

## The winter of my discontent

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"Dear and esteemed professor, we are greatly impressed by your continuing high level contribution to the medical literature. This is to let you know that 5<sup>th</sup> issue of our new and high visibility journal ..... lacks only one contribution before it goes to the printers. You might consider contributing a critical editorial, a review or even an original article about your line of work for our upcoming issue. Should you decide to send in this manuscript we will surely give you a 50% discount on the current \$ 200 page charge." I am sure many of you also regularly receive similar such, if you will, malicious mail and you, as I do, delete it instantly and would not, again like I, bother replying to this mail by the fine English it really deserves. On the other hand, I can assure you my younger readers, one would perhaps never receive such offers to publish 30-40 years ago and if she or he ever did, such a rascally proposal would receive its deserved answer by a very harsh reprimand over at least a phone call while a repeated such proposal would be given to the press. Yet such proposals are perhaps the new normal these days.

On the other hand this transition to the current loose publication ethics has surely been not very abrupt. It was perhaps as expected as the recent invasion of the Capitol building in the USA where, one group of people let falsehoods dominate the public scene with a most unfortunate permissiveness for so long. No, I will not continue with political examples. Now I plan to give a very medical example of this disturbing indifference. Here is a quote: "... an important consideration, given the large cost differential among agents (aprotinin being far more costly than either aminocaproic acid or tranexamic acid); and nearly all investigations were sponsor- supported and therefore carried unavoidable bias." This quotation, which I frequently discuss in my talks, is from the introduction of an original study in N Engl J Med (1), some years ago. The authors unhesitatingly admit that all sponsor-supported work carry bias and most disturbingly state this bias is unavoidable. There is also the consideration that any acknowledging of any state of affairs can be considered as transparency, a very much in vague designation. I have considerable dislike for this popular designation. For if a caught liar admits that he/she is a liar and you find out that in fact, he/she has lied even more than you thought and he/she is not reprimanded in some way than this transparency would, I am afraid, surely, and simply encourage further lying.

Some years ago we had published work which showed that there was considerably less self-critique in basic science as compared to clinical papers in 3 main rheumatology journals (2). A short time after and on the occasion of a large international meeting I was in a session related to publishing where the editors of our most prominent journals had given talks. During the discussion I asked why there was practically no self-criticism in our highest impact basic science medical journals. The reply came from the editor of such a journal. "Because there is no such requirement of our authors to do so in our journal." Transparency or frank talk, indeed.

It was only last year an important transgression of publication ethics was highlighted in rheumatology. I became aware of it through the well known SLE expert Dr. D. Wofsy's eye opener editorial, "A Tale of Two Trials" in Arthritis and Rheumatology (3). We are told that two drug companies in partnership conduct two almost identical drug trials with the same medication in SLE. One trial finishes a month earlier without achieving its main outcome measure. After this realization, the said companies decide not to unblind the second trial but ask FDA to

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modify the main outcome measure, basically declaring a secondary endpoint of the first trial, the main outcome measure of the second trial and the second trial ends with this modified primary outcome achieved. Finally, the first trial gets published in *Lancet Rheumatology* (not yet a PubMed journal) while the second successful trial gets published in *N Engl J Med* with a reference to the previous publication (4). To top this all, in an accompanying editorial to the second trial the authors say "... Given the need to bring drugs to patients with SLE, the lupus community has urged regulators to consider trial designs that allow greater flexibility in defining success..." (5). So everything is again very transparent.

I want to conclude my display of the current situation with the fate of a letter to the editor of *Annals of Internal Medicine* I wrote some months ago. Apart from being an excellent journal, I have, over the years, repeatedly advised my younger colleagues to follow and aim their most important work to be published in this journal. I also have almost a sentimental tie to *Annals of Internal Medicine* in that my ever first publication had appeared there almost half a century ago (6). The article about which I had submitted a letter to the journal this time was an otherwise highly informative paper about vascular changes observed in 12 COVID-19 patient autopsies (7). My discontent was not about the content but the style. There were statements like "In 6 of the 9 men (two thirds) included in the study...." or "In this autopsy study of 12 consecutive patients who died of COVID-19, we found a high incidence of deep venous thrombo-

sis (58%).... Furthermore, diffuse alveolar damage was demonstrated by histology in 7 patients (67%), (*italics mine*)."

"I had commented in my letter that these percentages or the explanatory comment after the ratio given did not add but probably took away from the otherwise excellent credibility of what was presented. My letter immediately appeared in electronic format in the journal homepage. However, I noted that it was promptly deleted the next day.

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