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## An Answer to Tommy Wasserman's Response to my Reply

In my "Wasserman and Crucifixion" (in my web site) I showed that Wasserman's comments about my review of G. Samuelsson's book, entailed insinuations about my integrity, *e.g.* as to whether or not I had read the book. The policy here was that if he could compromise my honesty, then my review would be rendered worthless. At the same time attacking would be the best defence of an untenable position.

The simplest and, to my opinion, the more straightforward thing to do, would have been to admit that he had misrepresented me and that he had no reason to suppose that I had not read the book, etc. And thus the issue would have been resolved.

Instead, on the 5<sup>th</sup> of July Wasserman released a new missile. *In this he tries to confuse the reader about the real issue.* This is done by more insistently casting aspersions on my truthfulness, citing parts of my words but omitting enough to give them another meaning than I intended, and by defending Samuelsson on matters that I had never broached. (Naturally, citation must of necessity be brief, but not where the meaning is affected! He does, of course, refer to my writing in my web site). All this is done at the same time as he assures us that he has not read his friend's book yet! The natural question, How can he defend a book he has not read against a Reviewer who has read it? does not seem to bother him. I do not know where the scientific rule of not forming an opinion before one has studied the evidence has gone.

The reader who wants to know what exactly I have said must read my own words, not Wasserman's misleading half quotes. Thus,

1. making insinuations on my integrity he writes "Only C. (and ...) really knows the truth", while omitting my words: "[Any fair reader can see that my review does not betray ignorance of Samuelsson's interpretation, but, on the contrary, it points out that Samuelsson's interpretation of the Greek texts is unacceptable. This implies that I have actually read Samuelsson's book.](#)" (My having read the book comes out repeatedly in my Review and my "Wasserman and Crucifixion", not only in the many pages I refer to but also in my familiarity with the argument and the thesis, but all this is here ignored). Wasserman's reason for the attempt to cast aspersions on my

integrity is said to be that Samuelsson had “that very impression”! So, here impressions are raised to the level of categorical statements!

2. Wasserman continues: “C. goes on to ask why such a procedure – browsing, searching texts on one’s own and responding – would invalidate what he as a reviewer has to say about the dissertation” –implying that I admit to having done precisely what he attributes to me, i.e. made my own search without having read the book—and then replies to the effect that Samuelsson thinks that “C. is basically reproducing the traditional understanding”.

I am afraid my troublesome evidence cannot be explained away like this. Moreover, I have never admitted to anything of the kind. What I actually wrote was very different to what I am represented as having written: “But suppose for a moment that I did what Wasserman assumes that I have done”, namely, “Caragounis has browsed the work, made his own searches and then responded”. “In which way would such a procedure invalidate what I have to say about this dissertation? If I have done my own search and found that a proper study of the texts leads to another conclusion than Samuelsson arrived at, how can my evaluation of Samuelsson’s book be brushed aside with the above insensitive remark? Where is the logic in this reasoning?” This also indicates that in my criticism of this book, I am not simply following the “traditional understanding”.

3. When a little further down Wasserman quotes my words, he again skips the important “Is it not rather the case that here we have the exactly opposite situation, namely, that *precisely because Caragounis has read Samuelsson’s interpretation of those texts and found it wanting, that those are the best texts to cite to show that Samuelsson’s interpretation is wrong?*”.

4. Unnecessarily Wasserman tries to defend Samuelsson’s use of “a smaller number of occurrences” when he asks: “Why does Samuelsson treat ‘a smaller number of occurrences?’ Is it a sloppy omission or does it rather have to do with his method?...” The fact is that such a defense is totally superfluous, since I have never broached the issue. In fact, in my Review I have said quite plainly that “Samuelsson treats a smaller number of occurrences, but even these should have been sufficient to clarify the meaning of crucifixion” and in my Reply to Wasserman: “Here it is not a case that we need more texts about crucifixion (there are about 10.000 texts till the XVIth century and many more till the present day!), but that we need *to understand* what the texts are saying”. Quite clearly, then, my argument at this point is not that there are *other texts* unknown to

Samuelsson that contradict Samuelsson, but that the texts Samuelsson treats have not been interpreted correctly! Now I could not have said this unless I had read his interpretation! Nor has the fact that in my research I normally apply the diachronic approach to the study of Greek in any way interfered here, since, in this case, I show the existence of crucifixion not by later texts but by referring to pre-Christian evidence as well as to pagan texts from very early Christian times, where Christian influence is out of the question!

5. The end of this paragraph and the next two paragraphs, defending the limits of the dissertation, namely, that it relates to the background of the NT terminology, i.e. not going beyond the time of the NT, are questions that I never raised against Samuelsson! So, they are irrelevant in this connection. Nevertheless, since Wasserman now raises this issue, I should like to remark parenthetically that this procedure shows precisely the static view of the Hellenic language still held by many students of Greek and their cutting it up into several Hermetically isolated periods — a position that was exposed as untenable long ago by G. Hatzidakis. His groundbreaking work has been taken for granted in international Greek scholarship in general. The same point has been argued for by me for many years within the area of the New Testament. The Hellenic language needs to be approached in a natural way as a living organism with both continuity and change. But it must be a *critical* decision at every step. *The problem with many scholars of Greek is that they have assumed that the Hellenic language is dead, and that they can do as they please with dumb texts which cannot protest, when they are being misinterpreted.* But enough of this here.

6. Wasserman cites my Review: “Such an astonishing claim makes it incumbent on me ...” [important omission again, thereafter he continues] “to look critically into the kind of evidence that has led the author to such an extraordinary conclusion, namely, that Jesus most probably was not crucified, but died in some other way. e.g. through some kind of “suspension” (e.g. p. 372).” Then he asks: “Does Samuelsson really draw this conclusion, “that Jesus most probably was not crucified or has C. misunderstood him?” [important omission follows]. This question of Wasserman’s only shows that he is unacquainted with the contents of Samuelsson’s book and that he should, therefore, not argue with the Reviewer. However, whether I have misunderstood Samuelsson or not, can be settled right here and now. In addition to p. 372, we can turn to Samuelsson’s conclusion on p. 330. There he writes:

“If a suggestion of the holistic view of the terminology is heeded, that there was no distinct punishment of ‘crucifixion’ before the death of Jesus, it is plausible to say that the punishment of crucifixion, so to speak, came into being on Calvary — *or rather in the later Christian interpretation of the texts depicting the events on Calvary*” (my emphasis).”

Samuelsson’s above statement puts the matter beyond all doubt. There is no way in which his words can be explained away, nor can it be argued that I have misunderstood him, unless, of course, he does not mean what he writes!

While I appreciate Wasserman’s feelings for Samuelsson, I am sorry to say he writes more as a faithful friend rather than as an objective scholar.

7. Finally, since Wasserman mentions and quotes from his correspondence to me, I wonder why he did not find it in his heart to quote also from my correspondence to him? There I wrote:

“With regard to Gunnar’s frustration, I can feel for him. I have met him only once and I liked him very much. I would be the last person to want to hurt him. But this is not a question of friendly feeling; it is a question of Truth and Facts. I could not do otherwise”.

As it must have become obvious to the reader, in his Response to my “Wasserman and Crucifixion” Wasserman has not produced any material arguments or evidence that vindicate Samuelsson’s thesis.

What is important is to go beyond these ‘formalities’ and look into what the evidence has to say. This is what I have done in my review. We have always known that some of the words I have mentioned relevant to the subject have had a wider use in Hellenic literature (and I have quoted examples), a fact that has misled Samuelsson into thinking that the meaning of these terms is unattainable and that they hence do not refer to crucifixion but to some kind of “suspension”. This is where the problem with this dissertation lies. Nevertheless, (ἀνα)σταυρῶ, ἀνασκολοπίζω, προσηλῶ, as well as σταυρός and ἥλος constructed appropriately—as it also becomes obvious from randomly given supporting expressions—do describe the horrible punishment of crucifixion (i.e. by being nailed on a cross and involving the shedding of blood) and that this did occur in very old times, and was widely practised by the Romans. *As I have shown, this evidence is older than the crucifixion of Jesus and it is also attested in pagan authors of the first two centuries, who could not have been influenced by Christian teaching!*

With this I consider my exchange with Wasserman on this issue closed.

*Note:* Wasserman's response drew forth remarks from Rod Decker about my review. The reader will note the total absence of any scientific argumentation against my position or in favour of Samuelsson's thesis. Thus, his remarks are of no help to either Samuelsson or Wasserman. His reaction is simply a personal attack on me for old grudges against my theses that the Erasmian pronunciation is unnatural for the Hellenic language and that the Hellenic language is one from the beginning to the present (cf. "Decker and his recommend" under "Debate" in my web site)—a position that is taken for granted by enlightened scholarship (see most recently the statements by F. R. Adrados, J. Blomqvist, *et al.* in *Greek. A Language in Evolution*, ed. Chrys C. Caragounis, Hildesheim: Georg Olms, 2010) and appreciated by the senior members of the *B-Greek* blog (which see), Prof Carl Conrad and Dr Randall Buth, to which blog Decker also subscribes.

I have by now seen 12 comments. If you will pardon me, I must say that I am not only surprised but also dismayed, if our scholarly competence takes us only as far as making comments on the reviewer's expressions, but is incapable of judging the matter objectively and materially. The question is: Can it be maintained, in view of my review, that Samuelsson's interpretation of the relevant words as meaning some kind of suspension but not crucifixion, is correct? In science our primary interest is not to comment on one another's expression, but to look for facts and evidence. Only Maurice Robinson was able to distinguish between expression, which is of secondary importance, and facts and evidence, which are of primary importance!