

## Response to Mark Goodacre

Dear Mark,

I am honored that you have taken the time and the effort to patiently work through my book, and then respond to it at length on your blog. I am gratified by your positive comments and thank you for them – I am particularly gratified to hear your intention to use the book with Duke undergrads. But I am also thankful for your criticisms. These challenge me to think through my position anew; they also give me the opportunity to restate and defend my position, hopefully in fresh and clarifying ways.

As for the level/audience/pitch of the book – this was one of the most difficult aspects in writing it. While straddling on the monkey bars of popular and scholarly genres, it is inevitable that I shift my weight now onto the one foot (at the risk of frustrating some) and now onto the other (at the risk of shooting over the heads of others). Hopefully, there will be something for everybody, “as much as they were able to hear,” as it were.

My second frustration with writing the book was in not being able to engage the spectrum of positions – and you’re right about not giving due space to Tuckett and Snodgrass in the bibliography. An oversight, indeed. I also wish I could have given more space to Risto Uro, especially as his point regarding secondary orality is very salutary. However, unlike my three interlocutors, Uro has never drawn up a comprehensive and cohesive scenario.

Though you have made a number of other sage remarks I would like to pass over them in order to go straight to your first two points of criticism: (1) catchwords and (2) the Diatessaron and John. I think that will be enough to bite off in one blog.

### Catchwords

First, let’s talk about catchwords. Here you rely heavily on the reviews of Williams and Joosten. The former I have read; the latter I confess I have never heard of until now. I remember Williams contacting me shortly after the book was released as he was quite keen to review it. After reading his review for myself, however, I found it rather disappointing. Let me explain why by going to the portion you quoted:

Though this conclusion may seem impressively supported, in fact recurring problems in his reconstructions considerably reduce its support.

Now follow some examples:

Firstly, the reconstructions are not straightforward. Thus from the Coptic word 'earth' (saying 9) and the Coptic word 'world' (saying 10) he reconstructs the Syriac word 'earth', despite the fact that Syriac has a perfectly good word for 'world' (pp. 65-66).

Williams takes up my handling of GT 9 (‘did not take root in the *soil*’ [ΕΠΕΧΗΤ ΚΑΔ = ἐν τῇ γῆ? = ܠܐ ܝܚܘܿܩ?]). and GT 10 (‘I have cast fire upon the *world*’ [ΕΞΗ ΠΚΟCΜΟC

ΕΙΣ ΤΗΝ ΓΗΝ = ἐπὶ τὸν κόσμον? = ܠܘܘܢ?) and chides me for using ܠܘܘܢ in GT 10. I suppose he wanted me to use a different word, perhaps ܠܘܠܐ. My only reason for using ܠܘܘܢ, he implies, is because I want to make a connection between GT 9 and 10 in the Syriac. But if he has a problem with this, he should take it up with the author of the OS Luke 12:49a, where Jesus says, “I have come to cast fire upon the *earth* (ܠܘܘܢ),’ and the OS version of the Parable of the Sower in the triple tradition where ܠܘܘܢ also occurs just at this point (see, e.g. OS Matt 13:8 par.). (It seems to me rather hubristic – not to mention methodologically suspect – to try to improve on the word choice of the OS composer himself.) Thus there is every reason to believe that the *Diatessaron* used the exact same constructions in his rendering of Luke 12:49a and the Parable of the Sower, and if Thomas used the *Diatessaron* and wrote in Syriac, then these two words would have likely occurred in precisely this juxtaposition. So then, rather than being evidence for my being arbitrary, Williams’s illustration splendidly underscores the fact that for a large portion of this experiment my hands are tied; my word choice is controlled by the extant Syriac witness.

Williams continues with his charge of tendentiousness:

When it suits Perrin to render Coptic 'world' by Syriac 'earth' it is so rendered (p. 78), but on other occasions the Coptic word 'world' is rendered by Syriac 'world' (p. 83). The author is thus selecting the words used in his retroversion *in order to create catchwords*.

The first sentence I confess is true, for I try my best to use the appropriate Syriac given the context. But what about the second statement: am I really selecting words in order to create catchwords? Perhaps I am ‘creating catchwords’ in a sense. But in each of these instances, if I am tendentiously creating catchwords in the Syriac, what Williams does not tell the readers of his review is that I am showing the very same ‘favoritism’ to the Greek and Coptic! Because I have assigned the same catchword status to the Greek and Coptic versions on the basis of a semantic repetition, the precise Syriac reconstruction of the words in this case is moot. Williams’s criticism is equally non a propos when he says I’m tendentious in my linking ‘evening’ and ‘night’ (p. 115) and ‘belongings/estate’ and ‘house’ (p. 124), for again, all three languages earn ‘catchword points’ – again, on the basis of semantic overlap. The reader may be suspicious of such connections, but actually I am being generous to my devil’s advocate. If you think about a law of ratios, each and every three-way draw certainly doesn’t help my overall argument, but works against it. If these instances are among Williams’s best examples of my tendentiousness (cases in which my allegedly ‘creating catchwords’ actually blunts my argument), his charge does not even come close to being sustained.

Before I leave off with Williams, we note that he is unhappy with my translating ΠΤΩΜΑ (body/flesh/corpse) with ܠܘܘܢ (flesh): ‘similarly tendentious renderings from Coptic back to Syriac are 'corpse' rendered by 'flesh' (p. 106).’ The Coptic term has a wide range

so there are other ways to go with the Syriac. But are there any Syriac options necessarily *better* than ܦܠܫ (flesh)? Not in my mind. But forgive me for thinking that – whatever the precise Syriac words – *something* is going on when GT 55 contains the word ‘hate’ (which can be expressed in Syriac as ܦܠܫ) and then GT 56 contains the word

‘body/flesh/corpse’ (which can be expressed in Syriac as ܦܠܫ). Coincidence? Perhaps.

But then precisely the same coincidence reoccurs when a term denoting ‘body/flesh/corpse’ (GT 80) is again juxtaposed with a term denoting hatred (GT 81). (This by the way is as good an explanation as any for the doublets in *Thomas*.) There are similar such instances of word pairings, which I mention in *Thomas, The Other Gospel*; the statistical probability of such collocations being random is nil. I do think this pretty powerful evidence. And I don’t think, as Joosten apparently wants to say (and as Mick Jagger wants to sing), that this is ‘Just my imagination running away with me.’

Finally, while there are errors in the book (I am painfully aware), and while I am not the Aramaicist that Williams is, and confess to my error of adding a , to ܦܠܫ (p. 105). one example does not constitute ‘scores of errors.’ Even if Jesus felt that one *yod* was of crucial importance (Matt. 5:18), Mr Williams is going to need more than a misplaced *yod* to overturn my argument. (For the record, I had two Syriacists diligently serving on my dissertation committee – one being a leading Aramaicist/Syriacist – and they both expressed general satisfaction with the technical aspects of my argument, at least as far as the Syriac went.) I’m glad you note, Mark, there are reviews of *Thomas and Tatian* out there that are (far) more sanguine than the viewpoints of Williams and Joosten.

But let’s get back to the question of ‘fudging’ the catchword analysis, especially your concerns:

Perrin's answer to the perceived problem of "fudging" is to assert the statistical improbability of certain patterns of words occurring in the text by accident (pp. 87-8), but this avoids engaging with the most important question, which is not about "a blend of speculation and luck" (p. 87), but is rather about experimental bias, the selection of specific retroversions that make catchword links where other retroversions would not have done. Given the extent to which Perrin's case relies on the retroversion + catchword argument, criticisms of the earlier book need to be taken seriously. It may be that a good counter-argument can be made, but if so, it needs to be made rather than ignored.

I make no claims to pure objectivity, but I do try to be honest. The case I have set forth is cumulative as virtually any argument which seeks to discern an original language of composition beneath a translation must be. Undoubtedly, choices, even guesses, have to be made – hopefully they are good and reasonable cases, and attain to some level of certainty. This is something that has to be argued out on a case-by-case basis, which I try to do as concisely as possible in my footnotes. If this doesn’t satisfy your issue of experimental bias, may I ask you, how would it be possible to make *any* argument for a document having been originally written or rehearsed in a language other than the one

represented in the extant text? A number of scholars, for example, feel that there is a Hebrew or Aramaic wordplay going on behind Matt. 7:6. Perhaps you feel – because there is no way of eliminating experimental bias in discerning semitic puns there – that such arguments are a priori unsustainable? I am not so prepared to send the likes of Dalman and Black packing.

Of course if you're looking for a kind of objective, positivistic evidence that can be verified or falsified, you will be disappointed. Linguistic retroversion is based on educated guesswork. I have done my best in this regard – but the OS has provided a helping hand at a number of points too. Neither can I verify or falsify catchwords (be they semantic or phonological), any more than any one can prove the existence of a single pun in the Shakespeare corpus. But our inability to prove a pun, either in Thomas or in Shakespeare, doesn't mean that they don't exist. It is a rather narrow epistemological framework (and I have a sneaking suspicion this is what Joosten is laboring under) that regards the unverifiable as inadmissible. (By the way, while it may be within the linguist's remit to determine whether I have matched the right Syriac word-sound with the right Coptic word, you don't have to be a Syriac or Coptic scholar to judge whether word plays are going on. At those places where my matches are deemed reasonable, I would far more trust the judgment of a student of poetry than an unimaginative linguist or text critic.) If historiography is the act of asking readers to reconsider a limited set of facts by giving them a new way of looking at those facts, then I am simply doing history of a linguistic sort. Nor should it be said that the thesis of *Thomas and Tatian* doesn't amount to much because it is merely reconstruction. All history is reconstruction.

My chapter four has other arguments in addition to the catchwords argument. What about my argument that seven points of divergence between the Oxy. Fragments and the Coptic can be explained by a parent Syriac text? What about my argument regarding the Thomasine Syriac redaction? What about my argument that Tatian and Thomas Christianity eerily share many of the same distinctive practices, and that it would have been nigh impossible for Tatian to have inherited these practices from Thomas Christians, although the reverse is entirely possible (*Other Gospel*, 99-106)? Admittedly, catchwords are an important piece of the pie of *Thomas, The Other Gospel*, but it is only one piece of larger argument.

### **Diatessaron and John**

So as not to wear out the patience of your blog readers, let me say just a few things about the Diatessaron and John. I do remember reading Parker's review and the question he raises there. I suppose I am not as personally bothered as you or David. I don't, at any rate, consider the relative absence of Johannine material a "problem." Perhaps I should, but let me tell you why I don't.

First, it is very hard for me to think of any Johannine speech material of any length that would make any sense in a document like *Thomas*. Generally, when Jesus speaks in John, he is either speaking in the midst of narrative (which Thomas eschews given the nature of

what he trying to do), dealing with topics Thomas doesn't care about (pneumatology, unity) or he is speaking self-referentially (e.g., the I AMs) – while Thomas buys into Johannine protology, he would have been less happy with the claims Jesus makes for himself in the gospels as a whole.

Second, while people often make a big deal out of Q and Thomas (you and I know better!), what is more significant is Thomas's penchant for the parable material within the double tradition. Judging by Irenaeus' words (Adv. Haer. 2.27.1) – and he could not have been completely wacked on this – a number of sects were adapting the parables to 'ambiguous expressions.' In reading the parables, each such individual would 'discover for himself as inclination leads him.' And 'in accordance with the number of persons who explain the parables will be found the various systems of truth.' Apparently, at the end of the second century, parables held a certain fascination for a vast number of sects. Could Irenaeus be talking about Thomas Christians? It is entirely possible. If so, we might expect Thomas Christians to have something of a fixation with parabolic material, including Jesus' enigmatic statements like, 'Give to Caesar what is Caesar...'. As you know there parables are found only in the synoptic material, not the Johannine material.

I know you have raised more issues than I have covered, but for now I must stop – especially if I have any hope of your readers following me to the end. More later on the Diatessaron and the Greek fragments.

Again, Mark, my many thanks. I have appreciated the interaction.

All best wishes,  
Nick