

PRIMARY JOB OF A SCIENTIST: ORIGINAL RESEARCH OR FAULT-FINDING?

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Some years ago I published a paper (HOŁYŃSKI 2003) in which – as one of many serious drawbacks of the obligatory peer-reviewing system – I emphasized the “*very high – out of any reasonable proportion with the typically resulting (if any...) improvements – costs [.. in terms ... mainly of time and effort of all the involved parties – the reviewer, author and editor, who could use it much more profitably ...]*”. My paper has not provoked any reaction – people do not like to question the sanctity of Sacred Cows, and anyway it is much easier to repeat uncritically a dogma of the allegedly enormous importance of the peer-reviewing system to assure the high level of scientific publications than to find and formulate serious counter-arguments to opposite contentions – but facts supporting my conclusions are now more numerous and persuasive than ever before. One of such facts, known to editors of any “peer-reviewed” journal, is the increasing difficulty in finding appropriate and willing reviewers – the question raised, among others, by GRAUR (2014), who “*discovered a negative correlation between the number of papers that a scientist publishes per year and the number of times that that scientist is willing to accept manuscripts for review*”. Such correlation is perfectly expectable: some people spend their time on effective scientific research, some others are more interested in “looking for a mote in other’s eye” – the time is not infinite, one must choose, so the correlation must be negative! GRAUR (2014) concludes that “*the biggest consumers of peer-review seem to contribute the least to the process*”, but this is a glaring misconception: I, *e.g.*, do not consider myself a “consumer” but rather a “sufferer” of the obligatory peer-reviewing system, which costs me (and, of course, any other author) hours spent on superfluous or even harmful adjusting my formulations to those preferred by the reviewers, or at least on idle quarreling and lengthy explications why I consider my original formulation better! If **an author** asks me – as some of my Colleagues frequently do – to read his/her manuscript and **informally** comment on it, I of course never refuse because I consider my duty to offer the **desired** help, but I always reject the **editors’** requests to serve as **formal** “peer-reviewer” whose objections (just or groundless, honest or unfair...) may cause the rejection of the paper by the editor, or at least cause superfluous stress and waste of time of the author. Honest discussion is either that carried directly between me and the author, or – concerning already published paper – openly, by **also published and signed** comments, not by “shooting from behind a fence”, behind the double shield of anonymity and non-responsibility...

To conclude: GRAUR’s (2014) proposal to “*ask senior authors to provide evidence of their contribution to peer review as a condition for considering their manuscripts*” evidently aims at putting the cart before the horse: the primary job of a scientist is **doing original research**; to subordinate it to accessory task of reviewing others’ work would, at that, be profoundly unfair (demanding scientists to do what at least many of them consider superfluous or detrimental), harmful to

the scientific progress (forcing most effective researchers to spend time on unfruitful “finding quarrel in a straw” instead of productive studies), and finally provoking further multiplication of (already now by no means rare...) poor, hurried, superficial and/or malicious comments... The proper solution of the problem would be just to “*abolish the peer review altogether*”, not nearly “*tantamount to doing away with science as we know it*”: for centuries NEWTONS, DARWINS, EINSTEINS and innumerable others neither reviewed others’ papers nor expected their own to be reviewed (indeed some – like *e.g.* EINSTEIN – angrily protested against the very idea!), and nevertheless the “*science as we know it*” has been developed mainly by those “*Giants on whose shoulders we stay to see further*”! I do not wish to repeat here the many-sided argumentation presented in detail in the paper cited above (HOLYŃSKI 2003), but quotation of concluding remarks seems necessary: “*Obviously I do not advocate automatic acceptance of any dabblery or charlatanry, I only suggest not to apply the cure which is worse than the malady... I am sure that the “optional” peer-review system should be much more adequate and much less harmful: a submitted paper should be looked through by the editor (concentrating on its **scientific** value, **not** on formal “editorial standards”!) – in most cases it will be immediately evident to him/her whether the results are reliable and non-trivial (in which case the paper should be accepted) or not (what would warrant its rejection). Naturally in those (rather rare) instances when the editor suspects serious (**scientific!**) problems but feels not competent to decide, he/she could ask for the opinion of an “expert” referee, whose conclusions (if negative) should be sent to the author who can either accept them or not – in the latter case it is the editor’s duty and responsibility to evaluate the soundness of the arguments of **both** sides, and decide to reject the paper (if the “worthlessness” of presented results seems evident) or accept it (otherwise)*”.

LITERATURE CITED

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