An Aramaic Approach to Q

Sources for the Gospels of Matthew and Luke

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THE STATE OF PLAY

The present state of research into 'Q' varies from the chaotic to the bureaucratic. At the chaotic end of the spectrum, there is no agreement as to whether Q existed, nor as to what it was, if it did. At the bureaucratic end of the spectrum, an amorphous group of scholars have agreed that it was a Greek document. It was produced by a Q community, whose concerns can be worked out from it. Some of these scholars suppose that we can work out what this Q community did *not* believe from what was not in Q, to the point that the Q community did not have an atonement theology because Q has no passion narrative. Most scholars who believe this also believe that Q was the first Gospel, and that its picture of Jesus was that of some kind of Cynic philosopher. As we narrow down the group of scholars to more detailed agreements, so we see an increase in the number of common judgements made in the interests of a consensus of the group, with quite inadequate attention to evidence or argument. We also see the large-scale omission of Aramaic, the language in which Jesus taught.

The purpose of this book is to suggest that the use of Aramaic has something to contribute to the study of Q. In a previous book, I suggested that the Gospel of Mark consists partly of Aramaic sources which have been literally translated into Greek. Consequently, they can be partly reconstructed. In the light of recent research, including that stemming from the discovery of the Dead Sea scrolls, I sought to lay down the most fruitful way of doing this, and I exemplified this with reconstructions of Mark 9.11–13, 2.23–3.6, 10.35–45 and 14.12–26.[1] In this book, I propose to see what we can do for Q. After discussing the history of research, I consider again the most appropriate methodology for this kind of work. I then reconstruct and discuss the sources of Matt. 23.23–36//Luke 11.39–51 and Matt. 11.2–19//Luke 7.18–35. I turn finally to one of the 'overlaps' between Mark and Q, and discuss the recoverable Aramaic sources of Mark 3.20–30, Matt. 12.22–32, and Luke 11.14–23; 12.10. Throughout

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[1] P. M. Casey, *Aramaic Sources of Mark's Gospel* (SNTS.MS 102. Cambridge, 1998).

these discussions, I continue the work of my previous book in that I seek to contribute not only to our understanding of Q, but also to the recovery of the Jesus of history.

We must begin with a critical history of scholarship. Here I do not seek to catalogue all previous work, but to select from the history of scholarship significant advances and mistakes, so that we can see more clearly how to proceed, and what pitfalls to avoid. One of the pitfalls lies in different definitions of what Q was, or is. For clarity's sake, I therefore anticipate one outcome of this book by giving the definition which I use when I conduct my own discussion of Q: Q is a convenient label for the sources of passages which are found in both the Gospel of Matthew and the Gospel of Luke, and which have not been taken from Mark's Gospel. It will be clear that this entails some controversial conclusions, and that we must be careful to note that it does not entail others. It implies that Q was not a single document, and that Luke did not take all his Q material from Matthew; I shall argue for both of these hypotheses in detail. It means that we can meaningfully discuss whether a passage such as Matt. 11.28–30 is to be described as part of Q; it is not found in Luke, but we could discuss whether it was in the same documentary source as Matt. 11.25–27//Luke 10.21–2, whether Matthew added it, whether Luke knew it or whether Luke left it out. It also means that our evidence for Q is found in Greek; it does not specify that this is, or is not, how it reached the evangelists. I shall argue that some parts of Q reached both evangelists in the same Greek translation, and that other parts are due to two different translations being made, whether by the evangelists, their assistants or by more distant sources.

From Holtzmann to Tödt

Serious modern research into Q effectively began with Holtzmann, though this is not what he called it. In a book published in 1863, he suggested that there was one source A behind the Triple Tradition of the synoptic Gospels, and a second major source behind the Double Tradition. This source he called Λ, which stood for λόγια.[2] At this stage, however, the priority of Mark had still not been established, nor had anyone shown

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[2] H. J. Holtzmann, *Die synoptischen Evangelien: Ihr Ursprung und geschichtlicher Charakter* (Leipzig, 1863). For predecessors, cf. J. G. Eichhorn, 'Über die drey ersten Evangelien. Einige Beyträge zu ihrer künftigen kritischen Behandlung', in *Allgemeine Bibliothek der biblischen Literatur* 5 (Leipzig, 1794), pp. 761–996; F. D. E. Schleiermacher, 'Über die Zeugnisse des Papias von unseren beiden ersten Evangelien', *Theologische Studien und Kritiken* 5, 1832, 735–68; C. H. Weiße, *Die evangelische Geschichte kritisch und philosophisch bearbeitet* (2 vols., Leipzig, 1838), vol. I, ch. 1, esp. pp. 83–6.

what could be done with Aramaic. Meyer made the first major advance in our understanding of the Aramaic background to the synoptic Gospels in 1896. For example, he offered this reconstruction of Matt. 12.32:[3]

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

It is a great advantage that the complete sentence has been reconstructed, for this ensures that ברנש emerges as a normal term for man. It is also good that, even in an era long before the discovery of the Dead Sea scrolls, there are no problems with the late date of the Aramaic used. Moreover, the proposed reconstruction permits the understanding of Luke 12.10 as an alternative version 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. This was, however, as far as Meyer went, even in the best book then written on the Aramaic background of the Gospels, and the best for another fifty years.[4] The important advances which might have flowed from this work were simply not made.

The massive variety of other comments from the same period of scholarship included some brief but useful points. It was at about this time that this source was called Q. This has been noted in the works of Simons in 1880 and Weiß in 1890, and became popular partly because of the work of Wernle in a notable book published in 1899.[5] Thus Wellhausen could describe it simply as a 'zusammenhangenden Quelle, die man mit Q bezeichnet'.[6] Among Wellhausen's own more enduring suggestions was that at Matt. 23.26 καθάρισον correctly represents the Aramaic *dakkau* (reinigt), whereas at Luke 11.41 τὰ ἐνόντα δότε ἐλεημοσύνην represents a misreading of the same word as *zakkau* (gebt Almosen).[7] This is plausible, and a useful contribution to the whole question of the relationship between

[3] A. Meyer, *Jesu Muttersprache. Das galiläische Aramäisch in seiner Bedeutung für die Erklärung der Reden Jesu und der Evangelien überhaupt* (Freiburg i.B. and Leipzig, 1896), p. 94.

[4] For further discussion of Meyer, and a critical *Forschungsberichte* of the whole Aramaic question, see Casey, *Aramaic Sources of Mark's Gospel*, ch. 1.

[5] E. Simons, *Hat der dritte Evangelist den kanonischen Matthäus benuzt?* (Bonn, 1880); J. Weiß, 'Die Verteidigung Jesu gegen den Vorwurf des Bündnisses mit Beelzebul', *Theologische Studien und Kritiken* 63, 1890, 555–69; P. Wernle, *Die synoptische Frage* (Freiburg i.B., Leipzig and Tübingen, 1899). Simons uses 'der apostolischen Quelle' in reference to the work of B. Weiss, abbreviates it to 'Q.', defined carefully as 'Die apostolische Quelle nach W.' (p. 22), and proceeds to use 'Q.', almost always with a full stop, as an abbreviation of this. Weiß uses 'Quelle' in its normal sense, meaning 'source', introduces Q in brackets for the Redenquelle (p. 557), and then simply proceeds to use Q as a symbol for the sayings source lying behind Matthew and Luke.

[6] J. Wellhausen, *Einleitung in die drei ersten Evangelien* (2nd edn, Berlin, 1911), p. 58.

[7] *Ibid.*, p. 27.

the different forms of Q material.[8] 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 commented on the possible language of Q.
He noted that verbal agreement is sometimes so high as to require a Greek
Q, whereas in other passages there are variants which may be explained as
resulting from two translations of an Aramaic source.[9] It is the problems
posed by this facet of the evidence that still require resolution.

A comprehensive attempt to reconstruct Q in Greek was made by
Harnack.[10] Harnack began with those passages of Matthew and Luke
which have the highest level of verbal agreement. This enabled him to
argue that Q was a Greek document, and the argument from this first set
of passages is very strong indeed. The similar argument for the next set of
passages, in which the level of verbal agreement is lower, is more of a *tour
de force*. Harnack argues that a single Greek translation was used by both
of the evangelists, and that this was translated from Aramaic. He did not,
however, supply the detailed argumentation which would be necessary
to establish this position; indeed he has no detailed argumentation on the
Aramaic question at all. This has been a constant defect of Q research
ever since.

A major contribution to the study of Q was made by B. H. Streeter,
most notably in *The Four Gospels* (1924).[11] One reason why this became
a standard work is that it offered a complete solution to the synoptic
problem, including decisive arguments for the priority of Mark. Streeter
said very little about Aramaic, however. He treated Q as a document
written in Greek, and discussed whether passages where there was con-
siderable variation in wording between the Matthean and Lucan versions
belonged to it. A most inadequate treatment of Aramaic is just squeezed
into the discussion of the 'minor agreements'.[12] Here the changes which
Matthew and Luke made to Mark's excessive use of καί, and to his equally
excessive use of the historic present, are correctly seen as the reactions of

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[8] See further pp. 23–4, 82 below.   [9] Wellhausen, *Einleitung*, pp. 59–60.

[10] A. von Harnack, *Sprüche und Reden Jesu. Die zweite Quelle des Matthäus und Lukas*
(Leipzig, 1907); ET *New Testament Studies*, vol. II: *The Sayings of Jesus. The Second
Source of St Matthew and St Luke* (London, 1908).

[11] B. H. Streeter, *The Four Gospels. A Study of Origins* (London, 1924). See earlier B. H.
Streeter, 'On the Original Order of Q', in W. Sanday (ed.), *Studies in the Synoptic Problem
by Members of the University of Oxford* (Oxford, 1911), pp. 141–64: 'St Mark's Knowledge
and Use of Q', in Sanday (ed.), *Studies*, pp. 165–83: 'The Original Extent of Q', in Sanday
(ed.), *Studies*, pp. 185–208.

[12] Streeter, *Four Gospels*, pp. 296–8.

two Greek writers to one whose Greek has been influenced by Aramaic. Moreover, this is correctly seen as the reason for those minor agreements which consist of them both doing so in the same passages. Streeter did not, however, offer any reasonable demonstration that Aramaic was really the cause of unusual features in Mark's Greek, still less of his bald assertion that 'Mark's Greek is that of a person who had been brought up to think in Aramaic.'[13] More centrally, he offered no proper discussion of the possibility of Aramaic sources of Q at all. He did not even consider the possibility that some passages might be the result of two translations of Aramaic source material.

Streeter also stored up future trouble by arguing that very little was omitted from Q by Matthew and Luke.[14] His arguments for this position carry no weight at all. His first is that Matthew omitted very little from Mark. This, however, demonstrates nothing, since Matthew might have preferred Mark because it gave a coherent outline for the ministry, but felt that it needed expanding with some more of Jesus' teaching. He could have selected from a massive Q what he needed for this purpose, making a somewhat longer Gospel, and leaving most of Q out. The mere fact that he edits most of Mark does not tell us the size of his Q nor how much of it he used.

Moreover, Luke did leave out a lot of Mark. Streeter's second argument is that Luke used a mutilated copy of Mark.[15] This omitted most of the 'great omission', going straight from Mark 6.47 αὐτὸς μόνος to Mark 8.27 καὶ ἐν τῇ ὁδῷ. Streeter's arguments for this view are absolutely arbitrary. For example, he suggests that it would explain Luke's omission of the place-name Caesarea Philippi as the scene of Peter's confession (Luke 9.18). But this is the only mention of Caesarea Philippi in the Gospels, and it is a long way north of Galilee in an area which Jesus is not otherwise known to have visited. Luke may therefore have omitted it because he thought it must be a mistake. In short, Streeter's argument assumes his result: he regards this view as probable only because he thinks sources are more likely causes of changes than editorial alterations. It is, however, most unlikely that a copy of Mark would be mutilated in this way, and that so assiduous a collector of information as Luke would be unable to obtain an unmutilated copy. We would therefore require strong evidence to believe this, and we have none at all. Streeter adds special explanations of Luke's omission of other pieces of Mark, such as that the pith of the long discussion of divorce (Mark 10.1–12) is given in the last two verses,

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[13] *Ibid.*, p. 297. [14] *Ibid.*, pp. 289–91.
[15] *Ibid.*, p. 290, picking up on pp. 175–8.

for which Luke has an equivalent in another context (Luke 16.18).[16] This is quite misleading, for it still shows that Luke left out pieces of Mark. He is equally liable to have left out pieces of Q.

Most of Streeter's arguments that very little was omitted from Q by Matthew and Luke are of no weight for reasons of this kind. They also *presuppose*, rather than demonstrate, that Q was a single document. If it were several documents, Matthew and Luke might have included material from some documents and not from others because they either knew only some of them, or knew only Greek and not Aramaic, or because they selected some rather than others until they had sufficient material, or because they found some documents which fitted their purposes and others which did not. It follows that the treatment of Q in this standard and influential work is seriously defective. It omits any serious discussion of possible source material in Aramaic, and puts forward entirely spurious reasons both for considering it one unified document, and for imagining it as source material from which Matthew and Luke did not omit anything very much.

Burney included Q in his attempts to uncover Jesus' poetry.[17] For example, he devoted a whole chapter to 'The Use of Parallelism by Our Lord'.[18] Having first noted this as a formal characteristic of Hebrew poetry,[19] 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 Matt. 8.20//Luke 9.58.[20]

Burney's work was, however, vitiated by errors of method. Even the chapter on parallelism sets out Gospel passages in English, which underlines the fact that Burney never demonstrated the existence of Aramaic sources of Q. He has no detailed discussion of passages where the Matthean and Lucan versions are seriously different, so he never tackles the implications of Aramaic phenomena for our model of Q. For example, he notes that Luke 11.47 is different from Matt. 23.29. Having set out most of the Matthean version in parallel lines in *English*, all he does about the differences is to set out the Lucan version, also in parallelism in English too, and he simply declares that 'the second stichos' summarizes Matt. 23.30–1.[21] This is not sufficient to demonstrate anything. Some of the chapter on rhythm conducts the discussion in English too, which

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[16] *Ibid.*, p. 178.

[17] C. F. Burney, *The Poetry of Our Lord* (Oxford, 1925). For a more general assessment of Burney's work on the Aramaic background to the Gospels, see Casey, *Aramaic Sources of Mark's Gospel*, pp. 19–22.

[18] Burney, *Poetry*, ch. II.   [19] *Ibid.*, pp. 15–22.

[20] *Ibid.*, pp. 132, 169.   [21] *Ibid.*, p. 68, with n. 3.

is methodologically quite inadequate. The brief discussion of rhythm in Aramaic poetry includes for example Dan. 4.24, but apart from the rather arbitrary way in which Burney set it out in lines,[22] there is nothing to suggest that this is really poetry at all.

Burney uses the Matthean version of the Lord's prayer for a reconstruction which is supposed to consist of two four-beat tristichs; the formula may be said to be two (stanzas) × three (stichoi) × four (beats). This is said to have been an aid to memory. Burney then declares the Lucan version mutilated, and suggests that we can hardly hesitate as to which is the more original. Finally, he suggests that it has features of rhyme.[23] This hypothesis, which never receives properly detailed discussion, runs from the improbable to the impossible. It is surely improbable that we have two stanzas, in an orally transmitted poem with no break in the sense, from a culture not known to have had poems in two four-beat tristichs. Luke's opening πάτερ must surely go back to Jesus' rather distinctive אבא, whereas the Matthean version is more conventional. It is surely at least as probable that Luke's version is original, which upsets Burney's formula drastically, and underlines the fact that it is *Burney's* formula, not something which has arisen naturally from the culture under study. Consequently, the arrangement of the supposed reconstruction of the Matthean version is not a satisfactory criterion for determining authenticity.

Burney's discussion of rhyme is entirely spurious. Rhyme 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'.[24] 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. His first example from the Lord's prayer[25] is supposed to have the ending -ā set in strophe 1a, with *dᵉbišmayyā́* for ὁ ἐν τοῖς οὐρανοῖς, rhyming at his strophe 3a with exactly the same form *dᵉbišmayyā́* for ὡς ἐν οὐρανῷ, and rhyming in strophe 3b with *bᵉarʿā́*. But the first example of דבשמיא is probably a Matthean addition. Burney does not explain the behaviour of the translator in putting two different Greek expressions for the same Aramaic one, and the whole notion of these words rhyming really only reflects the ending of the Aramaic definite state. Finally, some of the words

[22] *Ibid.*, p. 110.   [23] *Ibid.*, pp. 112–13, 161–2.
[24] *Ibid.*, p. 147.   [25] *Ibid.*, pp. 112–13, 161–2.

used by Burney are not correct. So, for example, *dᵉyōmā́* for ἐπιούσιον, supposedly giving the same rhyme with *ā́* in the second stanza, is surely wrong. It is not, however, *randomly* wrong: it is the translation of a late traditional understanding of ἐπιούσιον *into* Aramaic, which illuminates the nature of Burney's supposed reconstructions – they are translations *into* Aramaic, not genuine reconstructions at all.

For these reasons, Burney's work is faulty from beginning to end – he made many mistakes, and demonstrated nothing beyond the already obvious fact that some of Jesus' sayings show signs of parallelism.

The next piece of work on Q to have been considered important was that of Bussmann.[26] Bussmann argued that R, his abbreviation for *Redenquelle*, should be regarded as a composite of two collections. Those passages in which there is close verbal agreement between Matthew and Luke are dependent on a Greek source, which he sometimes labels T for *Täuferquelle*, passages which may have derived from circles close to John the Baptist. Those passages in which there is considerable verbal varia-tion between Matthew and Luke were derived from an Aramaic source, 'nur Reden enthaltende, also wohl das eigentliche R'.[27]

The important point made by Bussmann is his clear recognition of the two different types of source material, and his guess at the cause of this, that Matthew and Luke used one Greek translation for some Q passages, and used or made two Greek translations of Aramaic source material in others. He did not, however, discuss a single Aramaic word. It follows that Bussmann could not possibly *demonstrate* that the variations between Matthew and Luke were due to two translations. While his hypothesis was basically plausible, it could not be defended in this inadequate form against the argument that the variations are due to heavy editing, and that what really happened was that Matthew and/or Luke edited much more vigorously in some passages than in others. Nor did Bussmann offer a sufficient argument for believing that the supposed Aramaic source was a single document.

If Q was a document, it deserves a commentary. The first real attempt to supply one was made by T. W. Manson, who also offered a commen-tary on the teaching peculiar to each of Matthew and Luke.[28] Manson's Q does not include all the material in which Matthew and Luke overlap. He asserts that translation and even mistranslation account for some vari-ants, but he gives very few examples, at least the majority of which are

[26] W. Bussmann, *Synoptische Studien*, vol. II: *Zur Redenquelle* (Halle, 1929).

[27] *Ibid*., p. 137.

[28] T. W. Manson, *The Sayings of Jesus* (1937, as part II of *The Mission and Message of Jesus*, ed. H. D. A. Major et al. Reprinted separately, London, 1949).

taken from earlier scholarship.[29] For example, he repeats from Burney the view that Matt. 6.22–3 is rhythmically superior to the version of Luke, and refers back to Burney for a 'retranslation into Aramaic'.[30] It is not, however, clear that the original saying was verse in any meaningful sense, and Burney's work on rhythm is largely spurious.[31] For reasons of this kind, most of Manson's scattered comments on Aramaic are unhelpful. Manson's Q also includes passages such as Luke 3.7–9, which is almost verbally identical with Matt. 3.7–9 in Greek. This is difficult to reconcile with positing translation variants in other passages. We must conclude that, like Harnack and Streeter, Manson failed to investigate the Aramaic dimension of Q.

A brief article by Barrett is worthy of mention at this point, even though it was too short to deal with Aramaic reconstructions.[32] Barrett pointed out clearly and forcefully that the Q material does *not* have common order in the two Gospels as a whole. The common order at the beginning is partly explained by common use of Mark which has, for example, the temptation of Jesus after his baptism, and this was sufficient to cause Matthew and Luke to put Q material about John the Baptist, and then the temptations of Jesus, in the same order. Barrett also noted that whereas some passages are verbally identical in Greek others were explicable as resulting from two translations of Aramaic source material. The evidence so clearly summarised by Barrett is enough to show that Q was not a single document.

The next noteworthy book was the most important contribution to the study of the Aramaic substratum of the Gospels: M. Black, *An Aramaic Approach to the Gospels and Acts* (1946).[33] In this book, Black gathered together the best of previous work, and added many points of his own. Hence its position as the standard work on its subject. In his review of previous work, Black laid down a number of correct principles. For proposed mistranslations, he laid down that 'the mistranslation must at least be credible; and the conjectured Aramaic must be possible'.[34] This excludes a high proportion of suggestions, and in this matter Black unfailingly observed his own principles. He also followed Driver in calling for

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[29] One or two more are given in T. W. Manson, *The Teaching of Jesus* (Cambridge, 1931, 2nd edn, 1935); 'Some Outstanding New Testament Problems. XII. The Problem of Aramaic Sources in the Gospels', *ET* 47, 1935–6, 7–11.

[30] Manson, *Sayings*, p. 93, referring to Burney, *Poetry*, p. 131.

[31] Casey, *Aramaic Sources of Mark's Gospel*, pp. 19–22; pp. 6–7 above.

[32] C. K. Barrett, 'Q: A Re-examination', *ET* 54, 1942–3, 320–3.

[33] M. Black, *An Aramaic Approach to the Gospels and Acts* (Oxford, 1946).

[34] *Ibid.*, p. 7.

the presentation of whole sentences.[35] 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.[36] He
discussed whole features of the Aramaic language as well as detailed re-
constructions; for example, he has a whole section on asyndeton.[37] 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.

It is all the more regrettable that Black was not able to make a signif-
icant positive contribution to the study of Q. He did repeat some useful
suggestions from earlier work. For example, he repeated Wellhausen's
suggestion that at Matt. 23.26 καθάρισον correctly represents the Aramaic
dakkau (cleanse), whereas at Luke 11.41 τὰ ἐνόντα δότε ἐλεημοσύνην
represents a misreading of the same word as *zakkau* (give alms), and he
correctly defended this against Dalman's criticism.[38] Such suggestions
cannot, however, take us far enough, because they deal only with single
words. Black was moreover in no position to produce the much needed
revolution in Q studies, not least because scholars who had written exten-
sively on Q had not made significant use of Aramaic. Equally, of those
who had written on Aramaic, only Meyer showed first-class ability, and
he wrote when the study of Q was still in its infancy.[39] Black's main
problem was accordingly that there was no established model of Q, nor
any established methodology for studying a possible Aramaic substratum
of it. Believing correctly in the historicity of much of the Q material, and
knowing that Jesus spoke Aramaic, Black tried to reconstruct as many
Aramaic features in the supposed source(s) of Q as he could. Unfortu-
nately, he thereby *produced* Aramaisms rather than reconstructing them,
and when they are genuinely to be found in our texts, he moved verses to
put them beside each other in a supposedly original source. Too influenced
by Burney, he did not always reconstruct the original Aramaic at all.

For example, Black sets out Matt. 3.12//Luke 3.17 in parallel lines
in English, without reconstructing the supposed Aramaic.[40] It is, how-
ever, simply not obvious that Jesus, Q, Matthew or Luke was aware of
this parallelism. Black then suggests that φυγεῖν at Matt. 3.7//Luke 3.7

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[35] *Ibid.*, p. 12.   [36] *Ibid.*, ch. II.   [37] *Ibid.*, pp. 38–43.
[38] *Ibid.*, p. 2, referring to Wellhausen, *Einleitung*, p. 27.
[39] Meyer, *Jesu Muttersprache*: see p. 3 above.
[40] Black, *Aramaic Approach* (1946), p. 106.

represents the Aramaic *ᵃraq*, that ῥίζα at Matt. 3.10//Luke 3.9 represents *ʿiqqar*, and that 'if the word-play was to be effective, verse 10 must have followed directly on verse 7 . . .'[41] Here Black's Aramaic is sound, but the use he has made of it is not, for altering the text like this is quite arbitrary. At Matt. 5.43–48//Luke 6.27–36, Black uses his ideas of parallelism to alter the order of the text; here too this criterion is too arbitrary to be accepted. Black also declares that 'Alliteration, assonance and word-play are all prominent features of the Aramaic of these verses.'[42] He therefore uses Luke 6.32–3 rather than Matt. 5.46 to 'reconstruct' these features, including *ṭebhu* for χάρις and *ḥabbibhu* for ἁμαρτωλοί. It is, however, most unlikely that Matthew would put μισθόν for *ṭebhu*; it is surely very likely that Luke would alter it to χάρις (a term never used by Matthew, but used eight times in Luke's Gospel and seventeen times in Acts), precisely because it makes the text more suitable for Gentiles. Accordingly, we should reconstruct מה אגר איתי לכון, a probable source which has less wordplay than that proposed by Black. Likewise, it is not probable that Matthew wrote τελῶναι for *ḥabbibhu*, whereas Luke might well alter מכסיא or τελῶναι to ἁμαρτωλοί, a term which he uses seventeen times and which might make better sense for him here. Accordingly, bearing in mind the reading οὕτως (D Z 33, with Latin and Syriac support), we should reconstruct הלא מכסיא עבדין כן. This further reduces the extent of the proposed wordplay.

This straining after features which do occur in Aramaic, but less frequently than in Black's reconstructions, sometimes leads to problems with the actual Aramaic. For example, Black has an arbitrary combination of Matt. 6.19–20 with Luke 12.33, apparently because shifting from Matthew to end with Luke 12.33 enables him to reconstruct *qarebh* behind the Lucan ἐγγίζει to give wordplay with *ruqba*, proposed behind the Matthean σής for 'moth', and with *marqebh*, proposed behind the Matthean ἀφανίζει for 'corrupteth'.[43] The first problem is ἐγγίζει at Luke 12.33, whereas Matthew has διορύσσουσιν (Matt. 6.20). That Matthew should translate קרב or edit ἐγγίζει into διορύσσουσιν is surely most improbable. It is, however, entirely plausible that Jesus said פלשין, and that Luke, writing for Greeks who lived in cities, should find פלשין/διορύσσουσιν quite strange and replace it with ἐγγίζει. More serious is *ruqba*. Black gives no attestation for this, and appears to have repeated from an unfortunate entry in Jastrow a word which did not really exist. סס is found at *Ahiqar* 184, and in Hebrew, later Jewish Aramaic, Christian Palestinian Aramaic and Syriac, including the Syriac versions

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[41] *Ibid.*, p. 106.   [42] *Ibid.*, p. 137.   [43] *Ibid.*, pp. 135–6.

here. סס is surely the word which should be reconstructed, and this ruins the proposed wordplay. Nor did Black give attestation for *marqebh*, which is not known to me from early sources. There is much more extensive attestation of חבל, including 4Q203 8 11. It is therefore quite impossible that Black's wordplay occurred in an Aramaic source.

It must therefore be concluded that Black did not succeed in carrying forward research into the Aramaic substratum of Q. During the next few years, both he and Bussby wrote short articles which again drew attention to the evidence that there is an important Aramaic dimension to Q.[44] These treatments were too short to achieve the massive amount of progress which was theoretically possible. The whole matter was ignored by Farrer, in a famous essay which attempted to dispense with Q altogether.[45] His argument that Luke used Matthew does not even list, among the objections which he proposed to meet, any cases in which Luke may be thought to have made or used a different translation from that of Matthew.[46] Farrer also argued very strongly against the production of an unknown source which mysteriously disappeared, suggesting that his hypothesis was better for utilising only known sources. This argument should have been forceful enough to prevent the emergence of the American view of Q, a supposedly real Gospel supposedly produced by a unique community. Such an important work would surely not have been discarded merely because it was used by Matthew and Luke. Farrer's argument did not, however, have the effect it should have had. It should not be allowed great weight against the existence of smaller sources, which no one would have such great reason to preserve after their incorporation by Matthew and Luke into their Gospels.

Taylor wrote two essays on the order of Q which are worthy of mention here.[47] They began from the fact, posed with such clarity by Barrett, that the Q material as we have it is not in common order. Taylor proposed to remove from Q some material in which the level of verbal agreement is notoriously lower than in others. While his work was not detailed enough to make his case demonstrable, it was potentially fruitful in suggesting a

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[44] M. Black, 'Unsolved New Testament Problems. The Problem of the Aramaic Element in the Gospels', *ET* 59, 1947–8, 171–5; F. Bussby, 'Is Q an Aramaic Document?', *ET* 65, 1953–4, 272–5.

[45] A. M. Farrer, 'On Dispensing with Q', in D. E. Nineham (ed.), *Studies in the Gospels. Essays in Memory of R. H. Lightfoot* (Oxford, 1957), pp. 55–88; likewise e.g. B. C. Butler, *The Originality of St Matthew* (Cambridge, 1951), chs. 1–2.

[46] Farrer, 'Dispensing with Q', p. 63.

[47] V. Taylor, 'The Order of Q', *JThS* NS 4, 1953, 27–31; 'The Original Order of Q', in A. J. B. Higgins, ed., *New Testament Essays: Studies in Memory of T. W. Manson* (Manchester, 1959), pp. 246–69.

possible model for the Q material, with one Greek document to which a significant proportion of the Q material did not belong.

The next work to be considered important is that of Tödt.[48] Kloppenborg regards it as important because Tödt recognised that Q presented a kerygma organised along its own distinctive lines: Son of man Christology. He declares that his thesis 'marked a decisive shift in this historical, theological and hermeneutical valuation of Q'. Tuckett likewise describes it as a 'decisive contribution in the development to see Q as a self-contained entity'. He particularly notes that Tödt interpreted Q 'independently of the passion kerygma so dominant in Mark and Paul'. Hoffmann likewise regards it as 'The most incisive and significant contribution to the study of the sayings source, and also the foundation and impetus for recent discussion.'[49] Yet Tödt's work has since been shown to be deeply flawed, not least in his use of the term Son of man (Menschensohn). His chapter I is entitled 'The transcendent sovereignty of the Son of man figure (Menschensohngestalt) in late Jewish (spätjüdischen) Apocalyptic'. Both 'the Son of man figure (Menschensohngestalt)' and the description 'late Jewish (spätjüdischen)' should warn us that we are to be treated to German tradition, not to real Judaism. The discussion is carried through at the hand of German translations of Dan. 7.13–14, 4 Ezra 13 and the *Similitudes of Enoch*, without any discussion of the Aramaic (א)נש(א) בר, or of the original text of any of these documents. The discussion of Dan. 7.13–14 already introduces 'the figure of a transcendent Perfecter', and bluntly casts aside the interpretative section, which is said to rob him 'completely of his individuality and puts "the people of the saints of the Most High" in his place'.[50] This arbitrarily removes the Jewish people from the original text of Dan. 7, and alters the interpretation of אנש כבר to a figure from German tradition more convenient for Tödt's work.

When he reaches the New Testament material, Tödt follows other scholars in declaring Luke 12.8–9 more original than its parallels, on the ground that it discriminates between the 'I' of Jesus and the Son of man, without any discussion of a possible Aramaic original in which this matter would

[48] H. E. Tödt, *Der Menschensohn in der synoptischen Überlieferung* (Gütersloh, 1959); ET *The Son of Man in the Synoptic Tradition* (London, 1965).

[49] J. S. Kloppenborg, in J. S. Kloppenborg (ed.), *The Shape of Q. Signal Essays on the Sayings Gospel* (Minneapolis, 1994), pp. 7–8; J. S. Kloppenborg, *The Formation of Q. Trajectories in Ancient Wisdom Collections* (Philadelphia, 1987), p. 23; C. M. Tuckett, *Q and the History of Early Christianity* (Edinburgh, 1996), p. 51; P. Hoffmann, 'The Redaction of Q and the Son of Man. A Preliminary Sketch', in R. A. Piper (ed.), *The Gospel Behind the Gospels. Current Studies on Q* (NT.S 75. Leiden, 1995), pp. 159–98, at 160.

[50] Tödt, *Menschensohn*, pp. 19, 21; *Son of Man*, pp. 21, 24.

look quite different.[51] At the beginning of chapter III 'The sayings con-
cerning the Son of man's activity on earth', he sets up the question of 'how
the name Son of man came to be used in this new way', without discussing
בר (א)נש(א), which would have shown that the supposedly new usage was
in some cases the original one. His discussion of Matt. 8.20//Luke 9.58
concludes that the 'name Son of man (Menschensohnname)' is used to ex-
press Jesus' 'sovereignty, his supreme authority', without any discussion
of a possible Aramaic original in which this could hardly be the case.[52]
This inadequate methodology becomes crucial when Tödt proceeds to
argue, 'In the understanding of the Sayings Source the Identification of
Jesus with the coming Son of man is no doubt complete. Accordingly, we
find in Q a Son of man Christology (Menschensohnchristologie).'[53] This
supposed result has been produced by treating the Jewish texts only in
German and ignoring the Aramaic level of the tradition of Gospel sayings.

Tödt's specific discussion of Q in general is equally unsatisfactory. It is
largely discussed in German, with only occasional treatment of the Greek
text, and no consideration of a possible Aramaic substratum. This means
that Tödt can put together patterns which would not have occurred to any
bilingual translators. Equally, assuming Q to have been a document, he
can put forward categories contrary to the editing procedures of Matthew
and Luke. Thus freed from the major constraints under which the text
was actually produced, he can form effectively circular arguments with
his own culture. Without discussion of Aramaic source material, there
is no proper discussion of the variability in verbal overlap in different
parts of the document, nor of the extent to which this coincides with lack
of common order. Thus Tödt effectively omits consideration of the main
reasons why we should not believe that Q was a single document.

It is as a result of these unsatisfactory procedures that Tödt can imagine
a serious Gospel document which had no passion narrative. Such are his
(inherited) criteria that all he needs to come to this conclusion is for either
Matthew or Luke to follow Mark; then the evidence of other material (in
fact largely Lucan) does not count. This might have been the case even
if Q had been a single document, partly followed in the passion by Luke
but only twice by Matthew; if Q was not a document, then the document
without a passion is merely a construct of modern scholars. Tödt offers
no proper discussion of the two exceptional passages: at Mark 14.65
προφήτευσον, both Matthew (26.68) and Luke (22.64) have τίς ἐστιν

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[51] Tödt, *Menschensohn*, p. 51; *Son of Man*, p. 55.
[52] Tödt, *Menschensohn*, pp. 112–14; *Son of Man*, pp. 120–3.
[53] Tödt, *Menschensohn*, p. 241; *Son of Man*, pp. 264–5.

ὁ παίσας σε, and they end the story of Peter's denial καὶ ἐξελθὼν ἔξω ἔκλαυσεν πικρῶς (Matt. 26.75//Luke 22.62). Such strong evidence of an alternative version of part of the passion narrative should surely not have been ignored.[54] Similar problems attend his assertion that the sayings about the suffering and rising *Menschensohn* 'do not occur in the Sayings Source, but first in Mark'.[55] This position is achieved without considering Aramaic originals, which would be earlier than Mark or Q; and without considering that it might simply be a consequence of Matthew and Luke feeling that they had a central group of predictions already from Mark, so they did not agree at points when they reproduced others (Luke 13.31–3) or edited in such additions as they did feel they needed (Matt. 26.2b). A document without passion predictions has been produced by assuming that some rather disparate material was a document, not by demonstrating anything of the kind. Finally, though it is not expounded as it was to be in later American scholarship, we do meet *die Gemeinde*/the community, which asserts things over against *die Juden*/the Jews.[56] Tödt did not, however, show that there was such a community, and Matthew and Luke have an unambiguous frame of reference which puts Jesus and all his earliest followers among the Jews, not over against them.

We must therefore conclude that Tödt's work was methodologically unsatisfactory from beginning to end. If this book was as important for the study of Q as subsequent Q scholars have asserted, the future of the study of Q was bound to be problematic.

From Robinson to Kloppenborg

It is generally agreed that J. M. Robinson's 1964 essay has been a fundamental influence on subsequent study of Q.[57] Kloppenborg describes it as a 'major step toward the solution of the hermeneutic of Q'.[58] What Robinson is often thought to have done is to have established the genre of Q; it is less often noticed that his apparent establishment of its genre stopped many scholars from troubling over whether Q really existed.

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[54] See further pp. 26–9 below.

[55] Tödt, *Menschensohn*, p. 215; *Son of Man*, p. 235.

[56] Tödt, *Menschensohn*, p. 252; *Son of Man*, p. 277.

[57] J. M. Robinson, 'ΛΟΓΟΙ ΣΟΦΟΝ: Zur Gattung der Spruchquelle Q', in E. Dinkler (ed.), *Zeit und Geschichte. Dankesgabe an Rudolf Bultmann zum 80. Geburtstag* (Tübingen, 1964), pp. 77–96; revised ET '*LOGOI SOPHON*: On the Gattung of Q', in J. M. Robinson and H. Koester (eds.), *Trajectories through Early Christianity* (Philadelphia, 1971), pp. 71–113, and in J. M. Robinson (ed.), *The Future of Our Religious Past: Essays in Honour of Rudolf Bultmann* (London, 1971), pp. 84–130.

[58] Kloppenborg, *Formation of Q*, p. 27.

Evidence that it was not a unity was turned into evidence that *it* had phases of composition and the like. The title of Robinson's essay indicates the *Gattung*, or genre, which he found for Q: λόγοι σοφῶν, which he translates as 'sayings of the sages', or 'words of the wise'. Robinson sought to establish the existence of this genre by referring to a wide variety of sayings collections. While his use of late sources such as the *Apocalypse of Adam* makes one uneasy at times, he established beyond reasonable doubt from documents such as *Ahiqar* and Proverbs that collections of sayings had been made for a long time. Accordingly, the existence of such documents as *m. Abot* and the *Gospel of Thomas* showed that a collection of sayings of Jesus was a possible document: scholars who had maintained that a document in the form of the proposed Q was impossible had been shown to be wrong. That should have been an important gain, and it is regrettable that problems with this genre have prevented it from being such.

The major problem is the nature of this genre itself. If we chop a sonnet in half, we get two halves of a sonnet. A sayings collection is like a worm: if we chop it in two, we get two sayings collections, perhaps a little damaged at the ends. Similarly, if we have an epode and we add another epode, we get two epodes. A sayings collection is like a glass of *Trockenbeerenauslese*: pour it into a bigger glass with another glassful, and we still have one glass of *Trockenbeerenauslese*, and some of us like it better for being bigger; add a sayings collection to another sayings collection and we get one bigger sayings collection. In practice, this meant that all kinds of tricks could be played with Q. It could be thought to have grown in stages, or to have had different versions, merely because of differences in its supposed parts, but these might have belonged to different documents altogether.

Problems should also be found over such passages as Matt. 8.5–10// Luke 7.1–9; Matt. 26.68//Luke 22.64; Matt. 26.75//Luke 22.62, which show that, Mark apart, Matthew and Luke had access to more than 'sayings', or 'words', Robinson's translations of λόγοι in the title of his *Gattung*. The problem is not, however, the Greek word λόγοι itself. In Hebrew, the book of Jeremiah is introduced as דברי ירמיהו; in Greek, the historical account of Nehemiah is introduced as λόγοι Νεεμια (2 Esdras 11.1 LXX); a longer account of the reign of Jehoshaphat is referred to as in ספר דברי הימים למלכי יהודה, βιβλίῳ λόγων τῶν ἡμερῶν τῶν βασιλέων Ιουδα (1 Kings 22.46). It follows that a collection of λόγοι might by no means be confined to sayings: it might include chronicles and prophecy. This makes it all the more necessary to be wary of inferences to and from a supposed genre of λόγοι.

A more serious problem is to be found with the σοφῶν. Not only are λόγοι in general found abundantly without σοφῶν as well as with them; more seriously and surely quite devastatingly, Jesus is *never* described in Matthew or Luke, or in the rest of the New Testament for that matter, as a σοφός. The contents of Q are not altogether those of a wisdom collection either. Neither the narratives noted above, nor the account of John the Baptist, are generally comparable to wisdom collections. We must surely conclude that Robinson set the stage for scholarship to go down a blind alley. The breadth and manoeuvrability of the proposed form is especially to be noted; it will be recalled when we reach chreiae.

The next major development was that of redaction criticism (Redaktionsgeschichte). I propose to examine especially the work of Lührmann, which is said to have had the greatest influence on subsequent research, and that of Schulz, who wrote what is effectively the largest commentary on Q.[59] Lührmann simply assumes that Q was a single document, and never pauses to establish this. He offers no meaningful discussion of Aramaic sources. He consequently never faces the problem posed by the wide variety of agreement and disagreement between Matthew and Luke in Q passages. While he notes the variability of the ordering of the Q material, he simply infers from this that we cannot tell the original position of something in Q; he does not face the problem which this poses for his model of Q. For example, in discussing Luke 11.39–52 and its Matthean parallels, he suggests that some uncertainty about the position of this passage in Q arises from Matthew and Luke positioning it differently with respect to the Marcan context.[60] Yet this passage is not only out of sequence from any perspective of a common order, it has substantial variations between Matthew and Luke, some of which are explicable as due to two translations of Aramaic source material. These problems should have been discussed. Yet the whole book proceeds as if the existence and unity of a single documentary Q had been established, which it had not.

Lührmann's *redaktionsgeschichtliche* judgements are often unsatisfactory too. For example, he declares Luke 7.33–5//Matt. 11.18–19 secondary.[61] This saying contains the expression ὁ υἱὸς τοῦ ἀνθρώπου, the

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[59] D. Lührmann, *Die Redaktion der Logienquelle* (WMANT 33. Neukirchen-Vluyn, 1969), on which I cite the verdict of Tuckett, *Q and the History*, p. 54; S. Schulz, *Q: Die Spruchquelle der Evangelisten* (Zurich, 1972). See also especially P. Hoffmann, *Studien zur Theologie der Logienquelle* (NTA 8. Münster, 1972); A. Polag, *Die Christologie der Logienquelle* (WMANT 45. Neukirchen-Vluyn, 1977).

[60] 'Da sowohl Lk als auch Mt den Komplex jeweils verschieden in den Mk-Kontext einfügen, läßt sich über seine Stellung in Q nichts Sicheres sagen' (Lührmann, *Redaktion*, p. 44).

[61] *Ibid.*, p. 29.

most notorious Semitism in the New Testament, but Lührmann offers no discussion of any possible Aramaic underlay. His supposed reasons for his judgement are quite arbitrary. He lists differences between this passage and the earlier sayings, noting for example that in this passage John and Jesus stand over against each other, without any perceptible devaluation of John.[62] This is true, but tells us nothing about the authenticity of any saying. Lührmann then focusses on the very last line (Luke 7.35), but the difficulties of this problematical saying cannot make the Son of man saying inauthentic. Lührmann then proceeds to turn 'this generation' (Matt. 11.16//Luke 7.31) into the people of Israel (das Volk Israel), so that the criticism of 'this generation' becomes the opposition between John and Jesus on the one hand and the people of Israel on the other.[63] The *Sitz im Leben* of this judgement is in German rejection of Jews, not in first-century Israel, where both John and Jesus carried through successful popular ministries in Israel. This becomes quite a theme,[64] but it is not justified by the primary source material.

Lührmann's decision to regard Luke 11.49–51//Matt. 23.34–6 as redactional is equally unsatisfactory. Lührmann begins his argument for Q redaction with the observation that this passage does not correspond to the woes (den Weherufen) in form or content.[65] This requires a source to have a degree of form-critical uniformity which is quite remote from existing texts. In Matthew, this passage forms a suitable conclusion to the woes which have gone before it, while Luke has moved one of them to form a narrative ending; the perfectly true observation that this passage is not the same as the woes cannot tell us whether it was the work of Jesus or an earlier editor or the final redaction of Q, if there was one. Lührmann adds the use of 'this generation' (Matt. 23.36//Luke 11.51), but we have seen that his interpretation of this is not satisfactory.

Lührmann begins a chapter labelled 'The Community (Die Gemeinde)' by suggesting that there are Q sayings which permit inferences about the self-understanding of the community which stood behind the redaction of Q.[66] Unfortunately, however, Lührmann has never demonstrated that there was a single Q, let alone a single community behind it. Consequently, this is not what any of his observations demonstrate. For example, he uses the word 'harvest' at Matt. 9.37–8//Luke 10.2 to show that there were Gentiles in the Q community.[67] But the metaphorical use of so normal a

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[62] 'Hier stehen also Johannes und Jesus nebeneinander, ohne daß eine Abwertung des Johannes zu erkennen ist' (*ibid.*, p. 29).

[63] 'Der Gegensatz zwischen Johannes und Jesus einerseits . . . und dem Volk Israel' (*ibid.*, pp. 30–1).

[64] E.g. *ibid.*, pp. 43, 68, 93.   [65] *Ibid.*, p. 45.   [66] *Ibid.*, p. 49.   [67] *Ibid.*, p. 60.

phenomenon cannot be controlled by selected biblical texts. Jesus' ministry was directed at Jews, who must therefore surely be in mind, as Matt. 10.5 says and Luke evidently assumed. The sayings are therefore likely to be genuine. Moreover, even if this passage were secondary, it could only tell us about the Q community if it had already been demonstrated that there was a single Q produced by a single community.

That this book should have been so influential indicates how bureaucratised the study of Q had already become. The basic assumptions of the existence of one document, and of a community behind it, were simply taken for granted, without any attempt to demonstrate them. The document was then treated as if it had been a Gospel like Matthew, Mark and Luke, and subjected to the *redaktionsgeschichtliche* procedures then fashionable among academics.

Similar comments apply to the commentary on Q by Schulz. Schulz does not at any point argue that Q was a single document. He orders the Q material into two layers (Schichten). He labels his discussion of the older one 'The Kerygma of the Jewish-Christian Q Community (Das Kerygma der judenchristlichen Q-Gemeinden)', and of the other one 'The Kerygma of the Later Syrian Q-Community (Das Kerygma der Jüngeren Q-Gemeinde Syriens)'. He declares that these two are *traditionsgeschichtlich*, *formgeschichtlich* and *religionsgeschichtlich* not a unity.[68] Nonetheless, he never stops to show that the document was a unity, that either of the proposed layers was a documentary unity, nor that there was a community behind either of them. His *Forschungsberichte* is weighed down with previous German scholarship, the judgements of which are repeated in numerous footnotes throughout the book. Like that of Lührmann, this learning demonstrates only the bureaucratisation of scholarship, since many of the judgements are arbitrary and the main points, the existence of a document and the two communities behind it, were never demonstrated.

Schulz discusses the different passages of Q in an order all his own. He never justifies the validity of this order, to the point where it is not fully clear that it is meant to be an original order, or if so, of what (each layer?). He knows that the order of the material is sometimes uncertain. For example, he begins his discussion of Matt. 23.37–39//Luke 13.34 by saying that it belongs to the Q material, even if its position in the context of the Q source can no longer be determined. A massive footnote retails the judgements of previous scholars, giving reasons why Matthew and/or Luke should have moved this passage to these two places, and the

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[68] Schulz, *Q*, e.g. pp. 57, 177, 481.

like.[69] At no stage, however, does he stop to demonstrate that there was
a common order from which this piece was twice displaced. If Matthew
and Luke be supposed to have moved it to its present places, why should
we not believe that they each inserted a *separate* saying in these places?

Schulz mentions Aramaic only occasionally. He omits it for example
from his discussion of Matt. 12.32//Luke 12.10. He declares Matthew's
κατά secondary, on the kind of statistical comparison only with the other
synoptics which is a feature of his work – Matthew likes κατά + genitive
(sixteen times; Mark, eight ; Luke, six).[70] He does not even consider the
possibility that it was selected as a translation variant, not even because
Matthew liked it. He declares the Marcan variant secondary, with a long
note on previous scholarship but without consideration of the possibility
that this too is an alternative translation, perhaps indeed an edited one.[71]
Yet Matt. 12.32 was reconstructed by Meyer![72] In this way Schulz re-
frained from discussing the possibility that some parts of the Q material
derived from more than one translation of an Aramaic source.

Failure to deal properly with Aramaic evidence is generally associated
with other arbitrary features. For example, he begins his discussion of
verses from Matt. 23 and Luke 11 by declaring that they are prophetic
threat-oracles (prophetischen Drohsprüche), originally independent woe-
oracles of early Christian prophets (selbständige Wehesprüche urchristli-
cher Propheten).[73] While similar comments are made in a more general
way in his introduction,[74] Schulz does not at any point justify the
putting of sayings in this category, nor even the existence of early
Jewish-Christian prophets who created sayings of Jesus and transmitted
them as such. He proceeds to reject Wellhausen's suggestion that Luke
11.41 τὰ ἐνόντα δότε ἐλεημοσύνην resulted from mistranslation of an
Aramaic source, but he simply declares that Haenchen and others were
right about this, without discussing the Aramaic. In the very next verse,
he declares the proposal that τὸ πήγανον resulted from a misreading of
שבתא = ἄνηθον as שבדא (sic!) unnecessary, without discussion of whether
an argument of cumulative weight might be formed from different
suggestions.[75] This is especially important in this particular passage,
since the degree of verbal agreement is lower, and this might be due to
two different translations, which would ruin Schulz's assumption of an
entirely Greek Q. In the next Matthean phrase, τὰ βαρύτερα τοῦ νόμου
(Matt. 23.23), he declares τοῦ νόμου redactional on the ground that it is

[69] *Ibid*., p. 347.   [70] *Ibid*., p. 246, with n. 476.   [71] *Ibid*., p. 247.
[72] Meyer, *Jesu Muttersprache*, p. 94; see above, p. 3.   [73] Schulz, *Q*, p. 94.
[74] *Ibid*., pp. 57ff.   [75] *Ibid*., pp. 96 (with n. 20), 100.