝ* . . . id est, puella surge, “girl, get up”. He inferred from this that Christ used the Syriac language.⁹

One of the most impressive text editions was the Walton polyglott, published in 1655–7.¹⁰ The title page declares its contents, including the text of the Bible, with the Samaritan Pentateuch, the Targums, the LXX, the Syriac and other versions, with Latin translations of the oriental texts and versions. While paying tribute to his predecessors, Walton noted his improvements, including more extensive presentation of Aramaic and Syriac versions. Two pieces of prolegomena are also especially relevant. Proleg. XII *De Lingua Chaldaica & Targumim*, ‘On the Chaldaean Language and the Targums’, was a very sound introduction for its day. Correct information includes sorting out the different names for this language: ‘appellata est Syriaca à regione Syriae, Aramaea ab Aram, & ab Assyria Assyriaca: aliquando etiam dicta est Hebraea . . . quod populus Hebraeus post captivitatem Babylonicam hac usus sit pro vernacula’.¹¹ It is not surprising that Walton found the Targums difficult to date. One of his errors of method is still found among New Testament scholars: he used New Testament parallels in arguing for an early date for whole Targums.¹² In the mid seventeenth century that was a reasonable thing to do. We have now noticed, however, that it is some traditions which are thus shown to be early, and these may be incorporated in Targums which did not reach their present form until centuries later. Another useful prolegomenon was XIII, *De Lingua Syriaca, et Scripturae Versionibus Syriacis*, ‘On the Syriac Language, and the Syriac Versions of Scripture’. This contains a very learned and coherent discussion of the dialect spoken by Christ and the apostles. Taking up claims that Jesus spoke Syriac, Walton concludes that this is the right language, but not the right dialect.

⁸ J. A. Widmanstadius and M. Merdenas (eds.), *Liber Sacrosancti Evangelii de Jesu Christo Domino et Deo nostro* (Vienna, 1555).

⁹ *GRAMMATICA SYRIACA, SIVE CHALDAICA*, Georgij Michaelis Amirae Edeniensis è Libano, Philosophi, ac Theologi, Collegij Maronitarum Alumni (Rome, 1596), (unnumbered) p. 7.

¹⁰ B. Waltonus et al. (eds.), *Biblia Sacra Polyglotta* (6 vols., London, 1655–7).

¹¹ *Ibid.*, vol. I, p. 81.

¹² *Ibid.*, vol. I, p. 85.

Throughout this period, the publication of texts, commentaries and works of reference formed an important contribution to knowledge. Significant analytical developments had to await the massive explosion of knowledge which took place in the Victorian era. New discoveries included two hitherto unknown Syriac versions of most of the canonical Gospels, the Old Syriac and the Christian Palestinian Syriac lectionary. They too caused excitement because they were in the right language. Cureton, who discovered the first part of the Old Syriac, declared, 'this Syriac text of the Gospel of St. Matthew which I now publish has, to a great extent, retained the identical terms and expressions which the Apostle himself employed; and . . . we have here, in our Lord's discourses, to a great extent the very same words as the Divine Author of our holy religion himself uttered in proclaiming the glad tidings of salvation in the Hebrew dialect to those who were listening to him, and through them, to all the world'.¹³ The Palestinian Syriac lectionary of the Gospels was from the right area, as well as in the right language. The first codex came to light in the eighteenth century, and the discovery of two further codices in 1892–3 led to the publication of the standard edition.¹⁴

Others scholars turned to Jewish Aramaic. In 1894, Dalman published his *Grammatik des jüdisch-palästinischen Aramäisch*.¹⁵ This was a valuable study of the Aramaic which it investigated. At the same time, some obvious problems were looming, if this Aramaic was taken as the major source for reconstructing sayings of Jesus. Dalman used sources which were much later in date than the Gospels, and he made extensive use of selected Targums. If we used this Aramaic to 'reconstruct' Gospel narratives and sayings of Jesus, we might end up with the wrong dialect, and with translation Aramaic rather than the natural language.

The Victorian era also saw the production of the major rabbinical dictionaries of Levy and Jastrow.¹⁶ These were fine pieces of

¹³ W. Cureton (ed.), *Remains of a Very Antient Recension of the Four Gospels in Syriac, hitherto unknown in Europe* (London, 1858), p. xciii.

¹⁴ A. S. Lewis and M. D. Gibson (eds.), *The Palestinian Syriac Lectionary of the Gospels, Re-edited from two Sinai MSS. and from P. de Lagarde's Edition of the 'Evangelium Hierosolymitanum'* (London, 1899).

¹⁵ G. Dalman, *Grammatik des jüdisch-palästinischen Aramäisch nach den Idiomen des palästinischen Talmud und Midrasch, des Onkelostargum (Cod. Socini 84) und der jerusalemischen Targume zum Pentateuch* (Leipzig, 1894. ²1905).

¹⁶ J. Levy, *Chaldäisches Wörterbuch über die Targumim und einem grossen Theil des rabbinischer Schriftthums* (2 vols., Leipzig, 1867–8; ³1881); J. Levy, *Neuhebräisches und chaldäisches Wörterbuch über die Talmudim und Midraschim* (4 vols.,

work, and in their own right significant contributions to knowledge, which greatly facilitated the study of rabbinical literature. More than a century later, they remain indispensable for serious research workers. Problems have arisen in the work of scholars who have used them to reconstruct sayings of Jesus, but who have not always had first-hand acquaintance with rabbinical texts. Such faults are not those of the compilers.

While these major developments were in train, a number of detailed suggestions were made, which brought both progress and problems of method. For example, Nestle suggested that Luke's 'cities' in his version of the parable of the talents (Matt. 25.14–30// Luke 19.11–27) was due to the misunderstanding of ככרין, 'talents', which had been read as כרכין, 'cities'.¹⁷ At one level, this was a bright idea. Matthew and Luke have parallel passages with many variations: alternative translations of Aramaic sources were a possibility worth exploring, and misunderstandings and mistakes might seem to be a good way of verifying that something has gone wrong. This example has, however, all the problems which have attended such attempts. In the first place, the Lukan version makes sense on its own. Jesus might have said both parables, for they are very different, or the Lukan version might have been told and retold by people who liked it better in the Lukan form. Secondly, כרכין is not the only Aramaic word for 'cities': the choice of this word is especially arbitrary when the Lukan version is sensible.

A number of suggestions were made in a long series of articles by J. T. Marshall.¹⁸ Some of his points were perfectly sound, though not necessarily new. He explained that Ἐβραϊστί at John 5.2; 19.13, 17 must mean 'in Aramaic' rather than 'in Hebrew' because of the Aramaic endings of Βηθεσδα, Γαββαθα and Γολγοθα. He

Leipzig, 1876–89. 2nd edn, Berlin/Vienna, 1924); M. Jastrow, *A Dictionary of the Targumim, the Talmud Babli and Yerushalmi, and the Midrashic Literature* (2 vols. London, 1886–1903. Rep. New York, 1950).

¹⁷ An almost off-hand comment in a book review, *TLZ* 20, 1895, 565.

¹⁸ J. T. Marshall, 'The Aramaic Gospel', *Expositor*, 4th series, 3, 1891, 1–17, 109–24, 205–20, 275–91, 375–90, 452–67; 4, 1891, 208–23, 373–88, 435–48; 6, 1892, 81–97; cf. W. C. Allen, 'The Aramaic Gospel', *Expositor*, 4th series, 7, 1893, 386–400, 454–70; S. R. Driver, 'Professor Marshall's Aramaic Gospel', *Expositor*, 4th series, 8, 1893, 388–400, 419–31; J. T. Marshall, 'The Aramaic Gospel', *ExpT* 4, 1892–3, 260–7; C. Campbell, 'Professor Marshall's Theory of an Aramaic Gospel', *ExpT* 4, 1892–3, 468–70; J. T. Marshall, 'The Aramaic Gospel: Reply to Dr. Driver and Mr Allen', *Expositor*, 4th series, 8, 1893, 176–92; E. Nestle, 'The Semitic and the Greek Gospels', *ExpT* 8, 1896–7, 42–3, 138–9; J. T. Marshall, 'The Semitic and the Greek Gospels', *ExpT* 8, 1896–7, 90–1.

correctly explained *ταλιθα κουμι* (Mark 5.41) and *ἀββα* (Mark 14.36), and he associated *σαβαχθανι* (Mark 15.34) with the Aramaic form of Ps. 22.1. He consulted the Targums in passages where the LXX has words found in the Gospels. He also has a good account of problems which arise when material is translated from one language to another.¹⁹ His work has, however, a number of problems which proved difficult to resolve. One was over vocabulary. Allen accused him of coining for words meanings which they did not possess. His examples included **אַרְבַּעָה**, ‘bed’, which Allen argued was used only of cattle, with the meaning ‘act of lying down’. Marshall correctly pointed out that **רַבַּע** does mean ‘recline’ of human beings, but it is only with the discovery of 4Q Tobit that we have early evidence of this, and Driver responded rightly by commenting that this did not justify the production of a noun, **אַרְבַּעָה** or **מַרְבַּעָה**, ‘bed’.²⁰ The underlying problem was the absence of Aramaic from the right period. Creative Aramaists responded somewhat like native speakers, extending the semantic areas of words to provide whatever meanings they needed: accurate critics pointed out that they had gone beyond the evidence of extant texts.

Another problem was the reconstruction of Jesus’ dialect: Marshall proposed to use Talmudic evidence fleshed out with the Samaritan Targum.²¹ All this evidence is late, and the available text of the Samaritan Targum was hopelessly corrupted by mediaeval scribes. Marshall’s model of the synoptic problem was also difficult: he interpreted variants as translation variants without proper consideration of whether one might be due to secondary editing. For example, he interpreted *ὀφειλήματα* (Matt. 6.12) and *ἁμαρτίας* (Luke 11.4) as alternative translations of one Aramaic original, without considering whether *ἁμαρτίας* may be a Greek revision of the Semitising *ὀφειλήματα*. Allen properly pointed out that the rare word *ἐπιούσιον* (Matt. 6.11//Luke 11.3) implies a single Greek translation.²² Some of Marshall’s points depend on misreadings which are at best very hypothetical. For example, he suggested that the difference between *ἀπολέσωσιν* (Mark 3.6) and *ποιήσαιεν*

¹⁹ Marshall, ‘Aramaic Gospel’, *Expositor*, 4th series, 3, 1891, 10, 11, 276–8, 116ff.

²⁰ Allen, ‘Aramaic Gospel’, 388, 395–6; Marshall, ‘Reply to Dr. Driver and Mr Allen’, 183; Driver, ‘Professor Marshall’s Aramaic Gospel’, 392–3.

²¹ Marshall, ‘Aramaic Gospel’, *Expositor*, 4th series, 4, 1891, 208ff.

²² Marshall, ‘Aramaic Gospel’, *Expositor*, 4th series, 3, 1891, 124; Allen, ‘Aramaic Gospel’, 468–9.

(Luke 6.11) was due to a confusion between **אבד** and **עבד**. Equally, however, Luke may have edited Mark. This is the preferable hypothesis because it makes sense both of passages where verbal identity is too great for us to posit separate translations and of Lukan editing. It is also a disadvantage that Marshall is dealing with single words. Driver correctly demanded whole sentences before the method could be seen clearly enough for a final judgement to be passed on it.²³

It follows that Marshall's work could not take this set of problems to an acceptable conclusion. At least, however, Marshall, Driver and Allen worked with the correct view that Jesus spoke Aramaic. This was not clear to everyone. A few scholars argued that Jesus taught in Hebrew. At the end of the nineteenth century, Resch argued this in some detail, and he sought to reconstruct **יְשׁוּעַ דְּבַרְי יְשׁוּעַ** in Hebrew.²⁴ This work has a number of faults of method which still recur in scholarship. One is Resch's basic failure to distinguish between an edited translation *into* Hebrew, which he offers, and serious reconstruction. So, for example, he follows Matthew and Luke in omitting Mark 2.27, and puts what is effectively a translation of Luke 6.5 (//Matt. 12.8//Mark 2.27–8) as his verse 29, *after* Mark 3.5//Matt. 12.13// Luke 6.10, its position in Codex Bezae:

וַיֹּאמֶר אֵלֵיהֶם בְּנֵי-הָאָדָם גַּם אֶדְוֶן הַשַּׁבָּת

This does not permit an explanation of *why* Jesus used **בְּנֵי-הָאָדָם**, a traditional translation of $\delta\ \upsilon\iota\delta\acute{\omicron}\varsigma\ \tau\omicron\upsilon\ \alpha\nu\theta\rho\acute{\omega}\pi\omicron\upsilon\varsigma$ *into* Hebrew, or of why Mark added 2.27 and put $\acute{\omega}\sigma\tau\epsilon$ at the beginning of 2.28, or of why anyone moved what has become a statement of Jesus' sole authority over the sabbath away from the end of the two pericopes in which sabbath halakhah is disputed. Ironically, Resch has made it more rather than less difficult to explain Jesus' teaching in its original cultural context.

Another major fault is to suppose that synoptic parallels are to be explained from misreadings of a Hebrew underlay. For example, at Matt. 10.10//Luke 10.7 Resch suggests that the original reading was **מַחִיָּה**, correctly translated as $\tau\rho\phi\eta\varsigma$ (Matt. 10.10). This was

²³ Marshall, 'Aramaic Gospel', *Expositor*, 4th series, 3, 1891, 465–6; Driver, 'Professor Marshall's Aramaic Gospel', 430–1.

²⁴ A. Resch, *Aussercanonische Paralleltex te zu den Evangelien* (TU X. 5 vols., Leipzig, 1893–7); A. Resch, *Die Logia Jesu* (Leipzig, 1898); A. Resch, *ספרי תלדות ישוע המשיח* (Leipzig, 1898).

corrupted to **מחיתו**, so the other translator decided to read **מחירו** and translated τοῦ μισθοῦ αὐτοῦ (Luke 10.7).²⁵ But it is difficult to see why the original text should have had **מחיה** rather than **שכר**, conjectured misreadings are not enough to solve the synoptic problem, and in this case Matthew had good contextual reason to alter μισθοῦ to τροφῆς.

Thirdly, Resch could not explain Aramaic evidence. At Mark 15.34//Matt. 27.46, he follows Codex Bezae in supposing that Jesus said Ps. 22.1 in Hebrew, and he suggests that the Aramaic version in most manuscripts was produced when Hebrew was no longer understood.²⁶ This argument cannot cope with the weight of attestation in favour of the Aramaic version, nor is it a convincing explanation of change in Greek Gospels. The Greek translations also supplied at Mark 15.34//Matt. 27.46 make it unnecessary for everyone to understand the Aramaic, and it is very doubtful whether Aramaic was better understood by Greek-speaking congregations than Hebrew was. The variant reading of Bezae, however, is readily explained as assimilation to the canonical text of Ps. 22.1. Resch was not helped by supposing that $\acute{\epsilon}\lambda\omega\acute{\iota}$ = **אלהי** and **למא** must both be Hebrew, which led him to describe the Gospel evidence as a *Mischtext*. This should not be accepted. Any source is most unlikely to have been vocalised, so the ω is the decision of a transliterator who was not very good at transliteration, may have suffered from interference from the Hebrew **אלהים**, and may have pronounced the Aramaic \bar{a} as \bar{o} , for this shift is attested elsewhere (for example **אנוש** for **אנש**, 1QapGen XXI.13).²⁷ The word **למה** is perfectly good Hebrew. Resch should have known that it was Aramaic too from Ezra 4.22 (cf. 7.23), and from later evidence: early attestation is more abundant now (for example 1QapGen XXII.32). Resch's description of the quotation of Ps. 22.1 as a *Mischtext* is accordingly the kind of mistake which was understandable a century ago, and which we should no longer make.

While a few scholars argued that Jesus taught in Hebrew, some argued that he taught in Greek. In 1767, Diodatus argued this, primarily on the basis of the hellenisation of Judaism.²⁸ Having surveyed the evidence down to 1 Maccabees, noting towards the

²⁵ Resch, *Aussercanonische Paralleltexpte* III, pp. 182–4.

²⁶ *Ibid.*, pp. 355–61.

²⁷ K. Beyer, *Die aramäischen Texte vom Toten Meer* (Göttingen, 1984), p. 137.

²⁸ D. Diodati, *DE CHRISTO GRAECE LOQUENTE EXERCITATIO* (Naples, 1767; rep. London, 1843).

end the evidence of extreme hellenisation at 1 Macc. 1.14–17, he inferred that Greek was widely spoken in Judaea. He argued that at this stage Jewish people were bilingual, but he supposed that Aramaic died out in the succeeding years. As evidence he noted documents such as the Wisdom of Solomon written in Greek, and the need for Ecclesiasticus to be translated into Greek. He also noted evidence such as the inscriptions on Herod's coins being in Greek.²⁹ He added the evidence of the New Testament being written in Greek. He then made a crucial point, commenting that if we consider Judaea at the time of Christ, we find no document written in Chaldaean or Syriac.³⁰ This was true when Diodatus wrote it, and made his view a great deal more reasonable than it has been since the discovery of the Dead Sea scrolls. His presentation had, however, three serious weaknesses which could be seen at the time. His presentation of the hellenisation of Judaism is undifferentiated by identity: the hellenisation of people like Herod and those mentioned in 1 Macc. 1.14–17 does not entail the hellenisation of faithful Jews. Secondly, his detailed arguments from Gospel evidence assume the literal truth of its surface narrative. In Luke 4, for example, the LXX is quoted when Jesus reads from the Bible,³¹ but the story may not be literally true, or it may use the Bible of Greek-speaking Christians to communicate with them, even though Jesus read from a Hebrew scroll. Thirdly, Diodatus could not cope with Gospel evidence that Jesus spoke Aramaic. He suggests that the Aramaic words in the Gospels show occasional use of Aramaic words, not that Aramaic was the vernacular,³² but he was quite unable to explain this occasional use.

In 1888, Roberts attempted a more thorough and extensive presentation of the same view.³³ He suffers from the same problems as Diodatus. The assumption that the surface level of the Gospels is literally true is worse than ever. For example, at John 12.20ff., some Greeks come to see Jesus. However, the result of this is not that Jesus sees the Greeks, but that he comments on his death, which had to take place before Greeks could enter the churches. We should infer that Jesus did not see them. Roberts, however,

²⁹ *Ibid.*, pp. 37, 76, 85ff.

³⁰ *Ibid.*, p. 153.

³¹ *Ibid.*, pp. 123–5.

³² *Ibid.*, p. 163.

³³ A. Roberts, *Greek: The Language of Christ and his Apostles* (London, 1888); similarly T. K. Abbott, *Essays, Chiefly on the Original Texts of the Old and New Testaments* (London, 1891), pp. 129–82.

imagines that they must have been present for this speech, so Jesus did see them, and must have spoken Greek so that they could understand him.³⁴ This is not a convincing interpretation of the text, and ignores the fact that most of the fourth Gospel is not historically true. This was also easier to do in 1888 than it is now. Roberts also generalises Diodatus' argument from the LXX. He notes correctly that Jesus is represented as relying on written scriptures. He suggests that Hebrew was a dead language, and declares that there were no written Targums (that shown to Gamaliel, t. Shab 13.2; y. Shab 15c. 5–7; b. Shab 115a, being unofficial and not accepted). This leaves the LXX as the only version which Jesus could have used.³⁵ Here too we must note that in 1888 the opinion that Hebrew was a dead language was reasonable, given the prevailing views of Daniel and of Mishnah, and the fact that the Hebrew documents from the Dead Sea had not been discovered. Equally, the view that there were no written Targums was reasonable when the Dead Sea Targums had not been found.

It follows that Roberts gets into his most obvious tangles in trying to explain the Gospel evidence that Jesus spoke Aramaic. He follows Diodatus in supposing that the Aramaic words mean that Jesus used Aramaic occasionally. At once, he has to admit that there is no evident reason for Jesus to have used Aramaic when the Gospels attribute Aramaic words to him.³⁶ This is an important weakness, mitigated in 1888 by the fact that no one understood translators well enough to explain why they retained some few words and not more. On Mark 5.41, Roberts suggests that the girl to whom Jesus spoke in Aramaic was the daughter of a strictly Jewish family and therefore not familiar with Greek.³⁷ This is reasonable in itself, but Roberts does not seem to have realised that it is reasonable only if we undermine his reasons for thinking that everyone spoke Greek. At Mark 15.34 he is in such a quandary that he argues that Jesus said Ps. 22.1 in Hebrew, repeating his distinguished type (David),³⁸ but the evidence is clearly Aramaic and there is no typology in the text.

These weaknesses are sufficiently severe for the view that Jesus

³⁴ Roberts, *Greek*, pp. 157–9.

³⁵ *Ibid.*, *Greek*, ch. V.

³⁶ *Ibid.*, *Greek*, pp. 96–8.

³⁷ *Ibid.*, *Greek*, pp. 105–6.

³⁸ *Ibid.*, *Greek*, pp. 108–9.

taught in Greek to have remained that of a small minority. It is none the less significant that such views were more reasonable when they were first put forward than they are now, following the discovery of the Dead Sea scrolls.

The view that Jesus spoke and taught in Aramaic was accordingly the prevailing view in 1896, the first watershed in the study of our subject. This year saw the publication of the first major monograph which attempted to see behind the Greek Gospels to the Aramaic sayings of Jesus: Meyer, *Jesu Muttersprache*.³⁹ Meyer assembled the main evidence for believing that Jesus spoke Aramaic, and supplied a sensible discussion of what Aramaic sources should be used. He tried to go for Galilean Aramaic, since this was Jesus' dialect. For this purpose he used both the Jewish Aramaic of the Palestinian Talmud and Christian Palestinian Syriac. He stated openly that these sources were too late in date, but since earlier ones were not available, he used them all the same. The great advance which he made was to offer reconstructions of whole Aramaic sentences, which he located in their original cultural context. For example, he suggested this for Mark 2.27–8:⁴⁰

שבתא בגין ברנשא איתעבידת ולא ברנשא בגין שבתא:
בגלל כן מריה הוא דשבתא ברנשא.

The great advantage of this is that it enables the final example of **ברנשא** to appear as it must appear in Aramaic, as a normal term for man. Only a whole sentence can do this, and whole sentences cannot fail to do it. For this reason, the procedure as a whole was an essential step forward. This is a particularly good example, because the son of man statement of Mark 2.28 is closely tied to the unambiguously general statement of 2.27. At the same time, the proposed reconstruction has problems. One is positing **ברנשא** behind both examples of ὁ ἄνθρωπος in 2.27. This made it difficult to understand the translator, and Meyer made no serious attempt to do so. The use of the late expression **בגלל כן** behind the difficult ὥστε is also problematical: it would surely have been more likely to have given rise to διὰ τοῦτο.

³⁹ A. Meyer, *Jesu Muttersprache: Das galiläische Aramäisch in seiner Bedeutung für die Erklärung der Reden Jesu und der Evangelien überhaupt* (Freiburg i. B. / Leipzig, 1896).

⁴⁰ *Ibid.*, p. 93.

Meyer's reconstruction of Matt. 12.32 further illustrates these points:⁴¹

כל דיימר מלא על ברנש ישתביק לה וכל דיימר על
רוחא דקודשא לא ישתביק לה.

Here too, it is a great advantage that the complete sentence ensures that **ברנש** emerges as a normal term for man. It is also good that there are no problems with the late date of the Aramaic used. Moreover, this is a Q saying, and the proposed reconstruction permits the understanding of Luke 12.10 as an alternative understanding of the same Aramaic. This might have led to important advances in our understanding of Q. Also helpful was Meyer's reference to Mark 3.28, πάντα ἀφεθήσεται τοῖς υἱοῖς τῶν ἀνθρώπων, where he saw a clear echo of **ברנשא** in the original saying. None the less, he had insufficient appreciation of the need to understand the translator. The use of **ברנש** in the indefinite state, which is entirely reasonable on Aramaic grounds, requires an explanation of the consistent use of the articles in ὁ υἱὸς τοῦ ἀνθρώπου. Meyer compared the use of **בר נשא** with **ההוא גברא**,⁴² but this is a different expression, and he was not able to show that the one was used like the other.

As we consider Meyer's work a century afterwards, his great advance is his attempt to produce complete reconstructions of some sayings. At the same time, however, his work left five definable problems which continue to require attention.

1. Much of the Aramaic which he used was from sources which were too late in date. Meyer knew this perfectly well, but there was nothing that he could do about it. One consequence was simply that a lot of his work could not be verified. A second result has remained concealed ever since: no one could see how far his ability to produce puns and the like really resulted from his use of a wider range of Aramaic than was ever available to Jesus.

2. It follows that his work contains too many puns. For example, at Matt. 3.9//Luke 3.8, Meyer suggests a *Gleichklang* between **אבניא** for λίθων and **בניא** for τέκνα. That is not unreasonable, but it does involve the selection of **בניא**, which might well have been translated υἱους, rather than **ינקין**, which was bound to be rendered τέκνα. He then suggests that the difference between δόξητε at Matt. 3.9 and ἄρξησθε at Luke 3.8 is due to the

⁴¹ *Ibid.*, p. 94.

⁴² *Ibid.*, pp. 95–6.

difference between תְּשָׁרִין and תְּשָׁרִין, and that this is evidence of an Aramaic *Grundlage*.⁴³ There are two things wrong with this. One is that the case for this difference in Aramaic cannot be confirmed for Aramaic of the right period. The other is that it does not permit a plausible model for the behaviour of the translator(s). The whole of the surrounding context is verbally identical in Greek. This does not make sense of having two translators. If, however, there was only one translator, it is more plausible to suppose that this part of Q reached the evangelists in Greek, and that one of them altered it for stylistic reasons, as both of them altered Mark. It follows that what particularly impressed Meyer as evidence of an Aramaic *Grundlage* cannot function as such.

3. This is part of the larger problem that Meyer could not see how translators worked. His treatments of both ὁ υἱὸς τοῦ ἀνθρώπου and of δόξατε//ἄρξῃσθε are examples of this. It is still a serious problem. We shall see that recent research in Translation Studies in general and the LXX in particular can be fruitfully applied here.

4. Several suggestions for a common underlay for more than one passage of Greek form bright suggestions which have never been worked through thoroughly enough to show that there ever was such an underlay. For example, at Matt. 21.31–2//Luke 7.29–30, Meyer suggests that Matthew's βασιλείαν τοῦ θεοῦ represents מְלִכְוּתָא דִּיהוּהּ, while Luke's βουλήν τοῦ θεοῦ represents מִלְכָּתָא דִּיהוּהּ. He further suggests that Matthew's προάγουσιν represents זְבִי (Peal), whereas Luke's ἐδικαίωσαν represents זְבִי (Pael). From this, Meyer concludes that Jesus said either 'זְבִי קוֹמִיכוֹן מִלְכָּתָא דִּי' or 'זְבִי קוֹמִיכוֹן לְמִלְכָּתָא דִּי' (galil. für קְדָמְכוֹן).⁴⁴ Here there are two major problems. One is the massive difference between either proposed pronunciation and the Lukan passage, in which ἐδικαίωσαν is positioned a very long way from βουλήν τοῦ θεοῦ. This underlines the second major problem, that a complete reconstruction would be required for this hypothesis to be confirmed, together with a proper account of the processes of translation and editing which led to the two passages which we now read. In short, we have not been given reason to believe that these two passages derive from one Aramaic underlay. It has been a perpetual illusion of Aramaists working on the Gospels that when two meanings of the same or similar Aramaic

⁴³ *Ibid.*, p. 79.

⁴⁴ *Ibid.*, pp. 86–7.

words are proposed, differences between Gospel passages have been explained. This example demonstrates that this is not sufficient. It is also regrettably typical that the two meanings of זכי cannot be validated for Aramaic of the right period, that the plausible-looking Galilean form may also be too late in date, and that יהוה is not likely to have been written for אדני, but אלהא is much more probable for θεοῦ.

5. With such loose methods, examples can be posited in the Gospel attributed to John, which consists largely of secondary rewriting in Greek.⁴⁵ For example, at John 8.34 a proposed wordplay fuels Meyer's reconstruction: **כל עבדי (דְעַבְדִּי) עוויא עבְדָא הוא דעוויא**.⁴⁶ Suggestions like this have the potential to damage the quest for the historical Jesus by making the latest and most unreliable of the Gospels appear early and authentic. Scholars have not realised how easy it is to produce supposed wordplays from mildly Semitic Greek.

These five problems have dogged the most learned and serious scholarship ever since. From an historical perspective, however, they must not be allowed to detract from the brilliance of Meyer's achievement. His work is very learned, and extremely ingenious, and it is not to be expected that creative pioneering scholarship should get everything right first time. Meyer had no proper models as a basis for his innovations. He advanced knowledge by reconstructing whole sentences, and by a variety of suggestions which required further critical assessment, and which should have led to increasingly refined work. It is a measure of his achievement that it was fifty years before it was seriously improved upon, and that scholarship still suffers from the problems which he left behind.

In the same year, Lietzmann surveyed the use of **בר (א)נש(א)** in the Targums of Onkelos and Jonathan, the Palestinian Syriac Gospels, and several tractates of the Palestinian Talmud.⁴⁷ This massive survey of Aramaic source material convinced him correctly that **בר נש** was a straightforward term for a person, but he went on to conclude that ὁ υἱὸς τοῦ ἀνθρώπου was a technical term of Hellenistic theology. This was hardly a satisfactory conclusion for a

⁴⁵ P. M. Casey, *Is John's Gospel True?* (London, 1996).

⁴⁶ Meyer, *Muttersprache*, p. 79, using the wordplay suggested by A. Smith Lewis, *A Translation of the Four Gospels from the Syriac of the Sinaitic Palimpsest* (London, 1894), p. xv.

⁴⁷ H. Lietzmann, *Der Menschensohn: Ein Beitrag zur neutestamentlichen Theologie* (Freiburg i. B. /Leipzig, 1896).

term absent from Acts (except 7.56) and from the Epistles. Where he did believe that **בר נשא** was original in a son of man saying, Lietzmann did not offer reconstructions, and his simple comments on the inappropriateness of \acute{o} υἱὸς τοῦ ἀνθρώπου rather than \acute{o} ἄνθρωπος as a translation show that he belonged to a period of scholarship when translators could not be fully understood.

The next major work was that of Dalman, *Die Worte Jesu*.⁴⁸ This was a less helpful contribution than has sometimes been thought. A useful introduction sets out reasons why we should suppose that Jesus spoke Aramaic, and should use this knowledge to illuminate his words. It has, however, significant problems of method. For example, Dalman argued that ἀποκριθεὶς εἶπεν must go back to *Hebrew*, not *Aramaic*. He concluded that it was not genuine Aramaic, so that the evangelists will have known it from the Hebrew Bible, whether directly or through the LXX.⁴⁹ Yet Dalman knew perfectly well that **ענה ואמר** was used in biblical Aramaic (for example Dan. 3.24). Dalman could not have known texts such as 4Q550 V.8, but the fundamental problem is his concept of genuineness. Nothing should be excluded from first-century Aramaic because it was originally Hebrew.

Dalman has a number of criticisms of other scholars, many of which are valuable. For example, criticising Resch in particular, he makes the point that, where different Gospel writers have synonyms, merely pointing out that one Hebrew word could lie behind both does not provide sufficient evidence of a Hebrew original.⁵⁰ When, however, he has to tackle the serious question of whether there is sufficient evidence in Q passages of an Aramaic original, all he can do is point out where others have made mistakes. For example, he comments on Nestle's suggestion that at Matt. 23.23 ἔλεος represents **רחמי**, which was confounded with **רחמתא** to give ἀγάπη at Luke 11.42, to which τοῦ θεοῦ was appended. Dalman points out that it is at least equally credible that the synonyms ἔλεος and ἀγάπη were interchanged and τοῦ θεοῦ added when this editing had been done in Greek.⁵¹ This gets us nowhere. What was needed was the reconstruction of whole passages, to see

⁴⁸ G. H. Dalman, *Die Worte Jesu*, vol. I, *Einleitung und wichtige Begriffe* (Leipzig, 1898. There was no second volume); ET *The Words of Jesus. I. Introduction and Fundamental Ideas* (Edinburgh, 1902; ²1930).

⁴⁹ Dalman, *Worte*, p. 20; ET p. 25.

⁵⁰ Dalman, *Worte*, pp. 34–5; ET pp. 44–5.

⁵¹ Dalman, *Worte*, p. 54; ET p. 68.

if a decision could then be taken as to whether an Aramaic underlay was probable, or whether we should adopt an alternative theory of some kind of Q which was transmitted in Greek and edited twice. From this point of view, the absence of Luke 11.40 from Matthew is just as important as the plausibility of רחמיא being misread as רהמתא, and the main point is that all such bits of evidence need to be discussed together. Dalman, like those whom he criticised, took only one small piece of evidence at a time, a process which never could lead to the uncovering of written Aramaic sources.

The main section of Dalman's book is organised around 'Begriffe', which are hardly what Jesus had. They are culturally German, and barely at home in first-century Judaism. The nearest thing to a 'Begriff' in Jesus' teaching is the kingdom of God, and in discussing this Dalman made an extraordinary and extraordinarily influential mistake: he attributed to Jesus the use of מלכותא דשמיא rather than מלכותא דאלהא on the ground that he was avoiding the divine name.⁵² But אלהא is not the divine name! It was the ordinary Aramaic term for 'God'. It was not the only term for 'God', and some texts do use other expressions (for example מלך שמיא, Dan. 4.34), but it continued in use, whereas the Tetragrammaton could be lawfully used only by the high priest on Yom Kippur. Dalman's section on 'Son of Man' is equally disastrous, not least because בר (א)נש(א) is not a 'Begriff'.⁵³ He begins with the Hebrew בן אדם, which is in the wrong language. He then infers that the singular בר (א)נש(א) was not in use in Jewish Palestinian Aramaic of the earlier period. It was not his fault that neither the Sefire inscriptions nor the Dead Sea scrolls had been discovered, but errors of method must still be attributed to him. He could have followed other scholars in taking more notice of the mundane nature of בר (א)נש(א) in thousands of examples in later sources, especially as the plural was already extant, in the definite state and with mundane meaning, at Dan. 2.38; 5.21.

Dalman was so impressed by the difficulty of doing adequate reconstructions of whole sentences that he could hardly see the point of this work. In the foreword to *Jesus-Jeshua*, he deliberately prescinds from making an Aramaic translation of Jesus' discourses, seeing no point in another Targum of the Gospels when there were

⁵² Dalman, *Worte*, pp. 75–7, ET pp. 91–4, and the same mistake at pp. 75–9, 159–62, 223; ET pp. 91–4, 194–7, 272.

⁵³ Dalman, *Worte*, pp. 191–219; ET pp. 234–67.

Aramaic and Hebrew translations already.⁵⁴ There were, and another was not required. Dalman failed to distinguish between translating material *into* Aramaic and the more difficult task of reconstructing what Gospel writers translated *from* Aramaic into Greek. It was accordingly rather inconsistent of him to offer Aramaic versions of several sentences. They are remarkably unilluminating. For example, he offers this version of Matt. 5.19:

*ūman dimebaṭṭēl ḥadā min hālen miṣwātā ze'ērāiyā umeal-
lēph ken libnē nāshā (biryātā) hū 'a'īd lmitḳerāyā ze'ērā
bemalkhūtā dishemaiyā. Ūman dimeḳaiyēm yāthēn
umeallēph ken hū 'a'īd lemītḳerāyā rabbā bemalkhūta
dishemaiyā.*⁵⁵

The idea of doing the whole verse is a potentially fundamental advance, as we have seen in discussing Meyer. Apart from being difficult to read because it is in the wrong script, however, Dalman's version is unhelpful because it is most unlikely to have existed before he made it up. This saying is attested by Matthew only, and has an excellent Sitz im Leben in the early church, where it accepts assimilation in the Gentile mission, but criticises metaphorically those who were not observant. It is difficult to see that it has any Sitz im Leben in the ministry of Jesus, where this was not an issue. The vocabulary is largely Matthean, and the most probable view of its origin is that Matthew composed it in Greek as part of his introduction to the Sermon on the Mount, which he constructed from traditional material which he vigorously edited.⁵⁶

Without proper criteria for distinguishing the authenticity of Aramaic versions of Gospel sayings, there is nothing to stop us from producing versions of material in the fourth Gospel. Dalman preferred Matthew and Luke, but Johannine efforts include *mushlam* for Τετέλεσται (John 19.30). As far as any understanding of the historical Jesus goes, this is irrelevant, because the Johannine material is secondary and was produced in Greek.⁵⁷ Dalman knew that it might have been, and it follows that his detailed discussion

⁵⁴ G. Dalman, *Jesus-Jeschua: Die drei Sprachen Jesu* (Leipzig, 1922), p. III; ET *Jesus-Jeshua: Studies in the Gospels* (London, 1929), p. xi.

⁵⁵ Dalman, *Jesus-Jeschua*, p. 58; ET p. 62. The transliteration given here follows the English version.

⁵⁶ Cf., e.g., W. D. Davies and D. C. Allison, *A Critical and Exegetical Commentary on the Gospel according to Saint Matthew* (3 vols., ICC, Edinburgh, 1988–97), vol. I, pp. 495–8.

⁵⁷ Casey, *Is John's Gospel True?*

of Aramaic and of other Jewish sources does not bear properly on the questions which should be at issue in the discussion of such a verse.⁵⁸

We must conclude that Dalman's major contribution to knowledge lay in the Jewish background to the New Testament, rather than in understanding sayings of Jesus.

The next major attempts to contribute to this work were those of Burney and Torrey.⁵⁹ Both showed learning and ingenuity, but were so unsound of method that very few of their suggestions have survived criticism. Burney pointed out a number of features of Semitic writing in the Gospels. For example, he pointed out how common parataxis is, and noted that it is characteristic of Semitic style, whereas Greek has many particles and subordinating participles.⁶⁰ Again, he devoted a whole chapter to 'The Use of Parallelism by Our Lord'.⁶¹ Having first noted this as a formal characteristic of Hebrew poetry,⁶² he set out many Gospel sayings in such a way as to draw attention to this feature of them. Burney also offered complete Aramaic reconstructions of several passages, including, for example, the whole Johannine prologue, and Matt. 8.20//Luke 9.58.⁶³ He also distinguished carefully between Semitisms, Aramaisms and Hebraisms.⁶⁴

How promising this sounds! Yet the whole exercise was vitiated by errors of method – even the case for the Gospels being translation Greek was not properly made. For example, parataxis is also found in Greek papyri, so it can hardly function on its own as evidence of translation Greek. Moreover, Johannine Greek, with its relatively restricted vocabulary, repetitive mode of expression, and lack of distinctively Greek particles, is very well adapted for communication between people who had several different first languages, and Greek as their second language.⁶⁵ This might have been just as important as Hebrew and Aramaic in the emergence of

⁵⁸ Dalman, *Jesus-Jeschua*, pp. 190–6; ET pp. 211–18.

⁵⁹ C. F. Burney, *The Aramaic Origin of the Fourth Gospel* (Oxford, 1922); C. C. Torrey, 'The Aramaic Origin of the Gospel of John', *HThR* 16, 1923, 305–44; C. F. Burney, *The Poetry of Our Lord* (Oxford, 1925); C. C. Torrey, *The Four Gospels: A New Translation* (London, 1933); C. C. Torrey, *Our Translated Gospels* (London, 1937).

⁶⁰ Burney, *Aramaic Origin*, pp. 5–7, 56–8.

⁶¹ Burney, *Poetry*, ch. 2.

⁶² *Ibid.*, *Poetry*, pp. 15–22.

⁶³ Burney, *Aramaic Origin*, ch. 1; *Poetry*, pp. 132, 169.

⁶⁴ Burney, *Aramaic Origin*, introduction.

⁶⁵ Casey, *Is John's Gospel True?*, p. 94.

Johannine Greek. Parataxis in the fourth Gospel is therefore different from parataxis in Mark, where there are other reasons to believe in Aramaic sources.

Parallelism is an equally unsatisfactory criterion. Burney set out most of his evidence in English, which underlines the fact that anyone familiar with Hebrew poetry can write in parallel lines in other languages, Greek included. Johannine examples may be entirely of this kind. Some of Burney's examples are also very dubious examples of parallelism. He comments on Mark 3.4//Luke 6.9, 'Instances of synonymous distichs or tristichs occurring singly or in groups of two or three are frequent.' He then sets it out:

Is it lawful on the sabbath to do good or to do harm?
To save a life or to kill?⁶⁶

I shall argue that this verse was indeed taken from an Aramaic source. It is, however, most unlikely that Jesus, Mark's source or anyone else thought that this was poetry.

This is even more marked with rhyme, which should not be regarded as a feature of ancient Semitic verse at all. Burney brought forward no evidence that rhyme was a feature of Aramaic verse. He discussed Hebrew poetry instead, and commented that 'the few occurrences which can be collected seem for the most part to be rather accidental than designed'.⁶⁷ His examples are indeed all produced at random by the fact that Hebrew words have a limited number of endings, with the result that similar ones occasionally occur together in groups. Burney produced the same effect with Aramaic versions of selected sayings of Jesus. For example, Burney translated John 10.1ff. into what he called 'rhymed quatrains, with the exception of the second stanza, which on account of its weight stands as a distich'.⁶⁸ He set out the first verse like this:

man d' lēt 'ālēl b' tar' ā
l' d' ir ā d' ' an ā
w' sālēk b' ' ah' ar ā yā
hū gannāb ūl' istā' ā

In the first place, it is difficult to see that this rhymes in any reasonable sense. In so far as it does so, this is because so many Aramaic words have similar endings. Moreover, Burney writes

⁶⁶ Burney, *Poetry*, p. 64.

⁶⁷ *Ibid.*, p. 147.

⁶⁸ *Ibid.*, pp. 174–5.

ūlīstā'ā for 'robber', using the fourth Gospel's Greek word, ληστής, as an Aramaic loanword and putting it in the definite state. While the Greek ληστής was eventually borrowed into Aramaic, it is not probable that this had already happened, and the native Aramaic ܠܢܐ is surely more probable. We must conclude that Burney's attempt to write rhyming Aramaic verse is entirely spurious.

Burney's versions also contain mistakes. For example, in Matt. 8.20// Luke 9.58, he has *ḵinnīn* for Q's κατασκηνώσεις. This is the wrong word. The Aramaic ܩܢܝܢ means 'nests', so any reasonable translator would have translated it as νοσσιάζς, using the straightforward Greek word for 'nests'. Accordingly, the Aramaic must have been a word such as ܡܫܟܢܝܢ, a general term meaning somewhere to stay, and reasonably used for the many trees round Capernaum in which native and migrating birds roost in large numbers. Other possibilities are ܡܫܬܠܠܝܢ and ܡܕܪܝܢ.⁶⁹ Burney's mistake was not, however, a random one. His tradition told him that the term 'nest' occurred in this saying (so, for example, the text of the RV in both places). He therefore translated this *into* Aramaic. This is the central fault at the basis of his reconstructions: they are not really *reconstructions* at all; they are *translations* of the kind that Dalman warned us against.

This is the fault at the basis of Burney's discussion of supposed mistranslations. They are for the most part not mistranslations of Aramaic sources, but creative work by Burney. For example, Burney argued that the frequency in John of the Greek particle ἵνα was due to the influence of the Aramaic relative particle ܕ, and that in some cases it had been mistranslated. His examples include John 6.50: 'This is the bread which comes down from heaven, so that one may eat of it and not die.'⁷⁰ Burney supposed that this originally meant 'which a man shall eat thereof and shall not die'. This is quite arbitrary. The evangelist's purpose clause makes excellent sense. In his view, Jesus did become incarnate in order to bring salvation, and as the metaphor of bread is carried through, it becomes clear that the Christian Eucharist is essential for salvation.⁷¹ This is the misplaced creativity which runs through the whole of Burney's discussion. Secondly, Burney's judgement that John uses ἵνα so

⁶⁹ P. M. Casey, 'The Jackals and the Son of Man (Matt. 8. 20//Luke 9. 58)', *JSNT* 23, 1985, 3–22, at 8, 20–1; cf. pp. 69–71 below.

⁷⁰ Burney, *Aramaic Origin*, pp. 69–78, at 76.

⁷¹ Cf. Casey, *Is John's Gospel True?*, pp. 42–51.

frequently that his usage requires this kind of explanation is based on comparing the fourth Gospel with three other documents, the Gospels of Matthew, Mark and Luke. Some Greek documents use the particle *iva* more frequently than they do, so that a more thorough comparison would be needed before we could regard Johannine usage as non-Greek.⁷²

Once we are prepared to assert mistranslations like this, the way is clear for us to read all sorts of things into an imagined Aramaic substratum. Burney found the virgin birth behind John 1.13.⁷³ Burney notes that the plural of the verb, אִיתִלִּידוּ, 'were born', is the same as the singular אִיתִלִּיד, 'was born', with the addition of the one letter ו, which on its own is the word for 'and', the first word of John 1.14. So Burney suggests an accidental doubling of this letter ו, which mistakenly caused the verb to be taken as a plural. He reconstructs the Aramaic source like this:

לְמַהִימְנִין בְּשִׁמְיָהּ. דְּלָא מִן דְּמָא (or דְּמִין?) וְלָא מִן
צְבוֹת בְּסִרְא וְלָא מִן צְבוֹת גְּבֵרָא אִילָהִין מִן אֱלֹהָא
אִיתִלִּיד.
וּמִימְרָא בְּסִרְא אִיתְעַבִּיד. . .

. . . to those that believe in His name; because He was born, not of blood, nor of the will of the flesh, nor of the will of man, but of God.

8. And the Word was made flesh . . .'

Thus he presents the author 'drawing out the mystical import of the Virgin-Birth for believers'. The supposed corruption, misreading and consequently erroneous translation are an entirely spurious part of this argument, the function of which is to find Christian doctrine in a document from which a Christian scholar believed it should not be absent.

Torrey recognised that Burney's proposed mistranslations were not satisfactory, which is very ironical, for Torrey proceeded to major on mistranslations as his central criterion for believing that the Gospels were translated from Aramaic. He had one or two good ideas. At Luke 12.49, he noted that the Greek text (τί θέλω εἶ) must mean 'what do I desire if'. He reconstructed this

⁷² Cf. E. C. Colwell, *The Greek of the Fourth Gospel: A Study of its Aramaisms in the Light of Hellenistic Greek* (Chicago, 1931), pp. 92–3.

⁷³ Burney, *Aramaic Origin*, pp. 34–5, 41–2, followed by Torrey, *Translated Gospels*, pp. 151–3.

מָה צָבֵא אֲנֵא הֵן, and translated it 'how I wish that'.⁷⁴ Torrey did not fully understand the translator, who was not only rendering word for word, but also suffering from interference. His Aramaic is perfectly correct, however, and should be accepted as an explanation of the Lukan expression. Torrey also commented plausibly on Luke 1.39, εἰς πόλιν Ἰούδα. Noting correctly that Judah was not a city, Torrey reconstructed מְדִינַת יְהוּדָה, meaning 'to the province of Judah'.⁷⁵ At the same time, however, Torrey was very dogmatic and not altogether convincing about the behaviour of the translator. He was quite sure that מְדִינָא in Hebrew and Jewish Aramaic always meant 'province', and that in Gentile usage it always meant 'city'. He thought that the trouble was simply caused by Luke's being Gentile. It is not, however, obvious that Luke was the translator, and Torrey had insufficient grounds for his assertion about Gentile usage. Moreover, we should add the problem of interference. A bilingual who was used to the first of the two nouns being in the construct, and the second not having case, might have read the expression in what has become the traditional way, 'to a city of Judah'.

Despite a small number of gains of this kind, Torrey's work suffers from serious defects. It is very badly set out. Aramaic usage is often authoritatively declared without supporting evidence. Torrey also deals often with only one or two words, which greatly facilitates playing tricks. Some suggestions are plausible, but doubtful because we do not have sufficient reason to believe that there ever was an Aramaic original to the passage discussed. For example, he describes the single word εἰσῆλθον at Luke 7.45 as 'An especially clear case of false rendering'.⁷⁶ He reconstructs the word as עֵלַת. He supposes that the source meant עֵלַת, 'she came in', but was misread in an unvocalised text as if it were עֵלִית, 'I came in', partly because the translator rendered עֵלַת correctly with εἰσῆλθον in the previous verse. This explanation is plausible, and gives a better account of the translator than Torrey usually does. Doubts remain because this piece is in Luke only, and is fluently written in Greek. It may be therefore that the author was not quite as sure as Torrey that the woman came in after Jesus, and really meant 'since I came in, she has not stopped kissing my feet', which is very entertainingly put.

⁷⁴ Torrey, *Translated Gospels*, pp. 31, 34.

⁷⁵ *Ibid.*, pp. 82–6.

⁷⁶ *Ibid.*, pp. 98–100.

More serious are cases where Torrey has creatively rewritten the text with Aramaic backing. For example, for μετ' ὀργῆς at Mark 3.5 Torrey reconstructs ברנו. This is not an unreasonable version of the two words taken in isolation: viewing the sentence as a whole, I shall use the same word for ὀργή in a complete reconstruction of the verse, beginning ופנה עליהון לרגו. Torrey, however, was quite sure that Jesus was not angry. He argues that the word signifies 'distress, deep sorrow', citing *Tg. Ps.* 6.8; *Job* 17.7, both of which are too late in date, together with *2 Sam.* 19.1, which is in the wrong language.⁷⁷ Torrey's argument is thus a mixture of good and bad method. It was right to find the underlay of ὀργῆς, and wrong to take it in isolation from the rest of the sentence. It would have been entirely right to offer a careful outline of the semantic area of רגו, and not to be hidebound by the translator in considering what Mark's source really meant and what Jesus really felt. It was wrong to have such a conviction that Jesus could not have been angry, and to rummage around texts in either Hebrew or Aramaic, written centuries before and afterwards, to find a meaning which fits a conviction that Jesus was not angry.

With such method, the rewriting can go much further astray, especially in dealing with texts which were first written in Greek. I have noted the Johannine prologue, where Torrey accepted Burney's use of a supposed Aramaic substratum to introduce the virgin birth where they felt that it should not have been left out.⁷⁸ Torrey was equally sure that the imperfect ἦν at 1.15, and the participle ὄν at 1.18, could not be right. So he suggested that ἦν at 1.15 was due to the misrendering of הוּא, which should have been rendered 'is', but which the translator interpreted as הָיָה, 'was': while at 1.18, הָיָה, which should have been rendered 'was', was read as הוּא, and misrendered 'is'.⁷⁹ Both suggestions arise from Torrey's lack of sympathy for the Johannine way of putting things. Here the author looks back on the ministry of Jesus from the perspective of the church, seen through the rewritten witness of John the Baptist. This is the reason for the past tense. Similarly at 1.18, Jesus is in the bosom of the Father, for that is where he has been since the time of the ministry, the narrative of which begins at 1.19. Torrey's comment on 1.15 is classic: 'The fact that the Grk. translator of the Gospel erred here is placed beyond doubt by the

⁷⁷ *Ibid.*, p. 68.

⁷⁸ See p. 22 above.

⁷⁹ Torrey, *Translated Gospels*, pp. 117–18.

subsequent examples of the same mistake.’ What this really means is that Torrey was very good at naughty tricks and played this one elsewhere too. At this stage, a supposed Aramaic stratum has become an excuse for altering difficult texts to something more convenient.

Torrey’s suggestions also involved some poor work by translators, and at times this goes beyond the reality of this world, in which some poor translating is indeed done, into the realms of lunacy. For example, at John 7.38 he suggests that מִן גִּיהָ, ‘out of the midst of her’, was misread as מִן גִּיהָה, ‘out of his belly’.⁸⁰ This gives us a sensible original meaning, and enables us to find scriptures which refer to the flowing of water out of Jerusalem. However, Torrey’s account of the translator is an account of an extraordinary blunderer. That cannot be excluded *a priori*, and we would have to believe it on the basis of good evidence, but on the basis of conjectures of this sort it is hardly convincing.

Not only did Torrey fail to give proper details of the attestation of difficult Aramaic, but in some cases he got it wrong. For example, at Mark 7.3 he suggested that πῦγμα was a translation of לְגִמְדָּה, ‘with the fist’, whereas the translator should have read לְגִמְדָּה, and should have translated this ‘at all’.⁸¹ It is not, however, clear that these were Aramaic words. Neither occurs in Aramaic of anything like the right period, and as far as I know there is no Aramaic word גִּמְדָּה = ‘fist’. Finally, some suggestions are not properly worked through. For example, he makes the claim that ‘Lk. 16:18, last clause, gives an exact verbal rendering of the Aram. here conjectured for Mk.’⁸² He does not, however, explain this.

We must therefore conclude that, like Burney, Torrey took work on the Aramaic substratum of the Gospels backwards rather than forwards. He had learning and ingenuity, but no serious controls, and he understood neither texts nor translators. Some of the contemporary discussion of his work was equally poor. Goodspeed argued that there could not have been any Aramaic Gospels, because there was no Aramaic literature at that time.⁸³ His wild

⁸⁰ *Ibid.*, pp. 108–11.

⁸¹ *Ibid.*, pp. 93–4.

⁸² *Ibid.*, p. 95.

⁸³ E. J. Goodspeed, *New Chapters in New Testament Study* (New York, 1937), pp. 127–68. Cf., e.g., D. W. Riddle, ‘The Logic of the Theory of Translation Greek’, *JBL* 51, 1932, 127–38; D. W. Riddle, ‘The Aramaic Gospels and the Synoptic Problem’, *JBL* 54, 1935, 127–38; E. J. Goodspeed, ‘The Possible Aramaic Gospel’, *JNES* 1, 1942, 315–40.

polemic failed to come to terms with indications that there was such Aramaic literature, and his second-rate analysis of Gospel evidence shows that he had not taken seriously, and perhaps had not read, the work of Meyer. From this grim retrospect, we can see the more clearly what a shining light Meyer had been, and Black was to be. Before offering an assessment of Black, we must draw attention to some features of the work done in the intervening period by scholars who did not write whole books about the Aramaic substratum of the Gospels.

Firstly, some scholars whose prime purpose was to illuminate the Gospels and their accounts of the historical Jesus used their knowledge of Aramaic to do so. One of the most helpful was Wellhausen, some of whose suggestions have withstood subsequent criticism. For example, at Mark 3.4 he argued for *achi* behind σῶζειν, a suggestion which I have adopted.⁸⁴ After more than one attempt, he suggested that at Matt. 23.26 καθαρισον correctly represents the Aramaic *dakkau* (*reinigt*), whereas at Luke 11.41 τὰ ἐόντα δότε ἐλεημοσύνην represents a misreading of the same word as *zakkau* (*gebt Almosen*).⁸⁵ This is also plausible, and a useful contribution to the whole question of the relationship between the different forms of Q material. At the same time, however, the fact that Wellhausen normally confined himself to single words meant that this was a very conjectural process, which could never lead either to a complete understanding of Gospel sources or to a proper understanding of translators. Wellhausen also noted that Codex Bezae had a greater claim to preserve the original text of the Gospels in various passages, more so than Westcott and Hort had bargained for.⁸⁶ This work was carried further by Wensinck, who argued that Bezae and its allies represent more faithfully the original form of the text of Luke, and that a corrected edition was issued by him.⁸⁷ While Wensinck's theory should not be accepted, his comprehensive collection of Semitisms was useful, and the realisation that the more Semitic readings in the western text might be more original is important in permitting the reconstruction of some passages.

If specialists Dalman, Burney and Torrey could be as un-

⁸⁴ J. Wellhausen, *Das Evangelium Marci* (Berlin, 1903), p. 23.

⁸⁵ J. Wellhausen, *Einleitung in die drei ersten Evangelien* (Berlin, 2nd 1911), p. 27.

⁸⁶ *Ibid.*, p. 9.

⁸⁷ A.J. Wensinck, 'The Semitisms of Codex Bezae and their Relation to the non-Western Text of the Gospel of Saint Luke', *Bulletin of the Bezan Club* 12 (Leiden, 1937), 11–48.

successful as the account above has indicated, it is not surprising that other New Testament scholars could also make some unconvincing suggestions. One of the most famous was by C. H. Dodd. In the wake of the work of Schweitzer and Weiss, some good Christian people would rather that Jesus had not expected that the kingdom would come at once, and had not been mistaken, for this is a mistake with severe consequences for orthodox Christology. Some texts are problematical from this point of view, including Mark 1.15, where Jesus says ἤγγικεν ἡ βασιλεία τοῦ θεοῦ, which means that the kingdom of God is at hand, about to come. Dodd suggested that the word behind ἤγγικεν was *m'ta*, and that it meant the same as ἔφθασεν at Matt. 12.28//Luke 11.20, so we should translate 'The Kingdom of God has come.'⁸⁸ Here we see the same flaws of method as in the work of Burney and Torrey. The text is inconvenient, the Aramaic substratum is not extant. A single word is therefore suggested to clear up the problem. The translator is not properly explained, though Dodd went to the LXX in an effort to do this. He noted, for example, the rendering of אֲשׁוּמ with ἐγγίζω at LXX Dan. 4.9, 19.⁸⁹ The translator of LXX Daniel is not a good model, and in these two passages he has rendered interpretatively, so that the tree reached up to heaven but didn't quite get there. In this, as in much else, the LXX was corrected by Theodotion. Examples like this should never be used to equate the semantic areas of different words in the same or in different languages. Moreover, by using one word Dodd avoided the main questions. Have we sufficient reason to believe in an Aramaic version which meant something different from the text which we have got? When we reconstruct a whole sentence, can we see how and why the translator changed the meaning of the text when he translated it? The second question is especially important in this case, since Dodd proposed to attribute to Jesus a theory of realised eschatology supposedly of central importance, and have the translator fail to transmit it by means of the obvious rendering of אֲשׁוּמ with φθάνω.

All the suggestions made during this period can hardly be said to amount to a significant contribution to scholarship. The basic reason for this was the methodological flaws common to all this work. The standard of verification was too low, and some scholars

⁸⁸ C. H. Dodd, *The Parables of the Kingdom* (London 1935, ²1961), pp. 36–7.

⁸⁹ C. H. Dodd, "'The Kingdom of God has Come"', *ExpT* 48, 1936–7, 138–42, at 140–1.

were only too happy to make up stories about Aramaic originals which had convenient results.

A fundamental achievement of scholarship in the first half of this century was the discovery and editing of important texts. Aramaic papyri were discovered at Elephantine and elsewhere, including letters, a copy of the Bisitun inscription of Darius I and the proverbs of Ahiqar. Sachau published major finds in 1911, and the standard edition of many of these documents was published with English translation and notes by Cowley in 1923.⁹⁰ This formed an important contribution to our knowledge of Aramaic vocabulary and grammar. The discovery of the Sefire inscriptions was also important, though it was some time before the standard editions were produced.⁹¹

Many texts which were known only to specialists in Syriac were also published, and so were made available to New Testament scholars who learn Syriac. Of particular importance were the two major series, *Patrologia Orientalis* and *Corpus Scriptorum Christianorum Orientalium: Series Syriaca*. Another significant part of the advancement of relevant knowledge was the compilation of works of reference, especially dictionaries and grammars. These included F. Schulthess, *Lexicon Syropalaestinum* (1903) and *Grammatik des christlich-palästinischen Aramäisch* (1924).

There were also continuing attempts to carry forward the task of dating texts and understanding the development of Aramaic, including the relationship of its several dialects. Of particular importance was the work of Kahle, though his dating of Targumic materials has not survived criticism.⁹² Segal contributed an important essay on Mishnaic Hebrew as a living language.⁹³ We have seen that it was often regarded as a dead language: what Segal did was to demolish all attempts to support this by means of analysing the language itself. Unfortunately, he later went on to declare that Hebrew was the lingua franca of Judaea and spoken in Galilee, at

⁹⁰ E. Sachau, *Aramäische Papyrus und Ostraka aus einer jüdischen Militär-kolonie zu Elephantine* (Leipzig, 1911); A. Cowley, *Aramaic Papyri of the Fifth Century B. C.* (Oxford, 1923).

⁹¹ The best is J. A. Fitzmyer, *The Aramaic Inscriptions of Sefire* (BibOr 19, 1967. Rev. edn, BibOr 19A, 1995).

⁹² P. Kahle, *Masoreten des Westens II* (Stuttgart, 1930); *The Cairo Genizah* (Schweich Lectures, 1941. London, 1947), esp. pp. 117–32; 2nd edn (Oxford, 1959), esp. pp. 191–208.

⁹³ M. H. Segal, 'Mishnaic Hebrew and its Relation to Biblical Hebrew and to Aramaic', *JQR* 20, 1908, pp. 647–737.

least by the educated classes.⁹⁴ This is a classic case of a scholar beginning with careful and innovative analysis of the available evidence, and then going on to conjecture authoritatively in a gap. At the same time, Segal did not make the errors of method which we shall find in later work which made similar claims:⁹⁵ he suffered from the absence of Aramaic documents which were subsequently discovered.

One other feature of New Testament scholarship in this period must be briefly noted. Most scholars writing on Jesus and the Gospels left Aramaic out. For example, C. H. Turner omitted it from most of his studies of Markan style, including, for example, his discussion of some twenty-six occurrences of ἄρχομαι, an obvious translation of אָרַח. Zerwick almost omitted Aramaic from a whole book on Markan style.⁹⁶ Streeter virtually omitted the Aramaic dimension from his discussions of the priority of Mark and the nature of Q: a most inadequate treatment is just squeezed into the discussion of the 'minor agreements'.⁹⁷ Bultmann and Dibelius gave only a very occasional mention to an occasional item in their pioneering works of Formgeschichte, in which they bred unnecessary scepticism about the historical worth of sayings and narratives which they failed to see in their cultural context.⁹⁸ Aramaic receives only the briefest mention in Headlam's *Life of Jesus*.⁹⁹ This omission of Aramaic drastically inhibited the task of seeing Jesus against the background of his own culture.

We must now consider the work of Matthew Black, *An Aramaic Approach to the Gospels and Acts* (Oxford, 1946). In this book, Black gathered together the best of previous work, and added many points of his own. In his review of previous work, Black laid down a number of correct principles. For proposed mistranslations,

⁹⁴ M. H. Segal, *A Grammar of Mishnaic Hebrew* (Oxford, 1927), pp. 5–20.

⁹⁵ See pp. 57–63 below.

⁹⁶ C. H. Turner, 'Markan Usage: Notes, Critical and Exegetical, on the Second Gospel. VIII. Auxiliary and Quasi-auxiliary Verbs', *JThS* 28, 1927, 349–62, reprinted with other studies in J. K. Elliott (ed.), *The Language and Style of the Gospel of Mark* (NT. S LXXI, 1993); M. Zerwick, *Untersuchungen zum Markus-Stil* (Rome, 1937).

⁹⁷ B. H. Streeter, *The Four Gospels: A Study of Origins* (London, 1924), pp. 296–8.

⁹⁸ M. Dibelius, *Die Formgeschichte des Evangeliums* (Tübingen, 1919. ²1933). ET *From Tradition to Gospel* (New York, 1965); R. K. Bultmann, *Die Geschichte der synoptischen Tradition* (FRLANT 29, NF 12. Göttingen, 1921). ET *The History of the Synoptic Tradition* (Oxford, 1963).

⁹⁹ A. C. Headlam, *The Life and Teaching of Jesus the Christ* (London, 1923).

he laid down that 'the mistranslation must at least be credible; and the conjectured Aramaic must be possible'. This excludes a high proportion of suggestions, and in this matter Black unfailingly observed his own principles. He also followed Driver in calling for the presentation of whole sentences.¹⁰⁰ We have seen Meyer do this, and the faults of not doing so: this was a necessary principle, which, however, Black did not keep to all the time. Black also offered a sound overall summary of the range of available Aramaic sources, of Aramaic dialects, and of the languages which Jesus is likely to have known. He concluded that Jesus will have taught almost entirely in Aramaic, and that his task was to determine the extent of Aramaic influence in the Gospels.¹⁰¹ He discussed whole features of the Aramaic language as well as detailed reconstructions. For example, he has a whole section on asyndeton.¹⁰² This includes discussion of whether the extent of asyndeton in John's Gospel might be due to Jewish or Syrian Greek, rather than actual translation. Black's separation out of these possibilities was much more careful than the work of his predecessors.

Helpful reconstructions attempted by Black include Mark 4.31b: *di kadh z^eri^c b^es^aar^a z^eer hu' min kullhon zar^ain dib^era^a*.¹⁰³ Here the choice of script is regrettable, because it makes the sentence so difficult to read, and the vocalisation is for the most part seriously uncertain, as Black noted.¹⁰⁴ The main point, however, is that a play on words between the words for 'sowing', 'seeds' and 'earth' is inevitable. It is also easier to appreciate for being part of a completely reconstructed line. Useful comments on passages which are not reconstructed include Luke 14.5, where there must indeed be wordplay on 'son' (*b^era*) and 'ox' (*b^eira*), and the proposed word for 'pit' (*b^era*) is also perfectly plausible.¹⁰⁵ Helpful discussions of linguistic features include aorists such as ἐβάπτισα at Mark 1.8, which must represent a Semitic perfect.¹⁰⁶ Thus, in addition to sound principles, Black contributed the largest number of sound examples of Aramaic influence in New Testament Greek so far collected.

Black also contributed helpful criticisms of previous work. For

¹⁰⁰ M. Black, *An Aramaic Approach to the Gospels and Acts* (1946), pp. 7, 12.

¹⁰¹ *Ibid.*, ch. 2.

¹⁰² *Ibid.*, pp. 38–43.

¹⁰³ *Ibid.*, p. 123.

¹⁰⁴ *Ibid.*, p. v.

¹⁰⁵ *Ibid.*, p. 126.

¹⁰⁶ *Ibid.*, p. 93.

example, he did not continue Burney's spurious efforts to find rhyme in Aramaic poetry. He also criticised Torrey's Aramaic, noting that some of his supposed words are not attested.¹⁰⁷

The extensive and sober discussions of all these features, with ample examples, are the major points which make this book the best so far published on this subject. It still left serious problems, however, mostly the same ones as were bequeathed by Meyer.

1. There is first of all the perennial problem of the meagre remains of Aramaic from the time of Jesus. We can see some of the effects of this in the light of subsequent discoveries. For example, Black declared that 'the construct has largely fallen into abeyance in all Aramaic dialects'.¹⁰⁸ There are now too many examples of it from documents near the time of Jesus for us to agree with this: for example אַנְשׁ 1QapGen XX.16; בֵּית 1 En. 22.4. Again, in assessing Wellhausen's suggestion that an ambiguous 'shkahl might lie behind variant readings at Luke 13.24, Black very cautiously points out that שְׂכַח was not known in Palestinian Aramaic: it is now extant at 1QapGen XXI.13, where it represents יֹכַל of MT Gen. 13.16.¹⁰⁹ The small amount of extant Aramaic made documents even more difficult to date than they are now. This is one reason why Black accepted faulty early dates for the Palestinian Pentateuch Targum and the Targum to the Hagiographa.¹¹⁰

2. Even though Black largely saw through Burney and Torrey, he still has too much stress on wordplay. For example, he still has the doubtful pun on אַבְנִיא for λίθον and בְּנִיא for τέκνα at Matt. 3.9//Luke 3.8.¹¹¹ More seriously, he has several unconvincing arguments involving Johannine passages, and variant readings in all the Gospels. For example, he suggests that at John 3.33 the Aramaic שְׂדַרְיָה, 'sent him', has been misread as שְׂרִירָא, ἀληθής, and he approves of the parallelism which he thinks he has restored.¹¹² The trouble with this is that the text of the fourth Gospel makes sense as it is, and we have no reason to believe these conjectures. The textual variants, though often fascinating, are also dubious. For example, Black retails Cureton's suggestion that at Matt. 20.21 the Sinaitic and Curetonian Syriac have מְרִי, 'My

¹⁰⁷ *Ibid.*, pp. 105, 8–9.

¹⁰⁸ *Ibid.*, p. 68.

¹⁰⁹ *Ibid.*, p. 96.

¹¹⁰ *Ibid.*, pp. 20–2.

¹¹¹ *Ibid.*, p. 107; see pp. 13–14 above.

¹¹² *Ibid.*, pp. 110–11.

Lord', in place of the Greek εἰπέ, because they read an Aramaic text in which אַמַר, correctly rendered εἰπέ, had been corrupted to מַרִי.¹¹³ The trouble with this is that we have no reason to believe that they read an Aramaic text at all, and an alternative explanation is readily available: the Syriac translator felt both that Zebedee's wife was bound to address Jesus respectfully, and so put מַרִי and that his Syriac sentence flowed better without a literal equivalent of εἰπέ. All this comes partly from not realising how easy it is to create wordplays in Aramaic.

3. While Black's attempts to understand translators were an improvement on his predecessors', severe problems remained. For example, Black argued that at Mark 14.25 'πίνω καινόν is impossible in Aramaic and can scarcely have been original'.¹¹⁴ What is impossible is to translate πίνω καινόν literally *into* Aramaic, which is what we are tempted to do if we can only envisage a translator translating absolutely literally from Aramaic into Greek. I shall suggest that Mark's source read אַשְׁתִּינָה וְהוּא חֲדַת, and that Mark's text is a solution to the problem of translating this into decent Greek.¹¹⁵

4. Some suggestions are still not fully worked through. For example, Black suggests that the difference in Aramaic between two phrases in Mark 8.38 and Luke 9.26 is slight, and is due to differing interpretations of an Aramaic proleptic pronoun.¹¹⁶ He does not, however, offer proper reconstructions of either, so does not get involved in how בַר (אִנְשָׁא) could have functioned in either saying, or in the (surely improbable) model of the synoptic problem which is implied.

5. Despite his greatly improved methods, Black was still left with many examples of Semitic phenomena in the Gospel attributed to John. I have noted his treatment of John 3.33. Proper explanation of this would not be possible until scholars realised the ease with which wordplays can be produced and the limited significance of features such as parallelism, and came to terms with Johannine Greek as either a form of Jewish Greek or an adaptation of Greek for speakers of several different first languages.

As in the case of Meyer, the problems outstanding must not be allowed to obscure the brilliance of Black's achievement. This was

¹¹³ *Ibid.*, p. 186.

¹¹⁴ *Ibid.*, p. 171.

¹¹⁵ See p. 243 below.

¹¹⁶ Black, *Aramaic Approach*, p. 73.

the most learned and ingenious book on this subject, the only good one for fifty years and still the best fifty-two years later. It was so because Black carefully gathered together all that was known, saw through most of what could be seen to be false, ingeniously added much, and wrote it all up with great care.

The most important feature of work since Black has been the discovery, editing, examination and use of the Dead Sea scrolls. Several scrolls and fragments are written in Aramaic. They have provided many examples of words which were only known from later documents in other dialects. They have permitted extensive grammatical analysis, as a result of which it has become possible to date other Aramaic documents with greater precision. They have fuelled the question of exactly which sort of Aramaic should be used to reconstruct sayings of Jesus, a question which I hope to resolve with this book.

The first major document to be made available was the Genesis Apocryphon, the most legible columns of which were first published in 1956, and studied over the following years.¹¹⁷ The next was the Targum of Job, published in 1971.¹¹⁸ With most documents being in Hebrew, it was especially important to have these two major ones in Aramaic, to reinforce the established view that this was the language spoken by most people in Israel, in which Jesus will therefore have taught. The Genesis Apocryphon is a Haggadic piece of a relatively popular kind, whereas most of the Hebrew documents are relatively learned. The Job Targum is a quite literal translation. Its existence is pointless unless there were Jews who wanted to know what the book of Job said, and who could

¹¹⁷ N. Avigad and Y. Yadin, *A Genesis Apocryphon: A Scroll from the Wilderness of Judaea. Description and Contents of the Scroll, Facsimiles, Transcriptions and Translation of Columns II, XIX–XXII* (Jerusalem, 1956); E. Y. Kutscher, 'The Language of the Genesis Apocryphon: A Preliminary Study', *Scripta Hierosolymitana IV, Aspects of the Dead Sea Scrolls*, ed. Ch. Rabin and Y. Yadin (Jerusalem, 1957), pp. 1–35; J. A. Fitzmyer, *The Genesis Apocryphon of Qumran Cave I: A Commentary* (BibOr 18. Rome, 1966; BibOr 18A. ²1971); T. Muraoka, 'Notes on the Aramaic of the Genesis Apocryphon', *RQ* 8, 1972, 7–51.

¹¹⁸ J. P. M. van der Ploeg and A. S. van der Woude (eds.), *Le Targum de Job de la Grotte 11 de Qumrân* (Leiden, 1971); M. Delcor, 'Le Targum de Job et l'Araméen du temps de Jésus', *RevSR* 47, 1973, 232–61; M. Sokoloff, *The Targum to Job from Qumran Cave XI* (Ramat Gan, 1974); J. A. Fitzmyer, 'Some Observations on the Targum of Job from Qumran Cave 11', *CBQ* 36, 1974, 503–24, rep. in J. A. Fitzmyer, *A Wandering Aramean* (SBLMS 25. Missoula, 1979), pp. 161–82; T. Muraoka, 'The Aramaic of the Old Targum of Job from Qumran Cave XI', *JJS* 25, 1974, 425–43; T. Muraoka, 'Notes on the Old Targum of Job from Qumran Cave XI', *RQ* 9, 1977, 117–25.

understand an Aramaic translation but not the Hebrew text. We have to infer that, whereas learned Essenes could read and write in Hebrew, most Jews spoke Aramaic.

The other major single find was the fragments of the books of Enoch, including the related book of Giants, but not the Similitudes. The extant fragments belong to a document partly extant also in Greek, and the most extensive text of what we call 1 Enoch survives in Ge'ez. This find therefore increased the amount of material available for us to study the techniques of people who translated from Aramaic into other relevant languages. A proper edition was produced by J. T. Milik, but not until 1976.¹¹⁹

A large number of works survived only in fragments. The amount of this literature further strengthens the argument that Aramaic was the language primarily spoken by most Jews at the time of Jesus. It is also especially important that there is one, perhaps two, further Targums. The most important is 4Q156, a fragment containing what survives of an Aramaic translation of Lev. 16.12–21.¹²⁰ This is too small for us to be sure that it is from a complete Targum, but this is the most likely possibility. Accordingly, this piece demonstrates that there were Jews who were so observant that they wanted to know what Leviticus said, but who could not read or understand Hebrew. This is natural, since the reading of the Torah in the synagogue would require an Aramaic version unless everyone spoke Hebrew or Greek. It would therefore make very little difference if this piece should have been from some kind of lectionary, or other composite work: it is unambiguous evidence of faithful Jews who needed the text of the Torah in Aramaic because they could not cope with the instructions for Yom Kippur in Hebrew. There is also an Aramaic version of Tobit, which requires further study to determine whether it is an original text or a Targum.¹²¹ Our

¹¹⁹ J. T. Milik, *The Books of Enoch: Aramaic Fragments of Qumrân Cave 4* (Oxford, 1976); cf. L. T. Stuckenbruck, 'Revision of Aramaic-Greek and Greek-Aramaic Glossaries in *The Books of Enoch: Aramaic Fragments of Qumrân Cave 4*, by J. T. Milik', *JJS* 41, 1990, 13–48.

¹²⁰ J. T. Milik and R. de Vaux (eds.), *Qumrân Grotte 4. II. I. Archéologie II. Tefillin, Mezuzot et Targums (4Q128–4Q157)* (DJD VI. Oxford, 1977), pp. 86–9, 92–3; J. A. Fitzmyer, 'The Targum of Leviticus from Qumran Cave 4', *Maarav* 1, 1978, 5–23; A. Angerstorfer, 'Ist 4Q Tg Lev das Menetekel der neueren Targumforschung?', *BN* 15, 1981, 55–75; Beyer, *Die aramäischen Texte*, pp. 278–80; A. Angerstorfer, 'Überlegungen zu Sprache und Sitz im Leben des Toratargums 4Q Tg Lev (4Q 156), sein Verhältnis zu Targum Onkelos', *BN* 55, 1990, 18–35.

¹²¹ Cf. J. A. Fitzmyer, 'The Aramaic and Hebrew Fragments of Tobit from Qumran Cave 4', *CBQ* 57, 1995, 655–75.

appreciation of these pieces was inhibited by the scandalous delay in publishing them, so that it has only recently become possible to utilise them fully in the reconstruction of sayings of Jesus.

Taken together, the scrolls have massively increased the number of Aramaic words known to have existed before the time of Jesus. For example, **אבר**, 'limb', was previously known from later Jewish Aramaic, Samaritan Aramaic and Christian Palestinian Aramaic: it is now found in 4Q561. **בטן**, 'to be pregnant', was known from later Jewish Aramaic, Samaritan Aramaic, Christian Palestinian Aramaic and Syriac: it is now found in 4QEn^a (1 En. 7.2). **דול**, 'to quiver, shake', was known with certainty only in Syriac: it is now found in 4Q560 (cf. 1Q20). **חרו**, 'to string', was known from later Hebrew and Aramaic, especially Syriac: it is now found at 11QtgJob XXXV.4.

Some words which we knew in biblical Hebrew and later Aramaic are now extant also in the Aramaic in the scrolls. Perhaps the most important is the noun **מכאב**, now extant at 4QTLevi VIII.1; VIII.3, with . . . **מכאבן** at VII.3, so there should be no doubt that the verb **כאב** could also be used in the Aramaic of our period. It was previously known with certainty only in Hebrew and later Aramaic, Syriac being the only sort of Aramaic in which it was common. This clears up the previously insoluble problem of which word Jesus could have used to say that a/the son of man suffers (cf. Mark 8.31; 9.12). Other words include **אבל**, 'to mourn', previously known in biblical and later Hebrew, and later Jewish Aramaic, Samaritan Aramaic, Christian Palestinian Aramaic and Syriac: it is now found at 4QTLevi V.2 and 4QGiants 428. **בוז**, 'to rob, plunder', was known from biblical as well as later Hebrew, later Jewish Aramaic, Samaritan Aramaic, Christian Palestinian Aramaic and Syriac: examples from our period now include 1QapGen XXII.11 and 4Q318.III. 8. Taken all together, this group of words show that the Aramaic of our period was more influenced by Hebrew than we had previously realised, a natural result of a much longer period of diglossia than we previously knew about.

Some words already known from before the time of Jesus are now extant in a sense previously known only from a later date. For example, we knew the word **רבע** meaning 'lie down' in general, and used of reclining at table in Christian Palestinian Aramaic and in Syriac, long after the time of Jesus. It is now extant at 4Q196 (Tobit 2.1) of Tobit reclining at table, so I have used it

for ἀνακειμένων in my reconstruction of Mark 14.18. The word זָהָר was known in several dialects meaning 'take care', but with the quite different meaning 'shine' only in Syriac: it is now found meaning 'shine' at 11QtgJob XXX.4.

Of particular importance are the examples of בַּר אַנּוּ(ו)שׁ in the ordinary sense of 'man', 'person'. This occurs as a rendering of the Hebrew בֶּן אָדָם at 11QtgJob IX.9; XXVI.3. At 1QapGen XXI.13 בַּר אַנּוּשׁ corresponds to the Hebrew אִישׁ at Gen. 13.16, so its occurrence is not due to mechanical translation, and with כּוֹל and the negative לֹא it means 'no one', in a general statement. It has therefore been chosen deliberately in as mundane a sense as possible. It also occurs in the plural at 1 En. 7.3; 22.3; 77.3 [4QEnastr^b 23]; 1QapGen XIX.15; 4QGiants 426; 11QtgJob XXVIII.2. Not only does it not occur as a title, but it is not possible to see how it could be used as a title at the same time as it was in normal usage in this mundane sense.

The overall effect of these discoveries has been to make it possible to rely primarily on Aramaic from approximately the time of Jesus to reconstruct his sayings. Moreover, they have made dialectal differences less important than they were. When we had only early and late evidence available, the differences between Galilean and other Aramaic appeared great, and it seemed important that we did not have direct access to Jesus' dialect. Now it seems clear that the differences were small, not remotely comparable with the common habit of New Testament scholars of dealing with his sayings, and Gospel sources, in the wrong language altogether. The degree of interpenetration from Hebrew, emerging from centuries of diglossia, is also important: we can no longer assume that evidence of a Hebraism in a text such as Mark 14.25 means that it was not originally in Aramaic – rather, we must note the idiomatically Hebrew use of the Aramaic and Hebrew word יִסְף at 4Q198 (Tobit 14.2); 11QtgJob XXV.8, and infer that this usage had penetrated Aramaic.¹²²

The Dead Sea scrolls were not the only discovery to be made during this period. The most famous of the other texts made available was Codex *Neofiti I*, a Targum to the Pentateuch. While this was at first thought to be earlier in date than now seems reasonable, careful studies have slowly enabled us to date documents more accurately. The work of York and Kaufman has made

¹²² Cf. pp. 86, 242–3 below.

it clear that rabbinical Targums cannot be dated as early as the time of Jesus.¹²³ Work on the Dead Sea scrolls has proceeded slowly, and its effects have been gradually combined with work showing that these Targums must be dated later. When I reconstructed Mark 2.23–8 for publication in 1988,¹²⁴ I made use of as much of the Qumran material as was available, and could not imagine how Mark 9.11–13 could possibly be done: more Qumran material has been available for writing this book, together with a gradually clearer idea of the dating of other sources. The overall effect of all this work is to reduce our dependence on later source material, and to make the reconstruction of Gospel sources more possible.

A number of other texts which were previously known only in manuscript were made available for the first time, including Narsai's homily on the Ascension of Elijah and Enoch.¹²⁵ Some texts previously known in old and unreliable editions were published by modern critical scholars on the basis of a much wider range of manuscripts. Such works included the *Testament of Mar Ephraem*, previously known only from Assemani's 1740 edition, now made available in a critical text edited by Beck, who has contributed a number of fine editions.¹²⁶ This contains three examples of the idiomatic use of **בר (א)נשא** in a general statement used by the author with particular reference to himself. It is also an unusual work in that a Greek translation is extant. So, for example, we know that at line 124 the Greek translator rendered **ברנשא** with $\alpha\tilde{\nu}\delta\rho\alpha$, and for **כל ברנשא** at T. Ephraem 944 he put $\tau\eta\varsigma \tau\omicron\upsilon\theta \epsilon\omicron\upsilon\delta \epsilon\kappa\kappa\lambda\eta\sigma\iota\alpha\varsigma$. The interest of these renderings underlines the desirability of a modern critical edition of the Greek translation. Other

¹²³ A. D. York, 'The Dating of Targumic Literature', *JSJ* 5, 1974, 49–62; S. A. Kaufman, 'On Methodology in the Study of the Targums and their Chronology', *JSNT* 23, 1985, 117–24; S. A. Kaufman, 'Dating the Language of the Palestinian Targums and their Use in the Study of First Century CE Texts', in D. R. G. Beattie and M. J. McNamara (eds.), *The Aramaic Bible: Targums in their Historical Context* (JSOT. SS 166. Sheffield, 1994), pp. 118–41. Cf. further, e.g., T. Muraoka, 'A Study in Palestinian Jewish Aramaic', *Sefarad* 45, 1985, 3–21.

¹²⁴ P. M. Casey, 'Culture and Historicity: The Plucking of the Grain (Mark 2. 23–28)', *NTS* 34, 1988, 1–23.

¹²⁵ J. Frishman, 'The Ways and Means of the Divine Economy: An Edition, Translation and Study of Six Biblical Homilies by Narsai', Ph. D. thesis, Rijksuniversiteit, Leiden (1992).

¹²⁶ E. Beck (ed.), *Des Heiligen Ephraem des Syrers Sermones IV* (CSCO 334–5, SS 148–9. Louvain, 1973).

works which have been produced in proper critical editions for the first time include the Samaritan Targum.¹²⁷

A number of texts already published in critical editions have been produced in better ones. These include Porten and Yardeni's edition of Aramaic documents from Egypt.¹²⁸ The Sinaitic and Curetonian versions of the Gospels have been republished in a synoptic edition with the Peshitta, with a much-needed critical edition of the Harklean version.¹²⁹ A large number of texts in Syriac were made available, either for the first time or in better editions than previously, in two important series already noted from previous years: *Patrologia Orientalis* and *Corpus Scriptorum Christianorum Orientalium: Series Syriaca*.

A number of texts were also made available together with tools of study. The work of W. Strothmann was especially notable, in publishing concordances together with texts. This made it much easier to see, for example, that **בר (א)נשא** does not occur in long stretches of Syriac, and then does occur in a very mundane and straightforward way. For instance, in Jacob of Serug's *Three Poems on the Apostle Thomas in India*, **בר(א)נשא** does not occur until line 671, and only four times in over 2,500 lines: this may be compared with **בנינשא** 11 times, **אנשא** 27, **גברא/ה** 26, and with common words such as **אזל** 110, **אמר** 134, **חזא** 166.¹³⁰ This highlights the unsatisfactory standard of judgement unthinkingly employed by scholars who suppose that it is 'rare' in the literature of our period.

A significant number of tools of study were also published separately. These included a concordance to the Babylonian Talmud and to Targum Ps.-Jonathan. A start was also made to a concordance to the Palestinian Talmud and to a grammar of Christian Palestinian Aramaic. Fitzmyer and Kaufman produced a comprehensive bibliography to the older Aramaic. Macuch produced the first modern critical grammar of Samaritan Aramaic,

¹²⁷ A. Tal (ed.), *התרגום השומרוני לתורה The Samaritan Targum of the Pentateuch* (3 vols., Tel-Aviv, 1980–3).

¹²⁸ B. Porten and A. Yardeni, *Textbook of Aramaic Documents from Ancient Egypt, Newly Copied, Edited and Translated into Hebrew and English* (4 vols. Jerusalem, 1986–).

¹²⁹ G. A. Kiraz (ed.), *Comparative Edition of the Syriac Gospels, Aligning the Sinaiticus, Curetonianus, Peshittā and Harklean Versions* (4 vols. NTTs XXI. Leiden, 1996).

¹³⁰ W. Strothmann (ed.), *Jakob von Sarug: Drei Gedichte über den Apostel Thomas in Indien* (Göttinger Orientforschungen, 1 Reihe: Syriaca, vol. 12. Wiesbaden, 1976).

and Sokoloff edited a new dictionary of Jewish Aramaic in the periods of the Talmuds and Midrashim.¹³¹

The majority of these text editions and tools of study are unproblematic. They form straightforward contributions to knowledge. Taken together, they have enabled us to see more clearly the nature and development of the Aramaic language over a period of many centuries. Consequently, they have made the task of reconstructing sayings of Jesus, and interpreting these reconstructions, more possible.

Two problems were not satisfactorily resolved. One was the nature of Galilean Aramaic. In a significant series of studies, Kutscher pointed out various difficulties in reconstructing Galilean Aramaic at all, especially those provided by corrupt non-Galilean texts.¹³² Reinforcing the first of these points with a devastating review of the state of relevant text editions, Sokoloff pointed out that even what is Galilean at a late date is not necessarily *specific* to Galilee.¹³³ The real consequence of these observations is that texts conventionally regarded as witnesses to Galilean Aramaic do not provide us with the sort of Aramaic spoken by Jesus in first-century Galilee, let alone with what was written in Mark's sources, which may have been in Judaeian Aramaic for all we know. A related problem is that of oral as opposed to written Aramaic. The secondary literature is full of scattered comments which propose to privilege some documents, especially Targums, but nothing solid has ever been demonstrated.¹³⁴ It therefore remains doubtful whether there were any significant differences between oral and

¹³¹ Ch. J. and B. Kasowsky (eds.), *Thesaurus Talmudis Concordantiae Verborum Quae in Talmude Babylonico Reperiuntur* (42 vols., Jerusalem, 1954–89); E. G. Clarke, with W. E. Aufrecht, J. C. Hurd and F. Spitzer (eds.), *Targum Pseudo-Jonathan of the Pentateuch: Text and Concordance* (Hoboken, 1984); M. Kosovsky (ed.), *Concordance to the Talmud Yerushalmi* (Jerusalem, 1979–); Ch. Müller-Kessler, *Grammatik des Christlich-Palästinisch-Aramäischen*, part I, *Schriftlehre, Lautlehre, Formenlehre* (Hildesheim, 1991); R. Macuch, *Grammatik des Samaritanischen Aramäisch* (Berlin, 1982); J. A. Fitzmyer and S. A. Kaufman, *An Aramaic Bibliography. Part I: Old, Official, and Biblical Aramaic* (Baltimore/London, 1992); M. Sokoloff, *A Dictionary of Jewish Palestinian Aramaic of the Byzantine Period* (Ramat-Gan, 1990).

¹³² E. Y. Kutscher, *Studies in Galilean Aramaic* (Hebrew, Jerusalem, 1952. ET Jerusalem, 1976).

¹³³ M. Sokoloff, 'The Current State of Research on Galilean Aramaic', *JNES* 37, 1978, 161–7.

¹³⁴ For a recent survey, see L. T. Stuckenbruck, 'An Approach to the New Testament through Aramaic Sources: The Recent Methodological Debate', *JSP* 8, 1994 (*sic!*), 3–29, esp. 17–28.

written Aramaic during our period, beyond those which are so inherent in the ways people communicate that they are found in written texts, including the Gospels and, for example, the sayings of rabbis in the Palestinian Talmud.

Three scholarly books¹³⁵ on the Aramaic substratum of the teaching of Jesus have been published in the last fifty years, including the third edition of M. Black, *An Aramaic Approach to the Gospels and Acts* (1967).¹³⁶ This contained some significant changes from the 1946 edition. It made use of the work of Wilcox on the Semitisms of Acts, which do not fall for discussion here.¹³⁷ Black also gave clear recognition to the importance of the Qumran material.¹³⁸ Unfortunately, however, very little of it was available for him to study, so there was no point in trying to reorganise the whole book around it. This edition is accordingly an updated 1946 work, not a new book. The use of *Tg. Neofiti* was equally problematic. It was not published, so the portions which Black had seen, together with the small quantity of material from the Dead Sea, did not give him reason to reject the early dating fed to him. This was particularly the case as he already believed in too early a date for other Palestinian Pentateuch Targums. Thus he followed Kahle in supposing that the non-Mishnaic interpretation of Exod. 22.5–6 must date from before the time when the oral Law codified in Mishnah had any validity, and hence that a written Targum must have existed in very ancient times.¹³⁹ The assumption that what is non-Mishnaic must be that old is no longer accepted, and in any case what is dated early is an interpretative tradition of a passage, not a whole written Targum.

This edition had additional appendices. Appendix C, on the unpublished work of A. J. Wensinck, has some good examples of the valid use of Aramaic from long after the time of Jesus to illuminate Gospel expressions. For example, Wensinck noted two Targumic occurrences of **יְלִיד אֲתַתָּא** in the debate between Moses

¹³⁵ I do not discuss G. R. Selby, *Jesus, Aramaic and Greek* (Doncaster, 1990), which is too ignorant to be taken seriously; or B. Fletcher, *The Aramaic Sayings of Jesus* (London, 1967), an overtly amateur work.

¹³⁶ M. Black, *An Aramaic Approach to the Gospels and Acts* (Oxford, ³1967). I do not treat separately the second edition of 1954, which had relatively few alterations.

¹³⁷ M. Wilcox, *The Semitisms of Acts* (Oxford, 1965).

¹³⁸ Black, *Aramaic Approach* (³1967), pp. 39–41.

¹³⁹ *Ibid.*, pp. 38–9. See further M. Black, 'Aramaic Studies and the Language of Jesus', in M. Black and G. Fohrer (eds.), *In Memoriam Paul Kahle* (BZAW 103. Berlin, 1968), pp. 17–28, rep. in S. E. Porter (ed.), *The Language of the New Testament: Classic Essays* (JSNT. SS 60. Sheffield, 1991), pp. 112–25.

and the Red Sea, at Exod. 14.29. This provides a genuine parallel to the Q expression $\gamma\epsilon\nu\nu\eta\tau\omicron\iota\ \gamma\upsilon\nu\nu\alpha\iota\kappa\omega\delta\upsilon\nu$ at Matt. 11.11//Luke 7.28.¹⁴⁰ This is not a natural Greek expression, and the parallel is so close that, taking account also of the Hebrew יְלִיד אִשָּׁה (Job 14.1; 15.14; 25.4; Sir. 10.18; 1QS XI.21; 1QH XIII.7; XVIII.12–13), we must infer the use of this expression by Jesus, long before an example is extant in our meagre Aramaic texts.

In appendix E, Black published a seminal paper by G. Vermes on the use of the term בֵּר (א)נִשְׂא(א) in Jewish Aramaic.¹⁴¹ After a brief critical review of previous work, Vermes laid out the basic uses of בֵּר (א)נִשְׂא(א) as a normal term for ‘man’. He then proceeded to the most important part of the paper, its use as a circumlocution for ‘I’, which enabled him to produce a solution to the son of man problem. It is the evidence collected under this heading which made this paper a seminal one, for several scholars who did not altogether agree with Vermes’ interpretation were none the less impressed by the evidence which he brought forward. In 1976, I proposed that the idiom was the application of a general statement by a speaker to himself, a theory which I have developed and refined in subsequent publications.¹⁴² Vermes’ mistake stemmed partly from the fact that Pragmatics had still not got off the ground. For example, he commented on GenR VII.2, where Jacob of Nibburaya is threatened with a flogging for his incorrect halakhic judgement that fish should be ritually slaughtered, and asks:

א"ל בר אינש דאמר מילא מן אורייתא לקי.

Vermes translates, ‘Should בֵּר נִשְׂא be scourged who proclaims the word of Scripture?’ He comments: ‘Theoretically, of course, *bar nāsh* may be rendered here as “one”, but the context hardly suggests that at this particular juncture Jacob intends to voice a general principle.’¹⁴³ This presupposes that we have to choose between a general statement and a reference to the speaker, but we should not do this. On the contrary, it is precisely because the statement remains a general one that it is so well adapted to

¹⁴⁰ Black, *Aramaic Approach* (³1967), p. 298.

¹⁴¹ G. Vermes, ‘The Use of $\text{בֵּר נִשְׂא/בֵּר נִשְׂא}$ in Jewish Aramaic’, appendix E in Black, *Aramaic Approach* (³1967), pp. 310–28; reprinted in G. Vermes, *Post-Biblical Jewish Studies* (Leiden, 1975), pp. 147–65.

¹⁴² P. M. Casey, ‘The Son of Man Problem’, *ZNW* 67, 1976, 147–65; see pp. 111–21 below.

¹⁴³ Vermes, ‘Use of בֵּר נִשְׂא ’, p. 321.

functioning in the circumstances which Vermes correctly outlined; 'In most instances the sentence contains an allusion to humiliation, danger, or death, but there are also examples where reference to the self in the third person is dictated by humility or modesty.'¹⁴⁴ In such circumstances, people use general statements to influence others. Jacob of Nibburaya was trying to avoid being flogged precisely by hoping that the general principle that a person who expounded the Torah should not be flogged would be accepted and applied to him.

The next book on this subject was F. Zimmermann, *The Aramaic Origin of the Four Gospels* (1979). Zimmermann presented himself as carrying forward the work of Torrey, and his work is full of methodological errors. The majority of his examples are changes in single words, supposedly mistranslations of an Aramaic substratum. For the Aramaic behind the Gospels, he selects what he calls 'proto-Syriac', but for his examples he uses ordinary Syriac, and later Jewish Aramaic too. This gives him a larger vocabulary than any Aramaic speaker ever had, with which to play tricks. He omits the Dead Sea scrolls and all earlier Aramaic sources, which is methodologically unsound, because the words in the scrolls were in existence in Aramaic within a relatively short time of the ministry of Jesus and the writing of the Gospels. At no point does he justify his assumptions about the content of 'proto-Syriac'.

With so much Aramaic to play with, Zimmermann does get one or two examples almost right. He notes the peculiarity of Mark 3.3, ἔγειρε εἰς τὸ μέσον, and sees this as a misinterpretation of קום.¹⁴⁵ He does not, however, explain what the translator should have put. I shall suggest that קום בגוא has been translated literally by a bilingual suffering from interference.¹⁴⁶ Zimmermann correctly notes the peculiarity of ἀπὸ μιᾶς at Luke 14.18, and that the Syriac *mehada*, 'at once', explains it. He claims, however, that it is found only in Syriac and that it is evidence that 'The Aramaic written Gospels are a product of the Diaspora of Syria.'¹⁴⁷ He does not tell us where in Wellhausen he found this, nor does he answer Black's point: 'min h^adha, mēhda, appears in Palestinian Syriac too

¹⁴⁴ *Ibid.*, p. 327.

¹⁴⁵ Zimmermann, *The Aramaic Origin of the Four Gospels* (New York, 1979) p. 83.

¹⁴⁶ See pp. 180–1 below.

¹⁴⁷ Zimmermann, *Aramaic Origin*, p. 20.

frequently to be dismissed as a borrowing from Syriac'.¹⁴⁸ Zimmermann's argument in any case falters on the meagre quantity of extant Aramaic. There is virtually no Galilean Aramaic of the right period for this or any other expression to be absent from, nor is there sufficient Aramaic of any dialect in the right period. Luke's ἀπὸ μιᾶς is so un-Greek, and corresponds so precisely to מן חדא, that we must accept that it is a bilingual's mistake, but we may not deduce anything as precise as Zimmermann does.

The majority of Zimmermann's examples are not to be accepted. For example, at Matt. 18.24 Zimmermann thinks that the servant could not have owed his master 10,000 talents, the equivalent of 10 million dollars. He therefore suggests that 'The translator misvocalised the form רבו, det. רבותא . . . rendering "10,000" instead of רבותא rabbutha . . . meaning in this context "large amount, considerable sum."' The passage now carries the meaning that the servant owed the master *much money*.¹⁴⁹ Here, as so often, Zimmermann has rewritten a text which he does not like, at the hand of an Aramaic excuse. Matthew's 10,000 talents makes perfect sense as a deliberately ridiculous sum, and hyperbole was part of Jesus' teaching technique (cf., for example, Mark 10.25; Matt. 23.24). Matthew's text is therefore perfectly in order.

With methods like this, many texts can be rewritten, not least from the fourth Gospel. Zimmermann does not like Jesus' statement at John 8.28, "Ὅταν ὑψώσητε τὸν υἱὸν τοῦ ἀνθρώπου, τότε γνώσεσθε ὅτι ἐγὼ εἶμι. He gives as his ground for this that the expression 'lifting up' with reference to the crucifixion 'is incongruous', that 'lifting up' in this sense was done by the Romans, and that it is difficult to have Jesus forecasting his own crucifixion.¹⁵⁰ None of this is satisfactory. The fourth Gospel has Jesus predict his crucifixion and be in charge of it when it happens, it holds the Jews responsible, and considers Jesus' identity revealed in it. Zimmermann may not like this, but that is no excuse for rewriting the text. Zimmermann offers כד מתרים לכון בר נשא . . . אנה אתי: this was supposed to mean 'When I will no longer be with you, you will know who I was/am.' The first part of this is entirely spurious. Secondly, Zimmermann does not discuss בר נשא, which cannot

¹⁴⁸ Black, *Aramaic Approach* (31967), p. 113, properly citing Wellhausen, *Einleitung* (21911), p. 26.

¹⁴⁹ Zimmermann, *Aramaic Origin*, p. 34.

¹⁵⁰ *Ibid.*, p. 146.

function as a simple substitute for the first person like this. Nor does Zimmermann explain the behaviour of the translator. Moreover, his אֲנִי אֵלֵּא merely shows that he does not have any sympathy for Johannine 'I am' statements either. His discussion does nothing to show that his supposed original ever existed before he made it up.

We have seen Torrey and Burney use such methods to write the virgin birth into the Johannine prologue.¹⁵¹ There is, however, nothing particularly orthodox or Christian about this arbitrariness, and Zimmermann uses his method to remove the λόγος!¹⁵² His criticism of this, that it is not used in the rest of the Gospel and appealed to later Christian philosophers, is not sufficient foundation for supposing that it is not really there. The fourth Gospel uses λόγος until the incarnation, and other terms thereafter, beginning with Ἰησοῦ Χριστοῦ (John 1.17), which is not difficult to understand. Zimmermann supposes that the original was אֲמֵרָא, which was intended to mean 'lamb'. He notes correctly the importance of the Lamb of God in John 1, and at the crucifixion, but that does nothing to justify his creative rewriting of the text, nor does he provide a reasonable account of Johannine theology about a pre-existent Lamb.

We must conclude that Zimmermann's methods are not satisfactory. Like Torrey and Burney, he would have taken scholarship backwards, if enough scholars had followed him.

The remaining monograph on this area as a whole is that of G. Schwarz, *'Und Jesus sprach'*.¹⁵³ This is a very learned book. Schwarz provides a list of the Aramaic words in the Gospels, with explanations of their use. He presents Aramaic reconstructions of numerous passages. These are accompanied by detailed evidence of the attestation of words in primary source material. Schwarz's reconstructions are also ingenious. For example, Schwarz offers a basically reasonable discussion of the difficult passage Matt. 8.22// Luke 9.60. Using an earlier suggestion of Perles, he explores the possibility that the most difficult part of this verse might be reconstructed שְׂבוּק מִתִּיא לְמִקְבְּרֵי מִתִּיא. This originally meant 'Leave the dead to the gravediggers', which is a reasonable thing

¹⁵¹ See pp. 22, 24 above.

¹⁵² Zimmermann, *Aramaic Origin*, pp. 167–70.

¹⁵³ G. Schwarz, *'Und Jesus sprach': Untersuchungen zur aramäischen Urgestalt der Worte Jesu* (BWANT 118 = VI, 18. Stuttgart, 1985, ²1987).

for Jesus to have said. It was misread by the translator, a misreading no more far gone than many in the LXX.¹⁵⁴

It would therefore be good if we could hail this as an important contribution to knowledge, but its weaknesses of method are too great for us to do so. One major problem is the choice of Aramaic source material. This is frequently late in date. Nor is it always particularly Galilean, the justification occasionally offered for preferring one form to another.¹⁵⁵ For example, in discussing Matt. 6.2–3, Schwarz cites 2 Chr. 5.12 **הצצר** **מהצצרין**; what he calls Tg Jer, and should call Tg. *Ps-Jonathan*, at Lev. 19.13 for **סוטרא**; and Tg. *Neof.* Exod. 10.2, with Tg. *Onq.* Gen. 44.1, for **שוי**.¹⁵⁶ All these sources date from long after the time of Jesus, none of them is particularly Galilean, and it is a further disadvantage of method that they are all translation Aramaic too. **סוטרא** is particularly unlikely for these reasons, for it is not attested in earlier Aramaic at all. **אגר** is attested in earlier and later Aramaic, and we now have it with the right meaning at 4QTobit 12.1. We should surely infer that **אגר** was the word which Jesus used, where Matthew has $\mu\tau\theta\acute{o}\nu$. **שוי**, though abundantly attested, is the wrong word because it has the wrong meaning! Its semantic area includes that of English words such as ‘put’, ‘place’, and it is used to render the Hebrew **שים** at Tg. *Neof.* Exod. 10.2 because of its semantic overlap with **שים**. Hence Onqelos has **שוייתי**, whereas the Peshitta alters to **עבדת**. This illustrates the perils of using translation Aramaic: Tg. *Neofiti* and Tg. *Onqelos* have suffered interference from **שים**, and consequently cannot provide evidence that **שוי** normally means ‘do’. At Gen. 44.1, **שוי** is used by Tg. *Onqelos* to render **שים** with the meaning ‘put’. Hence Tg. *Neofiti* and the Samaritan Targum have it too, and the Peshitta retains **שים**, which is common in Syriac as well as in Hebrew. Schwarz more or less notices this with the second example, and suggests an original meaning *einlegen*, ‘put in’, the use of $\pi\omicron\iota\epsilon\acute{\iota}$ (Matt. 6.3) being a mistranslation. We shall see that this sort of conjectural alteration is Schwarz’s second major fault. In this case, we must rather treat the translator’s use of $\pi\omicron\iota\epsilon\acute{\iota}$ as the most straightforward possible evidence that Jesus said **עבד**, a word which is widely attested in all

¹⁵⁴ *Ibid.*, pp. 91–7, using R. Perles, ‘Zwei Übersetzungsfehler im Text der Evangelien, I. Mt 8, 22 (= Lk 9, 60)’, *ZNW* 19, 1919–20, 96.

¹⁵⁵ Schwarz, ‘*Und Jesus sprach*’, e.g., p. 50, preferring the Galilean **מוספה** to the Babylonian **אוסופי**.

¹⁵⁶ *Ibid.*, p. 202.

periods of Aramaic and properly corresponds to the semantic area of ποιεῖ.

Sometimes Schwarz's suggestions can be improved by means of additional references to earlier primary sources, which indicates how his methods are a matter of deliberate choice. For example, he suggests מסגרא for φυλακήν at Matt. 5.25//Luke 12.58.¹⁵⁷ As evidence for this, however, he quotes only one passage of *Tg. Psalms* (142.8), which is sufficient to show only that this word was known to some people who spoke Aramaic more than half a century later than the time of Jesus. We might at once prefer בית עגן from 1 En. 22.4. We must, however, consider also early Aramaic evidence of the use of the root סגר in the sense of 'confine', 'restrain'. It is used at Dan. 6.23 of God's angel who 'shut', 'closed', the mouths of the lions, so it is right to take account of the many examples of סגר used in biblical Hebrew with a similar semantic area, and with it the use of the Hebrew מסגור of a dungeon (Isa. 24.22, cf. Isa. 42.7; Ps. 142.8). Its existence in Aramaic is confirmed by 1QapGen XXII.17, and Dupont-Sommer is probably right to restore the actual word [מ]סגרא on an ostrakon from Elephantine.¹⁵⁸ Late examples of סגר in both Jewish Aramaic and in Syriac may then reasonably be used to confirm that the word became Aramaic by the time of Jesus, that it was not just a rare loanword in the cited texts. We should infer that מסגרא was used for 'prison' in Aramaic at the time of Jesus, and follow Schwarz in this particular reconstruction.

Schwarz's second major fault of method is the arbitrariness of his alterations to the text, both in ferreting out supposed mistakes and variants in translation, and in deliberate emendation. For example, in his discussion of Matt. 5.20,¹⁵⁹ Schwarz first seeks to establish that צמקתא may signify 'almsgiving', which is not exactly wrong, but which is not sufficient to justify Schwarz's view that the translator should have put ἐλεημοσύνη because צדקתכון really meant 'euer Almosen'. He then proceeds to the centrally arbitrary notion that τῶν γραμματέων καὶ Φαρισαίων is a gloss. He offers two points in justification of this. Firstly, he would follow Schlatter in expecting τῆς before τῶν. This is not a proper criterion, because a

¹⁵⁷ *Ibid.*, p. 190.

¹⁵⁸ A. Dupont-Sommer, 'Un ostracon araméen inédit d'Eléphantine (Collection Clermont-Ganneau n° 44)', in D. Winton Thomas and W. D. McHardy (eds.), *Hebrew and Semitic Studies Presented to G. R. Driver* (Oxford, 1963), pp. 53–8.

¹⁵⁹ Schwarz, 'Und Jesus sprach', pp. 79–85.

glossator is not more likely to produce our text than a translator, a possibility which Schwarz does not explore. Nor does he discuss the proposed parallel at Maxim. Tyr. 15.8d. We might suggest the following reconstruction of the whole phrase:

הן לא ישנא צדקתכון יתיר מן דספרין ודפרושין

The translator's *πλεῖον* for יתיר is as straightforward as possible. He will have been used to the genitive of comparison as an equivalent of מן in expressions like this, and he will have been accustomed to putting nouns in the genitive as an equivalent of ד. This is how he came to put simply *γραμματέων* and *Φαρισαίων* in the genitive, adding one τῶν because he felt that the expression needed to be made more definite. This is mildly conjectural in detail but entirely plausible, whereas Schwarz is certainly wrong about a main point.

Schwarz's second argument is to repeat Grundmann's dissatisfaction with the text: Grundmann declared that in the time of Jesus there were different groups in Judaism, whereas this expression reflects the situation after the 'Jewish' war of 66–70 CE. This is not satisfactory. 'Scribes and Pharisees' is not an expression characteristic of sources after 70 CE, whereas Mark provides clear evidence of opposition to Jesus from Pharisees (Mark 2.24; 3.6) and from 'scribes who came down from Jerusalem' (3.22). This opposition was early and important enough to give rise to polemical sayings of this kind.

By this means Schwarz obtains a supposed original:

אמין אמד אנא לכוּן
דאין לא ספיק צדקתכון סגי
לא תיעלון למלכותא דאלהא.

Amen, ich sage euch:

Wenn euer Almosen nicht sehr reichlich ist,

dürft ihr nicht eingehen in die Königsherrschaft Gottes.

One might render this in English: 'Amen I say to you: if your almsgiving is not munificent, you will not enter the kingdom of God.' This is a saying of Schwarz, which has no Sitz im Leben in the teaching of Jesus. Schwarz repeatedly alters texts in this way, and this repeated mistake is sufficient to ensure that most of his results are not valid.

Schwarz also makes too much of structural neatness, alliteration

and what he regards as other signs of Aramaic poetry. For example, at Matt. 5.3, Schwarz replaces αὐτῶν ἐστὶν ἡ βασιλεία τῶν οὐρανῶν with αὐτοὶ πλουτισθήσονται. Among his reasons is that this is precisely antithetic to οἱ πτωχοὶ.¹⁶⁰ This is not a sufficient reason for making this drastic change to Matthew's text. Again, for ὁμοία ἐστὶν at Matt. 11.16 (//Luke 7.31), Schwarz has מתיילא. While a stronger case can be made for this than the single passage quoted from *Tg. Psalms* (101.8), מלא is at least as likely. Schwarz's case is surely not helped by observing that he gets twenty-five examples of ל in five lines.¹⁶¹ It is not just that the use of genuine synonyms, and the employment of a wider range of Aramaic than was ever available to any one speaker, permits one to increase the number of לs in a piece. It is that large amounts of the same letter do not necessarily give us a more original text. The use of criteria like this produces neat patterns, not original sources.

With methods like this, we can once more find examples in the Gospel attributed to John. For example, Schwarz does not like ἀληθῶς with Ἰσραηλίτης at John 1.47. He would prefer the author to have written ἀληθινὸς Ἰσραηλίτης. This does not justify his description of ἀληθῶς as *attributiv*, or his consequent classification of it as different from 4.42; 6.14; 7.40; 8.31. Still less does it mean that there was once an original Aramaic which read ישר.¹⁶²

These faults of method permeate Schwarz's work. We must therefore conclude that most of his results are wrong. Similar comments apply to his 1986 monograph on the son of man problem.¹⁶³ Schwarz follows Meyer and Vermes in arguing that (א)נש(א) בר simply = *ich*,¹⁶⁴ which is unfortunate because it biases the whole discussion. He attaches particular importance to a Geniza fragment of a Targum to Gen. 4.14, comparing it with the MT and *Tg. Neofiti*. The Targumic passages are as follows:

1. *Tg. Neof.* Gen. 4.14

הא טרדת יתי יומא דין מעילוי אפי דארעא ומן קדמין]ך
לית אפשר לי למטמרה.

Look! You have banished me this day from upon the face

¹⁶⁰ *Ibid.*, pp. 159–64.

¹⁶¹ *Ibid.*, pp. 260–6.

¹⁶² *Ibid.*, pp. 112–14.

¹⁶³ G. Schwarz, *Jesus 'der Menschensohn': Aramaistische Untersuchungen zu den synoptischen Menschensohnworten Jesu* (BWANT 119 = VI, 19. Stuttgart, 1986).

¹⁶⁴ *Ibid.*, pp. 73–7.

of the earth, and from before you it is not possible for me to hide.

2. Leningrad, Saltykov-Schedrin, MS Antonin Ebr III B 739v, at Gen. 4.14 (Klein, p. 9).

הא טרדת יתי יומה הדן מן עלוי אפי ארעא ומן קדמיך
אדני לית אפשר לברנש למטמרה.

Look! You have banished me this day from upon the face of the earth, and from before you, Lord, it is not possible for a son of man to hide.

Here the meaning of the Hebrew text was not acceptable to some Aramaic translators, for they believed that Cain could not be hidden from God because no one can be hidden from God.¹⁶⁵ *Tg. Neofiti* simply has Cain say that he cannot hide from God, but the Geniza fragment has generalised. It gives us a general statement which refers particularly to Cain. The main point is that the Geniza fragment is *different* from the MT and *Tg. Neofiti*, and it should not be interpreted as if it were the same. ברנש was so well established as a general term for ‘man’ that the only way to remove the general level of meaning would have been to say what *Tg. Neofiti* says instead. Both versions are perfectly straightforward. We must infer that the Geniza piece has made Cain use a general statement which has particular reference to himself. It follows that Schwarz’s central point is wrong. He must miss the general level of meaning in those sayings which have one, and he cannot use its presence or absence as a criterion of authenticity.

In other respects, Schwarz’s book suffers from the same defects as his more general monograph. Once again, he emends the text on the basis of unsatisfactory criteria. For example, he shortens Mark 10.45, on the ground that each *Stichos* is differently constructed, so that the verse has a rhythm unknown in Semitic poetry.¹⁶⁶ But Mark 10.45 is written in Greek prose, we have no reason to think that its source was in Semitic verse rather than Aramaic prose, and Schwarz has never demonstrated the fruitfulness of his analysis of Semitic verse. By these means¹⁶⁷ he deletes οὐκ . . . διακονηθη̅ναι ἀλλὰ διακονῆσαι, which removes the connection of this saying

¹⁶⁵ On translations which contradict the text, see M. L. Klein, ‘Converse Translation: A Targumic Technique’, *Bib* 57, 1976, 515–37.

¹⁶⁶ Schwarz, *Menschensohn*, p. 89.

¹⁶⁷ For the complete discussion, see Schwarz, *Menschensohn*, pp. 89–94, 171–6.

with the immediately preceding context, and ἀντὶ πολλῶν, which reduces the clarity of the saying. This leads to the following 'reconstruction':

אתא בר נשא למיתן נפשיה פורקן

Ich kam, um mich selbst als Lösegeld zu geben.

This interpretation of **בר נשא** removes the connection of the saying with Jesus' answer to Jacob and John earlier in the passage. This is not reconstruction of an original saying in its cultural context: it is the destructive removal of it from reality.

As in his more general monograph, Schwarz does not give early attestation of Aramaic words. For example, he suggests **למשבוק** behind ἀφιέναι at Mark 2.10, citing only *Tg. Onqelos* Num. 14.19; Deut. 29.19.¹⁶⁸ He should have noted that there are abundant early occurrences of this word, including 4QPrNab and 11QtgJob XXXVIII.2, in both of which it is used with reference to sins. He should also have discussed the semantic area of **שבק**, which overlaps with ἀφίημι and is seriously different from 'forgive' and *vergeben*. In some cases, Schwarz uses words which are attested only in Aramaic too late for the time of Jesus, though this is not as extensive a problem as in some of his other work. There are also places where Schwarz has the wrong word. For example, he puts the traditional **קניין** behind κατασκηνώσεις at Matt. 8.20//Luke 9.58, and translates 'Nester'.¹⁶⁹ As we have seen, however, the Aramaic **קניין** really does mean 'nests', so much so that any reasonable translator would have translated it as νοσσιάζς. Schwarz's mistake facilitates the interpretation of the saying of Jesus alone, for nature does not provide birds with nests, so the general level of meaning has been lost.¹⁷⁰

We must therefore conclude that Schwarz's methods are not satisfactory. Despite his learning and ingenuity, therefore, we cannot accept many of his results.¹⁷¹

A few other scholars used Aramaic words to assist with their interpretation of Gospel passages when they were writing mono-

¹⁶⁸ *Ibid.*, p. 111.

¹⁶⁹ *Ibid.*, pp. 191–2.

¹⁷⁰ See p. 21 above and pp. 69–71 below; Casey, 'Jackals'.

¹⁷¹ I therefore do not discuss his other work, which includes many learned articles, and G. Schwarz, *Jesus und Judas: Aramaistische Untersuchungen zur Jesus-Judas-Überlieferung der Evangelien und der Apostelgeschichte* (BWANT 123. Stuttgart, 1988).

graphs on aspects of the life and teaching of Jesus. Jeremias is perhaps the most famous, and some of the work which he did is new and right. For example, it was often suggested that the Greek ἄρτος could not designate unleavened bread.¹⁷² Since Mark has ἄρτος at the Last Supper, it seemed to follow that the Last Supper was not a Passover meal. In a fine scholarly discussion, securely based in primary source material retailed in its original languages, Jeremias showed that both ἄρτος and the Aramaic and Hebrew **חלה** were normal terms for referring to the unleavened bread at passover.¹⁷³

Jeremias also made use of the work on Aramaic done by his predecessors. For example, he improved on Burney's reconstruction of the Lord's prayer.¹⁷⁴ Though regrettably printed in English letters, this formed a sound basis for exegesis:

'Abbā

yithqaddāsh sh^emākh / tethé malkhuthákh

lahmán d^elimhár / habh lán yoma dhén

ush^ebhoq lán hōbhain / k^edhish^ebhāqnan l^ehayyabhain

w^ela tha'elinnan l^enisyón.

Among the advantages of seeing this in Aramaic are the possibility of expounding **למחר**, an improvement on Burney's *d^eyōmā*, whereas ἐπιούσιον, when treated only in Greek, was effectively an insoluble problem; and the Aramaising Greek for 'sins' and 'sinners', which opens a window onto the Aramaic tradition, and illustrates the fact that Luke is more inclined to remove such evidence than Matthew. It is also a great advantage that the whole passage can be seen.

At the same time, Jeremias suffered from the faults of his generation. He did not have the Dead Sea scrolls to work with, so he used Aramaic of all periods, including translations *into* Aramaic, and he did not offer complete discussions of the translation process. For example, in discussing Luke 14.8–10, he alleges that Luke's γάμους and Matthew's δειπνήσαι (Matt. 20.28 D it sy) both go back to an original Aramaic *mištutha*.¹⁷⁵ He does not, however,

¹⁷² E.g. by J. Wellhausen, "Ἄρτον ἔκλασεν, Mc 14, 22", *ZNW* 7, 1906, 182.

¹⁷³ J. Jeremias, *The Eucharistic Words of Jesus* (2nd ET London, 1966), pp. 62–6.

¹⁷⁴ J. Jeremias, *The Prayers of Jesus* (ET London, 1967), p. 94, with extensive discussion; *New Testament Theology*, vol. I, p. 196.

¹⁷⁵ J. Jeremias, *The Parables of Jesus* (ET London, ²1963), pp. 25–6.

give any attestation for this word, nor does he discuss *how* two independent translators could have arrived at such different forms, nor does he seek to explain the place of the passage in the manuscript tradition of Matthew. He does not offer a complete reconstruction, and comments only on words which fit his model of two translations from Aramaic. He suggests that the Syriac and Christian-Palestinian versions be used as 'an additional means of controlling the process of retranslation', which illustrates beautifully his failure to distinguish between reconstructing an original and translating the Gospels *into* Aramaic. Consequently, he could not handle passages where a translator had made changes to a more Greek idiom, leaving the Semitic underlay not apparent to someone translating literally into Aramaic. So at Mark 14.22 he regards the genitive absolute ἐσθιόντων αὐτῶν as a 'graecism' which is 'unknown in Semitic', and he regards that as supporting the common view that it is a redactional link.¹⁷⁶

These serious problems should not be allowed to conceal the value of Jeremias's work. He showed, albeit intermittently, that normal exegetes of the Gospels can gain further insights into the life and teaching of Jesus by careful consideration of the Aramaic level of the tradition.

Another scholar who has made important contributions to this work is J. A. Fitzmyer. Perhaps his most permanent contributions have been to the Aramaic background to the New Testament. He has produced important editions of texts, including the Sefire inscriptions and the Genesis Apocryphon.¹⁷⁷ These editions are meticulously presented, and include discussions of Aramaic grammar and syntax which form genuine contributions to knowledge in their own right. Other learned articles have made a similar contribution, for example to the analysis of 11QtgJob, and the interpretation of 4Q246.¹⁷⁸ Fitzmyer is also responsible for two standard working tools, an edition of Qumran texts and a comprehensive bibliography to older Aramaic.¹⁷⁹ His article on the languages of Palestine at the time of Jesus is a model of learning,

¹⁷⁶ Jeremias, *Eucharistic Words*, p. 184.

¹⁷⁷ Fitzmyer, *Aramaic Inscriptions of Sefire; Genesis Apocryphon*.

¹⁷⁸ J. A. Fitzmyer, *Essays on the Semitic Background of the New Testament* (London, 1971, rep. Missoula, 1974); *Wandering Aramean*; '4Q246: The "Son of God" Document from Qumran', *Bib* 74, 1993, 153–74.

¹⁷⁹ J. A. Fitzmyer and D. J. Harrington, *A Manual of Palestinian Aramaic Texts* (BibOr 34. Rome, 1978); Fitzmyer and Kaufman, *Aramaic Bibliography*.

clarity and sound judgement.¹⁸⁰ He has also made significant contributions to the study of individual NT words and expressions, as, for example, κορβαν (Mark 7.11), the Aramaic קרבן now extant on an ossuary.¹⁸¹ As far as Aramaic reconstructions of sayings of Jesus are concerned, Fitzmyer has reaffirmed the earlier principle that whole sayings should be reconstructed.¹⁸²

Fitzmyer has also attempted a potentially fruitful yet hazardous classification of Aramaic into different phases.¹⁸³ His five phases are (1) Old Aramaic, from roughly 925 BCE to 700 BCE, which includes the Sefire inscriptions: (2) Official Aramaic, from roughly 700 BCE to 200 BCE, which includes the Elephantine papyri and the Aramaic of Ezra: (3) Middle Aramaic, from roughly 200 BCE to 200 CE, which includes the Dead Sea scrolls and the documents from Muraba'at: (4) Late Aramaic, from roughly 200 CE to 700 CE, which includes the Talmuds, Samaritan Aramaic and a large amount of Syriac, both biblical versions and church fathers: (5) Modern Aramaic. The advantage of this classification is that it enables us to focus clearly on the changes which took place, as we seek to decide which Aramaic can legitimately be used to reconstruct sayings of Jesus.

Equally, however, this classification can be very problematic if it is interpreted too strictly. This is most obvious with the Aramaic of Daniel. From a technical point of view, Fitzmyer's classification of it in Official Aramaic is reasonable, but its date is too late, c. 166–5 BCE. This highlights the lack of any clear moment when people passed from one phase of Aramaic to another: 200 BCE was not a watershed. The second problem arises from Fitzmyer's attempt to exclude Late Aramaic from work on the substratum of the teaching of Jesus. There is still too little Aramaic extant from the Second Temple period for this to be satisfactory. For example, at Luke

¹⁸⁰ J. A. Fitzmyer, 'The Languages of Palestine in the First Century A. D.', *CBQ* 32, 1970, 501–31, rev. *Wandering Aramean*, pp. 29–56. A more up-to-date account, 'The Languages that Jesus Spoke', was discussed by the Historical Jesus seminar at the SNTS meeting in Chicago, August 1994. See further J. A. Fitzmyer, 'Problems of the Semitic Background of the New Testament', in J. M. O'Brien and F. L. Horton (eds.), *The Yahweh/Baal Confrontation and Other Studies in Biblical Literature and Archaeology: Essays in Honour of E. W. Harrick* (New York, 1995), pp. 80–93.

¹⁸¹ J. A. Fitzmyer, 'The Aramaic *qorbān* Inscription from Jebel Ḥallet et Tûri and Mk 7:11/Matt 15:5', *JBL* 78, 1959, 60–5, rev. edn *Semitic Background*, pp. 93–100.

¹⁸² E. g. J. A. Fitzmyer, *The Gospel According to Luke* (AB 28–28A. 2 vols., New York, 1981–5), vol. II, p. 947.

¹⁸³ Fitzmyer, *Wandering Aramean*, pp. 57–84.

14.18 we have the expression ἀπὸ μιᾶς. We have seen that this is a literal translation of the idiomatic Syriac expression מן חדא, which means 'all at once'. We must infer that מן חדא was in use in the Aramaic of our period, a possibility which Fitzmyer himself takes seriously.¹⁸⁴ The most serious example of this problem was Fitzmyer's argument that examples of the idiomatic use of בר (א)נש(א) collected by Vermes should not be accepted because they do not have the prosthetic א. Subsequent work has shown that the semantic area of בר (א)נש(א) is not affected by whether it has the prosthetic א, and it is in any case entirely possible that the prosthetic א was not pronounced by Galileans.¹⁸⁵ If we do not use Late Aramaic at all, we shall also find that we cannot fulfil Fitzmyer's perfectly sound requirement that we always reconstruct whole sentences. It follows that we must take positive advantage from Fitzmyer's excellent work on the Aramaic background of the Gospels. We must not, however, adopt a literal interpretation of some of his principles, but must rather seek a more nuanced understanding of how to move forward.

Towards the end of this period, Wilcox provided useful summaries of the status quaestionis, together with learned and incisive comments of his own.¹⁸⁶ Three points of method are especially worthy of note. Faced with evidence that some apparent Semitisms have parallels in Greek papyri, Wilcox reiterated a more nuanced view of the position of Wellhausen: the mere fact that a locution is found in papyri does not show that it is not a Semitism, when it occurs in a source which we have other reasons to believe was a translation from Aramaic. Secondly, faced with Fitzmyer's view that only Aramaic from the time of Jesus and earlier should be used in reconstructions of his sayings, Wilcox accepted the importance of Aramaic of early date, but also brought forward specific examples to justify careful use of later source material. These

¹⁸⁴ Fitzmyer, *Luke*, p. 1055; see pp. 42–3 above.

¹⁸⁵ Cf. G. Vermes, *Jesus the Jew* (London, 1973), pp. 188–91; G. Vermes, 'The Present State of the "Son of Man" Debate', *JJS* 29, 1978, 123–34, at 127–30; G. Vermes, 'The "Son of Man" Debate' *JSNT* 1, 1978, 19–32, at 23–5; J. A. Fitzmyer, 'The New Testament Title "Son of Man" Philologically Considered', in *Wandering Aramaean*, pp. 143–60, at 149–53; J. A. Fitzmyer, 'Another View of the "Son of Man" Debate', *JSNT* 4, 1979, 58–68, esp. 61–4; Schwarz, *Menschensohn*, pp. 71–3, 84; P. M. Casey, 'The Use of the Term בר (א)נש(א) in the Aramaic Translations of the Hebrew Bible', *JSNT* 54, 1994, 87–118.

¹⁸⁶ M. Wilcox, 'Semitisms in the New Testament', *ANRW* II. 25. 2 (1984), pp. 978–1029; M. Wilcox, 'The Aramaic Background of the New Testament', in Beattie and McNamara, *Aramaic Bible*, pp. 362–78.

included אִפְתָּח in Vat. Ebr. 440 of Genesis 49.1, a clearly Aramaic text with the same assimilation of the ת to the פ as presupposed in ἐφφαθα of Mark 7.34; and שָׁכַח in the sense of ‘be able’ in the Targumic Tosefta of Genesis 4.7, found in Oxf. Ms. Heb. c. 74(P), as well as at 1QapGen XXI.13, a document in which it also means ‘find’ (for example 1QapGen XXI.19).¹⁸⁷ Most fundamentally, noting my article on Mark 2.23–8, he properly related Aramaisms to Jewish culture: ‘the whole approach to the Aramaic and Hebrew background of the New Testament must be linked in with as full an historical, social and midrashic perspective as possible, and that the atomistic “spot the Aramaism” endeavours of the past, whatever their merits, must give way to that new approach’.¹⁸⁸

Many people working in other fields of study have made contributions to knowledge which are important in their own right, and essential for progress in the reconstruction of sayings of Jesus. Since the late 1960s, there has been a massive explosion of knowledge in the fields of Bilingualism and Translation Studies. In 1989, Heidi Schmidt gathered together a collection of essays on the phenomenon of interference.¹⁸⁹ A correct understanding of interference is essential if we are to understand our Gospel translators, and consequently essential if we are to have any confidence in our Aramaic reconstructions. Švejcic discussed one particular problem which is especially important for understanding the production of the translation ὁ υἱὸς τοῦ ἀνθρώπου: ‘Literal Translation as a Product of Interference’.¹⁹⁰ This is only one example of the way in which scholars in other fields have contributed knowledge which is essential for us.

There has also been a massive increase in our understanding of the early translations of the Hebrew Bible. For the LXX, we now have the programmatic essays of Barr, Brock and Tov.¹⁹¹ Among

¹⁸⁷ Wilcox, ‘Semitisms’, pp. 998–9, 1011–12.

¹⁸⁸ Wilcox, ‘Aramaic Background’, pp. 376–7, noting Casey, ‘Plucking of the Grain’.

¹⁸⁹ H. Schmidt (ed.), *Interferenz in der Translation* (Übersetzungswissenschaftliche Beiträge 12. Leipzig, 1989).

¹⁹⁰ A. D. Švejcic, ‘Literal Translation as a Product of Interference’, in Schmidt, *Interferenz*, pp. 39–44.

¹⁹¹ J. Barr, *The Typology of Literalism in Ancient Bible Translation* (NAWG 11. Göttingen, 1979); S. P. Brock, ‘Aspects of Translation Technique in Antiquity’, *GRBS* 20, 1979, 69–87; S. P. Brock, ‘Towards a History of Syriac Translation Technique’, in R. Lavenant (ed.), *IIIe Symposium Syriacum* (OCA 221, 1983), pp. 1–14, reprinted in S. P. Brock, *Studies in Syriac Christianity: History, Literature and Theology* (London, 1992); E. Tov, ‘Die griechischen Bibelübersetzungen’,

other things, these essays make clear the differing degrees of literalness which may be found within the work of a single translator. There have also been a number of detailed studies of particular translators and of particular words and constructions. For example, in a detailed study of the translation of כִּי with ὅτι, Aejmelaeus showed that it is often used incorrectly by the standards of monoglot Greek speakers precisely *because* it is so often used correctly.¹⁹² This set up too close an association between the two words in the minds of translators who were suffering the double level of interference which is inevitable when translators translate texts.

There have also been detailed studies of other biblical versions. The most important for our purposes are the Peshitta and the Targums. For example, Taylor wrote a monograph on the Peshitta of Daniel.¹⁹³ This is not just a compendium of information about this translation, but a careful analysis which pays proper attention to translation technique. I contributed a detailed study of the translation of the words for 'man' in the Peshitta and in several different Targums.¹⁹⁴ This comparison of the reactions of several different translators to the same problems illuminated some false assumptions in the conventional secondary literature to the Son of man problem.

The study of these versions is especially interesting from our point of view because the same languages are being used. At the same time, we have to be very careful because these translators were not working in the same direction. Similar comments apply to the Syriac versions of the New Testament. For example, Joosten's study of Syriac versions of Matthew shows careful analysis of translation technique, going *from Greek into Syriac*.¹⁹⁵

ANRW II. 20. 1 (1987), 121–89; S. P. Brock, 'Translating the Old Testament', in D. A. Carson and H. G. M. Williamson (eds.), *It is Written: Scripture Citing Scripture, Essays in Honour of Barnabas Lindars* (Cambridge, 1988), pp. 87–98; E. Tov, 'The Septuagint', in M. J. Mulder and H. Sysling (eds.), *Mikra* (CRINT II, 1. Assen/Maastricht/Philadelphia, 1988), pp. 161–88.

¹⁹² A. Aejmelaeus, 'OTI causale in Septuagintal Greek', in N. Fernández Marcos (ed.), *La Septuaginta en la investigación contemporánea (V Congreso de la IOSCS)* (Madrid, 1985), pp. 115–32 = A. Aejmelaeus, *On the Trail of the Septuagint Translators: Collected Essays* (Kampen, 1993), pp. 17–36.

¹⁹³ R. A. Taylor, *The Peshitta of Daniel* (MPI. VII. Leiden, 1994).

¹⁹⁴ Casey, 'Use of the Term (א)נש(א) בר'.

¹⁹⁵ J. Joosten, *The Syriac Language of the Peshitta and Old Syriac Versions of Matthew: Syntactic Structure, Inner-Syriac Developments and Translation Technique* (Leiden, 1996).

The general standard of recent research into Bilingualism, Translation Studies and the ancient versions of the Bible has been very high. Most studies have made a genuine contribution to knowledge. Consequently, this work puts us in a position to make a much more informed study of the Aramaic sources of the Gospels.

For this purpose, we must of course be right about what language the sources were in! There have been periodic attempts to argue that Jesus taught in Hebrew or Greek rather than Aramaic, and these have continued even since the discovery of the Dead Sea scrolls. We must briefly examine some of them, to uncover their faults of method.

The two most significant attempts to argue that Jesus taught in Hebrew are those of Birkeland and Carmignac. Birkeland begins by discounting literary remains as evidence of a popular tongue.¹⁹⁶ This is methodologically unsound. There is no doubt that scribes wrote in Hebrew: they did not have reason to use Aramaic unless it was a popular tongue. Birkeland also ignores the evidence of inscriptions, such as the Aramaic inscriptions on the shekel trumpets in the Temple (m. Sheq 6.5). Birkeland turns to expressions such as Ἑβραϊδι διαλέκτω (for example Papias, at Eus. *HE* III.39.16).¹⁹⁷ He pours scorn on the normal view that terms such as Ἑβραϊδι in expressions like this could refer to the use of Aramaic: he insists Hebrew must be referred to. This argument has a classic fault of method, that of proceeding logically in the wrong language. In English, 'Hebrew' means 'Hebrew' and not 'Aramaic'. This is not, however, true of Greek expressions such as Ἑβραϊδι διαλέκτω. Greek speakers continued to use words such as Ἑβραῖος to refer to Jews (for example Phil. 3.5): it was therefore natural for them to use such expressions as Ἑβραϊδι διαλέκτω to refer to the native tongue of most Jews, which was Aramaic. It is therefore expressions of this kind which need examining, without an assumption derived from the wrong language.¹⁹⁸

For example, Josephus tells us that *σάββατα* means 'rest' κατὰ τὴν Ἑβραίων διαλέκτον (*AJ* I, 33). The form *σάββατα* is distinc-

¹⁹⁶ H. Birkeland, *The Language of Jesus* (Avhandlingar utgitt av Det Norske Videnskaps-Akademi i Oslo II. Hist. Filos. Klasse. 1954. No. 1. Oslo, 1954). For criticism, J. A. Emerton, 'Did Jesus Speak Hebrew?', *JThS* NS 12, 1961, 189–202.

¹⁹⁷ Birkeland, *Language of Jesus*, 13. See p. 1 above.

¹⁹⁸ The same mistake is made, e.g., by J. M. Grintz, 'Hebrew as the Spoken and Written Language in the Last Days of the Second Temple', *JBL* 79, 1960, 32–47, at 32–3, 42; P. Lapide, 'Insights from Qumran into the Languages of Jesus', *RQ* 8, 1975, 483–501, at 488–90.

tively Aramaic because of the ending, so it follows that Josephus was happy with the description of Aramaic as τὴν Ἑβραίων διαλέκτον. Birkeland takes such examples to be a confusion arising from the Aramaisation of the vocabulary of the spoken Hebrew of that time. This is another error of method, that of explaining away the evidence of our primary sources instead of explaining it. Birkeland has done so because he has taken an incorrect frame of reference from his logically abstracted use of the English word 'Hebrew'. Birkeland also notes *talita kumi* attributed to Jesus at Mark 5.41, and he agrees that this is Aramaic. He suggests that this is quoted in Aramaic *because* Jesus normally spoke Hebrew.¹⁹⁹ This is extraordinarily contorted. If Jesus normally spoke Hebrew, he would not have reason to change at this point, and the translator's means of conveying this information is equally odd. On the normal view, however, the translator has simply decided to quote his actual healing words, which is an intelligible thing to do. Birkeland also finds himself unable to explain the need for Targums, and never discusses any details of a possible Hebrew substratum of Greek Gospels.

It follows that Birkeland's methods are too weak to demonstrate anything.

The work of Carmignac was more learned and ingenious, but still unsatisfactory of method.²⁰⁰ Carmignac begins by telling us how easy he found it to translate the synoptic Gospels *into* Hebrew. This reflects his ability as a Hebraist, and the fact that these Gospels arose from a Semitic substratum. It does not, however, mean that Gospel sources were in Hebrew rather than Aramaic. Nor is it sufficient to show that the Gospels are wholly translations. Moreover, all existing attempts at translation into Hebrew have problems with some expressions. Carmignac would have to discuss and justify his proposed Hebrew source for expressions such as *πάσχα* (for example Mark 14.12), *ἐπιβαλὼν* (Mark 14.72) and *ὁ υἱὸς τοῦ ἀνθρώπου*. He would also have to explain how Jesus could use Hebrew in a culture where Aramaic was pervasive enough to require the existence of Targums.

Carmignac's proposed Hebraisms in the Gospels have three major problems. The first is the difficulty of distinguishing some of

¹⁹⁹ Birkeland, *Language of Jesus*, pp. 24–5.

²⁰⁰ J. Carmignac, *La Naissance des Evangiles synoptiques* (Paris, 1984). For criticism, P. Grelot, *L'Origine des Evangiles: Controverse avec J. Carmignac* (Paris, 1986).

them from Septuagintalism. For example, he discusses ἐγένετο ἐν τῷ . . . καὶ. Carmignac's figures for this are Mark two, Matthew six, Luke thirty-two, and he says correctly that this can be a translation of the Hebrew בַּיְהוָה.²⁰¹ Equally, however, he notes that this translation is used many times in the LXX, which opens up the possibility that Gospel examples are due to familiarity with the LXX, not to direct translation. Carmignac argues that this cannot be the case because in the New Testament this locution is confined to the synoptic Gospels. This is an unsatisfactory basis of comparison, because the New Testament is of composite authorship. It is entirely intelligible that some authors should use any given Hebraism or Septuagintalism, and that others should not. In this case, moreover, we have a locution which is obviously more suitable for narrative than for anything else, so only the four Gospels and Acts really provide a comparative base. We should infer that Mark and Matthew needed something to make them use this locution, that Luke liked it very much but reconsidered his decision when he came to write Acts, and that the authors of the fourth Gospel did not like it.

Carmignac considers Luke 9.28, and raises the crucial question of whether it comes from Luke himself or from his supposed Hebrew source. The answer is surely that it comes from Lukan editing, because Luke is so clearly editing Mark in the context. At 9.27, his wording is very close to that of Mark until he drops the words ἐληλυθυῖαν ἐν δυνάμει. This is part of the process of altering Jesus' inaccurate prophecy of the coming of the kingdom, and it is carried further at 9.28, where the use of ἐγένετο is part of Luke's means of dating the Transfiguration eight days after Jesus' prediction, to present it as a fulfilment of that prediction. This editing has such an excellent Sitz im Leben in Luke's life situation that we must attribute it to him, not to a Hebrew source. This is the second major problem with Carmignac's suggestions: they are often given as explanations of independent translations made by the synoptic evangelists, when editing of Mark by Matthew or Luke is a more probable explanation.

The third major problem with Carmignac's Hebraisms is that he or his predecessors have created the occurrence of many of them, using tricks now familiar to us from scholarly discussion. For example, he proposes that in the Lord's prayer *acquitter* = אָדֹנָי,

²⁰¹ Carmignac, *Evangelies synoptiques*, p. 35.

dettes and *débiteurs* = נִשָּׂה, which is not so in Aramaic, and *tentation* = נִסָּה.²⁰² We have, however, no reason to believe that this series of puns existed before mediaeval scholars made it up. Only Carmignac's conviction that similar words are of central importance justifies his selection of נִשָּׂא/נִסָּא rather than the common סָּלַח for 'forgive'.²⁰³ There are no problems in the way of an Aramaic reconstruction of Matt. 6.12–13a//Luke 11.4:

וּשְׁבַק לְנָא חוּבִינָא כַּאֲף אַנְחָנָא שְׁבַקְנָא חִיבִינָא,
וְאֵל תֵּאַעְלֹנָא לְנִיִּסְיוֹן.

It is not an advantage of Carmignac's theory that he increases the number of puns, because we have no reason to believe that Jesus used more puns than this.

These problems are so serious that Carmignac's hypothesis cannot be accepted. Nor should we accept two recent attempts to illuminate a Gospel from translation *into* Hebrew. When he translated Mark into Hebrew, Lindsey declared that it was easier to do this to Luke. He regarded this as a serious argument for the priority of Luke, and a group of scholars periodically repeat his comments.²⁰⁴ His arguments have never been presented with sufficient scholarly rigour. Inadequate comments include the declaration of 'the Hebraic perfection' of Luke 12.10, without offering any reconstruction of it.²⁰⁵ This verse includes the term ὁ ὕψος τοῦ ἀνθρώπου: a ridiculously brief discussion assures us that Jesus used the Aramaic בַּר אַנְשׁ when speaking Hebrew, but there is no proper discussion of Aramaic usage, either here or when it is declared a 'deity-laden' expression.²⁰⁶ Uncritical comments include the bare declaration that the Aramaic version of Ps. 22.1 at Mark 15.34 is a replacement of Jesus' saying at Luke 23.46, without any explanation of why Mark should do anything so peculiar.²⁰⁷

Howard has argued that pre-Matthean material can be recovered from the sections of translation of the Gospel of Matthew into

²⁰² *Ibid.*, p. 38.

²⁰³ Carmignac's full discussion, with a complete reconstruction, is given in J. Carmignac, *Recherches sur le 'Notre Père'* (Paris, 1969).

²⁰⁴ R. L. Lindsey, *A Hebrew Translation of the Gospel of Mark* (Jerusalem, 1969). Cf., e.g., R. L. Lindsey, *Jesus Rabbi and Lord: The Hebrew Story of Jesus Behind Our Gospels* (Oak Creek, 1990); D. Bivin, 'A New Solution to the Synoptic Problem', *Jerusalem Perspective* 4, 1991, 3–5; B. H. Young, *Jesus the Jewish Theologian* (Peabody, 1995).

²⁰⁵ Lindsey, *Hebrew Translation*, p. 37.

²⁰⁶ *Ibid.*, pp. 71–2; *Rabbi and Lord*, pp. 51–2.

²⁰⁷ Lindsey, *Hebrew Translation*, p. 63.

Hebrew found in the *Evan Bohan*, a fourteenth-century Jewish anti-Christian treatise by Shem-Tob ben-Isaac ben-Shaprut.²⁰⁸ Howard's only points of substance are that some of the translation is older than the treatise of Shem-Tob, and that his work incorporates genuinely old tradition. The work in its present form, however, has many late features and a rather wild text. For example, for ὁ υἱὸς τοῦ ἀνθρώπου at Matt. 8.20, Shem-Tob has **בן אדם בן הבתולה**, and for κατασκηνώσεις the interpretative rendering **קנים**. The expression **בן אדם** is not a possible underlay for ὁ υἱὸς τοῦ ἀνθρώπου, but a rather inadequate translation *into* Hebrew which would have to be explained to Hebrew-speaking Christians whose natural language did not contain it. In this example, it is turned into a specific description of Jesus by the expression **בן הבתולה**, 'the son of the virgin', which must be older than Shem-Tob because he would have no motivation to add it, but which remains a secondary Christian addition which may not be *much* older. **קנים**, 'nests', is an interpretative rendering of κατασκηνώσεις already found, as **קנא**, in the Sinaitic Syriac at Luke 9.58 (but not at Matt. 8.20), and, as **קנין**, in the Palestinian Syriac lectionary of both passages. Howard does argue that the *Evan Bohan* contains late revisions and explanatory additions, but there is no evidence in the text that they are later than the production of the Hebrew translation. At Matt. 4.23, τὸ εὐαγγέλιον τῆς βασιλείας is rendered **זבד טוב לעו מאוונג'ייליי ממלכות שמים**, which is another obvious attempt to translate Matthew's Greek text *into* Hebrew. Howard regards this as early because **זבד** forms a word connection with **זבדיאל** at 4.21 in Hebrew but not in Greek.²⁰⁹ The whole expression, however, is an obvious attempt at explicitation, so we must rather infer that Howard's criteria are unsatisfactory.

Howard draws attention to textual variants which are also found in older sources. For example, he notes the addition of **המלך** at Matt. 2.19, where sin cur pesh read **מלכא**.²¹⁰ All that evidence of this kind shows is that the late wild text of the *Evan Bohan* collected some readings which are found earlier. This particular reading could have arisen twice. Herod is called 'king' at Matt. 2.1, 3, 9, and by the *Evan Bohan* at Matt. 2.7, and by sin cur at Matt. 2.15. It

²⁰⁸ G. Howard, *The Gospel of Matthew according to a Primitive Hebrew Text* (Macon, 1987); rev. edn, *Hebrew Gospel of Matthew* (Macon, 1995).

²⁰⁹ Howard, *Hebrew Gospel*, pp. 184–90, esp. p. 185.

²¹⁰ *Ibid.*, pp. 194–6.

may be a copyist's addition more than once at 2.19, especially as Howard records its omission at 2.19 by manuscripts ABDEFG of the *Evan Bohan*. Howard's most dramatic suggestion is that the ending of the Gospel in the *Evan Bohan* supports a shorter ending known to Eusebius.²¹¹ This should not be accepted either. All our Greek manuscripts have the longer ending, which authorises the Gentile mission in accordance with the needs of the early church, but after the resurrection of Jesus when it took place, not before, when everyone knew that there was no Gentile mission. Unlike the *Evan Bohan*, the shorter early text includes the Gentile mission. The short text of the *Evan Bohan* has a Sitz im Leben among Jews who were not happy about Gentile Christianity, so it has a perfectly good Sitz im Leben in the mediaeval period.

Howard also draws attention to old traditions which are found in the *Evan Bohan*. For example, he notes the use of תלה with regard to Jesus' death at b. San 43a and in the *Tol'doth Yeshu*, and he argues that this word refers to hanging rather than crucifixion.²¹² Points of this kind show that Shem-Tob used older Jewish tradition, but they do nothing to show that his Hebrew text of Matthew is older than the fourteenth century.

Accordingly, Howard's most crucial arguments are those which purport to show that the quotations of Matthew in the *Evan Bohan* are not an edited translation, but an original composition. His first argument is from the language of the *Evan Bohan*. This is a mixture of biblical Hebrew, Mishnaic Hebrew and later rabbinical and even mediaeval Hebrew. Howard asserts that biblical Hebrew would be dominant if this were a late composition, on the ground that it is dominant in other works. This is an arbitrary assertion: Howard fails to show that there was a standard habit which this author was bound to follow. Howard proceeds to argue from puns, wordplays and alliteration.²¹³ He thinks they go far beyond what a translator would have created, and that they enhance the text of Matthew in a way that an anti-Christian author like Shem-Tob would not have done. There is some truth in the second point, but this only shows that parts of the text were inherited rather than done by Shem-Tob himself. Howard completely fails to demonstrate the first point. We have seen an example of word connection by the translator at 4.23. A different sort of example is found at 4.21, where Shem-Tob has

²¹¹ *Ibid.*, pp. 192–4.

²¹² *Ibid.*, pp. 207–8.

²¹³ *Ibid.*, pp. 184–90.

שני אחים אהרים for ἄλλους δύο ἀδελφούς: Howard does not explain what else he could have put. The Syriac versions are similar: for example, sin cur have תרין אחין אהרין. At 7.6, Shem-Tob has חזיר for τῶν χοίρων and, later in the verse, יחזור for στραφέντες. One cannot see why we should suppose that the translator noticed, let alone why this is more likely to have been done by an author than a translator. All such evidence is explicable as the work of translators.

We must therefore conclude that Howard's hypothesis is completely unsatisfactory. He did not take seriously the gross improbability of arguing that pre-Matthean material is to be found in a mediaeval anti-Christian tractate, nor did he test the inadequate methodology of his argumentation.

Some scholars, notably Turner and Porter, have continued to argue that Jesus taught in Greek. Both omitted major pieces of evidence which show that Jesus preached in Aramaic. Turner proposed that Jesus spoke Jewish Greek, or biblical Greek.²¹⁴ Some of the time, he calls this 'a distinguishable dialect of spoken and written Jewish Greek',²¹⁵ and he has been heavily criticised for not bringing forward enough evidence to justify its being a separate dialect.²¹⁶ This is a valid criticism, but it is not the main point. The study of Jewish languages has uncovered a wide range of phenomena, including variant forms of languages which are not extensive enough for conventional classification as separate dialects.²¹⁷ What we need to know, therefore, is whether the Gospel evidence is satisfied by supposing that Jesus spoke such a form of Jewish Greek, so it is important that Turner's arguments do not show this. Against the possibility that Matthew was written in Aramaic, Turner puts up its use of μέν . . . δέ and the genitive absolute. Both are specifically Greek constructions much commoner in the Gospels than in the translation Greek of the LXX, and Turner uses this as an argument against an Aramaic or Hebrew

²¹⁴ N. Turner, 'The Language of Jesus and his Disciples', in *Grammatical Insights into the New Testament* (Edinburgh, 1965), pp. 174–88, rep. in S. E. Porter (ed.), *The Language of the New Testament: Classic Essays* (JSNT. SS 60. Sheffield, 1991), pp. 174–90.

²¹⁵ Turner, *Grammatical Insights*, p. 183.

²¹⁶ G. H. R. Horsley, 'The Fiction of "Jewish Greek"', in G. H. R. Horsley, *New Documents Illustrating Early Christianity*, vol. 5, *Linguistic Essays* (Marrickville, 1989), pp. 5–40.

²¹⁷ J. A. Fishman (ed.), *The Sociology of Jewish Languages*, *IJSL* 3, 1981; J. A. Fishman (ed.), *Readings in the Sociology of Jewish Languages* (Leiden, 1985); J. A. Fishman (ed.), *The Sociology of Jewish Languages*, *IJSL* 67, 1987.

Q, and, by implication, any of the synoptic tradition.²¹⁸ But this argument is valid *only* against an Aramaic Matthew, which we should not indeed believe in. It cannot be an argument against Aramaic sources, for these could be revised. For example, we find Matthew using μέν when revising his Markan source at Matt. 13.8 (cf. Mark 4.8), and a genitive absolute at Matt. 9.10, revising Mark 2.15. Turner's argument from statistics comparing Gospel with LXX usage presupposes that Gospel translators could not differ from LXX translators. In fact they could: they might have noticed that the LXX has too few occurrences of such Greek features because literary monoglot Greeks told them so, and they might therefore have made increasing use of them, a process evidently carried further by Gospel editors.

Turner also repeats Abbott's argument that we cannot explain Mark's supposedly 'peculiar' practice of reproducing a few Aramaic words. He suggests that Jesus spoke Aramaic on these occasions, contrary to his usual practice.²¹⁹ But it is difficult to see why he should do so, and Turner's suggestion that he may have been addressing individuals whose sole language was Aramaic is ridiculous for Jesus addressing God (Mark 14.36; 15.34), and difficult to reconcile with Turner's general reasons for thinking that Jesus spoke Greek, since these imply that everyone else did. It is much more likely that we have one of the many translators who leave occasional words in the original language for dramatic effect, and it is coherent that Matthew and Luke tend to omit them (Matt. 9.25 and Luke 8.54 omit *ταλιθα κουμ*; Matt. 15.5 omits *κορβαν*, and Matthew omits Mark 7.34, while Luke omits the whole of Mark 7; Matt. 26.39 and Luke 22.42 omit *ἄββα*; Luke omits the cry from the cross at Mark 15.34–5, while Matthew re-edits the Aramaic just this once). This is surely because the translators were in direct touch with the source material, whereas the editors felt free to edit because they were not suffering from the degree of interference unavoidable in translators.

Turner then looks for evidence of composition in Greek. All he demonstrates, however, is that parts of Matthew, Luke and John were written or edited in Greek. For example, he notes the expression *ἐν καρδίᾳ καλῆ καὶ ἀγαθῆ* at Luke 8.15.²²⁰ This has a

²¹⁸ Turner, *Grammatical Insights*, pp. 176–9.

²¹⁹ *Ibid.*, p. 181, repeating Abbott, *Essays*, without precise reference to Abbott, and without any reference to Diodatus or Roberts: see pp. 10, 11.

²²⁰ *Ibid.*, p. 181.

traditional Greek phrase with no direct Aramaic equivalent, and there is alliteration in the Greek. However, we already knew that Luke was editing Mark 4.20, from which this phrase is absent. Evidence of this kind demonstrates only that all the synoptics are not literal translations of Aramaic Gospels: it does nothing to show that they were written without Aramaic sources. Again, Turner comments on John 3.3, 7.²²¹ These verses were indeed written in Greek, and fundamentalist assumptions are required for us to imagine that they could possibly contain words of Jesus.

It follows that Turner failed to show that Jesus taught in Greek. The arguments of Porter are no more convincing.²²² Porter suggests that Jesus is not recorded as using Aramaic apart from quotations. He notes that these are often taken as evidence that Jesus spoke Aramaic, but he suggests the contrary: 'By this reasoning it is more plausible to argue that Jesus did most of his teaching in Greek, since the Gospels are all Greek documents.'²²³ This is a quite unsatisfactory attempt to sidestep one of the central pieces of evidence. The Gospels were written to communicate the good news about Jesus to Greek-speaking Christians. It follows that the language in which they are written does not tell us which language Jesus spoke. Mark's use of Aramaic words suggests that everyone knew that Jesus spoke Aramaic, not Greek, for these Aramaic words must be explained. That can be done by supposing that Jesus spoke the lingua franca of Jews in Israel during his ministry, and that some words were left in the original tongue by the translators. This is supported by peculiarities such as $\acute{\omicron} \upsilon \iota\acute{\omicron}\varsigma \tau\omicron\upsilon \acute{\alpha}\nu\theta\rho\acute{\omega}\pi\omicron\upsilon$, which is not normal Greek, and is intelligible as a translation of **בר (א)נשא**. This explanation cannot be upset by counting the extent of the use of each language in documents written for people who spoke Greek. This is why it is so important that Porter does not provide a satisfactory explanation of the presence of these Aramaic words, or any explanation of other features of Aramaic.

Porter lays great stress on general facts about the broad use of Greek in Israel, but he does not differentiate this material properly, either by identity or by date. For example, he has Galilee 'com-

²²¹ *Ibid.*, p. 182.

²²² S. E. Porter, 'Did Jesus Ever Teach in Greek?', *TynBull* 44, 1993, 199–235; revised as 'Jesus and the Use of Greek in Galilee', in B. Chilton and C. A. Evans (eds.), *Studying the Historical Jesus: Evaluations of the State of Current Research* (NTTS XIX. Leiden, 1994), pp. 123–54. See further P. M. Casey, 'In Which Language Did Jesus Teach?', *ExpT* 108, 1997, 326–8.

²²³ Porter, 'Jesus and the Use of Greek', p. 125, n. 9.

pletely surrounded by hellenistic culture'.²²⁴ This Hellenistic culture was, however, Gentile, and its presence in cities such as Tyre and Scythopolis is entirely consistent with its rejection by Aramaic-speaking Jews. Again, Porter refers to the Greek names of the musical instruments at Dan. 3.5.²²⁵ These are, however, the instruments of Nebuchadnezzar, and represent in real life the favourite instruments of the Hellenistic persecutor Antiochus IV Epiphanes. They are the only Greek words in the text of Daniel precisely because they represent Hellenistic persecution, so they reveal very little knowledge of Greek and absolute rejection of it. Among genuine evidence for Jews using Greek, Porter cites the funerary inscriptions from Beth She'arim.²²⁶ While he notes that they date from the first to the sixth century CE, he does not draw from this the necessary conclusion: they do not tell us how many Jews in first-century Capernaum used Greek. Jews who lived in Israel after the time of Jesus gradually spoke more and more Greek, and it is this which these inscriptions reflect.

This is supported by two inaccurate generalisations. Correctly noting that in a multilingual situation, one language may carry more prestige than another, Porter announces that 'In Palestine, the prestige language was Greek.'²²⁷ In whose view? We may imagine this view being held at the court of Herod Antipas, and in a technical sense among Aramaic-speaking Jews who used Greek for business purposes. Porter gives us no reason to believe that this was the view of chief priests, scribes, Jewish peasants or the Jesus movement. In a sense, the prestige language was Hebrew, since this was the language of the Torah, which provided the halakhah on the basis of which the whole of daily life was run. From another perspective, instruction in the halakhah was given to most Jews in Aramaic, into which the Torah was translated. This could be perceived as being the central factor, and peasants and craftsmen might decide to operate only among Aramaic-speaking Jews. From this perspective, politics, education and economics were run in Aramaic. Fundamentally, therefore, Jewish people could take a different view of what a prestige language was from that represented in the multicultural research on which Porter depends.

Porter discusses Jewish literature which survives in Greek.

²²⁴ *Ibid.*, p. 135.

²²⁵ *Ibid.*, p. 139.

²²⁶ *Ibid.*, pp. 146–7.

²²⁷ *Ibid.*, p. 133.

Noting that 2 Esdras and Judith survive largely in Greek, he comments ‘quite possibly reflecting Jewish linguistic priorities for preservation of religious texts’.²²⁸ As a commentary on a culture which produced the Hebrew Bible and the Dead Sea scrolls, this is quite surreal. Some texts were written, and others preserved, in Greek because so many Jews spoke Greek, the lingua franca of the eastern half of the Roman empire, including the massive diaspora communities of Greece, Asia Minor and Egypt. In our period the Hebrew Bible was completed, and most of the Dead Sea scrolls were written, in Hebrew and Aramaic, because these were the sacred tongue and the lingua franca of the vast majority of Jews in Israel. Mishnah was written in Hebrew, and the Palestinian Talmud in a mixture of Aramaic and Hebrew, because this situation continued later. This would be inexplicable if Porter were right.

Porter also misinterprets important pieces of detailed evidence. For example, he notes that Josephus acted as interpreter for Titus so that he could communicate with Jerusalem Jews. Porter suggests that Titus spoke Greek which his listeners did not understand sufficiently well, and comments that ‘it is not known whether the deficiency in this situation was with his listeners or with Titus’.²²⁹ It is perfectly well known: Titus was fluent in Greek (Suet. *Div. Tit.* III.2). He told Josephus to negotiate with the Jews τῆ πατρίῳ γλώσσῃ (*BJ* V.360–1), which was obviously not Greek, and was in fact Aramaic rather than Hebrew. Thus the lingua franca of Jerusalem Jews was Aramaic, a fact which fits all our evidence but not Porter’s frame of reference.

In the final section of his article, Porter’s not-too-hidden agenda emerges: ‘there is a possibility if not a likelihood that we have some of the actual words of Jesus recorded in the Gospels’.²³⁰ This is a fundamentalist’s dream, and uncritical assumptions are required to carry it through. One of Porter’s passages is John 12.20–8, already used like this by Roberts in 1888. We have seen that it is completely secondary.²³¹ Porter also discusses Jesus’ trial before Pilate. Porter concludes from the fact that interpreters are not mentioned in the scriptural accounts that there were none there, another inference already made by Roberts.²³² This requires the text to be quite

²²⁸ *Ibid.*, p. 140.

²²⁹ *Ibid.*, p. 141.

²³⁰ *Ibid.*, p. 148.

²³¹ See pp. 10–11 above.

²³² Roberts, *Greek*, p. 165.

stunningly sacred. Is it not enough that what scripture *does* say be thought true, without having to suppose further that what it does *not* say be taken so literally?

These attempts to show that Jesus taught in Greek are accordingly to be regarded as quite spurious.

Equal trouble can still be caused by omitting the Aramaic level of the tradition. This has been much less common in scholarship since the work of Black, but it is still found, especially in American scholarship which is heavily influenced by selected literary theories. For example, Robbins has the 'imprint of the hand of Mark' visible in his use of καί to join sentences in the passion narrative, with reference to earlier scholarship and without considering a possible source which used ܐܢܝܢܐ.²³³ He suggests that complex editing produced Mark 14.21, without considering the Aramaisms in this verse.²³⁴ Kelber virtually omitted consideration of Aramaic from *The Oral and Written Gospel*, thereby omitting evidence that Mark depends partly on written sources.²³⁵ It is mentioned only to tell us that even if features such as the third-person plural narrative and the historic present might be traceable to Aramaisms or Semitisms, 'this does not preclude their oral propensity'.²³⁶ The literary trends of scholarship reached a logical peak in the work of Burton Mack, for they are basically founded on literary approaches to fiction, and fiction is what Mack asserts Mark's Gospel is.²³⁷ So he tells us that it is 'impossible to regard the Son of Man sayings as early', and that 'very late' stories include Mark 2.23–8, without any discussion of ܒܪ (ܐ)ܢܫܐ or other Aramaic aspects of these passages.²³⁸ On the Last Supper, 'the Pauline texts must be given priority',²³⁹ but there is no discussion of the Aramaisms in the Markan account.

Aramaic has been very little used in some discussions of the synoptic problem. For example, it plays a very small role in

²³³ V. K. Robbins, 'Last Meal: Preparation, Betrayal and Absence (Mark 14:12–25)', in W. H. Kelber (ed.), *The Passion in Mark: Studies on Mark 14–16* (Philadelphia, 1976), pp. 21–40, at 23–4, with n. 6.

²³⁴ Robbins, 'Last Meal', pp. 31–4.

²³⁵ W. H. Kelber, *The Oral and the Written Gospel* (Philadelphia, 1983). For criticism, J. Halverson, 'Oral and Written Gospel: A Critique of Werner Kelber', *NTS* 40, 1994, 180–95.

²³⁶ Kelber, *Oral and Written Gospel*, p. 66.

²³⁷ B. L. Mack, *A Myth of Innocence: Mark and Christian Origins* (Philadelphia, 1988). For criticism, L. W. Hurtado, 'The Gospel of Mark: Evolutionary or Revolutionary Document?', *JSNT* 40, 1990, 15–32.

²³⁸ Mack, *Myth of Innocence*, pp. 102, 197, cf. 242.

²³⁹ *Ibid.*, p. 298.

Farmer's recreation of the Griesbach hypothesis, apart from a quite unconvincing discussion of the Aramaic words in the text of Mark, all of which would have to be secondary if Farmer were right. Aramaic is omitted, for example, from Farmer's discussion of Mark 1.41. Thus the supposed omission of *σπλαγχνισθείς* by both Matthew and Luke forms a minor agreement which is very difficult to explain on the hypothesis of Markan priority, whereas the independent omission of *ὀργισθείς*, which should be read as a translation of *גָּרַח*, is quite easy to explain.²⁴⁰ Perhaps because of its omission by Farmer and others, Aramaic plays very little part in Tuckett's otherwise devastating critique of the Griesbach hypothesis: it might have been especially helpful in the discussion of Mark 3.28–9, Matt. 12.31–2 and Luke 12.10, but the main point is that Aramaic reconstructions have a potential which this discussion did not exploit.²⁴¹ Further work on the Semitisms of Codex Bezae and other manuscripts could also be fruitful.²⁴²

If, however, Aramaic is omitted, at least we can all see that it is omitted. One of the most remarkable features of some recent contributions to the Son of man problem is that they purport to discuss a major Aramaism, whereas the logic of their argument is entirely dependent on its being conducted in English. An extensive example is provided by Burkett.²⁴³ His article was written to refute a solution to the Son of man problem put forward by a series of scholars, including myself. After a number of objections which I discuss elsewhere,²⁴⁴ Burkett declares that the most serious objection is exegetical. In presenting this objection, however, Burkett does not even interact with the theory which I have proposed. I proposed *Aramaic* reconstructions of sayings of Jesus because Aramaic is the language which Jesus spoke: Burkett's criticisms are, however, entirely directed at *English* translations, and the distortions which this involves are horrendous. Burkett criticises my interpretation of Matt. 8.20, but he neither quotes nor discusses the

²⁴⁰ W. R. Farmer, *The Synoptic Problem: A Critical Analysis* (London/New York, 1964), esp. pp. 172–4, 145.

²⁴¹ C. M. Tuckett, *The Revival of the Griesbach Hypothesis* (MSSNTS 44. Cambridge, 1983), esp. pp. 87–9.

²⁴² Cf. p. 26 above. There is a very brief treatment in D. C. Parker, *Codex Bezae: An Early Christian Manuscript and its Text* (Cambridge, 1992), pp. 188–9, 253–6. It is virtually omitted from D. C. Parker and C.-B. Amphoux (eds.), *Codex Bezae: Studies from the Lunel Colloquium June 1994* (NTTS XXII. Leiden, 1996).

²⁴³ D. Burkett, 'The Nontitular Son of Man: A History and Critique', *NTS* 40, 1994, pp. 504–21.

²⁴⁴ See pp. 118–21 below.

Aramaic reconstruction of Matt. 8.20//Luke 9.58, which I offered and translated as follows:

לתעליא איתי להון חורין ולצפרי שמיא משכנן
ולבר אנש לא איתי לה אן דיסמוך רישה בה.

The jackals have holes, and the birds of the air have roosts,
and a son of man has nowhere to lay his head.

Burkett also quotes one of my exegetical comments on this Aramaic reconstruction: ‘the divine provision of resting-places for jackals and birds is contrasted with the lack of such provision for men, who have to build houses to have anywhere to stay’.²⁴⁵ He then suggests, ‘Casey subtly changes the verb of the saying from “have” to “be provided”’. This is untrue. The Greek verb ἔχω does not have a literal equivalent in Aramaic. However, it sometimes functions in the same way as the Aramaic expression **ל איתי**, which is not a verb. It may therefore be used to translate it (cf. Dan. 3.15; 1 En. 23.3). Accordingly, what I offered is not a subtle change, but the most straightforward reconstruction imaginable of idiomatic Aramaic which Jesus could have spoken, and straightforward behaviour by the translator. I used the term ‘provision’ in my explanation of the general level of meaning in English, which has no more precise equivalent of **ל איתי** either. Thus Burkett’s criticism ignores the proposed reconstruction altogether. Where the Aramaic uses an idiom not found in English, he has merely found that my translation and my description do not use the same word. They should not do so because it is essential that we bring out the meaning of these sayings by describing their cultural assumptions and their implications, and we cannot do this if we confine ourselves to repeating one translation of them.

Burkett then suggests that the proposed generalisation is not true, ‘since birds have to build their nests no less than humans have to build their homes’. Here Burkett uses the traditional translation ‘nests’, the inaccuracy of which I pointed out.²⁴⁶ This is a serious misrepresentation, because nature provides birds with roosts, and jackals with holes, and truly does *not* provide birds with nests. I gave all the necessary information about the behaviour of these

²⁴⁵ Burkett, ‘Nontitular Son of Man’, 517, quoting P. M. Casey, ‘General, Generic and Indefinite: The Use of the Term “Son of Man” in Aramaic Sources and in the Teaching of Jesus’, *JSNT* 29, 1987, 21–56, at 37.

²⁴⁶ Cf. p. 21 above.

creatures for the general level of meaning of my Aramaic reconstruction to be plausible. I particularly noted the position of Palestine on a route for migratory birds, which need roosts and do not build nests when migrating, and I mentioned the native Lesser Kestrel roosting in hundreds in the trees round Capernaum.²⁴⁷ Finally, I noted that the saying applies particularly to Jesus and his disciples. Burkett alleges that for this interpretation, ‘The indefinite “a son of man” would have to be qualified.’ There are two things wrong with this. In the first place, it is again in the wrong language. Whatever state it was in, **בר (א)נש(א)** was not ‘The indefinite “a son of man”’. Secondly, Aramaic generalisations do not have to be true of all people; indeed they are not necessarily true.²⁴⁸ Burkett, however, does not discuss Aramaic sentences.

Such criticisms are inappropriate in method. It is *part* of my proposed hypothesis that all Son of man statements in English, all Menschensohn statements in German and at least the majority of $\acute{o} \nu\acute{i}\acute{o}\varsigma \tau\acute{o}\upsilon \acute{\alpha}\nu\theta\rho\acute{\omega}\pi\omicron\upsilon$ statements in Greek are true of Jesus alone. This explains why this hypothesis is uncongenial to scholars who have a strong sense of logic, a good knowledge of Christian tradition and little or no knowledge of Aramaic. It is central to the proposed hypothesis that Aramaic usage was *different* from that of other languages, especially in that **בר (א)נש(א)** cannot lose a general or generic level of meaning. It follows that this hypothesis cannot be understood, let alone assessed, by means of Son of man statements in any language other than Aramaic.

When we look back over the scholarship of the last fifty years, we see a massive explosion of knowledge in subjects which form the background to the reconstruction of Jesus’ sayings in their original language. The Dead Sea scrolls have provided a decisive increase in our knowledge of Aramaic and other languages used in Israel towards the end of the Second Temple period. The discovery and editing of many other texts has massively increased our knowledge of the Aramaic language. Major tools of study have made our task more possible. Scholars in the fields of Bilingualism and Translation Studies have greatly increased our understanding of how bilinguals and translators function. Work on the LXX, the Targums and the Peshitta has increased our understanding of the large group of translators whose work most closely approximates

²⁴⁷ Casey, ‘Jackals’, 8–9, 20–1.

²⁴⁸ See pp. 111–18 below.

that of translators who worked to produce information about Jesus in Greek.

The same does not apply to the actual reconstruction of sayings of Jesus. The best book on the subject is still Black, *Aramaic Approach*, the first edition of which was published in 1946. Much recent work has taken scholarship backwards instead of forwards. The time is therefore ripe for a new attack on these problems. We must begin by elaborating a new methodology.