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Comparing Matthew with the *Didache* : Some Methodological Reflections

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Introduction

As times change, so do the controversies and opinions that go with them. This is as true in the arena of national politics as it is in the marketplace of scholarly ideas. It was not so long ago that the burning question of the day was whether *Barnabas* knew and used the *Didache* or vice versa. Today, thanks largely to Jean-Paul Audet's *La Didachè* [1958], the point is all but moot; the discussion has moved on. Still it has not moved on in all respects, for since the time of Harnack scholarship has been coming back again and again to certain enigmas within the larger enigma. Three basic questions remain decisive. The first has to do with whether the *Didache* is to be envisaged as a composite text, pieced together over the years by a series of editors, or whether the text was written more or less by one editor, more or less at one time. If we pursue the former option, the question then becomes, "What stylistic and material shifts must take place at any given point to justify the supposition that one pen has been laid down and a different, later pen has been picked up?" The answers to this question are almost as numerous as those seeking to respond to it. The second mystery of *Didache* research has to do with the socio-historical location of the document and/or its constitutive layers. How might we speak to the social setting behind each successive stratum? In particular, how might we describe the community which gave rise to the original core document – if we may put it this way – behind the *Didache*? Who packed the original snowball of teaching before it was rolled in the powder of later tradition? Related to the first two issues is a third: the interrelationship between the *Didache* and the synoptic tradition. How does the *Didache*, particularly its *sectio evangelica*, relate, if at all, to Matthew and Luke? These three issues – redaction, historical setting, and sources – are of course mutually informing.

While every effort to offer a comprehensive description of the *Didache* must engage all three of these fronts, it is unnecessary to insist that proper investigation begin with any one in particular.

"Comprehensive descriptions" necessarily require a *kind* of circular reasoning, that is, the framing of a coherent paradigm. Where one hops in on the circle, where one chooses to begin the discussion, is less important than the shape and explanatory power of the overall argument. At the same time, such explanatory power does not come easily without each of these three issues (redaction, socio-historic setting and sources) being treated, at least to some extent, on their own terms.

Setting aside socio-historical and redactional considerations, in this paper I will attempt to focus solely on the pressing question of sources. Does the *Didache* show signs of dependence on the synoptic tradition (particularly Matthew) in its finalized form or it more reasonable to surmise that the Didachist, being heir to a rich oral tradition, composed his or her work independently of the now-canonical texts? Stopping short of taking sides in what is now becoming perhaps a slightly entrenched debate, my intent here is to interact with the case for an orally-based *Didache*, at least as it is laid out in what is now one of the most recent (and will undoubtedly become one of the most significant) monographs on the subjects: Aaron Milavec's *The Didache: Faith, Hope and Life of the Earliest Christian Communities, 50-70 C.E.* Inasmuch as Milavec holds to what may be called a "consistently oral" position (tracing all, as opposed to none or just some, of the Jesus traditions in the *Didache* to an oral stream), his argument I think

represents a good test-case for both the strengths and weaknesses of the orally-based *Didache* position. In the second part of the paper, I offer suggestions as to how Milavec's thesis may be strengthened. These suggestions do not necessarily involve a criticism of the author's work, so much as a recognition that we are in the early stages of a difficult issue with no apparent, easy resolution.

1. *Faith, Hope, and Life*: An Argument for a Consistent Orality

The most significant, if not the boldest, claim in Aaron Milavec's *The Didache* is that the text of the *Didache* displays a thoroughgoing unity. On the possibility that this is true, and surely life would be much simpler if it were, this would mean that the laborious attempts to delineate various and sometimes complex editorial layers are simply wrong-headed – the question of redaction need hardly detain us at all. In regards to the historical situation, Milavec contends that the material contained in the *Didache* harkens back to a very early Christian community, one which existed prior to the period in which the synoptic gospels would come to circulate. But in order to make both these claims stick, a third piece must fall into place, namely, that the *Didache* in its final form depends exclusively on independent Jesus tradition. For the author this independence is demonstrable on a number of grounds, not least among them the “oral environment” (724) in which the document took shape.

Whilst Milavec confesses he will “expend only passing energy on issues of source and redaction criticism” (xii), the reader is heartened to find that the author's chapter 11 actually gives fairly energetic attention of the issues. After a brief *Geschichte der Forschung*, Milavec begins by critiquing an “older methodology,” represented by John M. Court's “The *Didache* and St. Matthew's Gospel,” (*SJT* 34 [1981] 109-20), which “consisted in isolating parallel citations and then drawing conclusions based upon an analysis of the degree of coincidence between the texts” (Milavec 698). On the basis of such parallels, Court concludes that the *Didache* was dependent on Matthew. But, Milavec insists, rightly, that this fails to take into account alternative explanations. Milavec then goes on to criticize Court on three further counts. First, he says that Court ignores the obvious differences in context between Matt 7:6 and its parallel *Did.* 9.5; Matthew's “not giving to dogs what is sacred” applies to judging, in the *Didache* the same saying applies to the eucharist (Milavec 700). Surely, it is reasoned, the dramatic difference in context checks any blithe attempt to connect the two passages directly. Secondly, Court is faulted for not giving sufficient heed to variants, for example the presence of the negative form of the Golden Rule (with parallels in Jewish and Christian writings), as opposed to Matthew's positive formulation (Milavec 700-01). Once again, Milavec's criticism is a valid one. The negative form of the Golden Rule is so prevalent in Jewish antiquity, it surely will not do for Court to argue dependence on Matthew on the basis of this non-Matthean wording. Third, while the tight parallel between the *Didache* and Matthew's version of the Lord's Prayer leads Court to conclude the former's dependence on the latter, Milavec demurs, for “[W]hen dealing with oft-repeated prayers, however, one might expect that the framers of the *Didache* did not borrow from Matthew but made use of the concrete prayer tradition within their own communities as source” (Milavec 701). Moreover, following Audet (*La Didachè*, 173), Milavec points out that there are minor variants between *Didache* and Matthew at this point, and “a community which put forward variant details of small significance ... must have relied upon its own unique practice and not have gone about copying and slightly modifying the text of Matthew” (Milavec 702). This too is a sound point, and one to which I will return later. So far, so good.

We draw closer to the heart of Milavec's argument when the author, invoking an unlikely ally, cites Christopher M. Tuckett with approval: “[C]ommon dependence on a prior source does not necessarily involve less close verbal agreement” (Tuckett, “Synoptic Tradition,” 207 [Milavec 702]). It is somewhat ironic that while Tuckett makes this statement so as to counter Helmut Koester's point that oral traditioning is reflected in the *Didache*'s minor divergences from Matt 24.10-12 (*Synoptische Ueberlieferung* [1957]) 184), Milavec is now turning this double-edged sword around to refute the one who first wielded it. The application of the principle to the case at hand goes something like this.

Assuming Matthew and the *Didache* did draw on a common source, it is not necessarily the case that the two documents would reflect less agreement than had one used the other directly. Conversely, a high degree of agreement between two texts does not necessarily imply a genetic relationship. On this basis Milavec justifies his agreement with Clayton Jefford, who writes: “[I]n most cases the relationship between the sayings collection in the *Didache* and the collection in the Matthew gospel is best explained by the hypothesis that the Didachist and the Matthean redactor have shared a common sayings source” (Jefford, *Sayings of Jesus*, 91). But, Milavec adds, Jefford is not entirely consistent. In the case of the Lord’s Prayer, Jefford thinks the Didachist knew Matthew. Our author claims that this viewpoint ignores “the possibility that a prayer recited three times each day might constitute an ‘oral source’ and that the multiple deviations from Matthew’s text must signify, minimally, that the framers of the *Didache* were not citing from an open gospel set out before them” (454). Although Milavec does not expressly say as much, it seems that he is concerned that Jefford, in conceding the Didachist’s occasional use of Matthew and Luke, has conceded too much. By allowing the Didachist to use the gospels directly, has not Jefford’s larger argument for Matthew and the *Didache*’s common dependence been somewhat weakened? Perhaps it is this weakness that Milavec wants to avoid.

But this turn also comes at a heavy price. Although, strictly speaking, a high degree of agreement between two texts does not *necessarily* imply a genetic relationship, the fact remains that the higher degree of agreement between any two given texts, the greater the probability of a genetic relationship. When Tuckett writes that “common dependence on a prior source does not necessarily involve less close verbal agreement,” and, again, “the measure of verbal agreement between the *Didache* and Matthew cannot be used to determine whether that agreement is due to direct dependence of one on the other or to common dependence on a prior source” (Tuckett, 207), this seems to be inconsistent both with his postulation of Q and the canons of logic.

To be sure, it may be remarked that there are several passages in the Double Tradition that are both closely if not exactly parallel and, per the Two-Source Hypothesis, clearly dependent on Q. But it must equally be remarked that very high correspondence between Matthew and Luke in the Double Tradition is the exception rather than the rule. There are far more instances in which either Matthew or Luke see fit to reword Q, sometimes dramatically. And so, since both Matthew and Luke are closer to Q than they are to each other, the Two-Source Hypothesis, which Tuckett maintains, provides a case in point that the measure of verbal agreement *can* be used “to determine whether that agreement is due to direct dependence of one on the other or to common dependence on a prior source.”

In considering an analogy closer to our day-to-day experience, let us say that a professor catches two students (Mr A and Mr B) cheating on an exam in class; they have been independently copying from various parts of Mr C’s test. If A left off his own work and copied down C at various stretches, and B made a similar number of changes, though not necessarily at the same points, one would expect that the amount of variation between A and B to be as much as twice that of, on the one side, A and C, and, on the other, B and C. Now let us say that Mr A and Mr B had sisters in another section of the same course: Ms B and Ms C. If the professor were to notice that the essay exams of Ms B and Ms C were curiously similar in wording, would not the most natural assumption be that Ms B had copied from Ms C or vice versa. It is, at any rate, rather unlikely that the professor would rifle through the remainder of essays looking for a third party, who, sharing the similarities of Ms B and Ms C, served as the exemplar for Ms B and Ms C. Whilst it is of course possible that this is precisely what happened, the more economical solution would be to start with the assumption that Ms B and Ms C were the only two parties involved. Thus follow two conclusions. First, whereas close verbal correspondence does not logically entail direct dependence, the greater the agreement, the more inclined we are to suspect direct dependence. Second, in cases where there is very close correspondence between two texts, the most economical and therefore the most likely solution is to suppose that direct borrowing is involved.

Thus I believe that Koester's point not only stands, but should be re-emphasized. Let us be clear: were the Didachean material tightly parallel with Matthew, it would be difficult to hypothesize anything but direct written dependence. But it is precisely the degree of verbal dissimilarity that gives life to the supposition of an antecedent source. For these reasons, I feel I somewhat uncomfortable with the leveling effect of Milavec's statement "that no degree of verbal similarity can, in and of itself, be used to conclude that the framers of the Didache knew and/or cited the written gospel. In every case, it is quite possible that both Matthew and the framers of the Didache relied upon free-floating sayings that they both incorporated into their material in different ways" (455). Certainly, it is "quite possible," but this is not the same thing as "quite probable." Although it may be granted that Matthew and the Didachist may have relied on the same pool of oral tradition at those points where they are extremely similar, the burden of evidence remains on those wishing to prove as much. Again, when Milavec writes that "[C]lose verbal agreement will always be incapable of establishing dependence..." (454), it seems he has overplayed his hand. If this axiom were applied to the synoptic problem, gospel scholars, including Tuckett himself, would have no choice but to declare the matter formally insoluble. So far, perhaps foolishly, we are still trying; the vast majority of gospel scholars are still assuming that there is general correlation between close verbal agreement and direct borrowing.

There is, I feel, a second caveat that must be registered, one regarding the so-called "bias of textuality." "Tuckett's conclusions," Milavec writes, "cannot be effectively refuted unless one calls into question the bias of textuality and the ignorance of orality which mark his methodology." (466) For Milavec it seems that Tuckett's argument is finally undermined on account of what Werner Kelber would call its "cheirographic bias." Of course Kelber himself has been criticized for exchanging one oversimplification (à la Overbeck and Bultmann, failing to differentiate written and oral media) for another (failing to recognize that the first-century world was a convergence of both oral *and* cheirographic cultures). But granting the merit of Kelber's basic insight, which calls into question the long standing tradition of 'scissors n' paste' source criticism, is the appeal to cheirographic bias strong enough to unseat Tuckett's entire methodology? In this case, I think not. If Tuckett has successfully identified both Matthean and Lucan redactional elements in the *Didache*, it will not do to nullify the force of this argument by playing the oral trumpcard. The appeal to oral tradition only pushes the question back a step further. Let us grant, for example, that the Didachist did rely on oral tradition in writing Did 1.4-5, this still does not answer the question as to how he happened to replicate characteristically Lucan and Matthean material. Was it pure chance? Did the Didachist here have access to (via oral tradition) to M or L? This must be spelled out more carefully. Granted, our author may ask, "Well, how long do you want my book to be?" But as stands, Milavec's argument comes dangerously close to "heads I win, tails you lose." The case for an oral-based Didache must be advanced either with fuller explanation or on a different basis.

2. Towards a Verification of Oral Sources

Pop psychologists, when discussing dysfunctional family systems, sometimes speak of "an elephant in the livingroom." The elephant in the livingroom is that which everyone knows is there, but for some reason, no one or almost no one dares to speak about. I suspect that the elephant in the livingroom of Didache studies and indeed gospel form criticism, is the fact that we know have virtually no common understanding as to how oral traditioning actually worked. How precisely do we envisage these sayings being transmitted? In what contexts? For what purposes? J. D. G. Dunn has established a taxonomy for three models of oral theory in NT studies: (1) informal, uncontrolled (Bultmann); (2) formal, controlled (Gerhardsson); (3) informal, controlled (Bailey). But presumably the degree of formality and control is a function of form, and vice versa. Given the diversity of forms in the *Didache*, we can derive no blanket solutions as to how faithfully oral Jesus traditions would have been preserved in this mysterious document.

Add to this the dynamic of secondary orality or vaguely remembered texts. Given the way copyist worked in antiquity, from text to voice, from voice to ear, from ear to text, we must remember there is no such thing as pure dependence on written sources. Even the use of written sources normally implied some amount of oral/aural mediation, a mediation which no doubt gave occasional hostages to fortune. Add to this, yet still, the very real possibility that the Didachist had access to a harmony, one perhaps much like Justin's. Divergence from Matthew might be explained by the fact that the Didachist drew on a hybrid text, conflating Matthew and Luke. Deviations are by themselves insufficient to prove oral sources. In fact, until we come to a stronger consensus regarding issues of how the early Christian used their written traditions, the use of harmonizing traditions in early Christianity, and the nature of oral transmission, we can do little more than sophisticated guesswork in regards to these media-critical questions.^f (Perhaps Ian Henderson ("Didache and Orality in Synoptic Comparison," JBL 111 [1992] 283-306) is correct *for now*, when he, although emphasizing the oral nature of the Didache, declares the media-critical question "insoluble.")

For this reason, I suggest that if Milavec's argument, the case for consistent orality, is to have a long and prosperous life, it needs to be redirected and reframed. The argument needs to be redirected because, whereas the current *status quaestionis* in oral theory cannot give an adequately firm basis for proving or disproving the oral nature of the sources, the hypothesized triangulation between Matthew, the Didache, and the oral tradition may be more easily borne out. The argument needs to be reframed because, rather than trying to prove the existence of an oral tradition behind the *Didache*, it seems to me more appropriate and more in keeping with the fragile nature of the evidence (for both sides!) to seek to establish a case for coherency. This means that while an oral-based Didache cannot be *proven*, such a hypothesis can at least be shown to be *consistent* with the facts.

This redirecting and reframing, I believe, can be done by appealing to a certain canon which – for better or worse – has for a long time been widely accepted by the scholarly community: the Two-Source Hypothesis. Apart from arguments based on Matthean redactional activity, it seems that one of the most common objections against the notion that the Didache and Matthew shared a common tradition is the high degree of agreement between the two texts. But returning to the rule, "the greater the agreement, the more inclined we are to suspect a direct dependence," might we be able to establish a tolerable level of agreement between Matthew and Didache in relation to the oral source, based on the analogy of Matthew and Luke in relation to Q? If most scholars accept that Matthew and Luke independently drew on Q and show a degree of verbal similarity on account of their mutual dependence on Q, then this degree of verbal similarity would also provide a credible baseline for levels of Matthean-Didachean agreement as they independently draw on their common source. Once it can be shown that the amount of agreement between Matthew and Luke is no greater than the amount of agreement between Matthew and the Didache, then the same scholars who accept Q, would have to accept that the theory of common tradition behind Matthew and the *Didache* is a reasonable one. Of course, this would not prove the existence of such a tradition, but it would establish it within the realm of plausibility. Perhaps, given the lay of the scholarly land, it is best to forego claims to having demonstrated the oral background of the Didache, and settle instead for a coherentist approach, one which depends on the socio-historical and/or redactional arguments to carry the weight.

If, on the other hand, it turns out that Matthew is much closer in wording to the Didache than it is to Luke in the Double Tradition, this certainly does not disprove Milavec's reconstruction, but it forces the consistent oral position to conclude that Matthew and the Didachist were more faithful in preserving their oral tradition than Matthew and Luke were in preserving their putatively written source. This is possible. But is it likely? I think not. May the weightier argument win.

