

Current practices of commercial scientific publishers and their implications for the scientific ethos

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There seems to be emerging, in recent years, a growing concern about the manner in which the commercial scientific publishers of today have helped distort the basic spirit of science by introducing essentially commercial ethos into scientific publishing. By attaching to their journals the rather dubious-valued index of 'impact factor' (IF), they have sought to enhance the perceptual value of their journals, making the authors go on a merry-go-round seeking to have their publication tagged by the high IF of the concerned journal. Furthermore, it has been noted that while the scientists provide the main research material for their publication and other services, like refereeing and serving on editorial boards, all for free, they are constrained to buy their own published product at exorbitant prices, which the journals have been charging at ever escalating scales, in terms of their price. This note seeks to analyse the situation and underscore the danger of scientists being exploited by the scientific publishers, and to forewarn of the dangers in the use of false-valued index like IF for the purpose of evaluation of a scientist's work and its deleterious effects on the spirit and ethos of science.

Historically science has usually been regarded as a rather sanctified profession devoted to the search for the truth, where many scientists have devoted their lifetime in the quest of unravelling the secrets of nature in all its myriad wonderful manifestations. This is not to suggest that the profession was ever entirely free from the basic human weaknesses and that some scientists have not erred on the wrong side bringing into focus the prejudices, rivalries and envies that the human mind is prone to. These natural though unpalatable traits notwithstanding, overall the scientific findings have resulted not only in the understanding of the intricate functioning of nature with respect to its physical aspects, but importantly, these have led to a complete transformation of the quality of life of the human race on this planet through their contributions in terms of fighting disease and hunger, and by providing freedom from drudgery through a variety of innovations in day-to-day life.

Scientists have been driven to this profession, not an easy one by any means, by a sense of curiosity and a quest to unravel the mysteries of nature. The only reward that they expect is a sense of consummation and satisfaction that they have been able to unravel another layer of the nature's beauty so far hidden to the ordinary eye. However, with the unprecedented expansion of the base of scientific activity and with an ever-increasing number of players, the original ideals of the profession have become

increasingly diluted. Over the past couple of decades there have been some disturbing trends in the entire scenario which leaves one with an uncomfortable feeling that all is not well with the spirit of science which, in my opinion, has been severely compromised as a result of some extraneous elements which have invaded and infiltrated the system for their own commercial gains.

A perceptive article by Ziman¹ entitled 'Is science losing its objectivity?' written almost two decades ago had already voiced apprehensions about the changing face of the pursuit of science. In response to a related question 'Can scientists produce objective knowledge in a world where their research is increasingly directed towards making money or meeting social needs?', he explores a wider canvas than what is being attempted here. It would be worthwhile, however, to carry forward some of the issues explored by Ziman in the present-day context.

Here I explore a limited issue: that of the role of the scientific publishing houses in the alteration of the scientific values as a result of practices which have now departed considerably from their original unwritten mandate. The publishers of scientific research were supposed to act, through their journals, as communication channels among authors all over the globe. Of particular significance were the journals run by Academies and Science Societies, like the Royal Society, American Physical Society, etc. which not only

provided communication channels, but also helped maintain high standards of scientific publications in a reasonably fair manner – notwithstanding an occasional conflict with the anonymous reviewers.

Besides the Academy and Society-run journals, there have always been journals brought out by publishing houses like Wiley, Elsevier and Springer. For a while the functioning of these publishing houses appeared to be rather 'reasonable', even though there were grudges and rumblings about the mounting costs of their journals. However, over the last decade or so their cost escalation to exorbitant levels coupled with other practices that have come into vogue, have led to some serious churning in the scientific community bringing into question the manner of conduct of these journals vis-a-vis the scientific community. Some of the practices and features of these journals have, willy nilly, directly and indirectly influenced the manner that we do science and have brought great distortions in our perception of the spirit of science.

This note is an attempt to examine how the scientific publishing houses have ended up influencing and engineering the minds of the scientists through measures like the 'impact factor' (IF), pitching it as a measure which they have been using to enhance the price of their journals to the point that recently, one of the wealthiest universities – The Harvard University – was forced to announce discontinuing many of the most expensive

journals. The greatest of the culprits have been named as Elsevier, Springer and Wiley.

A memo from the Harvard Faculty Advisory Council has summed up the profiteering by these publishing houses in terms which resonate with what many of us have also been thinking, by stating that ‘major publishers had created an “untenable situation” at the university by making scholarly interaction “fiscally unsustainable” and “academically restrictive”, while drawing profits of 35% or more. Prices of online access to articles from the two major publishers – Elsevier and Wiley – have increased 145% over the past six years.’

A sentiment which is equally shared by many researchers has been expressed as a paradox by Robert Darnton, Director of Harvard library, when he says that ‘We faculty do the research, write the papers, referee papers by other researchers, serve on editorial boards, all of it for free ... and then we buy back the results of our labour at outrageous prices’. Here, I shall try to analyse, what factors have led to this outrageous situation and what role have the scientists themselves played, albeit unwittingly, resulting in this situation.

In a recent post – Sick of Impact Factors – (occamstypewriter.org), Stephen Curry has highlighted the malady of IF afflicting the world scientific community. He sums up the paradox of the current situation in the following words ... ‘astonishingly for a group that prizes its intelligence, have acquired a dependency on a valuation system that is grounded in falsity. We spend our lives fretting about how high an impact factor we can attach to our published research, because it has become an important determinant in the award of grants and promotions needed to advance a career. We submit to time-wasting and demoralising rounds of manuscript rejection, retarding the progress of science in the chase of a false measure of prestige’. I believe the above quote says it all as to how, false-valued IF lures scientists into the trap of high-profile journals, whereby they continue to submit their papers to such journals enhancing further, in the process, their value perception in a positive feedback loop. It has thus acquired the growth characteristics of cancer which grows uncontrollably to the detriment of the main body. But it enables the scientific publishers to make windfall profits, since

they can tag the price of the journals to their IF.

It ought to be obvious, therefore, that much of the pain of the scientific community is self-inflicted since they, like any other human beings, like to find ways to express their superiority over their colleagues. The IF of the journals just happened to be such an index that they can flaunt for their papers, realizing little that it is ultimately a false measure of the value of their research.

Deleterious effects of the impact factor on the ethos and the basic spirit of science

Great distortions have appeared over the years in the manner that we do science, and the value that we attach to it, which I would now like to elaborate. The cult of IF is largely, if not wholly, responsible for this.

In my opinion, the gravest danger to the scientific environment locally as well as globally has arisen by the manner in which the peer scientific evaluation for promotions and career advancement of researchers has been almost entirely outsourced to IF. Earlier, we used to look for appreciation and approbation from our senior colleagues, who would give their personal comments, appreciative or critical, directly to us in a personal, interactive manner. The evaluation of the research work of scientists thus used to be based on the personal knowledge of the work, so acquired. There was implicit in it the process of mentoring of the young scientists.

As senior scientists became more and more involved with administrative responsibilities in a burgeoning environment, the time required to effectively interact with junior colleagues got compromised. There arose more committees to attend, to assess, to monitor and to plan the progress of science than those which progressed science, namely the close seminars and colloquia which gave opportunities for interaction among various scientists. These seminars and colloquia were serious affairs, where there were in-depth discussions on the topic presented, and it was a great learning process for everyone.

However, with the globalization of science, criteria of judgement too tended to become globalized, whereby the performance of scientists could be inter-

compared on a universal scale. It led to the evolution of the concept of ‘citation index’. The use of this index as a measure of one’s scientific productivity has now become so widespread and entrenched, that despite many of its reported pitfalls, it is now being used extensively, and if I may add rather indiscriminately and unwisely, as an evaluation device for the purpose of promotions and other such purposes. It needs to be emphasized that it is after all a proxy measure of one’s scientific productivity. It may, at best, serve as an indicator of one’s productivity in a particular field of current activity. But by no means can it indicate one’s creativity, which is different from productivity. This is indeed its greatest pitfall, apart from its other infirmities which have been discussed at length by other authors. Therefore, to effect any career advancement based largely on the citation index would only promote productivity and disincentivise creativity. It may well lead to complacency among scientists armed with high citation index, by giving them a somewhat exaggerated sense of achievement. The citation index as it is formulated is a highly flawed measure of one’s relative scientific merit among one’s peers because it does not discriminate between creative work and mere productivity. In fact, creative work may fare, in the short term, far worse in comparison, because many a time such work does not get recognized quickly enough to have an effect on one’s citation index.

Based on the citation index, there have now been invented new indices – the *h*-index and the *i10* index. In some sense the *h*-index is to an author what IF is to a journal. Both purport to provide a measure of the relative ‘status’ of the concerned entity – the IF among the other competing journals, and the *h*-index among the peers of the scientist. Together these indices have tended to convert science, which was meant to be a cooperative, enlightening activity guided by unwritten ethical values into a competitive, quasi-commercial activity, whereby journals make phenomenal profits essentially at the labours of scientists themselves. Scientists’ minds have become engineered to believe that the publication of their work in high impact journals will bring them greater laurels, enhancing in the process the perceived value of the journals. Since IF is determined by the citations, which too determine

the *h*-index, these indices get coupled together in an unholy alliance whose detrimental effects on the overall ethos of science are unfortunately not being generally appreciated.

The indices like the *h*-index and the *i10* index pose, in addition, a real danger to the very culture and ethos of science in yet another way. A scientist may tend to focus on the enhancement of his *h*-index, and the pleasures derived from making creative discoveries may get traded for 'pleasures' of accumulating high citation indices. When the highs of true scientific scholarship would get traded for the false 'highs' of high *h*-index, the true spirit of scientific adventure would stand severely compromised. It tends to create an ambience which does not encourage creating challenges for oneself and addressing such challenges.

I am given to understand that the desires and undesirable attempts to enhance one's *h*-index are no longer in the realm of mere possibilities. Young researchers in some parts of the world are already engaged in inventing ways and means of enhancing their *h*-indices in league with other researchers in a give-and-take mode. In other words, the *h*-indices are subject to manipulations. This is most alarming, if it is true.

The most unfortunate thing, as alluded to earlier, is that the *h*-index along with specification of IF of journals contributing to the *h*-index citations, is rapidly being adopted for the purpose of promotions, awards and grants. We are thus not only encouraging the insidious hold of the scientific publishing houses but, in the process, are also doing incalculable harm to the health of the scientific environment and the enlightening spirit of science. We have virtually handed over the controls of the defining spirit of science to the publishing houses, since they are the one's who seem to have set the tone now. One needs to pause a while and think now. The situation is well described by Curry in his recent post on Occamtypewriter.com as quoted earlier.

The fact, however, remains that both the *h*-index and IF are highly flawed measures as they are based on flawed premises and are therefore of little significance.

The next related question I wish to discuss is the exploitation of scientists by the publishing houses. This point has

already been made in the remark by Darnton quoted in the beginning of this note ... The situation portrayed above is indeed quite bizarre. I cannot recall any class of professionals, other than scientists, who have been so thoroughly and willingly exploited by the journals all in the name of professional duty, while the publishing houses merrily rake in millions at their expense, and continue to have a grip on the minds of the scientists, through the artifice of 'prestige' which the scientists have great weakness for – being a referee on a prestigious journal and being labelled as one of the 'best 100' referees and, of course, having published in 'high impact journals'. It has become quite a practice these days among authors to cite the IF of the journal in which their paper is published, while listing their publications.

When the journals were earlier run mostly by Science Societies, and Academies, the situation was different; they were run (and I believe even now they do so) on a non-profit basis, the (free) 'professional duty' was justified and appropriate. With most other journals now turning commercial enterprises, a (free) professional duty is simply exploitation. I believe that the time has come to recognize this crass exploitation and to question the tenability of this state of affairs.

Role of journals in dissemination of scientific discoveries in the evolving scenario – need for a paradigm shift

Science journals had obviously an important role to play in the past for the exchange of information among scientists, and for general dissemination of scientific thoughts that have been put forward periodically. With the advent of the internet, the relevance and the necessity of the print version of the journals is rapidly vanishing. The internet also provides an unprecedented connectivity among scientists of different disciplines. One may then well debate the relevance of the very existence of such journals now.

Given the fact, as pointed out above, that most of the work which the journals thrive on is done by the scientists themselves, namely writing the papers which provide the main material for the journals, evaluating the work of fellow scientists through the process of refereeing,

and serving on the editorial boards of journals, the question may well be asked as to what essential role do the journals play in the whole process of dissemination of knowledge so generated? It is essentially a managerial role. Should one then pay so heavily in a number of ways, for the mere managerial role? The simple answer is that we do not need a middleman manager. Time has come that scientists must introspect on the totality of issues involved and find ways and means to manage their own affairs and to rejuvenate and restore the spirit of science to its original pristine state. We must, therefore, banish the 'middleman' and stop the commercialization of science. The transition will not be easy, but a start has to be made. If we banish the journals as we know them, then all the problems associated with IF and citation indices will automatically vanish. Of course, it will require a deep psychological transformation on the part of scientists in the manner that we conduct our scientific affairs. It calls for a change in the mindset and practices which have been deeply ingrained in our psyche, so that we can end the dependency on this exploitative and degrading state of affairs.

A possible alternative approach to the management of scrutinization and dissemination of scientific findings

The main managerial task of the journals is essentially to get scrutinized the scientific findings of authors through their own peers acting as referees to determine not only the correctness of a submission, which is the most essential requirement, but also to gauge its efficacy and novelty content, and when reasonably satisfied have these published in them. It has been experienced by many a researcher that this process has often been rather tortuous. Journals seem to set their own standards for considering the publication of an article. This by itself is unexceptionable. But the trouble starts when this desire to maintain high standards of its published material degenerates into arbitrariness. Editors of some of the journals fail to judge the true merit of a submission because they have lost the ability to discriminate, if they had any, and are too frozen in the conventional framework to appreciate the novelty in a submission. An interesting counter-example may be

recalled here in the case of the Nobel laureate C. V. Raman, whose paper relating to the 'effect' was not recommended by the referee, but the then editor of *Nature* went ahead and published it over-ruling the referee. Or one can recall the case of Einstein's Nobel Prize paper, which was published in the *Annalen der Physik* even though the then Editor of the journal, Max Planck had reservations about the contents and conclusions of the paper. That is the kind of judgemental discretion, foresight and boldness displayed by the editors of yesteryears which enabled innovative and creative findings to come into light, which might have been otherwise lost or delayed.

In contrast, today a number of cases abound when so-called highly prestigious journals have rejected papers, which were subsequently awarded a Nobel Prize. A case in point is the Nobel Prize discovery of the vacuum tunnelling microscopy, which was rejected by the *Physical Review Letters*. There exists a whole list of such cases. One gets the impression that in most of the cases today, the editors are solely guided by the referees' comments and fail to exercise their own judgement, and in fact will sometimes choose the most negative of the referees' comments to reject a paper. In doing so they act more as 'post offices', and than true adjudicators of the submissions before them. (I am reminded of a case where S. Chandrasekhar, as the then editor of *Astrophysical Journal* had actually made a calculation himself to check the correctness of the conclusion of a submission by a friend of mine. That was in the late 1950s. It may be added that it was during his editorship that the *Astrophysical Journal* acquired the status that it has enjoyed since.)

The overall effect of today's scenario is that the authors get tossed around from one journal to another until a more enlightened and open-minded editor of some journal accepts to publish their work.

Editors of some 'prestigious' journals aided by their referees, pretend such high standards for their journals, that it is well nigh impossible to publish in them unless the submissions have a splash value. Journals like *Nature* and *Science* will send back your submission post haste as being unworthy of even a look. In fact, submissions to *Nature* and *Science* are prejudged, so as to determine whether a submission even deserves to be sent to

referees. For some reason of expediency, more and more journals are following this practice. This is perhaps all right, except that the procedure is rather opaque as one does not know whether some seasoned researchers are involved in this process of preliminary assessment or these submissions are ticked off by some inexperienced members of the editorial team. But one knows for a fact that in some cases *Nature* was constrained to tender apologies to some researchers whose work was rejected by the journal, but which went on later to be awarded a Nobel Prize. This only highlights the less than adequate and proper attention that some submissions receive by the editorial team of *Nature*. But it is in such journals that one has witnessed a spate of withdrawals of some fraudulent findings. The reason is that these journals have of late become more commercially oriented, and are more inclined to publish sensational and news-splashing discoveries, compromising the strict scrutiny that a finding must be subjected to.

Looking at the total scenario today in the matter relating to the dissemination of scientific findings to one's scientific peers and others, and the role that journals are playing and their conduct today have led to a total cultural transformation of science from its purely knowledge-based pursuits of yesteryears to its present IF-loaded pursuits which are linked to and driven by their commercial interests. They use the resources of the scientists themselves to advance their business interests. There ought to exist a more hassle-free mechanism for the availability of the work done by a researcher to his/her peers, and to be able to rid oneself of what may be referred to as the 'tyranny of journals'.

The alternative of 'open access journals' has been touted by some as a cure for the above-mentioned evil. But these so-called open access journals are hardly a cure, because the authors have to shell out rather large sums of money to have their articles published therein. Sensing the changing authors' mood in favour of the 'open access' journals, some publishers have started open access journals – such as *Scientific Reports* and *Physical Review X*. To be sure, they are now freely available to any reader worldwide. But eventually money has to be paid to the journals to publish them, to the tune of US\$ 1400 per publication, as processing charges. So the 'open access' option

is only a mirage! It does not constitute a viable solution as a truly freely available and affordable alternative.

A truly open access publication platform must be affordable to be viable and not commercially driven. It is then eminently desirable and indeed futuristic as being the ultimate objective. A rather disconcerting trend has, however, been lately witnessed whereby a number of journals, including some Indian ones, have gone into co-publishing agreements with the very publishing houses which have come under fire for being exorbitant and exploitative. While some have retained their portals as open access to their publications, others get to be approached only through the portals of these publishing houses. This is disconcerting because it is against the current trend of the desirability of being affordably open access. I am not sure whether the perceived gains to the journals from this co-publishing agreements could be more than marginal in terms of greater publicity of their contents and the possible enhancement of their 'IF', given the fact that for some of the Indian journals, as I have noticed, it hovers around ~ 1 . Of course, there could be monetary gains in terms of royalties. But whether such monetary gains are worth the academic compromise that they entail could well be debated. On the contrary, such journals may soon find themselves out of place because an affordable open access is the future, where 'IF' would be meaningless.

Lately there has been a plethora of journals of all kinds in the garb of 'open access', where they levy publication charges on the authors seeking to publish therein. They are essentially money-making enterprises with little scientific credibility, who wish to take advantage of the perceived proliferation of the research activity. Since all such journals will necessarily depend on the limited scientific manpower of competent researchers for the purpose of refereeing, these are entirely unsustainable and would quickly degenerate into mediocrity, as they will be forced to rely on not entirely competent researchers as referees.

A novel approach has been initiated by the biologists – the concept of the 'faculty of 1000', whereby regardless of where a paper is published, its novelty value is judged through a process of 'selection' by the faculty of 1000. A paper identified as being novel and of

sufficient interest then makes to a 'list of faculty selected papers'. Such a process can then eliminate the necessity of authors going around from one journal to another in ever-frustrating pursuits.

However, an affordable communication of the scientists' research findings to each other is still not visible on the horizon. A solution must, nevertheless, be found away from the standard mode where one still depends on academic publishers, whether through the open access route or otherwise.

One can go a step further and propose only one journal on-line worldwide for a given discipline; then one may well be able to rid entirely the concept of journals as we know it today. In fact, such a step has already been taken by the biologists in terms of a journal called '*eLife*', which is a collaborative initiative between researchers and its three prominent funders – the Howard Hughes Medical Institute, the Max Planck Society and the Wellcome Trust. This is a remarkable initiative, whereby the e-journal is managed by no fewer than 175 reviewing editors representing various disciplines of life sciences, overseen by an editor-in-chief and two deputy editors. The following is quoted from its website (<http://www.lifesciences.org>).

'Our Senior Editors and 175-member Board of Reviewing Editors work closely together to achieve *eLife*'s commitment to fast, fair and constructive editorial

decision-making while only publishing the most influential advances in life science and biomedicine.'

Making use of the full potential of the electronic dissemination of knowledge as against the restrictive print version, this concept represents a paradigm shift in the communication of scientific findings, and can lead to freedom from the stranglehold of the commercial publishers. Such an initiative needs to be replicated in other disciplines like physics and chemistry as well.

Some serious thoughts need to be given to the above concept, and to see how this can be implemented. Clearly, if the scientists have to be able to manage their own affairs, it would require some effort on their part as a voluntary service, which anyhow they have been rendering for the current journals. The *eLife*, in fact, provides a template for such an initiative in other disciplines which ought to be seriously considered. From an Indian viewpoint one could, to begin with, launch such an initiative with the support of all research institutions and funding agencies. This will provide a forum for a more constructive research environment with a single publishing platform, without different journals competing with each other for the relatively small number of prospective authors.

P.S.: Since the writing of this note, I came across an article by Subbiah Arunachalam, 'Cancel the subscription' pub-

lished in the *Indian Express* a few months back, which echoes some of the points made in this write-up.

P.S. to P.S.: Just as this note was being fine-tuned before sending for publication, I came across an editorial 'Research Assessment: Declaring War on the Impact Factor' P. Balam (Curr. Sci., 2013, **104**, 1267–1268) on the issue of IF prompted essentially by the San Francisco Declaration on Research Assessment, where he has traced the origins of the IF and pointed out its insidious effects on the spirit of science.

I had also learnt earlier about the San Francisco Declaration on Research Assessment, where many of the issues raised in the present article have been echoed. I strongly believe that we must all support this declaration and be enthusiastic signatories to it.

1. Ziman, J., *Nature*, 1996, **382**, 751.

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