

**CAN THE "OPEN DIALOGUE POLICY" BE  
MADE FOOLPROOF?  
Reflections following some recent articles  
published in *COP***

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*Old and New Concepts in Physics* is a unique forum in today's scientific community. It allows, at the Editor's judgment, publication of a paper which a referee advised to reject, with the negative report turned into a published Comment, followed by the author's Reply. As the Journal's subtitle indicates, this is an attempt to facilitate an open dialogue on the most profound issues in theoretical physics. In view of the field's unfortunate impoverishment observed during the last few decades (Smolin, 2006), this is a timely enterprise.

As a new member of *COP*'s editorial board I find this service an honor as well as a heavy responsibility. Giving up referee anonymity is bound to create expectable unease. Indeed, time and again I found myself in the role of the "bad guy," advising against a paper's publication. The Editor sometimes heeded my advice but on another time chose to publish a paper I advised to decline (Ignatovich, 2008a). Later, when I saw the forthcoming publication of a debate over an earlier paper (Georges, 2008; Sharma, 2008), I again strongly advised canceling the publication. The editor, understandably, chose not to do so.

Without doubt, no editorial decision can leave everyone happy, even the more so with *COP*. It is therefore only with Prof. Kapuscik's kind permission that I revisit the above cases and share with the readers my concerns about them. Disagreements aside, I concur with the Editor's authority in these matters which, with every new issue, face him with difficulties spared from editors of ordinary journals.

## 1 Minding Earlier Submissions History

Let me begin with Ignatovich's (2008a) paper, for which I served as the referee. Should I explain again why I found it unsuitable for publication? This would be falling into the very trap which I wished to avoid in the first place. Rather, in what follows I shall focus on the editorial question: *How can we avoid the open-dialogue policy turning the Journal into a refuge for papers that were duly rejected by other journals?*

In reviewing the paper I relied on another exhausting but constructive procedure of *COP*: An author whose article was rejected by other journals is requested to submit the correspondence pertaining to those earlier submissions. I began reviewing this correspondence.

My concern then went further back to Volume *IV* which contains another paper by the author (Ignatovich, 2007). Here the appendix "history of submissions and rejections" reveals an astonishing correspondence with three leading physical journals. With *Phys.Rev.Lett.* the author failed to supply the referee report. With *Am.J.Phys.* he "fought hard" (sic) with three negative reports. With *Phys.Rev.C* he had again three negative reports from two referees, and then a sequence of "appellations" (appeals?) followed, some 11 letters, until the exhausted Editor simply stopped replying. All in all, this correspondence spanned over 15 pages of *COP*, longer than that paper itself (!). Then, upon submission to this journal, the referee report was again negative, this time published as a Comment, followed, of course, with another lengthy Reply.

This alone should offer an important lesson: *Merely replying time and again, at whatever length, to all reviewers, does not in itself constitute a dialogue with them.* This, indeed, was the case with the present paper: My report (Elitzur, 2008) recounts an incredible series of further "fights" that exhibited much shrewdness and wit but barely addressed the referees' scientific objections. I was also struck by the fact that while the principles and findings that the article claimed to have refuted were basic (the uncertainty principle) and simple (EPR-Bell), the alleged refutations were so convoluted that I failed to comprehend them. This, of course, might merely reveal my (and the other referees') incompetence, but then, I believe that in view of that article's so pretentious claim, at least one more referee report would be warranted.

## 2 Sufficient Initial Refereeing can Save much Trouble

Even graver concerns are presented by the debate published in *COP* over a paper by Sharma (Georges, 2008; Sharma, 2008). Again, I will comment not on the paper's soundness but on the procedure it underwent.

The original paper (Sharma, 2006) claims that Einstein's  $E = mc^2$  is wrong. It was published alongside with a single, very sympathetic comment (Kant, 2006) by a chemist from Himchal Pradesh University. This, without doubt, is insufficient for a paper that makes such

a sensational claim. *Exceptional claims require exceptional examination, even within the open dialogue policy.*

Moreover, a very lengthy version of that paper has already been published in *Physics Essays* (Sharma, 2004), the article submitted to *COP* merely repeating the claims made earlier. Thus, on top of the earlier concerns, the paper was not original.

Alongside with his papers, Sharma posted his claim in numerous sites, often getting into ugly personal quarrels. Then his *Physics Essays*' article elicited a response in *Galilean Electrodynamics*, a journal devoted to refuting Einstein's theories (Georges, 2007). Sharma, again, chose to reply in the Internet. His "Open letter to Professor George" (sic) was posted some dozens of times in numerous sites. Then Georges' rebuttal was resubmitted to *COP*, repeating almost verbatim the 2007 version. Now, of course, Sharma had the right to respond in *COP*, which he certainly did: He copied his "open letter" - with the same abusive style and almost incomprehensible English - from the Web. All in all, *COP* echoed a highly undignified fight already spread *ad nauseam* on the Web.

### 3 Concluding Reflections

At the end of the day, then, it is possible that *Old and New Concepts in Physics* had been privileged this year to serve as the first forum for one/two of 21<sup>st</sup> Century's first major revolutions, namely i) the first proof against the uncertainty principle and its consequences and/or ii) the first refutation of Einstein's  $E = mc^2$ . It is also possible, however, that dubious claims, of the kind that abounds in the Web, now boast the credibility of a peer-reviewed journal, with a great portion of the journal wasted on argumentative letters with no scientific value. Which is the case?

Perhaps I can draw from my experience with similar editorial boards, such as the journal of the Center for Frontier Sciences in Temple University, *Frontier Perspectives*. Papers that were rejected by mainstream journals for a good reason, and then viewed as "revolutionary" by a too-lenient reviewer, are often followed by other submissions by the same author, now harder to decline. In the long term, then, permissive reviewing compromises the level of the open-minded journal. Worse: In the longer term, it pushes mainstream journals to be even more conservative.

## Can the "Open Dialogue Policy" be made Foolproof?

How, then, can the Open Dialogue policy be made immune to misuse? Perhaps the inevitable subjective aspect of reviewing unorthodox articles can be balanced with the aid of some more objective criteria:

1. Make a thorough use of *COP*'s procedure of reviewing the article's previous submissions and the author's debates with referees. Did the author exhibit willingness to mind flaws in their work?
2. Consider whether this correspondence, if eventually published alongside with the paper, will prove to be a genuine scientific discussion in itself or merely an obscure argument about technicalities.
3. Ask colleagues' advice! This is a perfectly appropriate procedure for a referee, and even more so with *COP*. If your colleague sends you a report, you can offer them the choice of either having their report published as a Comment under their name, or anonymously, or as part of your own Comment.
4. Consult (though not uncritically) studies of "pathological science," such as Langmuir's (Langmuir and Hall, 1989) classic or more modern versions (e.g., Baez, 1998; Turro, 1999).

Indeed, Turro's (1999) simple question

How can a scientist tell whether a remarkable idea may lead to the Nobel Prize, which is awarded for science that changes the way scientists think and know - or to the Ig Nobel Prize, awarded to work that exemplifies the scientific process apparently gone amok?

is especially important for this Journal. I leave the above concerns for the readers to consider, and, if Prof. Kapuscik finds it appropriate, to continue the discussion in forthcoming issues.

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