

JNCS

A CAUTIONARY TALE

Background and time line

This is an account of an apparent abuse of editorial power by Dr. Joseph Simmons, the current editor of the Journal of Non-Crystalline Solids, a journal where I published several earlier papers before Simmons became editor. I hope the reader will find this account of interest and value.

The present story begins with my submission of a manuscript to this journal in November 2008. It was entitled, “Problems with the original modulus formalism (OMF) data fitting method and the Ngai coupling model (NCM).” On 23 November I received an acknowledgment of receipt from the journal with the statement that the MS would soon be sent out for review. Unfortunately that was the last direct communication I received from them until 9 April 2009!

Meanwhile, the Journal’s information board soon showed the message “With the editor,” perhaps a sign of problems to come! After receiving no response to several emails requesting clarification, sent to the office and to the editor during December and January 2009, I requested the previous editor of the Journal to query Dr. Simmons about the situation. I believe he was kind enough to do so and by the end of January 2009 the information board stated that my MS had been sent for review.

As time went on and nothing changed, I sent several emails to the office and finally my one of 9 April 2009 was answered later that day by a “Program Coordinator” in the JNCS office, more than four months since submission. This response stated that two new reviewers, both with good turn-around times, had been appointed for the MS.

After two more months of inaction, I wrote the office again on 5/28/09, 6/15/09, and 6/21/09. As usual, no acknowledgments and replies were received. Finally, I wrote again on 7/13/09 and received an immediate reply that stated, in its entirety, **“7 authors have refused to review your paper to date. I am forwarding this to the editor for further explanation.”** This was indeed a surprise, since I have published many papers

and never before encountered such a response. If this statement were indeed true, it must have involved seven referees who were unfamiliar with the area, and none of them were thus likely to be among the five potential ones that I originally submitted to the journal, ones expert in the area and probably actually reviewers of some of my earlier papers in the Impedance Spectroscopy area.

So, if this statement is true, in the more than seven months since submission of the MS, no reviews had been received. I replied on 7/30/09 to the terse message of 7/13/09 and again provided the original list of five appropriate reviewers. But again, no response was received, and I wrote once more on 9/4/09, over nine months since submission of the MS.

Finally, on 9/20/09, I received a response from the editor, Joseph Simmons, more than **two months** after it was said to be sent to him from the JNCS office and more than **nine months** since submission. His response was a superficial review of the MS that did not discuss its scientific content at all. Although I replied to his message on 9/21/09, 9/24/09, 10/9/09, and 10/23/09 requesting clarification of statements in it, it was the first, last, and one and only one I received from him. In the next section, I discuss his review (attached at the end of this message), and speculate on why action on my MS was delayed so long and perhaps why inappropriate referees were selected.

Possible explanations

In his message, Dr. Simmons provided an ambiguous decision on my MS. On the one hand, he rejected it, while on the other he stated that if I emended it properly it could be reviewed for JNCS. I asked for clarification of this anomalous decision, including identification of specific unacceptable parts of the MS, but was unable to elicit any further response from him. Note that even if I followed his imprecise suggestions as best I could without specific inputs from him, the MS would have had to be submitted anew and would thus lose the original receipt date. What could be the reasons for the remarkable treatment accorded my submission by JNCS?

To suggest a plausible answer to that question, some background about the MS and its contents is needed. On 24 February 2004 a MS by I. M. Hodge, K. L. Ngai, and

C. T. Moynihan (HNM), entitled “Comment on the electric modulus function,” was submitted to JNCS and published in **351** (2005) 104. In the Abstract of this paper it was stated, “Arguments in favor of the electric modulus formalism are reviewed, and several misunderstanding and misrepresentations are corrected.” Unfortunately, no mention was made in this paper of previously published works by me that showed the modulus formalism to be incorrect and inapplicable. Although this criticism was well known to both Ngai and Moynihan, Ian Hodge, the first author of the HNM paper, later wrote me that **he was unaware of it and was not told of it by his co-authors!**

Using the numbering in the serial listing of my scientific papers in my website (<http://jrossmacdonald.com>), those in print before final submission of the HNM MS in 2004 that discussed the failure of the modulus formalism were #'s 208, 212, 224, 226, 230, and 232, all now available for downloading at this site. It seems likely that any reviewers in the field of the HNM MS would have been aware of one or more of these papers and even in 2004 a Google search on “modulus formalism” would have identified at least some of them. Thus the “seven” reviewers of the MS were either unaware of the earlier work, since it was not mentioned in the HNM manuscript, or ignored it. In either case, they and perhaps even the editor who presumably selected them did an inadequate job.

On what basis did Dr. Simmons reject my MS and are his reasons valid? First, he objected to its lack of equations defining the K1, K0, and CK1 models, ones crucial to the fitting results included in the MS and to its conclusions about the failure of the modulus formalism approach. But any reader interested in the area would already know that in general no closed-form expressions for these models are available and that, nevertheless they are available in numerical form for accurate fitting of data using the free LEVM program, a fact mentioned in the same paragraph where the models were discussed. Any readers not familiar with these models and with the modulus formalism approach would probably not be interested in the work of the MS in any case but would be able to learn more from the references provided if they wished to do so. Thus, this potential reason for rejection is invalid

Second, he states, “The paper contains specific attacks on other authors such as suggestions of intentional **misleading comments**, and knowingly hiding information. We

do not publish **judgmental comments.**” And he also stated “Some of the comments in the paper could be viewed as inflammatory.” Presumably, these statements by Simmons are related to the following content of the MS. First, I quoted Einstein, who said “The right to search for truth also implies a duty: one must not conceal any part of what one has recognized as true.” I believe that this injunction required me to cite the reasons why I had concluded that the modulus formalism was an incorrect approach and why, in addition, such information was omitted from the HNM paper. Had I not done so, I could be criticized for “knowingly hiding (relevant) information.” If one accepts Einstein’s command, my actions in following it should not be taken as reasons for rejecting my MS.

It seems to me that there is a vast difference between quoting the statement from Hodge mentioned above, a purely factual matter, not a “judgmental comment” and not in any sense a “misleading comment”, and “specifically attacking other authors.” The reader is free to make his own decision about whether Hodge was truthful when he stated that his co-authors did not inform him of relevant earlier work. If he was truthful, as I believe, then they did indeed conceal important information whether or not they believed it to be true. Such information was surely relevant to why the HNM paper contained no mention of reasons why it had been shown to be incorrect, additions which would have made most of its content irrelevant. Surely making facts available when they are relevant is different from attacking others, an *ad hominem* approach which I always try to avoid.

Finally, it is ironic that Simmon’s efforts to greatly delay and finally reject my MS, apparently because it might reflect on the inadequacy of some referees and raise unpalatable relevant facts, were fruitless in one sense. In a paper (#248) published in March 2009 in the Journal of Physics and Chemistry of Solids (70 (2009) 546), like the JNCS an Elsevier publication, the Hodge statement was included, along with mention of the inadequacies of the HNM work. This manuscript was quickly and positively reviewed and no negative comments from either reviewers or the editor were received.

I believe the above discussion is worthwhile to show the effects of editorial mismanagement, but I urge the reader not to castigate Dr. Simmons, who undoubtedly did his best.

Simmons’ rejection letter:

Subject: Your submission 10318

From: Joseph Simmons

Date: 9/20/2009

To: macd@email.unc.edu

Ref.: Ms. No. 10318

Problems with the original modulus formalism (OMF) data fitting method and the Ngai coupling model (NCM)

James. Ross Macdonald, D.Phil, DSc. (Oxon); James R Macdonald, D.Phil, D.Sc (Oxon)

Journal of Non-Crystalline Solids

Dear Dr Macdonald,

This paper has encountered unusual reluctance for an opinion from our reviewers. We have sent it to 7 different referees without anyone being willing to do a review. I have looked at the paper myself and while I worked in dielectric relaxation with Moynihan and Macedo, your argument concerns developments that took place long after I stopped detailed studies of the field. However, I did find several problems with the paper.

(1) As one reads the paper, it is very disconcerting that the key arguments are based on what you call K1, K0 and CK1 models that don't have representative equations in the paper and the reader has to go to your numerous (13) self-references to find equations to be able to follow your argument. This alone suggests declination of publication and may explain why our referees have not wanted to spend the time.

(2) The paper contains specific attacks on other authors such as suggestions of intentional misleading comments, and knowingly hiding information. We do not publish judgmental comments.

(3) Some of the comments in the paper could be viewed as inflammatory.

Now, I fully understand your annoyance with researchers who in your opinion ignore your publications, however, our journal only publishes technical results and not opinions. Consequently, I regret that we have to decline the opportunity to publish your paper. If you decide to amend the manuscript and only refer to your results and to the fact that the analysis that the OMF model fails to describe the data adequately, I am sure that our reviewers will agree to examine the paper and may give it an appropriate review.

Thank you for considering the Journal of Non-Crystalline Solids for publication of your work.

Sincerely,

Joseph H. Simmons, PhD