Scientific Gatekeeping and Exposure

Copotoiu Sanda-Maria*

Editor-in-Chief

When reading some of the main medical journals, one realizes that the tasks of the editors imply also an activity known as "scientific gatekeeping". It basically means a triage of the submited articles to dispose of those who do not comply with the rigours of correct medical research or to strategically avoid alien fields of interest to the journal. Editors are supposed to master clinical and/or basic research in order to fit to the job position and thus take responsibility for these operations. They are also accountable for their actions.

Facts speek for themselves: fraudulent, fabricated articles sieved by the gatekeepers' selection process; shallow peer-review process; pression exerted in the intent of using influence to promote publication; loading by dues to authors, moods' driven unfair rejections. Other facts could be added to the list, emerging as mushrooms fueled by frustration. One of them is mannerism in scientific writing. An impeccable form of written study, correct statistics, conforming IRBs end up in being published and often cited when appearing in important journals. Still, not all of them contain significant clinical findings. The package is attractive, the content dull. It is selling though. The terminal phrase "… further studies are needed to confirm our findings" is sometimes just a defensive tool to prevent challenge. I wonder how many of these studies are included in meta-analyses and/or cited and an analysis of this issues would be a step forward in enlighting the scientific writing process.

When a journal decides to take a short cut and publish on the fast track ahead of print certain articles, the suspicion of shallow peer-review process emerges naturally. Every and each submitted article should receive due and even attention from the editor and further if the case, from the peer-reviewers. Only that the editor has the duty to discriminate and reject articles that do not conform to the themes of the journal. What happens to the initially rejected articles? It heavily depends on the reason of rejection. If not on the agenda of the journal, the articles may be submitted to the appropriate publication. Experience tells us that some of them are rewritten and resubmitted. On the other hand, reconsideration of an initially rejected article might be successful. Peter Glass confesses in an editorial of Anesthesia ana Analgesia that this joournal "has a higher acceptance rate for appealed rejections than for original submissions" (1). He further speculates on atributing this to the fact that only authors certain of the value of their research and thus of the mistaken rejection bother asking for reconsideration. Of course a reviewer has to remain unbiased by nationality, gender or, more importantly, previous published research. Reputed and proliphic researchers turn to be a fraud, or if initially confirming to the standards of good clinical research, ulteriorlly project themselves on the slippery slope of mischievous conduct. A huge number of published articles by the same authors, if considering the time needed to initiate and then complete a research, even if parallel studies are managed, preclude the authors from being equally envolved in the research and moreover, constitutes a sometimes uncontrollable burden for the mentors, if they accept to author the articles. Thus when comparing the CVs of most published authors with their envolvement in actual research or clinical practice, one is stunned by the number of hours a working day streches to comprise. And this, provided their teaching asignements, managind duties, family and social life were inexistent, which is quite unbelievable. All these asignements and activities are time consuming. There for, according to the challenges, doctors establish their own priorities. This means conflicting the stakeholders: colleagues of research, sponsors, students, managers, families. I know there are merituous people, champions of research who do things better than others, but it is not only them who get published.

"Scientific research is the most competitive of all human activities" wrote Braun and Diospatony ten years ago (2). The article featured 50 years of citation indexing. They used the redefined term "invisible college" for the severe journal gatekeepers, seen as a decisive factor in the self-organizing system of sciences. The indexed journals edited in the USA and the similar European ones were assessed using scientometric tools and ranked as a consequence. The results were tabled on a nation-based performance. Irrespective of the ranking relying mainly on citations, they concluded that "the main factor in the scientific health of nations is the decision power the invisible college of journal gatekeepers dispose of" (2). If excessively severe with the submitted articles, one would end being healthy but insignificant to invisible and thus unknowm by all means and decisesively ignored by our foreign peers. A practical "black stain", or a "land to be explored". Thus we would be confined and restrained if willing to use scientific evidence, to the studies produced abroad, while we do export educated brains and we practice medicine on a daily basis.

To our further discomfort, the majority of the studies' conclusions are cautiously formulated as "more likely then not" or seen as a standard phrase and rarely as the "nearly impossible burden of *beyond a reasonable doubt"* (3).

Correspondence to Sanda-Maria Copotoiu
E-mail: sanda_copotoiu@yahoo.com

Should we then publish only those who claim and are able demonstrate to be completely evidence-based insulated, or should we enlarge the overpooring ocean of medical scientific writings in the hope to discover some brute diamonds? I would give a chance to those who are envolved with research and are able to come up with interesting results. The not so silent academic watch tower would take care of if eggregious or skipped by the peer reviewers.

Recently, Goldacre and Heneghan launched the idea that "Focusing solely on existing trials and observational studies would represent a *failure of vision* and ambition in an era when medicine has both the need to and the opportunity to innovate" (4). They also prompted the academy to call for simple practical improvements that would address legitimate concern. The nominated domains are: publication bias, cost of trials, better evidence, shared decision making and declaration of conflict of interest (4).

Even more alarming is the fact that "in the United States, a new proposal by the White House budget office has raised concerns about the misuse of peer review for political purposes" [a].

However, according to Brooks Hayes, Springer had to retract 64 scientific papers with fake peer-reviews, but so had to do Elsevier, Taylor & Francis, SAGE and Wiley.[b]

Fang et al reviewed 2047 biomedical and life-science research articles indexed by PubMed and subsequently marked as retracted on May 3, 2012 and revealed that only 21.3% of retractions were attributable to errors compared to 67.4% attributable to misconduct. They wrote that the percentage of articles retracted because of fraud has increased ten fold since 1975. The temporal and geographic pattern of retractions may reveal – or shed light on - the underlying causes. The highest priority should be reduction of error and fraud compared to plagiarism, since this last one is easily detected. Increased attention to ethics in the training of scientists, which alone seems to be unlikely to successfully curb poor research patterns is seen as a must (5).

It comes as a truism the fact that leadership and championship may hide unauthorized fuels, such as undue material or societal high profile incentives and imbedded habits, such as reshaping previously published research. Narcicism can transform promising former "good researchers" into the today pointed "bad guys". Addiction to being published in high ranked medical journals could be tagged as "publicism" if it were not a requirement to keep the job position.

And I would like to quote Eileen Gay Jones who wrote that "In the end, the process of filtering scientific evidence involves weighting conflictual values" (6). This quotation tries in my opinion, to elegantly resume the motto previously Kendall inserted in his article: "It takes a special combination of thick skin and scientific enthuziasm to be a journal editor" (7).

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