

To the Editors of J. Phys. A:

I'm writing to enquire about the status of two "Comment" papers which I submitted on December 13, 2011. They both comment on the article "Sufficient conditions for uniqueness of the Weak Value" by J. Dressel and A. N. Jordan<sup>1</sup> (called DJ below), but they deal with completely different issues.

I am planning a long trip at the end of June, and probably won't be able to reply to correspondence for months thereafter. I am hoping to resolve the matter of the "Comment"s before then.

One of them, A/417115/COM, is very short and simple. According to your website, a report was received on January 18, but I did not receive a copy. Its evaluation is said to be still "In progress", which seems hard to understand given its simplicity and the long delay. I have wondered if its status might be somehow linked to the other "Comment", though I can't see how that could be considered proper.

The other, A/417459/COM, points out a serious error or gap in the proof of DJ's main result, which it calls the "General theorem" (GT). This "Comment" is listed only as "In progress". But since it has been that way for five months, it seems clear that something must have gone wrong. By comparison, DJ was accepted almost immediately, no more than a month after submission. It seems unlikely that a referee could have read carefully the dense proof of the GT in such a short time. (I could do it only because I happened to have covered much of the same ground in my own work, an unusual situation.) However, if the referee *had* read it carefully, it would take him<sup>2</sup> very little time to evaluate my objection to DJ's attempted proof.

If the referee hasn't produced a report in five months, then it seems clear that he has little interest in the correctness of the GT. Moreover, assuming it is the same referee that reported on DJ, he will have a vested interest in justifying his original favorable evaluation. This obvious vested interest along with the long delay should be taken into account in evaluating any report that he does produce.

If I may speak frankly, I imagine that DJ (like most physics papers) was probably accepted with minimal scrutiny based on an expectation of competence and integrity of the authors. Perhaps it is to be expected that any challenge to it would be subjected to a much higher level of scrutiny. I welcome genuine scrutiny. I would be surprised and happy to see a referee's report which made clear that he had actually read DJ's proof in detail, along with my objection. But, since referees are volunteers performing an often difficult and always tedious task, it is inevitable that the task will sometimes be done neither thoroughly nor well.

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<sup>1</sup>*J. Phys. A: Math. Theor.* **45** 015304, 1-14

<sup>2</sup>Or her, of course. I adhere to the long-standing and sensible grammatical convention that in contexts in which the antecedent of a pronoun is of indeterminate gender, either the male or female pronoun may be used to refer to the antecedent, i.e., either "him" or "her" is used in place of "him or her" or "her or him".

It seems to me that a commonsense way to proceed would be to ask the authors of DJ if they agree that the proof of the GT which appeared in J. Phys. A is in error or incomplete. There is some chance that they will recognize an error, thus saving the referee the time of carefully reading the proof.

Have you furnished copies of the “Comment”s to the authors? If so, when?<sup>3</sup> Did they return any reply? If so, may I have a copy? Most journals send “Comment” papers to the authors and replies to the author of the “Comment”.

There is one point which would be much easier to examine than the proof of the GT and whose resolution might be sufficient to break an impasse. The GT assumes a hypothesis, its (iv), which might look superficially meaningful to a referee who did not think carefully about it, but which I contend is so unclearly stated as to be almost meaningless.

Appendix 1 of an expanded version of A/417459/COM, posted in arXiv:1202.5604v3, analyzes in detail possible logical meanings of hypothesis (iv). DJ’s objection to my counterexample to the GT seems to require a great expansion of (iv) beyond any logical meaning. If after reading this appendix, a referee is willing to assert publicly that (iv) as written carries a clear, unequivocal logical meaning which is what DJ claims, then I suspect that we would have irreconcilable differences.

However, I do not know this, and if it does happen (which seems unlikely), I would still appreciate an explanation of how DJ extracts from (iv) as written the meaning which it uses to dispute the counterexample. At present, I am completely mystified.

When I submitted the “Comment”, I enclosed a copy of a letter to the authors asking for a clarification of (iv). They refused to reply. When subsequently DJ was accepted for publication, a main motivation for my (denied) request for the referees’ reports was in hopes of obtaining such an explanation. I am willing to consider other interpretations (and might withdraw the “Comment” if I agree that they have merit), but obviously I cannot do so if no one will discuss the substance of the matter. I hope you can understand my frustration.

On the other hand, suppose that after careful consideration, the referee agrees that readers could not reasonably be expected to guess the meaning of (iv) which DJ needs to refute the counterexample. In that case, I hope you would agree that professional ethics requires some sort of correction of DJ. Otherwise, it will be impossible for readers of J. Phys. A to understand what the GT actually asserts and to evaluate the counterexample and DJ’s attempted refutation of it. Publication has given implicit endorsement to DJ’s claims. If I am right, that will ultimately reflect negatively on J. Phys. A.

Clarifying (iv) would not resolve the larger question of the correctness of the GT, but it would be a start toward establishing a dialog and advancing toward some sort of agreement. The great frustration of my position is that no one seems competent or willing to discuss the GT in detail. The authors refuse to communicate with me. An offer (in my previous advisory report on DJ) to discuss in detail with the referee my objections to the proof was ignored. Had there been some discussion at that time, it might have been possible to have

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<sup>3</sup>The date is important to me for reasons which I omit for brevity.

done something about the proof errors before they were published, thus avoiding embarrassment and saving many people much work.

I am sorry that this letter has been so long, but I think that all of its points are important. I hope that the unifying thread of my frustration at a lack of communication is obvious. If there is anything wrong with the analysis of DJ in my “Comment”, I want to know about it so that I can correct it.

Sincerely yours,

Stephen Parrott

May 13, 2012