

T H R E E

## GOULDER AND THE NEW PARADIGM

A Critical Appreciation of Michael Goulder  
on the Synoptic Problem

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My first meeting with Michael Goulder was at the 1980 meeting of the Studiorum Novi Testamenti Societas in Toronto, but my first encounter was in 1975 when I had just begun graduate studies with a course on Matthean redaction, offered by one of Rudolph Schnackenburg's *Doktor-kinder*. Needless to say, perhaps, Goulder's *Midrash and Lection in Matthew* was not on the reading list. Nevertheless, I stumbled upon it in the library, read it, and although he did not persuade me of his Mark-without-Q world, he had a wealth of important and helpful insights into Matthew's editorial procedures. When I related some of these to my mentor, I vaguely realized that I had stumbled into the realm of heresy. Würzburg and Birmingham were evidently farther removed from one another than Birmingham and Toronto.

It was when I began teaching a regular doctoral seminar on the Synoptic Problem that I came to appreciate Goulder's contribution — second only to William R. Farmer's — in keeping the Synoptic Problem a vital issue. I fear that this essay might seem a shade more critical than appreciative, but it is offered here in the spirit of the serious and careful discussion that characterizes Goulder's own works.

Two scholars have been responsible in large measure for keeping the Synoptic Problem an interesting and debated issue, William R. Farmer (along with the team of scholars he has assembled) and Michael Goulder.

Farmer's 1964 volume *The Synoptic Problem* surveyed scholarship on the Synoptic Problem from Lessing and Griesbach to B. H. Streeter, pointing out the places where proponents of various source theories drew fallacious conclusions, where they failed to consider fully the merits of alternate solutions, or where they engaged in spurious argumentation.<sup>1</sup> Critical of the consensus that had formed in the late nineteenth century around the Two Document hypothesis (2DH), Farmer offered a defense of the Owen-Griesbach hypothesis, which suggested that Matthew was the first Gospel, that Luke used Matthew directly, and that Mark conflated Matthew and Luke.<sup>2</sup> A decade later, Goulder revived a theory of Austin Farrer,<sup>3</sup> which, like the 2DH, maintained Markan priority and Matthew's use of Mark, but which, like Griesbach, posited Luke's direct dependence on Matthew. This obviated the need to posit a sayings source, since Luke's double tradition material came directly from Matthew. Goulder developed this interesting thesis in two major monographs (*MLM*; *LNP*) and a long series of journal articles.<sup>4</sup>

Goulder's thesis has eight components (adapted from *LNP* 22–23):

1. W. R. Farmer, *The Synoptic Problem: A Critical Analysis* (New York: Macmillan, 1964; repr. Dillsboro: Western North Carolina Press, 1976).

2. The hypothesis now goes by the name Two Gospel hypothesis (2GH). The research team assembled by Farmer has published a defense of Luke's treatment of Matthew on the 2GH: A. J. McNicol, D. L. Dungan, and D. B. Peabody, *Beyond the Q Impasse — Luke's Use of Matthew: A Demonstration by the Research Team of the International Institute for Gospel Studies* (Valley Forge, Pa.: Trinity, 1996); and will soon publish an account of Mark's conflation of Matthew and Luke.

3. A. M. Farrer, "On Dispensing with Q," in *Studies in the Gospels in Memory of R. H. Lightfoot* (ed. D. E. Nineham; Oxford: Blackwell, 1955), 57–88; repr. in *The Two-Source Hypothesis: A Critical Appraisal* (ed. A. J. Bellinzoni; Macon, Ga.: Mercer University Press, 1985), 321–56.

4. "St. Luke's Genesis" (with M. L. Sanderson), *Journal of Theological Studies* n.s. 8 (1957): 12–30; "The Composition of the Lord's Prayer," *Journal of Theological Studies* n.s. 14 (1964): 32–45; "Characteristics of the Parables in the Several Parables," *Journal of Theological Studies* n.s. 19 (1968): 51–69; "Putting Q"; "Mark xvi.1–8 and Parallels," *New Testament Studies* 24 (1978): 235–40; "Farrer on Q," *Theology* 83 (1980): 190–95; "Some Observations on Professor Farmer's 'Certain Results,'" in *Synoptic Studies: The Ampleforth Conferences of 1982 and 1983* (ed. C. M. Tuckett; Journal for the Study of the New Testament Supplement 7; Sheffield: JSOT Press, 1984), 99–104 (Farmer's reply on 105–9); "Crank"; "A House Built on Sand," in *Alternative Approaches to New Testament Study* (ed. A. E. Harvey; London: SPCK, 1985), 1–24; "Luke's Compositional Options," *New Testament Studies* 39 (1993): 150–52; "Luke's Knowledge of Matthew," in *Minor Agreements: Symposium Göttingen 1991* (ed. G. Strecker; Göttinger theologische Arbeiten 50; Göttingen: Vandenhoeck & Ruprecht, 1993), 143–60; "The Pre-Markan Gospel," *Scottish Journal of Theology* 47 (1994): 453–71; "Juggernaut"; "Self-Contradiction in the IQP?" *Journal of Biblical Literature* 118 (1999): 506–17.

1. Part of Mark is based on reliable Jesus tradition; it is doubtful, however, that such is present in the non-Markan sections of the other Gospels.
2. Markan traditions were collected and transmitted by the Jerusalem community under Peter, James, and John, who amplified and eroded them.
3. These traditions were written by Mark approximately 70 c.E., with further editorial additions and attenuations.
4. There is no other collection of sayings, such as Q.
5. Matthew wrote his Gospel approximately 80 c.E. as an expansion of Mark for a Jewish-Christian community. Matthew was a highly creative author, elaborating Mark freely, (largely) without dependence on other tradition, oral or written. Matthew's Gospel is structured around a cycle of lections.<sup>5</sup>
6. Luke wrote his Gospel approximately 90 c.E. for a Gentile Christian community, combining Mark and Matthew. He rewrote Matthew's birth narratives with the aid of the Hebrew Bible and freely elaborated Matthew.
7. John wrote approximately 100 c.E. for a community in Roman Asia, drawing on all three Synoptics (especially Matthew) and elaborating them freely.
8. The Gospel of Thomas is a gnosticizing version of the Synoptics (especially Luke) and contains no historical tradition or material pertinent to the understanding of the development of the Synoptics.

A few preliminary comments are in order. First, in terms of the logical architecture of his thesis, Goulder's fourth point (on Q) is actually a corol-

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5. The lectionary theory of Matthew is presented in M. Goulder, *Midrash and Lection, the Evangelists' Calendar: A Lectionary Explanation of the Development of Scripture* (Speaker's Lectures in Biblical Studies 1972; London: SPCK, 1978); and idem, "Matthew's Gospel Round the Year," in this volume. This element of Goulder's thesis has attracted much criticism, since evidence of a fixed *siddur* of pentateuchal readings in the first century is highly ambiguous. See M. S. Goodacre, *Goulder and the Gospels: An Examination of a New Paradigm* (Journal for the Study of the New Testament Supplement 133; Sheffield: JSOT Press, 1996), 330–39: "Most of Goulder's evidence for the reading of the Pentateuch in an annual cycle beginning in Nisan is indirect and at best suggestive rather than probative" (339).

lary of his sixth: if a case can be sustained for Luke's direct use of Matthew, there is no need to posit a non-Markan source in order to account for the double tradition material in Luke. Second, his seventh point (on John) is not directly relevant to a discussion of the Synoptic Problem, although his assessment of the relative amount of historical tradition in John depends on a convincing defense of the thesis of Johannine dependence on the Synoptics.<sup>6</sup> Third, the parallel issue of Thomas's relationship to the Synoptics is more crucial to Goulder's thesis, simply because of the extent of Synoptic-like material in Thomas. If Thomas were to contain sayings parallel to Matthew's non-Markan material — for example, Thomas's parable of the weeds (Gospel of Thomas 57) — that could not be shown to be dependent upon Matthew (hence, logically pre-Matthean), it would not be so simple for Goulder to account for the parable as a Matthean elaboration of Mark 4:26–29 (thus Goulder, *MLM* 367–69). Rather than offering his own analysis of the Thomas tradition, Goulder merely refers the reader to the works of Schrage and Ménard. This is unfortunate, since both are badly outdated and the case for independent tradition in Thomas is much stronger than I think Goulder suspects.<sup>7</sup> Independent tradition in Thomas

6. The case for John's dependence on the Synoptics has found more defenders of late (e.g., F. Neirynck, "John and the Synoptics: 1975–1990," in *John and the Synoptics* [ed. A. Denaux; Bibliotheca ephemeridum theologiarum lovaniensium 101; Leuven: Peeters/Leuven University Press, 1992], 3–62), but is still far from *opinio communis*. See D. M. Smith, *John among the Gospels: The Relationship in Twentieth-Century Research* (Minneapolis: Fortress, 1992).

7. Goulder seems to have in mind W. Schrage's *Das Verhältnis des Thomas-Evangeliums zur synoptischen Tradition und zu den koptischen Evangelienübersetzungen* (Beihefte zur Zeitschrift für die neutestamentliche Wissenschaft 29; Berlin: Töpelmann, 1964) and J. E. Ménard's *L'évangile selon Thomas* (Nag Hammadi Studies 5; Leiden: Brill, 1975) (although he quotes neither) and refers to Schrage's thesis that the Coptic version of Thomas shows affinities with the Coptic versions of the New Testament. He fails, however, to note Schrage's later article, "Evangelienzitate in den Oxyrhynchus-Logien und im koptischen Thomas-Evangelium," in *Apophoreta: Festschrift für Ernst Haenchen zu seinem siebenzigsten Geburtstag* (ed. W. Eltester and F. H. Kettler; Beihefte zur Zeitschrift für die neutestamentliche Wissenschaft 30; Berlin: Töpelmann, 1964), 251–68, which found the Oxyrhynchus fragments not to resemble the Greek New Testament as closely. This clearly opens the possibility of secondary assimilation of Coptic Thomas to the Coptic versions of the New Testament (or vice versa). For more recent discussions of the problem, see F. T. Fallon and R. Cameron, "The Gospel of Thomas: A Forschungsbericht and Analysis," in *Aufstieg und Niedergang der römischen Welt* 2.25.6 (1988): 4195–4251; C. W. Hedrick, "Thomas and the Synoptics: Aiming at a Consensus," *Second Century* 7 (1990): 39–56; C. M. Tuckett, "Das Thomasevangelium und die synoptischen Evangelien," *Berliner Theologische Zeitschrift* 12 (1995): 185–200; S. J. Patterson, "The Gospel of Thomas and the Synoptic Tradition: A Forschungsbericht and Critique," *Forum* 8 (1992): 45–97; idem, *The Gospel of Thomas and Jesus* (Foundations and Facets: Reference Series; Sonoma, Calif.: Polebridge, 1993). M. Fieger's comments on the issue in *Das Thomasevangelium: Einleitung, Kommentar und Systematik* (Neutestamentliche Abhandlungen 22; Münster: Aschendorff, 1991) are largely derivative of Schrage's older work.

might not overturn Goulder's thesis, but it would no doubt require added nuance.

The inferences that Goulder draws from his thesis are bold. First, he does away with pre-Gospel sources. The Q document disappears as a result of his theory of Lucan dependence on Matthew. Even more strikingly, Goulder's view of Matthew and Luke as creative, even inventive, editors of Mark (and, in the case of Luke, of Matthew) eliminates the need for special Matthean (M) or Lucan (L) sources. While it is true that few contemporary critics think M or L to be discrete documents, Goulder does away with any need to appeal to pre-Matthean or pre-Lucan traditions. Thus Mark and Mark alone is the "beginning of the Gospel." Second and correspondingly, the degree of literary creativity in the later Gospels (and perhaps even in Mark) is very high. Not only is there no need for written documents such as Q, M, or L; there is little or nothing prior to or independent of Mark. Goulder's view of Luke's editorial procedure is consistent with the supposition of literary ingenuity, for he posits a Luke with extraordinary freedom in rewriting, manipulating, and rearranging his source material. One might observe that Goulder's thesis, if correct, would make reconstruction of the historical Jesus a rather simpler matter than is now usually imagined; or at least, he leaves us with fewer sources to worry about.

Goulder's thesis is articulated and defended with a combination of logic and brilliant rhetoric. It is perhaps this combination that accounts for the fact that in the United Kingdom at least, few scholars can take the 2DH as a given, and those who write on Q must at the same time be engaged in a defense of the 2DH; when they look over their shoulders, Goulder is always there.<sup>8</sup> Goulder has proved a formidable force in the British

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8. Several important British studies of the Synoptic Problem defend the 2DH: C. M. Tuckett, *The Revival of the Griesbach Hypothesis: An Analysis and Appraisal* (Society for New Testament Studies Monograph 44; Cambridge: Cambridge University Press, 1983); M. D. Hooker, "The Son of Man and the Synoptic Problem," in *The Four Gospels 1992: Festschrift Frans Neirynck* (ed. F. Van Segbroeck, C. M. Tuckett, G. Van Belle, and J. Verheyden; Bibliotheca ephemeridum theologicarum lovaniensium 100; Leuven: Leuven University Press/Peeters, 1992), 189–201; P. M. Head, *Christology and the Synoptic Problem: An Assessment of One Argument for Markan Priority* (Society for New Testament Studies Monograph 94; Cambridge: Cambridge University Press, 1997). Q specialists in the United Kingdom are, in comparison with North America and Germany, relatively few: D. R. Catchpole, F. G. Downing, R. A. Piper, and C. M. Tuckett are the most prominent. C. L. Mearns and G. N. Stanton have also written on Q.

New Testament establishment; and, though he complains of the lack of adherents his thesis has attracted, his influence is far from negligible.

Goulder calls his proposal a “new paradigm,” expressly invoking a term popularized by Thomas Kuhn in *The Structure of Scientific Revolutions*.<sup>9</sup> In this critical reflection, I would like to ask three questions: What does Goulder mean by “paradigm”? Is Goulder’s paradigm new? Does Goulder’s thesis offer a compelling critique of the 2DH?

### What Does Goulder Mean by “Paradigm”?

Ever since the publication of Kuhn’s volume, the term *paradigm* has become rather a buzzword among theological writers. A quick consultation of the ATLA database shows that the word appears in the titles and abstracts of one thousand monographs and articles published since 1980, almost twelve hundred since 1970. Though a handful of these discuss the paradigms in the Greek and Hebrew verbal systems, many others expressly invoke Kuhn by announcing “shifting,” “new,” and “emerging” paradigms.

Some of these usages, no doubt, are rhetorical attempts to lend some unearned legitimacy to novel theses by claiming that they are part of “new” or “emerging” paradigms. The rhetorical appeal of the term is obvious. No one, after all, would wish to adhere to the old and dying view that combustion and rusting involves the release of phlogiston when Lavoisier’s new paradigm of oxidation, reduction, and acidity is on the horizon. Nevertheless, it should be remembered that for Kuhn a paradigm is not merely an idea or a theory, but an achievement involving theories and practices (new ways of approaching the phenomena and new equipment) and promoting a new research program. The new research program in turn suggests new puzzles as well as solutions.<sup>10</sup> New paradigms, moreover, are more easily recognized with hindsight than at the moment of their birth,<sup>11</sup> precisely

9. T. S. Kuhn, *The Structure of Scientific Revolutions* (2d ed.; International Encyclopedia of Unified Science, Foundations of the Unity of Science 2/2; Chicago: University of Chicago Press, 1970).

10. See C. Strug, “Kuhn’s Paradigm Thesis: A Two-edged Sword for the Philosophy of Religion,” *Religious Studies* 20 (1984): 269–79, esp. 270.

11. As Kuhn notes, it is sometimes even difficult to identify the point at which a paradigm-shifting discovery occurs, as in the case of the discovery of oxygen in the 1770s. See *Structure of Scientific Revolutions*, 53–56.

because it takes some time for the paradigm to take hold in theory, in practices, and in the discursive modes adopted by practitioners.

Hence, advertisements of the birth of new paradigms (and the death of old ones) are always likely to be a bit premature. To be fair to Goulder, however, his rehabilitation of the Farrer theory, even if it has not attracted a significant group of practitioners, has been articulated over the course of twenty-five years and has achieved a practical embodiment in two significant commentaries, one on Matthew and another on Luke. It is not clear to me, nevertheless, that Goulder's theory has done what Kuhnian paradigms ought to do, namely, to suggest a new set of problems to be solved. And as I shall suggest below, Goulder's theory involves no substantially new practices or tools: Goulder remains a redaction critic, and a fine one at that, employing the standard tools of the trade in a relatively conventional manner.

### ***Falsifiability: Goulder's Popperian View***

In order to probe more deeply into what Goulder means by paradigm, it is useful to examine the opening paragraphs of *Luke: A New Paradigm*. There he refers to both Karl Popper<sup>12</sup> and Kuhn, arguing (with Popper) that knowledge progresses by conjectures, which are then subjected to deductive testing that attempts to refute them. For Popper, conjectures that survive are not thereby shown to be certainly true or even as probably correct, but they do appear to us to be better approximations of the truth than competing conjectures which fail deductive testing.<sup>13</sup> Falsifiability — the susceptibility of a hypothesis to refutation by means of empirical observations — is for Popper what sets scientific conjectures apart from nonscientific ones.<sup>14</sup>

12. K. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959 [first English ed.], 1968 [rev. ed.]); idem, *Conjectures and Refutations: The Growth of Scientific Knowledge* (London: Routledge & Kegan Paul, 1963).

13. Popper, *Conjectures and Refutations*, vii; idem, *Logic of Scientific Discovery*, 32–33.

14. This is Popper's criterion of demarcation between scientific and nonscientific (and metaphysical) statements: "I proposed that *refutability or falsifiability* of a theoretical system should be taken as the criterion of demarcation. According to this view, which I still uphold, a system is to be considered as scientific only if it makes assertions which may clash with observations; and a system is, in fact, tested by attempts to produce such clashes, that is to say by attempts to refute it. Thus testability is the same as refutability, and can therefore likewise be taken as a criterion of demarcation" (*Conjectures and Refutations*, 256; emphasis original).

Goulder describes the 2DH as a paradigm or “complex of hypotheses” that includes a conjectured sayings source Q and the hypothesis of the independence of Matthew and Luke. Since on most accounts Q’s contents did not include material parallel to the Markan passion narrative,<sup>15</sup> the 2DH implies that, while in Mark 1–13 there might be Matthew-Luke agreements against Mark that result from their incorporation of Q into the Markan framework, there should be no significant agreements of Matthew and Luke against Mark in the passion narrative. This is a simple deductive test, and one which, apparently, the 2DH fails: there is at least one significant minor agreement, Matt 26:67–68 || Luke 22:63–64 against Mark 14:65 (see n. 35 below).

I will return to the issue of the minor agreements later, but for the moment it is important to observe that Goulder’s understanding of a paradigm corresponds to Popper’s idea of the “problem situation” or “framework” into which the scientist fits her own work.<sup>16</sup> For Popper, it is a truism that the scientist works within a definite theoretical framework; but he emphasizes that “at any moment” the scientist can challenge and break out of that framework.<sup>17</sup> Popper in fact rejects Kuhn’s model of a single dominant, controlling paradigm. On the contrary, multiple competing theories exist at any given time; they are generally commensurable; and, if scientific, they are falsifiable. For Popper, science progresses in a continuous process of conjectures and refutations, with multiple conjectures vying for dominance.

This erodes Kuhn’s distinction between normal and extraordinary science, between those phases of scientific research generally informed on the one hand by one broad conceptual paradigm, related practices, and commending a certain research program and on the other hand by transitional

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15. Goulder’s statement is much stronger: “Now Q is *defined* as a body of sayings material and some narrative, beginning from the preaching of John and ending before the Passion” (LNP 6 [emphasis added]); and “there is no Q in the Passion story *ex hypothesi*.” This misleadingly makes it seem that the definition of Q is stipulative and, correspondingly, makes its refutation analytic (based on a simple understanding of the meaning of terms) rather than *a posteriori*. In fact, Q is only “defined” as the non-Markan source of Matthew and Luke. It *turns out* not to have passion sayings, but this is not part of the definition of Q.

16. K. Popper, “Normal Science and Its Dangers,” in *Criticism and the Growth of Knowledge* (ed. I. Lakatos and A. Musgrave; Proceedings of the International Colloquium in the Philosophy of Science 4; Cambridge: Cambridge University Press, 1970), 51–58, esp. 51.

17. *Ibid.*, 56.

periods when a prevailing paradigm has become problematized due to a critical mass of uncooperative data and anomalous observations, which eventually lead to its displacement by a new paradigm. Indeed Popper understands Kuhn's term *normal science* in a pejorative sense, connoting the activity of the "nonrevolutionary" and "not-too-critical professional," "the science student who accepts the ruling dogma of the day."<sup>18</sup>

Paradigm, for Goulder, has a Popperian ring, despite the fact that the subtitle of his Luke book advertises a new paradigm. To be sure, Goulder takes from Kuhn the idea that dominant paradigms often accommodate anomalies by making ad hoc adjustments. But while Kuhn takes such adaptations to be a standard and necessary part of normal science,<sup>19</sup> Goulder sees the attempts of the 2DH to accommodate anomalies by adjusting the theory — positing other intermediate documents, or oral tradition, or textual corruption — as leading to an "elastic," unfalsifiable, and therefore unscientific hypothesis. He observes, moreover, that paradigms resist displacement because researchers have invested careers in research programs informed by those paradigms. For Goulder, as for Popper, paradigms ought to be susceptible to deductive testing, which could well lead to their immediate collapse. The fact that they do not collapse is a symptom of the dangers of normal science.<sup>20</sup> Thus he bemoans the fact that generations

18. Ibid., 52. Similarly, *Logic of Scientific Discovery*, 50: "A system such as classical mechanics may be 'scientific' to any degree you like; but those who uphold it dogmatically — believing, perhaps, that it is their business to defend such a successful system against criticism as long as it is not *conclusively disproved* — are adopting the very reverse of that critical attitude which in my view is the proper one for the scientist. In point of fact, no conclusive disproof of a theory can ever be produced; for it is always possible to say that the experimental results are not reliable, or that the discrepancies which are asserted to exist between the experimental results and the theory are only apparent and that they will disappear with the advance of our understanding. If you insist on strict proof (or strict disproof) in the empirical sciences, you will never benefit from experience, and never learn from it how wrong you are."

19. T. S. Kuhn, "Logic of Discovery or Psychology of Research?" in *Criticism and the Growth of Knowledge* (ed. I. Lakatos and A. Musgrave; Proceedings of the International Colloquium in the Philosophy of Science 4; Cambridge: Cambridge University Press, 1970), 1–23, esp. 13: "It is important, furthermore, that this should be so, for it is often by challenging observations or adjusting theories that scientific knowledge grows. Challenges and adjustments are a standard part of normal research in empirical science, and adjustments, at least, play a dominant role in informal mathematics as well." Contrast Popper's statement in *Logic of Scientific Discovery*, 42: "For it is always possible to find some way of evading falsification, for example by introducing *ad hoc* an auxiliary hypothesis, or by changing *ad hoc* a definition. It is even possible without logical inconsistency to adopt the position of simply refusing to acknowledge any falsifying experience whatsoever. Admittedly, scientists do not usually proceed in this way, but logically such procedure is possible; and this fact, it might be claimed, makes the logical value of my proposed criterion of demarcation dubious, to say the least."

20. Popper, "Normal Science," 52–53. Goulder (*LNP* 4) asserts that Kuhn's use of "normal science"

of graduate students internalize and then perpetuate a paradigm which, he thinks, is logically flawed (“Juggernaut,” 668). Throughout his introduction to *Luke: A New Paradigm*, he refers to adherents of the 2DH with the apparently pejorative term *paradigmers*.

Kuhn’s reply to Popper is that paradigms, precisely because of their complexity, can seldom be cast in a form that is susceptible to the sort of deductive refutation that Popper (or Goulder) seeks. Tolerance of anomalies is not a special characteristic of paradigms that are about to collapse or of paradigms that irrationally resist falsification — it is a mark of all paradigms. For example, Schrödinger wave mechanics and his wave equation produce coherent results with electrons traveling much below the speed of light; but at higher velocities, and for other particles traveling near light-speed, Schrödinger’s equation does not work. The equation produces coherent results for electrons in the hydrogen atom, but not for atoms that have a more complex constellation of electrons and orbitals. But this is not a reason to abandon the Schrödinger wave equation. Nor does the fact that light and other electromagnetic radiation exhibit some of the features of particles (e.g., mass equivalence and momentum) imply that the wave theory of light is unusable. The wave theory continues to account for much observable data and serves as the basis for the construction of optical theory.

It is for reasons such as these that paradigms in Kuhn’s sense do not collapse when faced with bits of anomalous data. An old paradigm is declared invalid only when anomalies have accumulated to the extent that the paradigm cannot bear their weight and when a new candidate emerges to take its place. The choice to abandon one is simultaneously a choice to embrace another.<sup>21</sup>

Goulder’s Popperian perspective on the issue of the falsification of sci-

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is pejorative, but this seems to be a case of reading Kuhn via Popper. Kuhn (*Structure of Scientific Revolutions*, 65) in fact sees normal science not as antithetical to extraordinary science, but as embodying the practices that lead to its own displacement: “By ensuring that the paradigm will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm change will penetrate existing knowledge to the core. The very fact that a significant scientific novelty so often emerges simultaneously from several laboratories is an index both to the strongly traditional nature of normal science and to the completeness with which that traditional pursuit prepares the way for its own change.”

21. Kuhn, *Structure of Scientific Revolutions*, 77.

entific hypotheses and his Popperian sense of paradigms helps to account for the strategy he adopts in criticizing the 2DH: he proposes simple deductive tests — principally, the presence of minor agreements and the presence of Matthean vocabulary in Luke — with the expectation that such tests should refute the theory, leaving place for his own. This is also why he prefers as dialogue partners the so-called hard-liners Neiryck and Tuckett, who respond to the issue of the minor agreements by suggesting either coincidental redaction by Matthew and Luke or textual corruption.<sup>22</sup> This is in contrast to soft-liners, who appeal to multiple recensions of Mark, the interference of oral tradition, or intermediate Gospels. The hard-line position, because it requires its exponents to be able to supply plausible redaction-critical reasons for a coincidental agreement in altering Mark or to point to an early and reliable manuscript that eliminates the minor agreement, is, Goulder believes, easily falsifiable. This argumentative strategy can be seen not only in *Luke: A New Paradigm* but also in many of Goulder's articles and essays. Thus, while he invokes Kuhn, the key issue for Goulder is Popperian falsification. The existence of a paradigm accounts only for the resistance encountered by new proposals. It is a term connoting institutional inertia.<sup>23</sup>

I wish to suggest two reasons why Goulder's criticism of the 2DH has not proved effective and his own theory has not been embraced — a fact conceded by Goulder himself. First is that Goulder's Popperian view of falsification is not shared by most of those he criticizes. The 2DH is believed to be able to accommodate anomalies, just as Kuhn's paradigms routinely both produce and accommodate anomalies in the course of the puzzle solving of normal science. The 2DH is not perceived as imperiled by the problem of the minor agreements, and it provides a generally coherent

22. On the notorious minor agreement at Mark 14:65, see C. M. Tuckett, "On the Relationship between Matthew and Luke," *New Testament Studies* 30 (1984): 130–42; and F. Neiryck, "Τίς ἐστὶν ὁ παίσας σε: Mt 26,68/Lk 22,64 (diff. Mk 14,65)," *Ephemerides theologicae lovanienses* 63 (1987): 5–47; repr. with an additional note in *Evangelica II: 1982–1991 Collected Essays* (ed. F. Van Segbroeck; Bibliotheca ephemeridum theologiarum lovaniensium 99; Leuven: Leuven University Press, 1991), 94–138.

23. Goulder ("Juggernaut," 668–69) ventures that modern support for the 2DH is due to a combination of inertia, personal attachments, lack of academic integrity in admitting "that [proponents] have been wrong for years" and the daunting mass of scholarly literature on the topic that few can read or master. Missing from Goulder's list is the possibility that some embrace the 2DH on the basis of a careful examination of Synoptic data and the explanations that best account for those data.

account of a host of Synoptic data. Simply put, the 2DH is still an effective hypothesis. The second reason is that Goulder's thesis produces its own puzzles having to do with Luke's editorial procedures that Goulder has not sufficiently addressed. I will return to some of these later, but for the moment I would like to comment on falsification and the 2DH.

### *Are Synoptic Theories Falsifiable?*

When it was first formulated, the 2DH — or at least the supposition of (Ur-)Markan priority and the existence of sayings source — was advertised as incorrigible, beyond falsification. Albert Schweitzer, Holtzmann's erst-while student at Strasbourg, is a good example of such rhetorical bravado: "The [Markan] hypothesis has a literary existence, indeed it is carried out by Holtzmann to such a degree of demonstration that it can no longer be called a mere hypothesis."<sup>24</sup> A half century later Willi Marxsen echoed this:

This Two-Sources theory has been so widely accepted by scholars that one feels inclined to abandon the term "theory" (in the sense of "hypothesis"). We can in fact regard it as an assured finding — but we must bear in mind that there are inevitable uncertainties as far as the extent and form of Q and the special material are concerned.<sup>25</sup>

This is a grave logical mistake that has now been exposed, thanks in part to the efforts of John Chapman, B. C. Butler, W. R. Farmer, and Michael Goulder himself. Happily, it is difficult to find such inflated rhetorical claims made by contemporary adherents of the 2DH. The 2DH is and remains a hypothesis. Deductive testing might refute a hypothesis, but it can never prove one or establish it as fact.

Ironically, perhaps, proponents of other hypotheses have sometimes succumbed to the temptation of similar rhetoric. For example, in the

24. A. Schweitzer, *The Quest of the Historical Jesus* (preface by F. C. Burkitt; New York: Macmillan, 1910; repr. 1968), 202.

25. W. Marxsen, *Introduction to the New Testament: An Approach to Its Problems* (Philadelphia: Fortress, 1968), 118. Similarly, W. G. Kümmel (*Introduction to the New Testament* [rev. ed.; Nashville: Abingdon, 1975], 64) asserts that Luke's direct dependence on Matthew was "completely inconceivable" (*völlig undenkbar*), but acknowledges the existence of proponents of just this view: K. H. Rengstorff, A. Schlatter, J. H. Ropes, B. C. Butler, A. M. Farrer, N. Turner, W. R. Farmer, A. W. Argyle, R. T. Simpson, W. Wilkens, and E. P. Sanders.

defense of the 2GH offered by McNicol and his colleagues, McNicol repeatedly describes data in Luke (either the sequence of Lucan materials or certain phrases or words) as “evidence” of Luke’s direct use of Matthew.<sup>26</sup> It is not evidence or proof; rather, it is data for which the 2GH may offer a plausible accounting, but for which the 2GH is not the only plausible accounting. A yet more blatant example of rhetorical overstatement comes from Goulder’s mentor, Austin Farrer:

The Q hypothesis is a hypothesis, that is its weakness. To be rid of it we have no need of a contrary hypothesis, we merely have to make St. Luke’s use of St. Matthew intelligible; and to understand what St. Luke made of St. Matthew we need no more than to consider what St. Luke made of his own book. Now St. Luke’s book is not a hypothetical entity. Here is a copy of it on my desk.<sup>27</sup>

This is a sleight of hand. Luke indeed “existed” on Farrer’s desk, though Farrer did not seem to notice that the Greek text which he had on his desk was itself the *reconstruction* of text critics such as Tischendorf, Westcott, Hort, and Nestle who depended upon *hypotheses* concerning the transmission of the text of Luke. What did not exist on Farrer’s desk, however, was Luke’s *relationship* to Matthew. That was Farrer’s hypothesis, and that is his “weakness.”

Goulder engages in similar rhetorical flourishes when he asserts: “Luke’s use of Mark is a fact (or generally accepted as one), while Q is a mere postulate”; and further, “Q is now hardly defended in the University of Oxford.”<sup>28</sup> Goulder’s subordinate clause, “Q is a mere postulate,” is perfectly correct: Q is a postulate of the hypothesis that affirms both the priority of Mark to Matthew and Luke and the mutual independence of Matthew and Luke. One may quibble only with Goulder’s adjective *mere*.

26. McNicol, Dungan, and Peabody, *Beyond the Q Impasse*, 18 (sequential parallels between Luke 3:1–10:22, divided into five sections, and the sequence of Matt 3:1–18:5), 23 (agreements of Luke 4:31–32; 7:1 with Matt 7:28–29), 24 (Luke’s use of the Matthean absolute genitive + *idou*, Luke’s use of a participial form of *proserchomai* to introduce a finite verb [23:52], and Luke’s use of *skandalizein*). In the summary (318–19), McNicol claims that this data can be explained *only* on the 2GH, even though other explanations are not entertained.

27. Farrer, “On Dispensing with Q”; repr. 321–56, esp. 333.

28. Goulder, “Juggernaut,” 670, 668. The irony of the latter statement is that by the time that this article appeared, C. M. Tuckett, one of the ablest defenders of the 2DH, had come to Oxford.

Q is not a “mere” postulate; on the contrary, it follows *necessarily* from the two logically prior postulates.<sup>29</sup>

As to Luke’s use of Mark being a “fact” or even a “generally accepted fact,” neither is the case. The key piece of data from the Synoptic Gospels — that Matthew and Luke never agree against Mark in the sequence of triple tradition pericopes<sup>30</sup> — admits (logically speaking) of any explanation that places Mark in a medial position. This includes the 2DH and the Farrer-Goulder hypothesis (FGH). This datum, however, is also explicable, for example, on the 2GH and Boismard’s multistage hypothesis, which allows only a mediated relationship between Mark and Luke.<sup>31</sup> Such explanations are hypotheses, not unassailable facts. Moreover, whether Luke used Mark (2DH, FGH) or Mark used Luke (2GH) or both derived from some intermediary (Boismard) cannot conclusively be proved or disproved, since virtually all of the directional indicators are stylistic or theological, and most or all of the arguments are reversible. Luke’s use of Mark thus remains a hypothesis — a reasonable and effective hypothesis, in my view — but no volume of scholarly literature in its support (and no voting from Oxford — or Toronto, for that matter) will elevate its ontological status to anything more than that.

### *The Nature of Synoptic Hypotheses*

If we set aside the various rhetorical overstatements, we are still left with the broader conceptual question of the nature and function of competing Synoptic hypotheses. It is foolish to claim incorrigibility, but equally

29. Similarly, C. M. Tuckett, “The Existence of Q,” in *The Gospel behind the Gospels: Current Studies on Q* (ed. R. A. Piper; Novum Testamentum Supplement 75; Leiden: Brill, 1995), 21 = Q and the History of Early Christianity: *Studies on Q* (Edinburgh: T&T Clark; Peabody, Mass.: Hendrickson, 1996), 4, who rightly calls the Q hypothesis a “negative theory” insofar as it is predicated on the denial of a direct relationship between Matthew and Luke. Because Q is an integral part of the 2DH, it is infelicitous to speak of the Q hypothesis as if it were logically separable from the 2DH.

30. See E. P. Sanders, “The Argument from Order and the Relationship between Matthew and Luke,” *New Testament Studies* 15 (1968–69): 249–61; and the answers by F. Neirynck, “The Argument from Order and St. Luke’s Transpositions,” *Ephemerides theologicae lovanienses* 49 (1973): 784–815; and R. H. Fuller, “Order in the Synoptic Gospels: A Summary,” *Second Century* 6 (1987–88): 107–9.

31. This is characteristic of all of Boismard’s slightly varying solutions. In *Évangile de Marc: Sa préhistoire* (Études bibliques → QUERY: Should be Études bibliques? –JE ← n.s. 26; Paris: Cerf, 1994), Luke and Matthew are dependent on an intermediate version of Mark (Mark<sup>int</sup>), and the final version of Mark is dependent on a Marco-Lucan editor. His 1972 hypothesis lacked Mark’s dependence on a “rédacteur marco-lucanien” but affirmed the dependence of Mark and Luke on Mark<sup>int</sup>. See M.-E. Boismard, *Synopse des quatre évangiles en français*, vol. 2: *Commentaire* (preface by P. Benoit; Paris: Cerf, 1972).

mistaken to insist on the degree of simplicity that Goulder's requirement of falsifiability implies. In the formulation of hypotheses concerning the Synoptic Gospels, we are caught between two incompatible constraints: on the one hand, to formulate hypotheses that are as simple and clear as possible and that are generally falsifiable by reference to the array of Synoptic data; and on the other, to formulate hypotheses that are sufficiently attentive to the complexity of technological and human factors involved in the production and transmission of the Gospels to be a near approximation to what might have happened. Moreover, Synoptic hypotheses are not pictures or reconstructions of "what happened." They are only heuristic tools that offer convenient lenses through which to view data.

From the point of view of logic, it clearly is desirable to have a simple theory according to which Matthew and Luke used Mark — a supposition common to both the FGH and the 2DH. It is, however, most improbable from a historical perspective that Matthew and Luke used the same copy of Mark. Given what we know about the early transmission of manuscripts, it is highly unlikely that any two copies of Mark were in every respect identical. After all, none of the early papyri of the Gospels is identical with another. Two copies of Mark would at a minimum be subject to copyists' mistakes and conceivably to more substantial alterations. There is, moreover, no reason to suppose that Matthew's Mark and our Mark are identical, or that Luke's Mark was identical with either. The same considerations apply, *mutatis mutandis*, to Q on the 2DH or to Matthew and Luke on the 2GH.<sup>32</sup>

Synoptic hypotheses are convenient simplifications of what was undoubtedly a much more complex, and unrecoverable, process of composition and transmission. Under such circumstances, it seems perverse to insist on simple pictures that we know in advance to be too simple, merely because they are also easily falsifiable, or to bemoan the resort to more

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32. This assertion might appear odd, coming from one of the three editors of *Documenta Q* and the critical edition of Q by the International Q Project. I do not, however, believe that in text-critical labors or in the reconstructive efforts of the International Q Project it is realistic or theoretically possible to reconstruct *the* original text of a Gospel or Q. What is reconstructed is an imaginary point on a complex continuum of textual production and textual transmission. This imaginary point presumably approximates to a near degree a document that once existed; but its primary theoretical function is to account for preceding and subsequent textual history, to the extent that this is known.

ornate solutions on the grounds that certain features of the more ornate hypotheses place them beyond falsification.

### *The Minor Agreements and the Two Document Hypothesis*

The agreement of Matthew and Luke against Mark is the key problem for the 2DH. Some proponents of the 2DH accommodate the minor agreements by adjusting the model of the 2DH, suggesting recensions of Mark (Ur-Markus or Deutero-Markus) which agreed with Matthew and Luke; others account for the minor agreements by the supposition of interference from oral tradition; still others suggest textual (post-Markan) corruption; and a few posit intermediate Gospels. Goulder objects to most of these strategies since they place the hypothesis beyond falsification.

While Goulder's point is well taken, it is also the case that none of the adjustments mentioned above is inherently implausible. First, other literature existed in multiple recensions: the Greek and Coptic versions of the Gospel of Thomas, for example, display significant variations, including abbreviation and contractions of sayings and the relocation of one saying to a new context.<sup>33</sup> Second, as recent studies of ancient literacy show, oral performance was involved in literary production at several stages. Composition sometimes involved preliminary drafts, oral recitation before select audiences, and final editing taking into account the audience reaction. Moreover, the difficulties inherent in reading texts written *scripta continua* meant that in the performance/reading of a work, the text functioned in a manner to a musical score rather than a document to be recited.<sup>34</sup> This means that the way a work was heard was a combination of textual and (oral) performative features, or to put it differently, when we read ancient texts, we see only part of what was actually heard. Finally, the study of early New Testament papyri shows a remarkably unstable textual situation, with harmonization and parallel influence attested at a very early stage. If such variation is empirically attested in early New Testament manuscripts,

33. Compare the abbreviation of Oxyrhynchus papyrus 655.1.1–17 in Gospel of Thomas 36 and the expansion of Oxyrhynchus papyrus 654.5–9 in Gospel of Thomas 2. The second part of Oxyrhynchus papyrus 1.22–30 (= Gospel of Thomas 30) is found in Coptic as Gospel of Thomas 77.

34. K. Quinn, "The Poet and His Audience in the Augustan Age," *Aufstieg und Niedergang der römischen Welt* 2.30.1 (1982): 75–180, esp. 90.

it seems perverse to insist on a scenario that forecloses the possibility of transcriptional variations between multiple copies of Mark and Q.

Given these facts, it makes no sense to insist, as Goulder does, on mathematically simple hypotheses. The imperative of Synoptic Problem research is not to produce generalizations or schematic models simply so that they achieve a state of maximal falsifiability; the task of research is to produce models that account for as much of the data as possible. Any one of the adjustments to the 2DH noted above is capable of accommodating the few significant minor agreements — and they are relatively few.<sup>35</sup> If there is a difficulty with the 2DH, it is not that it cannot give an account of the minor agreements; rather, it is that we do not know which of the several available adjustments is preferable.

This view is bound not to please Goulder, but it is a realistic approach under the circumstances. It does render falsification of the 2DH more difficult but not, I think, impossible. The 2DH allows for a coherent accounting of both Matthew's and Luke's editorial practices and although it also generates some anomalies, these are no more serious than those generated on other hypotheses.

### Is Goulder's "Paradigm" New?

There is little doubt about the novelty of Goulder's theory of Synoptic relationships. While there are some anticipations in the nineteenth cen-

35. Although a raw tabulation of the minor agreements produces seemingly impressive numbers — 610 positive and 573 negative agreements by Ennulat's counting — many of these are "agreements" only in the most general sense and hardly imply any collaboration of Matthew and Luke; others are rather easily explained on the basis of Matthean and Lucan redactional habits. There are a small number of difficult cases:

Matt 13:11 || Luke 8:10 against Mark 4:11

Matt 9:20 || Luke 8:44 (*tou kraspedou*) against Mark 5:27

Matt 9:26 || Luke 4:14 (not in the same Markan context)

Matt 22:34–40 || Luke 10:25–28 (which some ascribe to Q) against Mark 12:12–34

Matt 26:67–68 || Luke 22:63–64 against Mark 14:65

Matt 26:75 || Luke 22:62 against Mark 14:72

Matt 28:1 || Luke 23:54 (*epiphōskein*) against Mark 16:1

See A. Ennulat, *Die "Minor Agreements": Untersuchung zu einer offenen Frage des synoptischen Problems* (Wissenschaftliche Untersuchungen zum Neuen Testament 2/62; Tübingen: Mohr [Siebeck], 1994); and my review in *Toronto Journal of Theology* 13 (1997): 101–3. The fundamental work on the minor agreements is still F. Neirynck, *The Minor Agreements of Matthew and Luke against Mark: With a Cumulative List* (Bibliotheca ephemeridum theologiarum lovaniensium 37; Leuven: Leuven University Press, 1974).

tury in the work of Karl Credner and closer comrades in J. H. Ropes and Morton Enslin,<sup>36</sup> Goulder's theory is far more learned and ambitious than those of his predecessors, for he accounts for most of the non-Markan material in Matthew and Luke as the result of redactional invention by the evangelists.

The Kuhnian question, however, is, "Is this a new paradigm?" The answer is, I think, "no." A paradigm for Kuhn is significantly larger than a particular theory or hypothesis. A paradigm both "stands for the entire constellation of beliefs, values, techniques, and so on, shared by the members of a given [research] community"; and it is "the concrete puzzle-solutions which, employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science."<sup>37</sup> Moreover, as Margaret Masterman observes, in the genealogy of a new paradigm it is often not the formal theory that comes first, but what she calls a "trick, or an embryonic technique, or picture, and an insight that is applicable to the field."<sup>38</sup> This combination of trick and insight constitutes the paradigm, which is then worked out in explicit theory and more advanced techniques.

As an example, one might think of advances in quantum mechanics. Faced with Compton's evidence that X-rays had particle as well as wave properties, de Broglie suggested that all matter has both particle and wave properties. De Broglie's hunch was tested with diffraction devices, which had been employed ever since Augustin-Jean Fresnel's 1815 experiments that had demonstrated wave theory in visible light. Instead of diffracting electromagnetic radiation, Davisson and Germer tried electrons and showed in 1927 that they too could be diffracted and hence exhibited wave properties. Only later did Schrödinger's wave mechanics provide

36. K. A. Credner, *Einleitung in das Neue Testament* (Halle: Waisenhaus, 1836), 201–5, who (a) posited Luke's dependence on Matthew and both on an Aramaic proto-Gospel also used by Mark and (b) invoked the *Logia* source to account for the double tradition; J. H. Ropes, *The Synoptic Gospels* (Cambridge: Harvard University Press; London: Oxford University Press, 1934; with new preface: London: Oxford University Press, 1960), 66–68, 93–94; M. Enslin, *Christian Beginnings* (New York, Harper, 1938), 431–34.

37. Kuhn, "Postscript — 1969," in *Structure of Scientific Revolutions*, 175. Kuhn calls the first sense "sociological" and the second sense a "shared example." M. Masterman ("The Nature of a Paradigm," in *Criticism and the Growth of Knowledge* [ed. I. Lakatos and A. Musgrave; Proceedings of the International Colloquium in the Philosophy of Science 4; Cambridge: Cambridge University Press, 1970], 59–89, esp. 67) refers to the second as a construct paradigm.

38. Masterman, "Nature of a Paradigm," 69.

the mathematical resolution of the two seemingly opposed characteristics of electrons.

The main paradigm shift in the study of the Gospels came, not with Goulder or even Holtzmann, but in the late eighteenth century, with Reimarus and Griesbach. Since the time of Tatian, Celsus, and Porphyry, the disagreements among the Gospels were well known, but were resolved in two different manners. Augustine, in his *De consensu evangelistarum*, treated differences among the Gospels as divergent yet ultimately compatible depictions of Christ: Matthew focusing on royalty, Mark on humanity, Luke on priesthood, and John on divinity. The differences among the Gospels could not gainsay the truth of the gospel, the latter understood in Augustine's Platonism as a transcendental idea that was partially and perspectively embodied in the text of the Gospels.<sup>39</sup> A different strategy was employed by harmonists, who referred disagreements to the artificially constructed metatext of the Gospel harmony. This practice came under increasing pressure with the advent, in the sixteenth century, of a verbally based doctrine of inspiration. Andreas Osiander's *Harmonia evangelica* insisted that even small variations in seemingly parallel accounts were significant and indicated discrete historical events.<sup>40</sup> Thus he desynchronized pericopes that earlier harmonists had treated as parallel, a policy that produced in his synopsis, and those that followed him, a string of absurd repetitions. Osiander, for example, had three healings of blind men near Jericho, three centurions' sons healed, three anointings of Jesus, and three cleansings of the temple.<sup>41</sup>

A new paradigm emerged with an insight concerning the "particularity" or individuality of each Gospel. Particularity had been a feature of Osiander's

39. S. McLoughlin notices Augustine's Platonism in "An Approach to the Synoptic Problem" (S.T.L. thesis, Katholieke Universiteit Leuven, 1963): "Augustine speaks of Mk following Mt, and while in our scientifically-minded age such a suggestion carries causal implications, it is much less certain that it does so for the Platonic-minded Augustine: a second witness, who adds nothing to what the first and principal witness had already said, could well in such a mentality be described as his follower" (28).

40. A. Osiander, *Harmoniae evangelicae libri quatuor Graece et Latine* (Geneva: Stephani, 1537).

41. See the discussion of pre-Griesbach synopses in H. K. McArthur, *The Quest through the Centuries: The Search for the Historical Jesus* (Philadelphia: Fortress, 1966), 85–101; and M. H. de Lang, "Gospel Synopses from the 16th to the 18th Centuries and the Rise of Literary Criticism of the Gospels," in *The Synoptic Gospels: Source Criticism and New Literary Criticism* (ed. C. Focant; Bibliotheca ephemeridum theologiarum Iovaniensium 110; Leuven: Leuven University Press/Peeters, 1993), 599–607.

der's harmony insofar as particularities of the Gospels were registered as discrete events in his metatext. But particularity took on a new significance when Hermann Samuel Reimarus argued that the contradictions among the Gospels implied that their authors had produced inconsistent, indeed fraudulent, accounts of Jesus' career.<sup>42</sup> For Reimarus the Gospels were discrete works, not related to some reconciling metatext. Instead, he viewed the Gospels in relation to a plausible historical reconstruction of their referent (Jesus) and their composers (the evangelists).

This turn toward the particularity of the Gospels was made possible by a new "device," the three-Gospel synopsis, created by J. J. Griesbach at virtually the same moment.<sup>43</sup> Griesbach's synopsis allowed for the first time a close comparison of agreements and disagreements among the Gospels. In retrospect, one might locate a paradigm shift in Griesbach's stated refusal to embrace the harmonist's program of creating a harmonious narrative, for this was impossible in principle.<sup>44</sup>

The "trick" of the new paradigm of Gospel study is Synoptic comparison, made possible first by the critical synopsis and subsequently by the many tabular comparisons of the sequence of Synoptic pericopes and the special and shared vocabulary of the Gospels, culminating in sophisticated tools such as Morgenthaler's *Statistische Synopse*, Neiryck's *New Testament Vocabulary*, and the Hoffmann-Hieke-Bauer *Synoptic Concordance*.<sup>45</sup>

The combination of an insight (the historical particularity of the Gospels) and a device (the synopsis) constituted a paradigm in which the puzzle-solving of normal science could take place. This took the form of a succession of attempts to solve the literary puzzle of the relationship

42. H. S. Reimarus, "Von dem Zwecke Jesu und seiner Jünger," in *Fragmente des Wolfenbüttelschen Ungenannten* (ed. G. Lessing; Berlin: Sanders [Eichhoff], 1778) = *Fragments* (ed. C. H. Talbert; trans. R. S. Fraser; Philadelphia: Fortress, 1970).

43. The synopsis originally appeared in 1774 as part of Griesbach's *Libri historici Novi Testamenti graece* (Halle: Curtius, 1774) and was printed separately two years later as *Synopsis Evangeliorum Matthaei, Marc et Lucae* (Halle: Curtius, 1776).

44. J. J. Griesbach, *Synopsis Evangeliorum Matthaei, Marci et Lucae, una cum iis Joannis pericopis; Quae historiam* → **QUERY: Should historian be historiae?** ← *passionis et resurrectionis Jesu Christi complectuntur* (2d ed.; Halle: Curtius, 1797), v–vi.

45. R. Morgenthaler, *Statistische Synopse* (Zürich and Stuttgart: Gotthelf, 1971); F. Neiryck and F. Van Segbroeck, *New Testament Vocabulary: A Companion Volume to the Concordance* (Bibliotheca ephemeridum theologiarum lovaniensium 65; Leuven: Peeters/Leuven University Press, 1984); P. Hoffmann, T. Hieke, and U. Bauer, *Synoptic Concordance: A Greek Concordance to the First Three Gospels in Synoptic Arrangement, Statistically Evaluated* (4 vols.; Berlin: de Gruyter, 1999–).

of the Synoptics, first in the rather loose and impressionistic theories of Lessing and Herder, then in the baroque source theories of Eichhorn, then in Saunier and de Wette's popularization of the Griesbach hypothesis, and finally in Holtzmann's rehabilitation of C. H. Weisse's 1938 Two-Source theory. What is common to these theories and to the Streeterian Four-Source hypothesis, the FGH, and the 2GH which followed them is that, as divergent as they may seem, they no longer refer differences to some meta-narrative, but understand differences to be the function of the literary, historical, and rhetorical particularity.

In the course of two centuries of Gospel study many variations of the paradigm have been introduced. Source criticism in the nineteenth century seemed to prefer a rather strict documentary approach, not entertaining the possibility of the influence of oral tradition upon the writers. This habit accounts both for the positing of intermediate documents such as Ur-Markus to account for slight differences between canonical Mark and the "Mark" which Matthew and Luke seemed to use and for the scouring of patristic sources for references to possibly important lost documents such as the Gospel of the Hebrews or Papias's *Logia*. In the early twentieth century, appeal to the interference of oral tradition became a standard part of many Synoptic theories; and by midcentury, the advent of redaction criticism obviated the theoretical need to posit earlier forms of Mark or Matthew, since differences, including additions, deletions, and modifications, could plausibly be referred to the creativity of the evangelists.

In this sense, Goulder's theory is a utilization hypothesis that accounts for the differences between Matthew and Mark and between Luke and Matthew/Mark by appealing to a now-standard lexicon of editorial maneuvers: abbreviation, expansion, transposition, rewriting, and free invention. All of this fits snugly within the prevailing paradigm of Synoptic research, despite its nonconformity with the 2DH, the dominant source theory (not paradigm) of Synoptic relationships.

Genuinely new paradigms might be on the horizon — one thinks of the narrative approaches, which often prescind from diachronic features of composition and focus on the narrative syntax of the Gospel text. There may even be a new device, the Gospel parallel, which allows each Gospel

to be viewed in its narrative integrity rather than cutting the text into source-critical or form-critical slices.<sup>46</sup> Or again, the turn toward social history has discovered the tool of the Mediterranean anthropologist's field report and has applied some of her conceptual categories to the Gospel text. Whether such innovations will turn out to offer a new paradigm may be doubted, however; both synchronic aspects of the Gospel text and the constellation of embedded social values have now been subsumed in socio-rhetorical exegesis.<sup>47</sup> In any event, neither narrative approaches nor social-scientific approaches have served to solve the puzzles endemic to the study of the Synoptic Problem.

### Is the Two Document Hypothesis Imperiled by Goulder?

Goulder's main challenge to the 2DH is threefold. First, he argues that there is no mention of Q in patristic literature. Of course, in the nineteenth century, Schleiermacher, Lachmann, Credner and Weisse supposed that Papias's *Logia* referred to Q or something very much like Q. But Goulder is quite right to insist that "Papias is sand" (LNP 33; "Juggernaut," 669). Indeed, Papias is sand, as Lührmann has most recently shown and as the majority of twentieth-century Synoptic scholars hold.<sup>48</sup> Hence, it is a misrepresentation to assert that "the Q hypothesis rests in part on a misunderstanding" ("Juggernaut," 669), if what is meant is that the hypothesis rests logically on Papias. It does not. A careful reading of Holtzmann shows that, despite the fact that he referred to Q with the siglum L (for *Logia*), Papias's testimony played no role in the architecture of Holtzmann's argument.<sup>49</sup> In the early twentieth century critics such as W. C. Allen, J. A. Robinson, F. C. Burkitt, B. W. Bacon, and J. C. Hawkins had already rejected the designation Q as "logia" on the grounds that it was

46. See R. W. Funk, *New Gospel Parallels*, vol. 1/2: *Mark* (rev. ed.; Foundations and Facets: Reference Series; Sonoma, Calif.: Polebridge, 1990); and idem, *Poetics of Biblical Narrative* (Sonoma, Calif.: Polebridge, 1988).

47. See V. K. Robbins, *The Tapestry of Early Christian Discourse: Rhetoric, Society and Ideology* (London: Routledge, 1996).

48. D. Lührmann, "Q: Sayings of Jesus or Logia?" in *The Gospel behind the Gospels: Current Studies on Q* (ed. R. A. Piper; Novum Testamentum Supplement 75; Leiden: Brill, 1995), 97–116.

49. H. J. Holtzmann, *Die synoptischen Evangelien: Ihr Ursprung und geschichtlicher Charakter* (Leipzig: Engelmann, 1863). See J. S. Kloppenborg Verbin, *Excavating Q: The History and Setting of the Sayings Gospel* (Minneapolis: Fortress; Edinburgh: T&T Clark, 2000), chap. 6.

question begging.<sup>50</sup> Even those who wished to maintain an Aramaic Q did not found their case on Papias's dubious statements, but on Wellhausen's theory of translation variants.<sup>51</sup>

Goulder also contends that the absence of mentions of Q in patristic sources provides an argument against its existence. Tuckett's point that other documents of the early Jesus movement have gone missing does not, of course, quite meet Goulder's objection. But it is not far off. After all, we know of Paul's "tearful letter" to the Corinthians not because it is listed in some patristic source, but only because of a quite passing comment made by Paul himself. Without this notation, we would have no hint at all of such a letter. Or again: we deduce the existence of a "Two Ways" document, not because it is mentioned in any patristic source, but because its existence follows logically from an analysis of the possible literary relationships among the *Didache*, Barnabas, and the *Doctrina Apostolorum*.<sup>52</sup> Hence, the nonmention of Q in early Christian sources is neither unparalleled nor, under the circumstances, particularly troubling.

Goulder's second objection has to do with the minor agreements. I have already indicated that these can be accommodated on several realistic versions of the 2DH. Of course, they can also be explained on the complex hypotheses of Boismard or on Robert Gundry's view that Luke had some access to Matthew as well as Q,<sup>53</sup> or on the 2GH or FGH.

Goulder's final and most important argument against the existence of Q has to do with the appearance of allegedly Matthean vocabulary in Luke. There are in fact two types of observations.

50. W. C. Allen, "Did St Matthew and St Luke Use the Logia?" *Expository Times* 11 (1899–1900): 424–26, esp. 425; J. A. Robinson, *The Study of the Gospels* (London: Longmans, Green, 1902), 70; F. C. Burkitt, *The Gospel History and Its Transmission* (Edinburgh: T&T Clark, 1906), 127, 130; B. W. Bacon, "A Turning Point in Synoptic Criticism," *Harvard Theological Review* 1 (1908): 48–69; J. C. Hawkins, *Horae Synopticae: Contributions to the Study of the Synoptic Problem* (2d ed.; Oxford: Clarendon, 1909; repr. 1968), 107.

51. E.g., J. C. Hawkins, "Probabilities as to the So-Called Double Tradition of St. Matthew and St. Luke," in *Oxford Studies in the Synoptic Problem* (ed. W. Sanday; Oxford: Clarendon, 1911), 95–140, esp. 104; G. D. Castor, *Matthew's Sayings of Jesus: The Non-Marcian Common Source of Matthew and Luke* (Chicago: University of Chicago Press, 1918), 17–18.

52. See J. S. Kloppenborg, "The Transformation of Moral Exhortation in *Didache* 1–5," in *The Didache in Context: Essays on Its Text, History and Transmission* (ed. C. N. Jefford; Novum Testamentum Supplement 77; Leiden: Brill, 1995), 88–109.

53. R. H. Gundry, "Matthean Foreign Bodies in Agreements of Luke with Matthew against Mark: Evidence That Luke Used Matthew," in *The Four Gospels 1992: Festschrift Frans Neirynck* (ed. F. Van Segbroeck, C. M. Tuckett, G. Van Belle, and J. Verheyden; Bibliotheca ephemeridum theologicarum lovaniensium 100; Leuven: Leuven University Press/Peeters, 1992), 1467–95.

Goulder argues that some of the minor agreements display characteristically Matthean and clearly un-Lucan vocabulary. If this observation were correct, the 2DH would hardly offer a convenient account. Rather than treating the minor agreements as the coincidental alteration of Mark by Matthew and Luke, it would be more likely that Matthean choices had directly or indirectly influenced Luke.<sup>54</sup> The real difficulty, however, lies with finding such vocabulary. Mark Goodacre, sympathetic to Goulder's thesis, surveys Goulder's candidates and finds that some cannot be considered Matthean on statistical grounds (occurring, for example, only once in Matthew).<sup>55</sup> Tuckett adds that other instances are not un-Lucan and others still are thoroughly Lucan.<sup>56</sup> Goodacre finds six instances of Matthean/un-Lucan vocabulary among the minor agreements.<sup>57</sup> But he also finds two instances of un-Matthean/Lucan vocabulary.<sup>58</sup> Goodacre

54. Goulder, "Putting Q." Goodacre (*Goulder and the Gospels*, 93–93) notes that Goulder has steadily made this criterion more stringent: in 1978 he looked for non-Lucan words or phrases that were "typical of, or plainly redacted by, Matthew" (219); similarly, "Farrer on Q," *Theology* 83 (1980): 195: "in some way characteristic of Matthew" and "in some way uncharacteristic of Luke." In "Luke's Knowledge of Matthew," 144, Goulder drops "in some way" from this criterion. Tuckett ("Relationship between Matthew and Luke," 130) agrees with the final formulation: "The Matthew-Luke agreement must be both positively Matthean and positively un-Lucan."

55. (a) Matt 4:13 || Luke 4:16 (*Nazara*) against Mark 1:14; see Goulder, "Putting Q," 219–21; Tuckett, "Relationship between Matthew and Luke," 131; Goodacre, *Goulder and the Gospels*, 101–2. The International Q Project argues that the MA is due to Q: S. Carruth and J. M. Robinson, *Q 4:1–13, 16* (ed. C. Heil; Documenta Q; Leuven: Peeters, 1996), 391–442; (b) Matt 26:68 || Luke 22:64 (*paisas* and *tis estin*) against Mark 14:65; see Goulder, "Putting Q," 226–28; Tuckett, "Relationship between Matthew and Luke," 136–37; Goodacre, *Goulder and the Gospels*, 102–5, who notes that *paiō* appears only once in Matthew and Luke, and *tis estin* occurs not only in Matthew (redactionally), but in Mark (5 times) and 13 times in Luke (5 times redactionally).

56. Tuckett, "Relationship between Matthew and Luke," 132–40:

Matt 9:2 || Luke 5:18 (*epi klinēs*) against Mark 2:3 (*krabbatos*)

Matt 17:5 || Luke 9:34 (*epeskiasen* [Luke: *epeskiazein*] *autous*) against Mark 9:7 (*episkiazousa autois*)

Matt 27:1 || Luke 22:66 (*presbyteroi* [Luke: *presbyterion*] *tu laou*) against Mark 15:1 (*presbyterōn*)

Matt 27:58–59 || Luke 23:52–53 (*houtos proselthōn tō pilatō ētēsato . . . enetylixen auto . . . kai ethēken auton*) against Mark 15:43–46 (*eisēlthen pros ton pilaton kai ētēsato . . . eneilēsen . . . kai ethēken auton*)

Matt 28:1 || Luke 23:54 (*epiphōskein*) against Mark 16:1

57. Goodacre, *Goulder and the Gospels*, 107–17:

Matt 12:15 || Luke 6:17 (*pantas*) against Mark 3:7

Matt 8:27 || Luke 8:25 (*hypakouousin*) against Mark 4:41 (*hypakouei*)

Matt 13:54 || Luke 4:15–16 (*synagōgē* [Luke: *synagōgais*] *autōn*) against Mark 6:2 (*synagōgē*)

Matt 14:13 || Luke 9:11 (*hoi* [Luke + *de*] *ochloi ēkolouthēsan*) against Mark 6:33

Matt 12:27 || Luke 20:32 (*hysteron*) against Mark 12:22 (*eschaton*)

Matt 26:47 (*idou . . . ēlthen*) || Luke 22:47 (*idou . . . proērchetō*) against Mark 14:43 (*paraginetai*)

58. Goodacre, *Goulder and the Gospels*, 117–22:

seems to take this as a matter of democracy, the Matthean vocabulary winning in a vote against the Lucan; but such a conclusion is dubious from a logical point of view. Moreover, the entire approach is weakened by the definition of Matthean/un-Lucan.

Goulder argues that Luke's use of the phrase *their synagogues* in a summary statement in Luke 4:15 reflects Matthew's (redactional) usage in Matt 13:54 (in a the story of Jesus' visit to Nazareth), where Mark has only the word *synagogue*. It is true that Matthew uses the phrase *their synagogues* five times (three times redactionally). Mark uses the phrase twice (1:23, 39). On any accounting, Luke's treatment of Mark 1:14–28 is complicated. Luke omitted Mark 1:15–20, substituted his own story of the visit to Nazara for one that occurred much later in Mark (6:1–6a), and created an introduction (Luke 4:14b–15) and summary (4:37) from elements in Mark 1:21, 23, 28:

Table here

Matthew's phrase in 13:54 ("he came to his hometown and began to teach the people in their synagogue") is a simple adaptation of Mark 6:1–2 ("he left... and came to his hometown, ... and ... on the Sabbath he began to teach in the synagogue"). But Luke does not take over Mark's Nazareth story (6:1–6a) and shows no affinities with Matt 13:54–58, apart from the proverb (Matt 13:57b = Mark 6:4), which in any case is quoted in a form that does not correspond with either Matthew or Mark (or John 4:44). Instead, Luke depends on other sources for his Nazara story and takes from the Markan context the elements that he uses in Luke 4:14b–15, 37. One hardly needs to appeal to Matt 13:54 in order to account for "in their synagogues" in Luke 4:15; it is better explained as the result of the reconfiguration of Markan materials. Conversely, to derive the phrase from Matthew then leaves us wondering why Luke failed to take over any of the other features of the Matthean Nazareth pericope.<sup>59</sup> Moreover, as Tuckett points out, Luke uses the phrase "synagogue of the Jews" in Acts.

Matt 14:21 || Luke 9:14 (*hōsei*) against Mark 6:44

Matt 22:35 || Luke 10:25 (*vomikos peirazōn* [Luke: *ekpeirazōn*] *didaskale*) against Mark 12:28

59. Goulder (LNP 299–300) agrees that Luke 4:14 (*kai phēmē exēlthen kath' holēs tēs perichōrou peri autou*) is taken from Mark 1:28 but does not notice that *in their synagogue* occurs also in Mark 1:23. Instead, he argues that Luke "unintentionally" carried over *their* from Matt 13:54.

**Mark 1**

<sup>14</sup>Now after John was arrested, Jesus came to the Galilee. . . .

(6:1–6a)

1:15–20

<sup>21</sup>And they entered Capernaum; and on the Sabbath he entered the synagogue and taught.

<sup>22</sup>And they were astounded at his teaching because he was teaching them as one having authority not like the scribes.

<sup>23</sup>And just then **in their synagogue** there was a man with an unclean spirit. . . .

<sup>28</sup>And at once his fame spread throughout the entire region of the Galilee. . . .

**Luke 4**

<sup>14</sup>Then Jesus, filled with the power of the Spirit, returned to the Galilee and a report about him spread through the entire region.

<sup>15</sup>He taught in their synagogues and was praised by everyone.

4:16–30

no parallels

<sup>31</sup>And he went down to Capernaum, a city of the Galilee, and was teaching them on the Sabbath.

<sup>32</sup>And they were astounded at his teaching because he spoke with authority.

<sup>33</sup>And **in the synagogue** there was a man who had the spirit of an unclean demon. . . .

<sup>37</sup>And a report about him reached every place in the region.

Hence it is hardly warranted to insist that Luke took his pronoun from Matthew when it was available in the immediate Markan context as well.

Or again: Luke's use of the biblicalizing *idou* at 22:47 is declared to be Matthean. But this is done only by narrowing the statistics to include only the usages of the particle after an absolute genitive (eleven times in Matthew [four times in Matt<sup>Red</sup>]; once in Luke).<sup>60</sup> But *idou* is common in all three Synoptics (Matt 62 times, Mark 17 times, Luke 57 times) and is added by Luke Mark four times where Matthew does not have it and seven times where Matthew also adds the word.<sup>61</sup> The situation is complex: *idou* is a Septuagintalism that occurs 1,074 times; Luke is known to imitate Septuagintal style; and he adds *idou* to Mark eleven times (including the four times where Matthew does not have the word). It seems rather dubious to ignore Markan usage, Luke's preferences (quite

60. Superscript <sup>Red</sup> means redactional and superscript <sup>S</sup> refers to texts appearing only in one Gospel.

61. Neiryneck, *Minor Agreements*, 273–74.

independent of Matthew), or Septuagintal usage when accounting for the agreement with Matthew at Luke 22:47.

The second form of Goulder's argument is similar. He argues that the presence of "many rare and striking phrases in Q that are also favorite expressions of Matthew" indicates that what is usually assigned to Q is in fact Matthean ("Juggernaut," 672). This is a form of the argument first articulated in 1843 by Eduard Zeller, who argued that when a word or phrase appeared in gospel A only where gospel B also evidenced it, but at several other points in gospel B, it was a valid inference that A used B.<sup>62</sup>

Goulder's "many" is rather an exaggeration; he mentions the following items:

*oligopistoi* (Q 12:28 + Matt 8:26<sup>Red</sup>; 14:31<sup>Red</sup>; 16:8<sup>Red</sup>)

*ekei estai ho klauthmos kai ho brygmōs tōn odontōn* (Q 13:28 + Matt 13:42<sup>S</sup>, 50<sup>S</sup>; 22:13<sup>S</sup>; 25:51<sup>Red?</sup>, 30<sup>S</sup>)

the concluding formula of Matthew's sermons (which does not appear in Luke [cf. Luke 7:1a])

*gennēmata echidnōn* (Q 3:7 + Matt 12:34<sup>Red?</sup>; 23:33<sup>Red?</sup>)

*poiein karpon* (Q 3:8–9; 6:43 [twice] + Matt 7:17<sup>Red</sup>, 19<sup>Red?</sup>; 13:26<sup>S</sup>; 21:43<sup>Red</sup>)

*ekkoptetai kai eis pyr balletai* (Q 3:9 + Matt 7:19<sup>Red</sup>)

*synagagein ton siton eis tēn apothēkēn autou* (Q 3:17; 12:24 + Matt 13:30<sup>Red</sup>; Luke 12:18)

Goulder's definition of Matthean and semi-Matthean vocabulary is highly problematic, but even so, Goodacre's sympathetic analysis shows

62. E. Zeller, "Studien zur neutestamentlichen Theologie 4: Vergleichende Übersicht über den Wörterrath der neutestamentlichen Schriftsteller," *Theologische Jahrbücher* (Tübingen) 2 (1843): 443–543, esp. 527–31. Zeller's results were mixed, with seventy-eight words and phrases allegedly supporting Matthean priority and thirty-one supporting Markan priority. Despite the seemingly inconclusive nature of his statistics, Zeller declared that an "overburdening proportion" (532) favored Matthew. C. M. Tuckett ("The Griesbach Hypothesis in the 19th Century," *Journal for the Study of the New Testament* 3 [1979], 29–60, 69 n. 57) notes that Zeller's list includes many instances where a word that occurs once in Mark and twice in Matthew or twice in Mark and three times in Matthew is declared to be characteristically Matthean.

that Q pericopes also contain Lucan, un-Matthean, and semi-Lucan words.<sup>63</sup> Presumably Goulder accounts for these by understanding the semi-Lucan words as “Luke-pleasing” words that Luke took over from Matthew and enhanced. The Lucan, un-Matthean words pose a more serious problem for Goulder’s thesis since, by the logic of his own argument, they should lead to the conclusion that Matthew used Luke. In fact, most of the semi-Lucan and semi-Matthean words could just as easily be treated as Q vocabulary embraced by Luke and Matthew, respectively, and the Lucan, un-Matthean and Matthean, un-Lucan words as Q vocabulary taken over by Luke and Matthew less “enthusiastically.” Moreover, the obvious Mattheanisms such as *he basileia tōn ouranōn* (Matt: 32x) are wholly lacking in minimal Q or in Luke.

The general difficulty with the argument is that it can hardly exclude the possibility that a singly or doubly attested word in gospel A (Q) appealed to the author of gospel B (Matthew), who expanded its usage. This is precisely the point made by Goulder against Farmer’s reiteration of Zeller’s argument.<sup>64</sup> Yet as Tuckett points out, Goulder himself uses the argument that he declared to be fallacious in his 1989 book on Luke.<sup>65</sup> Since Zeller’s principle is reversible, it is invalid for determining the direction of dependence.

63. Goodacre, *Goulder and the Gospels*, 42–88.

64. See W. R. Farmer, “Certain Results Reached by Sir John C. Hawkins and C. F. Burney Which Make More Sense If Luke Knew Matthew,” in *Synoptic Studies: The Ampleforth Conferences of 1982 and 1983* (ed. C. M. Tuckett; Journal for the Study of the New Testament Supplement 7; Sheffield: JSOT Press, 1984), 75–98; and Goulder’s reply (100): “But [Farmer’s argument] is a fallacy. A’s favorite expressions may not have appealed to B; indeed they may be classified as favorite *because* they did not appeal to B. Sometimes later B may copy in expressions of earlier A inadvertently; and sometimes a casual expression of earlier A may appeal strongly to B so that he uses it often.” Goulder then notes that he has taken over some of Farrer’s favorite phrases, some inadvertently, others used more often. He continues: “I do not wish to seem hard on Farmer, as I made virtually the same error of method myself in a paper to the SNTS seminar in 1981, where Farmer was present. Three of my instances of ‘inadvertent’ carrying over of a characteristic Matthean phrase by Luke were on Hawkin’s list on pp. 170–71: ‘you generation of vipers!’, ‘there shall be weeping and gnashing of teeth,’ ‘and it came to pass when Jesus finished these sayings.’ They might just be Q phrases that Matthew liked very much; though there are other reasons in fact for preferring the inadvertence explanation, which I offered.”

65. Tuckett, “Existence of Q,” 35–36.

### Conclusion

In this essay I have argued, first, that Goulder has adopted a view of paradigms that corresponds more closely to Popper's view of scientific theories than it does to Kuhn's. The significance of this observation lies in the fact that it seems apparent that Goulder's interlocutors do not share his rather strict Popperian view of falsification. Moreover, I have argued that whatever its applicability to hypotheses in the experimental sciences, Popper's view is inappropriate when it comes to the study of the Synoptic Problem. In place of a schematic and easily falsifiable model of Synoptic interrelationships, I have argued for a historically and technically realistic view. To adopt such a view, as I believe that most proponents do, renders ineffective most of the arguments that Goulder mounts against the 2DH.

Second, I have suggested that Goulder's own view is not a paradigm, at least by Kuhn's standards. It fits easily within the broad paradigm of Gospels studies that has prevailed since the early 1800s. This paradigm privileges the Gospels in their historical, literary, and socio-rhetorical particularity and seeks to establish a plausible theory of literary dependence. Formally speaking, Goulder's hypothesis is no different from the 2DH, the 2GH, or the several more complex hypotheses that are advocated today although, to be sure, Goulder has several interestingly different emphases in his explanatory theory.

Finally, I have argued that none of Goulder's points raised against the 2DH is especially telling. This is either because a realistic model of the 2DH can accommodate the few problematic minor agreements without collapsing or because the evidence that Goulder adduces of Mattheanisms in Luke (or Q) is both weak and admits of counterevidence: Lucanisms in Matthew.

Reflection on Goulder's hypothesis and its reception raises an interesting puzzle. Goulder has not been successful in offering decisive objections to the 2DH,<sup>66</sup> but even if he has not, it is a perfectly rational option to

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66. Goulder seems to concede this point, though he has varying explanations. In "Farrer on Q," 194, his explanation is *ad hominem*: "Q is not going to collapse: it has the highest vested interest of any New Testament hypothesis in that virtually ever scholar has written a book assuming its truth." Elsewhere ("Luke's Knowledge of Matthew," 144–45), his explanation has more to do with the nature of refutation: "In matters of this kind we cannot hope for proof. The four hypotheses [2DH, 2GH, FGH, and Deutero-Markan theory] are in competition with one another in plausibility. The Two-

embrace his hypothesis, since it provides a ready solution to the issue of the minor agreements and the allegedly Matthean vocabulary in Luke. It is possible to account for Lucanisms in Matthew by his notion of “Luke-pleasing” vocabulary. To be sure, there are some serious technical problems involved in rendering his thesis plausible, notably accounting for Luke’s almost complete detachment of the double tradition from the context in which he found it in Matthew and his relocation of that material elsewhere, or the puzzle that, when faced with material common to Mark and Matthew, Luke never prefers Matthew’s sequence over Mark’s and almost never prefers Matthew’s wording to Mark’s.

It might be that resistance to the FGH comes down to the inability of exegetes to imagine such activities on Luke’s part. But I doubt it. I think it rather more likely that ideological factors are at play. For much of its history, Q has served as a way to bridge the gap between Matthew and Luke in the 80s or 90s and the historical Jesus. Harnack believed Q to be a largely unadulterated collection of Jesus sayings. And despite the fact that most of the technical studies that have appeared on Q since Hoffmann’s 1972 *Habilitationsschrift* have tried to treat Q primarily as a document in its own right rather than as a source for the historical Jesus, outside of these specialist circles, Q is still regularly thought to offer immediate or near immediate access to Jesus.<sup>67</sup> Hence John P. Meier dismisses quickly studies that attempt to ascertain a composition history of Q as “hypothetical” — what in New Testament studies do we do that is not hypothetical? — and satisfies himself with the caricature of Q as a “grab bag.”<sup>68</sup> This allows him to use Q pretty much as Harnack had done and to quarry Q for Jesus tradition without having to inquire whether Q’s own editorializing and creativity renders those excavations problematic.

If theories of the stratification of Q have been regarded as imperiling

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Source hypothesis starts with the tremendous advantage of having been widely taught as *the* solution for at least fifty years.”

67. P. Hoffmann, *Studien zur Theologie der Logienquelle* (Neutestamentliche Abhandlungen n.s. 8; Münster: Aschendorff, 1972; 2d ed. in 1975; 3d ed. in 1980).

68. J. P. Meier, *A Marginal Jew: Rethinking the Historical Jesus*, vol. 2: *Mentor, Message, and Miracles* (Anchor Bible Reference Library; New York: Doubleday, 1994), 179–81. Meier’s characterization of Q as a grab bag without a coherent theology or a supporting community allows him to prescind from the literary and historical questions of its origin, tradents, and editorial tendencies, and to use Q simply as a “distinct and valuable source for sayings of Jesus and John.”

the quest of the historical Jesus, Goulder's thesis would have a yet more devastating effect. Not only is most of the double tradition treated as the result of Matthean editorial activity, but the special Lucan material, including the parables of the man going down the road and the lost son, so central to many constructions of the historical Jesus, would dissolve into Luke's exegetical imagination.

An examination of the history of solutions to the Synoptic puzzles can show how rather technical, almost algebraic, solutions to literary puzzles have functioned within broader theological paradigms and have been embraced not so much because they solved literary problems, but because they seemed to provide literary models that cohered with broader ideological interests. This is true of the use of the Griesbach hypothesis by F. C. Baur and his associates; and it is true of the use of the Markan hypothesis by liberal theology up to Harnack. The 2DH is difficult to displace not only because it still serves as an effective compositional hypothesis and because compelling counterevidence has not been produced; but it also allows for a relatively fulsome picture of Jesus and seems to lend support to general theological models concerning the gradual development and articulation of christological, eschatological, and ecclesiological doctrines.

In order for the FGH to be an effective counterforce to the 2DH, it is necessary to show not only that it solves literary puzzles more effectively than the 2DH, but that it holds the promise of addressing larger theoretical issues raised in the dominant paradigm, such as the relationship of the historical Jesus to the pictures of Jesus in the early Jesus movement and a comprehensive theory of the creation, use, and transmission of early Christian texts.<sup>69</sup>

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69. A shorter and slightly different version of this paper appeared as "Is There a New Paradigm?" in *Christology, Controversy, and Community: New Testament Essays in Honour of David Catchpole* (ed. D. Horrell and C. M. Tuckett; Supplements to *Novum Testamentum* 99; Leiden: Brill, 2000), 23–47.