

## Down with Canards!<sup>1</sup>

A canard is a falsehood that has gained the status of 'accepted truth' within a discipline. Once upon a time, a chap named Hermann von Soden affirmed that his **K<sup>r</sup>** was a revision of his **K<sup>x</sup>**, probably done in the 12<sup>th</sup> century. The information available in his own four-volume work demonstrates that affirmation to be false. Nonetheless, that falsehood gained the status of 'accepted truth' within the discipline of NT textual criticism. It became a canard.

Consider the following statement by Kirsopp Lake:

Writers on the text of the New Testament usually copy from one another the statement that Chrysostom used the Byzantine, or Antiochian, text. But directly any investigation is made it appears evident, even from the printed text of his works, that there are many important variations in the text he quotes, which was evidently not identical with that found in the MSS of the Byzantine text.<sup>2</sup>

Having myself spent an occasional year in the arcane halls of academia, I have observed that the uncritical repetition of things that 'everyone knows' is really rather common, in almost any discipline. New Testament textual criticism is no exception, as Lake observed above.

I take it that Hermann von Soden was the first to formally identify his **K<sup>r</sup>** as a distinct text-type, the 'r' standing for 'revision', since he considered it to be a revision based on his **K<sup>x</sup>**. Well now, by definition a 'revision' is perpetrated by a specific someone, at a specific time and in a specific place. Within our discipline I gather that 'revision' and 'recension' are synonyms. Hear Hort: "The Syrian text must in fact be the result of a 'recension' in the proper sense of the word, a work of attempted criticism, performed deliberately by editors and not merely by scribes."<sup>3</sup> It is not my wont to appeal to Fenton John Anthony Hort, but his understanding of 'recension' is presumably correct. A recension is produced by a certain somebody (or group) at a certain time in a certain place. If someone wishes to posit or allege a recension/revision, and do so responsibly, he needs to indicate the source and supply some evidence.<sup>4</sup>

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<sup>1</sup> Dictionaries offer a variety of definitions for 'canard', but they all agree that it is false information, and imply that it was created with malicious intent. Of course those who repeat the canard may do so without malice, albeit they do so without checking the evidence.

<sup>2</sup> Kirsopp Lake, *The Text of the New Testament*, sixth edition revised by Silva New (London: Rivingtons, 1959), p. 53.

<sup>3</sup> B.F. Westcott and F.J.A. Hort, *The New Testament in the Original Greek* (2 vols.; London: Macmillan and Co., 1881), *Introduction*, p. 133.

<sup>4</sup> Hort did suggest Lucian of Antioch as the prime mover—a suggestion both gratuitous and frivolous, since he had not really looked at the evidence available at that time. (Were he to repeat the suggestion today, it would be patently ridiculous.)

So, upon what basis did von Soden claim that his **K<sup>r</sup>** (that I call Family 35) was a revision of his **K<sup>x</sup>**, and created in the 12th century? Had he really paid attention to the evidence available in his own *magnum opus*, *Die Schriften des Neuen Testaments* (4 vols.; Göttingen: Vandenhoeck und Ruprecht, 1911-1913), he could not have done so, at least not honestly. But was he honest? At least with reference to John 7:53 - 8:11 (the *P.A.*), I think not. He claimed to have collated some 900 MSS for that pericope, and on that basis posited seven families, or lines of transmission, and even reproduced an alleged archetypal form for each one. Hodges and Farstad took his word for it and reflected his statement of the evidence in their critical apparatus; and I reflect the H-F apparatus in mine (for that pericope) for lack of anything better (except that I guarantee the witness of **M<sup>7</sup>** [my Family 35], based on my personal examination of Robinson's collations). Some years ago now, Maurice Robinson did a complete collation of 1,389 MSS that contain the *P.A.*,<sup>1</sup> and I had William Pierpont's photocopy of those collations in my possession for two months, spending most of that time studying those collations. As I did so, it became obvious to me that von Soden 'regularized' the data, arbitrarily 'creating' the alleged archetypal form for his first four families, **M<sup>1,2,3,4</sup>**—if they exist at all, they are rather fluid. His **M<sup>5&6</sup>** do exist, having distinct profiles, but they are a bit 'squishy', with enough internal confusion to make the choice of the archetypal form to be arbitrary. In contrast to the above, his **M<sup>7</sup>** (that I call Family 35) has a solid, unambiguous profile—the archetypal form is demonstrable, empirically determined.

Once upon a time I was led to believe that von Soden's work was reasonably reliable. This was important because his work is basic to both the Hodges-Farstad and Robinson-Pierpont editions of the Majority Text. However, the *Text und Textwert (TuT)*<sup>2</sup> collations demonstrate objectively that not infrequently von Soden is seriously off the mark. With reference to von Soden's treatment of codex 223, K.W. Clark wrote, "Furthermore, our collation has revealed sixty-two errors in 229 readings treated by von Soden".<sup>3</sup> 27% in error (62 ÷ 229) is altogether too much, and what is true of MS 223 may be true of other MSS as well. Please stop and think about that for a minute. 27% in error cannot be attributed to mere carelessness, or even sloppiness; mere

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<sup>1</sup> 240 MSS omit the PA, 64 of which are based on Theophylact's commentary. Fourteen others have lacunae, but are not witnesses for total omission. A few others certainly contain the passage but the microfilm is illegible. So, 1389 + 240 + 14 + 7(?) = about 1650 MSS checked by Robinson. That does not include Lectionaries, of which he also checked a fair number. (These are microfilms held by the *Institut* in Münster. We now know that there are many more extant MSS, and probably even more that are not yet 'extant'.) Unfortunately, so far as I know, Robinson has yet to publish his collations, thus making them available to the public at large.

<sup>2</sup> *Text und Textwert der Griechischen Handschriften des Neuen Testaments* (Ed. Kurt Aland, Berlin: Walter de Gruyter).

<sup>3</sup> *Eight American Praxapostoloi* (Kenneth W. Clark, Chicago: The University of Chicago Press, 1941), p. 12.

carelessness should not exceed 5%. It really does look like the reader is being misled, deliberately, and that is dishonest. H.C. Hoskier was not entirely mistaken in his evaluation.

Furthermore, how could  $K^r$  be a revision of  $K^x$  if  $K^x$  does not even exist? Soden himself was perfectly well aware that there is no  $K^x$  in the *P.A.* H.C. Hoskier's collations prove that there certainly is no  $K^x$  in the Apocalypse. We are indebted to the *Institut für Neutestamentliche Textforschung* for their *Text und Textwert* series. A careful look at their collations indicates that there probably is no  $K^x$ , anywhere. Take, for example, the *TuT* volumes on John's Gospel, chapters 1-10. They examined a total of 1,763 MSS (including fragments) (for 153 variant sets) and included the results in the two volumes. Pages 54 - 90 (volume 1) contain "Groupings according to degrees of agreement" "agreeing more often with each other than with the majority text". Only one group symbol is used, precisely  $K^r$ —the first representative of the family, MS 18, heads a group of about 120 MSS, but all subsequent representatives have only a  $K^r$ . Of the 120, the last six show 98%, all the rest are 99% (74) or 100% (40). I would say that Family 35 in the Gospels has over 250 representatives; the ranking here is based on only 153 variant sets (but see what happens below).

The group headed by MS 18 numbers 120, and is the only one that receives a group symbol, being by far the largest. But are there any other groups of significant size? I will now list them in descending order, starting with those that have 40 or more:

<u>Group</u>	<u>size</u>	<u>coherence</u>
2103	52	95% (15); 97% (20); 98% (13); 100% (4)
318	44	96% (1); 97% (24); 98% (6); 99% (10); 100% (4)
961	42	97% (1); 98% (4); 99% (34); 100% (3)
1576	42	97% (1); 98% (4); 99% (34); 100% (3)
1247	41	97% (1); 98% (4); 99% (33); 100% (3)
2692	41	97% (1); 98% (4); 99% (33); 100% (3)
1058	40	97% (1); 98% (17); 99% (15); 100% (7)
1328	40	98% (6); 99% (33); 100% (1)
1618	40	100% (all)
2714	40	98% (6); 99% (33); 100% (1)

Now then, 961, 1576, 1247, 2692, 1328, 1618 and 2714 all belong to Family 35 ( $K^r$ ), which leaves only 2103, 318 and 1058. As we look at the 'coherence' column we note that 961, 1576, 1247 and 2692 are the same, and upon inspection we verify that the lists of MSS are virtually identical—so we may

add 40 MSS to the 120 already designated  $K^r$ . 1618 and 2714 have heavy overlap, and 1328 partial overlap, so we may add at least another 20. Now let us look at the three that remain: 2103, 318 and 1058. Remembering that the threshold for  $K^r$  was 98%, we note that over half of the 2103 and 318 groups fall below it, so those groups are not solid. 1058 fares better, but almost half fall below 99% (all the  $f^{35}$  groups are heavily 99% or 100%). It may be relevant to observe that MS 1058 is probably fringe  $f^{35}$ . So where is  $K^x$ ?

I will now list the groups between 25 and 39, in descending order:

<u>Group</u>	<u>size</u>	<u>coherence</u>
1638	37	97% (2); 98% (2); 99% (29); 100% (4)
710	34	94% (18); 95% (1); 96% (13); 98% (2)
763	34	97% (1); 99% (33)
1621	32	98% (1); 99% (24); 100% (7)
1224	29	97% (1); 99% (28)
66	28	98% (1); 99% (26); 100% (1)
394	27	99% (all)
1551	26	99% (all)
1657	26	99% (all)
2249	26	99% (all)
685	25	99% (all)
1158	25	99% (all)

Guess what: they are all Family 35 except for 710; a glance at the coherence gives the clue. If 710 is really a group at all, it is rather 'squishy'. The last six lists are all but identical, and there is considerable overlap among the others. Even so, a few more MSS can probably be added to the Family 35 list, and an examination of the remaining 300+/- groups (depending on where the cutoff point is placed) will doubtless add even more. And so on. So where is  $K^x$ ?

Gentle hearer, allow me to whisper in your ear: There is no  $K^x$ , it only existed in von Soden's imagination. Obviously  $K^r$  cannot be a revision of something that never existed.<sup>1</sup>

And then there is the matter of demonstrated independence. By definition a revision/recension is dependent upon its source. If there is no demonstrable source anywhere in the extant/available materials (which for the NT are really rather considerable), then it is dishonest, irresponsible and reprehensible to

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<sup>1</sup> See also the section, "Archetype in the General Epistles— $f^{35}$  yes,  $K^x$  no" in Part III.

allege a revision/recension.<sup>1</sup>

And then there is the matter of demonstrated antiquity. There are hundreds of places where  $\mathfrak{f}^{35}$  is joined by early witnesses, where there is variation, but without pattern. The crucial point here is the lack of pattern; without pattern there is no dependency. There being no dependency,  $\mathfrak{f}^{35}$  is of necessity ancient. There are over 30 lines of transmission within the Byzantine bulk, and  $\mathfrak{f}^{35}$  is demonstrably independent of them all.

But why then do we not have  $\mathfrak{f}^{35}$  MSS from before the 11<sup>th</sup> century? Well, why do you suppose that with few exceptions only  $\mathfrak{f}^{35}$  MSS have the Lections marked in the margin? Could it be because the Greek speaking communities used them in their worship services and for reading at communal meals? And what effect does constant use have on any book? I suggest, for the calm, cool and collected consideration of all concerned, that any worthy MSS would be in constant use, and therefore could not survive for centuries. Copies that were considered to be of unacceptably poor quality would be left on the shelf to collect dust, and they are the ones that survived.

However that may be, I invite attention to the following list of  $\mathfrak{f}^{35}$  MSS from the 11<sup>th</sup> century. The MSS within ( ) appear to be marginal members of the family, or are mixed.

<u>MS</u>	<u>Location</u>	<u>Content</u>
35	Aegean	eapr
83	Munich	e
(125)	Wien	e
(476)	London	e ( $\mathfrak{f}^{35}$ in John)
(516)	Oxford	e
547	Karakallu	eapr
(585)	Modena	e
746	Paris	e
(1164)	Patmos	e
1384	Andros	eapr
1435	Vatopediu	e
(1483)	M Lavras	e
(1841)	Lesbos	apr (IX/X—may be $\mathfrak{f}^{35}$ in Paul)

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<sup>1</sup> Please see "Is  $\mathfrak{f}^{35}$  Ancient?" in Part III.

1897	Jerusalem	ap	(I have done a complete collation, and it looks just as old)
2253	Tirana	e	(Introductory material indicates an 11 <sup>th</sup> century date)
2587	Vatican	ap	
2723	Trikala	apr	
(2817)	Basel	p	

To begin, we note that there are 18 MSS listed, and each in a distinct location (of course, some of those presently in Western Europe may have been acquired from the same monastery). Further, since they are internally distinct, they represent as many exemplars. Since exemplars must exist before any copies made from them, of necessity, and since many/most/(all?) of those exemplars must also have been based on distinct exemplars in their turn, even if someone were to allege a recension, it could not have been perpetrated later than the 8<sup>th</sup> century—simply impossible. Surely, because one must account for the geographical distribution.

Did someone concoct the  $f^{35}$  archetype in the 8<sup>th</sup> century? Who? Why? And how could it spread around the Mediterranean world? There are  $f^{35}$  MSS all over the place—Jerusalem, Sinai, Athens, Constantinople, Trikala, Kalavryta, Ochrida, Patmos, Karditsa, Rome, Sparta, Meteora, Venedig, Lesbos, and most monasteries on Mt. Athos (that represented different ‘denominations’), etc. But the Byzantine bulk ( $K^x$ ) controlled at least 60% of the transmissional stream ( $f^{35}$  = a. 16%); how could something concocted in the 8<sup>th</sup> century spread so far, so fast, and in such purity? How did it inspire such loyalty? Everything that we know about the history of the transmission of the Text answers that it could not and did not. It is simply impossible that  $f^{35}$  could have been ‘concocted’ at any point subsequent to the 4<sup>th</sup> century. The loyalty with which  $f^{35}$  was copied, the level of loyalty for  $f^{35}$  being much higher than that for any other line of transmission, indicates that it was never ‘concocted’—it goes back to the Original.

We need to take up the question of ‘level of loyalty’ and the ‘quality quotient’, comparing various lines of transmission on that basis. For example, why is it that an average  $f^{35}$  MS will have only one variant for every two pages of printed Greek text, while an average Byzantine bulk MS will have at least three variants per page, and an average Alexandrian MS will have over fifteen per page? Does that suggest anything about attitude, about taking one’s work seriously? By ‘attitude’ I mean specifically toward the exemplar being copied—was it an object of respect or reverence?

And then there is the silence of history. Although I have touched on this elsewhere, it deserves specific attention. Allow me to borrow from my treatment of the 'Lucianic Recension'.<sup>1</sup> John William Burgon gave the sufficient answer to that invention.

Apart however from the gross intrinsic improbability of the supposed Recension,—the utter absence of one particle of evidence, traditional or otherwise, that it ever did take place, must be held to be fatal to the hypothesis that it *did*. It is simply incredible that an incident of such magnitude and interest would leave no trace of itself in history.<sup>2</sup>

It will not do for someone to say that the argument from silence proves nothing. In a matter of this 'magnitude and interest' it is conclusive. Sir Frederick G. Kenyon, also, found this part of Hort's theory to be gratuitous.

The absence of evidence points the other way; for it would be very strange, if Lucian had really edited both Testaments, that only his work on the Old Testament should be mentioned in after times. **The same argument tells against any theory of a deliberate revision at any definite moment** [emphasis added]. We know the names of several revisers of the Septuagint and the Vulgate, and it would be strange if historians and Church writers had all omitted to record or mention such an event as the deliberate revision of the New Testament in its original Greek.<sup>3</sup>

Come now, is there anything mysterious about what Burgon and Kenyon stated? Is it not obvious? Please stop and think about it for a minute. The silence of history 'must be held to be fatal to the hypothesis'. In passing we may note that whereas Burgon was a staunch defender of the Traditional Text of the NT, Kenyon most certainly was not, being an advocate of the so-called 'critical text'.

And then there is the matter of 'supply and demand'. Those who catalog NT MSS inform us that the 12<sup>th</sup> and 13<sup>th</sup> centuries lead the pack, in terms of extant MSS, followed by the 14<sup>th</sup>, 11<sup>th</sup>, 15<sup>th</sup>, 16<sup>th</sup> and 10<sup>th</sup>, in that order. There are over four times as many MSS from the 13<sup>th</sup> as from the 10<sup>th</sup>, but obviously Koine Greek would have been more of a living language in the 10<sup>th</sup> than the 13<sup>th</sup>, and so there would have been more demand and therefore more supply. In other words, many hundreds of really pure MSS from the 10<sup>th</sup> perished. A higher

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<sup>1</sup> *The Identity of the New Testament Text IV*, p. 84.

<sup>2</sup> J.W. Burgon, *The Revision Revised* (London: John Murray, 1883), p. 293.

<sup>3</sup> F.G. Kenyon, *Handbook to the Textual Criticism of the Greek Bible*, 2<sup>nd</sup> ed. (Grand Rapids: Wm. B. Eerdmans Publishing Co., 1951), pp. 324-25.

percentage of the really good MSS produced in the 14<sup>th</sup> century survived than those produced in the 11<sup>th</sup>; and so on. That is why there is a progressive level of agreement among the Byzantine MSS, there being a higher percentage of agreement in the 14<sup>th</sup> than in the 10<sup>th</sup>. But had we lived in the 10<sup>th</sup>, and done a wide survey of the MSS, we would have found very nearly the same level of agreement (perhaps 98%). The same obtains if we had lived in the 8<sup>th</sup>, 6<sup>th</sup>, 4<sup>th</sup> or 2<sup>nd</sup> century. In other words, THE SURVIVING MSS FROM THE FIRST TEN CENTURIES ARE NOT REPRESENTATIVE OF THE TRUE STATE OF AFFAIRS AT THE TIME.

To conclude, I trust that the reader will not consider me to be unreasonable if I request that henceforth all informed persons cease and desist from calling Family 35 (**K'**) a revision at any time. Enough is enough! **Down with canards!**