

Flagrant Misconduct of Reviewers and Editor: A Case Study

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Abstract A case of a particularly severe misbehavior in a review process is described. Two reviewers simply copied and pasted their critical comments from their previous reviews without reading the reviewed manuscript. The editor readily accepted the reviewers' opinion and rejected the manuscript. These facts give rise to some general questions about possible factors affecting the ethical behavior of reviewers and editors, as well as possible countermeasures to prevent ethical violations.

Keywords Reviewing process · Editor · Conflict of interests · Ethical conduct

Introduction

In the domain of academic ethics, a large amount of research is devoted to unethical behaviors of students (e.g., cheating) and scientists (e.g., data fraud, plagiarism,

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duplicate submission). In contrast, there are surprisingly fewer studies about ethical aspects of peer review and editor decision (e.g., Borkowski and Welsh 2000; Wager et al. 2009). Many publications are limited to simple and rather a content-free statement that peer reviewing and editor decision are processes that also have some ethical aspects. Accordingly, ethical codes are actively developed for authors but not for reviewers and editors. For example, editors require that authors state their conflict(s) of interest, but authors do not know whether editors and reviewers have any (Wager et al. 2009; Oleinik 2014).

This is surprising for at least two a priori reasons. First, the relation between the editor and the author is the relation between a powerful and a powerless person (Roth 2002), and it is a general feature of a democratic society that ethical flaws of people in power are prosecuted with more rigor than the vices of ordinary persons. It would be strange to put more responsibility on a simple worker in a factory than on a top manager of this factory. Second, reviewers remain anonymous much more frequently than the authors do, and the direct relationship between anonymity, on the one hand, and fraud and aggression, on the other hand, is common place in social psychology (e.g., Myers 2012).

Given the relative poverty of data (not to speak of theories), descriptions of (hopefully) rare and exceptional single cases can be useful. Such cases may indicate possible problems and give rise to working hypotheses to be tested in larger studies. Below, we report a case of blatantly unethical behavior of a highly renowned neurological journal.

Background

Several publications reported that patients with disorders of consciousness (DoC) significantly differ from healthy individuals and other neurological patients in terms of the extent of long-range functional connections that exist in the brain in resting state (Boly et al. 2008, 2009; Fernandez-Espejo et al. 2012; Laureys et al. 2008; Noirhomme et al. 2010). These data gave rise to a hypothesis that consciousness is particularly intimately linked with how the brain works at rest (i.e., in the absence of any task requirement), rather than how the brain responds to stimulation. Our group, for the first time, investigated brain functional connectivity in DoC patients not only in the resting state but also during two kinds of stimulation. The results were exactly the opposite of the above hypothesis: differences between patients and healthy controls increased from the resting state to simple stimulation, and from simple to more complex (affective) stimulation.

Case Description

We submitted the corresponding manuscript to a journal A and received two reviews with several critical comments, most important of which will be listed below. On the basis of these reviews, the journal rejected the manuscript. This decision might be regarded as doubtful, because the critical points were of technical

nature and merely required some additional analyses of the already obtained data; but the decision was by no way unethical because it is the unquestionable right of the editor to weigh critical question of reviewers in their balance with the aims and intentions of his or her journal. We took all the critical remarks into account, revised the manuscript accordingly (which, by the way, required only few days of work), and submitted it to journal B, one of the top journals in neurology. After about 4 months, we obtained a letter of rejection with two reviews attached. We had to read them twice, before we conceived of the reality: they were almost exact copies of the two reviews from journal A!

Importantly, the reviews were not just of similar content, but contained verbally identical assertions, e.g.:

1. “The major limitation of the study is that patients with complete different brain pathologies are mixed together within one group in the analysis of spatial characteristics and anatomical topography of fcMRI networks.”

On p. 11 of our manuscript we wrote that we conducted a separate analysis to test for differences between patients with different etiologies. Thus the point of the reviewer was simply false. Note, however, that even the grammatical error made in the first version of the review (“complete different brain pathologies”) was repeated in the second version. This is not simply the case that the reviewer did not read the manuscript that he was reviewing; he even did not read his own review!

2. “Besides the clinical diagnosis of the patients there is no information about the structural brain damage of the patients. To enhance comparability within and between patient groups authors exclude patients with major unilateral lesions without any further specification.”

Again, the structural brain lesions of patients who were included in, or excluded from, the analysis was done, first, in the manuscript (p. 15–16) in a brief form and, additionally, in the appendix (p. 2–3) with all detail.

The other reviewer was also not original in his second review, e.g., he copied and pasted from his review for journal A the following passages:

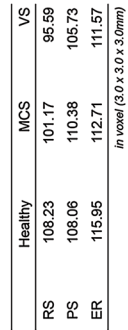
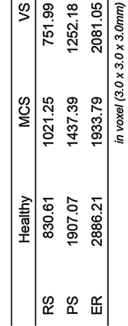
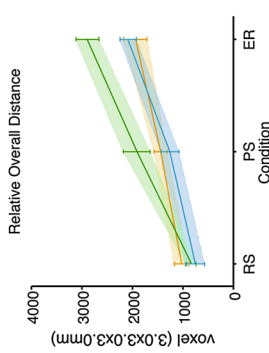
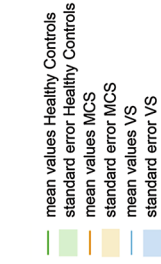
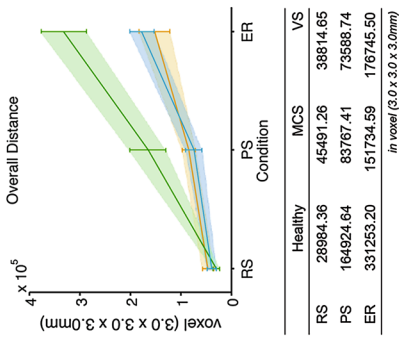
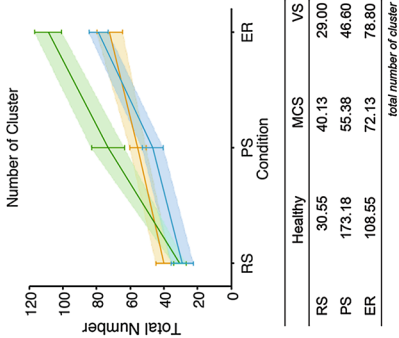
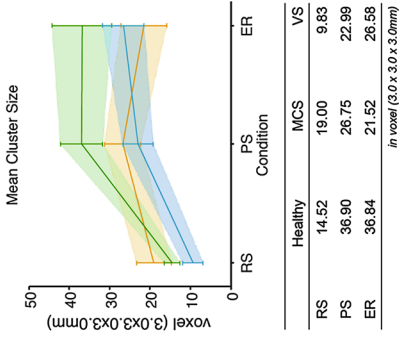
3. “Also, the repetition of the same analysis on each variable might either warrant a different approach, such as a MANOVA (or MANCOVA, see the next point), or some statistical adjustment of the significance threshold.”

As stated on p. 11 (line 9) of the manuscript, the required adjustment of the statistical threshold was done using the most widespread False Discovery Rate method (Benjamini and Hochberg 1995).

4. “Particularly in consideration of the authors’ discussion of subject variability it would be of great help to have error bars in the graphs, to understand the spread around the reported means.”

A brief look at our figure (see Fig. 1) might be sufficient to see the error bars. But if the only goal of the review is killing the manuscript with a slightly deviant point of view, it is not necessary to look at the figures.

5. “...along similar lines, in the discussion the authors mention results that are never presented to the reader (which have to do with the extent to which the passive stimulation task activates the ‘expected neural matrix’).”



◀ **Fig. 1** Results of the analysis of spatial metrics in three different conditions: resting state (RS), pain stimulation (PS), and emotional stimulation (ER). Each of the five graphs presents one spatial metric: Overall Distance, i.e., the sum of all Euclidian distances in a functional network; overall Number of Clusters; Mean Cluster Size, i.e., the sum of all cluster volumes divided by the number of clusters; Maximal Single Distance, i.e., the longest Euclidian distance to be found in the network; and Relative Overall Distance, i.e., overall distance divided by the number of clusters. All metrics except Number of Clusters are given in voxels (voxel size: 3.0 x 3.0 x 3.0 mm). Group means and standard errors of means are presented for healthy controls (*green*), Minimally Conscious State (MCS: *yellow*), and Vegetative State (VS: *blue*). In addition, group means can be found in a table below each graph. (Color figure online)

Quite straightforwardly, there is no such place at all in the manuscript, and a search using a Find function in Word or Adobe Acrobat indicates that the expression ‘expected neural matrix’ cannot be found.

Of course, we wrote a letter to the Editor-in Chief of the journal indicating all these scandalous errors. For some time, there was no response. After a second request, the Editor answered that the manuscript is nevertheless rejected on the basis of the negative review of an additional, third reviewer. This review was completely neutral concerning the issues attacked by the first two reviewers, but criticized other points that were not concerned by the first reviewers. Particularly, the third reviewer stated that our Introduction “is not sound”, although the first two reviewers found it fully unproblematic, and considered as a major problem the lack of significant differences between the two examined clinical groups (i.e., diagnoses Vegetative State and Minimally Conscious State). Again, this lack of differences did not bother any of the two first reviewers. The contradiction between the contents of different reviews did not, however, interest the editor; the negative tone was more important.

Discussion

To err is human. Two of the present authors are also reviewers in numerous journals and, possibly, we err in our opinions too. However, there are two minimal requirements to any review: the reviewer should (1) read the reviewed MS and (2) write a review in response to this MS, i.e., not in response to his/her own fantasies, memory contents, dreams, extrasensory insights, etc. The “reviews” in the present case did not fulfill even these minimal criteria.

To our best knowledge, such cases have not been described before. Both Borkowsky and Welsh (2000) and Resnik et al. (2008) discuss several kinds of reviewers’ unethical behavior and both regard “breaching confidentiality” and “borrowing ideas or methods from the reviewed manuscript” as the most severe sins that can be committed in this domain. However, breaching confidentiality at least implies that the manuscript has been carefully worked on, and a reviewer who steals an author’s ideas will, probably, further develop these ideas. The most frequent kind of reviewers’ misconduct, according to the same surveys, is harmless incompetence, which is nothing more than a random noise that can occasionally result in an error in the one or another direction, but cannot even bias the average.

Likewise, the most severe unethical practice of editors is considered “consistently steering papers to a reviewer whom the editor believes to be hostile to a particular theoretical, methodological, or substantive issue raised by the papers” (Borkowsky and Welsh 2000, Table III). In the present case, in contrast, the explicit aim of the reviewers’ and editor’s effort was just to kill the research they regarded as jeopardizing some interests. The reviewers did not notice that all their critical issues raised in their preceding reviews were adequately addressed and corrected; and the editor, faced with the demonstratively unethical behavior of the reviewers, did everything to cover the case.

An additional strong aspect of the story is the fact that it concerns a clinical study. Although the path is notoriously long from mere facts about how the brains of patients work to efficient diagnostics and treatment for the disease, the ultimate goal of all such studies is, of course, the development of methods for practical use rather than pure knowledge (e.g., Coleman et al. 2009; Kotchoubey et al. 2013). In the present case our data indicated the usefulness of testing patients under stimulation, and not only at rest. The intentional blockage of publication of these data may have serious ethical consequences for the clinical population in question, i.e., a potentially useful diagnostic test will not be developed, or its development will be substantially delayed. Notably, the opposite error would not yield big ethical problems. If, e.g., the approach indicated by our findings will, at the very end, be useless, this uselessness would become obvious after one or two replication trials, with no damage for patients.

We do not think that there is a means to completely rule out a severe misconduct. But there are some simple and cheap measures that can be applied to at least minimize its chance. As said above, two of the present authors are reviewers in several journals. As reviewers, we have never been asked to confirm that we have no particular interest to bias the editor’s decision and that, if we know the authors personally, this acquaintanceship would not affect our review. Given the very high probability of personal contacts between two research collectives in the same domain (Oleinik compared the population of experts with “a small village or even a settlement”: Oleinik 2014, p.62), the last requirement appears to be of importance. Similar questions can be posed to editors as well.

The process of reviewing and editing appears to be *the only situation in democratic societies* in which *people in power are completely free from any—even formal—obligations* to those who depend on them. At the same time it is very easy to ask both reviewers and editors a few questions, e.g., whether they personally know any of the authors, whether they believe this knowledge (if present) might affect their decision, and whether they have published data (or theories) that are strongly in line or strongly contradict to the data (or theories) presented in the manuscript. Even though very malicious people could, of course, give false answers to all such questions, most of us are not black-hearted but just strongly biased by our emotional sets and preferences; thus even such simple countermeasures may substantially decrease the degree of misconduct in reviewers and editors.

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