A Debate on Assessment System: Responsibility of Scientist

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Abstract: This article debates the responsibility of scientist. [Report and Opinion. 2009; 1(3):72-75]. (ISSN: 1553-9873).

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Publication has always been a central part of the research process, but there had, before this, been remarkably few serious investigations into its working (Rennie, 1986; Bailar and Patterson, 1985), though no shortage of opinion written in the absence of facts. The lack of information was particularly surprising given the strong discrimination being advanced for and against peer review (Rennie, 1986). Assessment of scientific quality is a dreadfully complicated problem which has no regular solution. Ideally, published scientific results should be scrutinized by true experts in the field and given scores for eminence and magnitude according to established rules. In practice, however, what is called peer review is usually performed by committees with general competence rather than with the specialist's insight that is needed to assess primary research data. Committees tend, therefore, to resort to secondary criteria like crude publication counts, journal prestige, the reputation of authors and institutions, and estimated importance and relevance of the research field, making peer review as much of a lottery as of a rational process (Cole *et al.*, 1981; Ernst *et al.*,1993).

In order to avoid any misinterpretation I would like to append the following restrictive remarks: scientists as a rule are not exceptionally ethical or modest in their personal lives, and there is no reason to expect them to behave better than or even differently from other people. Some scientists will even behave badly towards their fellow-scientists as soon as competition, priority and sometimes even prestige come into play. While these deficiencies are not linked very much they will eventually be ignored or tolerated by the scientific community, if a new researcher in question does admirable work and has never dishonored his real reputation, which is for being absolutely honest and trustworthy in scientific matters, that is, in obtaining and handling data and in using logic. To summarize: a scientist does not have to fit into the same categories in his work and in his life, but if he does not, he must be content to exist as a dual personality, and he must be aware of this fact.

These are some of the serious facts, but let me justify my statements as the problem involves at least two aspects: complexity and beliefs. Now the complexity; we all realize that in India the certain fields of science which are concerned with very complex systems are still in their infancy. In these fields the formulation of general proposition, laws, is still a risk, if in fact it is possible at all. Let me comment briefly on the second aspect, the impact of science and ideology on our present Indian Universities. A number of fields at our universities using the term science without deserving it, and as a consequence the formerly sharp distinction between speculative and scientific statements have rapidly disappeared.

Actually, in a philosophical view, Science can be defined operationally as an endeavor of human mind which aims at genuine knowledge. Genuine scientific knowledge is expressed as a true proposition: individual proposition (by means of data or facts) and then general propositions (or laws). True general propositions are those which describe the behavior of scientific models which are satisfying. But, if you start from beginning if after peer reviewing (as much of as a lottery) a true proposition (hypothesis) is satisfying for publication, and then it get published, the framework is then started with honesty and full scientific truth; but the individual proposition (by means of data or facts) get rejected because of an unfair, unjustified reviewing system, whether that data can be useful in framing future research priorities to the new world of the young unfolded mind.

Clearly, emerging scientific authors from India having limited resources do not necessarily publish their most citable work in journals of the highest impact because of such reviewing system, nor do their articles necessarily match the impact of the journals they appear in. This may be due to the journal's subject area and its relevance to the author's specialty, the fairness and rapidity of the editorial process, the probability of acceptance, publication lag, and publication cost (page charges) (Gordon 1984). Leading

researchers in a small field may thus be at a disadvantage compared with their colleagues in larger fields, since they lack access to journals of equally high citation impact and sometimes by the peer- review process.

If new scientific authors are not detectably rewarded with a higher impact by publishing their data, why are we so adamant on doing it? The answer, of course, is that as long as there are people out there who judge our science by its wrapping relatively than by its stuffing, we cannot afford to take any chances. For evaluation of scientific quality, there seems to be no alternative to qualified experts of the same field reviewing the manuscripts. Much can be done, however, to improve and standardize the principles, procedures, and criteria used in evaluation, and the scientific neighborhood would be well served if efforts could be concentrated on this rather than on increasing ever more stylish versions of principally ineffective markers. What is the function of science and what is the responsibility of individual scientist (the Critical reviewer) with regard to this significant problem? Reviewing can advise that how to accomplish goal but can not tell whether ought to choose that goal.

In case of peer review system, anonymous review system were used by most of the leading journals but there are no evidences, however it is assumed that this is better then an open review system. Walsh *et al*., 2006, have evaluated the feasibility of an open review system, in which they asked the reviewers of British journal of Psychiatry whether they would agree to have their name revealed to the authors whose papers they review; 408 manuscripts assigned to reviewers who agreed were randomized to signed or unsigned groups. After measuring the review quality, tone, recommendation for publication and time taken to complete each review, they came to a conclusion that a total of 245 reviewers (76%) agreed to sign. Signed reviews were of higher quality, were more courteous and took longer to complete than unsigned reviews (Table 1 and 2). Reviewers who signed were more likely to recommend publication. In overall this study supports the feasibility of an open peer review system and identifies such a system's potential drawbacks. In Indian context no such review system have been adopted or ever tried, the reason may be the same as in above case, those opposed to open peer review put forward convincing arguments in favor of maintaining their existing state of affairs. Low-ranked reviewers may hamper their career prospects by disparaging the work of powerful senior colleagues or be frightened into writing unsuitably favorable appraisal. Unwanted, inappropriate or even acrimonious dialogue may occur between author and reviewer, and professional relationships may suffer. Reviewers may become less critical, and scientific standard may decline. Some people ask why we should interfere with a system which appears to be functioning adequately without good evidences that there is a better way (Hyams, 1996). Although, these may be some of the drawbacks of the open peer-review system but Increased accountability in the reviewing process is essential, however, this is because it has become so important to publish in good journals, not only for the careers of individuals but also for research assessment exercise. From the desk of reviewer, one can say that reviewers give their valuable time free of charge and with little credit, yet they are performing an important job which plays a part in shaping our scientific future. It is critical that they do this job in the best possible way. By signing their name to a review they automatically become more accountable. Editors are forced to seek the best possible opinions for manuscripts and the editorial process is improved. Authors who are aware of the identity of their reviewer may also be less upset by hostile and discourteous comments (McNutt *et al*., 1990). These are some suggestions or recommendations for extrapolating the appraisal progression and construct new thoughts for framing the assessment procedures for Manuscripts of the juvenile scientists. But in short, in a free society which is necessarily pluralistic you will always find an array of goals, and for this reason any political decision is unavoidably a compromise and can never please everybody. The Logical Framework Analysis (LFA) can also be useful in this context (LFA, NORAD, 1999). Tracking progress next to cautiously defined output indicator provides a clear basis for monitoring progress; verifying rationale and purpose level progress then simplifies evaluation. But the alternatives of such distressful conditions in scientific community are still a topic of debate. To reveal these queries need internalized principles and patterns of behavior in each and every critic scientist by having three possessions: *Knowledge*, *Skills* and *Desire*. Knowledge reflects on "What to" and "Why to", Skill reveals on "How to" and the third one most important Desire means "Want to" ***think over it***!!.

Table 1 Review quality ratings by item in signed and unsigned groups

Quality item	Quality rating (mean (s.d.)) Signed group	Unsigned group	Difference (95% CI)
Importance of research question	3.02 (1.07)	2.85 (1.12)	0.16 (−0.06 to 0.39)
Originality	2.85 (1.16)	2.71 (1.18)	0.15 (−0.09 to 0.39)
Methodology	3.63 (1.00)	3.40 (0.93)	0.23 (0.02 to 0.43)*
Presentation	3.30 (1.03)	3.04 (1.01)	0.26 (0.05 to 0.48)**
Constructiveness of comments	3.75 (0.94)	3.48 (0.93)	0.27 (0.07 to 0.47)**
Substantiation of comments	3.45 (1.05)	3.25 (1.03)	0.20 (−0.01 to 0.42)
Interpretation of results	4.43 (1.16)	3.25 (1.14)	0.18 (−0.06 to 0.41)
Mean score	3.35 (0.86)	3.14 (0.86)	0.21 (0.03 to 0.39)*
Tone of review	4.51 (0.65)	4.27 (0.91)	0.25 (0.09 to 0.42)**
Time taken	2.05 (1.25)	1.65 (1.33)	−0.39 (−0.74 to 0.06)*

*$P < 0.05$, **$P < 0.01$.

Table 2 Recommendation on publication in signed and unsigned groups

Recommendation for publication	Signed group (*n* (%))	Unsigned group (*n* (%))	Total (*n* (%))
Accept without revision	41 (25%)	36 (24%)	77 (24%)
Accept with revision	53 (31%)	35 (23%)	88 (28%)
Resubmit with revision	43 (26%)	31 (20%)	74 (23%)
Reject	30 (18%)	51 (33%)	81 (25%)
Total	167 (100%)	153 (100%)	320 (100%)

Source: Walsh *et al.* (2006) *British journal of Psychiatry,* 176, 47-51

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