The disastrous boomerang effects of “citation mania”

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Abstract The recent publication of a commentary article by Dadkhah et al. (J Cell Commun Signal 11:181–185, 2017) which addressed issues raised by the citation of questionable scientific papers in current databases and the recent retraction of manuscripts dealing with the biological properties of the CCN1 protein by Lin et al. (J Biol Chem 291(53):27433, 2016) prompted us to examine how this situation reflects an evolution of the original citation system, endangering scientific communication. We argue that the increasing number of publication retractions that have been witnessed over several years is a direct consequence of the bias created by the inconsistency of citation metrics.

Keywords Scientific citation · Publication retraction · Scientific misconduct · Office for Research Integrity (O.R.I) · Retraction watch

Introduction

The evolution of science is based on an open communication of new data and ideas through publications of articles in specialized journals and newspapers, conferences and meetings on wide or selected topics during which scientists can directly exchange their new ideas and findings, that they can freely discuss to get feedback. « A scientist cannot readily continue to exist as a scientist unless he can communicate with other scientists » [(Leopold AC 1973 cited in Eugene Garfield 1974)].

As a co-organizer of the International Workshops on the CCN Family of Genes and Administrative Assistant of the Journal of Cell Communication and Signaling since their inception, I have participated in several international meetings over the past twenty years and witnessed a marked evolution, and significant improvement in the quality and quantity of scientific information that is shared both in oral presentations and in writing.

In our opinion, the dramatic increase in article retractions that have flooded the scientific literature over the past several years illustrates the perverse effects of a citation system which is no longer suited to the needs to enable a fruitful and rewarding propagation of valuable information. The reasons for the collapse of printed scientific communication have been pointed out over many years by a significant number of members of the scientific community. These have been the subject of numerous reviews that have proposed an in-depth analysis of the weaknesses and strengths of the various metrics that have been developed to estimate the impact of new findings.

In an attempt to understand what has gone wrong with a citation system that was expected to help the scientific community we will try to point out a few reasons that can account for a drift that puts a « once upon a time » fantastic tool for cross-examination and cross-evaluation of new data, and for critically constructive sharing of information.

We believe that using an informative tool as an evaluation instrument was a critical step in the wrong direction.

The citation process

Citation is a key process underlying advancement of science. It is a means to acknowledge the work accomplished by others and to recognize the impact of original ideas. It is a corner stone in building a foundation for sharing and communication

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of novelties upon which new concepts are erected and society conditions improve.

In his social analysis of the scientific citation process, B. Cronin (1984) wrote « The most common means of bestowing credit and recognition in science is via citations. With the emergence of commercially available citation indexes in the course of the last two decades, the significance of citation in the professional lives of career scientists has taken on a new dimension. Citation indexing rests squarely on the assumption that citations can be treated as units. Aggregate these units, so the theory goes, and one can arrive at a measure of an individual's or group's contribution to the growth of knowledge in a particular subject field. » He also quoted the work of Leopold who « identified the Citation Index game as one of the stratagems employed by scientists to increase their visibility among their peers » and the metaphor used by Gaston who compared competition in science with 'a race between runners in the same track and over the same distance at the same time' (Gaston 1971: 472).

Even though the context and the rules of the game have been greatly affected by the open access publishing turmoil, it remains that « The difference between track events and science, however, is that in the former there are medals for the runners-up. In science this is not so: originality alone is rewarded. Many run, but few are acclaimed.¹ ».

As suggested by Dadkhah et al. (2017) and by many others the huge increase in citations of all kinds that the scientific community has witnessed over the past years makes it extremely difficult to identify reliable citations in the most used databases. Aside from the fact that fake articles spread like weeds in the scientific community, the authors point out a deficiency in the databases stemming from the fact that fake articles or articles which have been retracted, are still contained within the databases and can be pulled out upon documentary searches. In addition to the fact that it is somewhat difficult to understand why the authors of such papers find their way to citation, even if it is clear that the value of their work was questionable, there is no doubt that such a collection of papers congests a system that is already extremely difficult to use. It does not really deal with papers that should not remain indexed.

In order to avoid sharing responsibility for the confusion raised by an overflow of published manuscripts, citation databases, that keep track of papers that should no longer serve the publicity of their authors, should clean up their data.

The need for quantifying the audience of publications has led to the use of several metrics that end up being unsatisfactory (Perbal 2015). In spite of their weakness, and efforts of companies promoting their indexing services, these metrics have a highly criticized influence. This is because they are used by institutions to finance the research performed by individuals who are immersed in an ocean of publications that make it impossible to achieve a comprehensive evaluation of the

scientific impact of any kind of work. Only the manuscripts published in the few « high rank » journals that everybody « must » read, remain afloat for a short time² and then rapidly fall into the endless pit of thousands of papers being indexed on Wos, Scopus, PubMed, and Google Scholar, to cite a few.

Since the same metrics are being used to evaluate the achievements of individuals, the weight that is put on researchers becomes unbearable. Complaints are more and more frequent and abuses of the system increase.

The weaknesses of the evaluation system have created a race for citations, which in itself surreptitiously reinforces the famous «publish or perish » system that has been enshrined by the most respected institutions worldwide. Both of these put at the verge of public recognition those who do not «fit in the mould».

Among the many ways used by individuals who desperately strive to be recognized and show no sense of scientific ethics, it is frequently observed that the benefits of the citation system, as it was originally conceived, are spoiled by some authors, who make incremental (at the best) advances in the field and voluntarily ignore the work of others, only give credit to those who belong to a small circle of friends or colleagues from whom they expect citations in return, or in an abusive manner cite their own work in long citation lists in which they include not only their original papers but review articles that they have previously published in which they already heavily cited their own work. This is questionable behavior.

In his famous essay on the Citation Index Game, Eugene Garfield (1974) once qualified these individuals as « citation baiters », and commented on the catch-22 of self citation. More recently, the incentives underlying the need for self-citation have been discussed by Fowler and Aksnes (2007) who concluded that because « there is no significant penalty for the most frequent self citers » this practice carries « important implications on the use of citations to evaluate performance and distribute resources in science ». As a reaction to this disturbing abuse of the citation system Thomson Reuters suspended 51 journals in their 2011 edition of the Journal of Citation Report, for « anomalous citation reports ».³ This decision points out the responsibility of editors and reviewers in accepting to publish papers in which authors overuse self citation.

The precious value of a citation system, originally meant to help researchers disseminate their work and get feedback on their achievements and concepts, has been discredited in different other ways⁴ by individuals who do not share the ethical values that were once attached to a noble world of scientific research based on free, open and honest communication. Many different types

¹ *Ibid* B. Cronin

² « five minutes in the sun » according to one of our good friend!

³ see P. Davis « Citation Cartel Journals Denied 2011 Impact factor ». (2012) The Scholarly Kitchen - <https://scholarlykitchen.sspnet.org/2012/06/29/citation-cartel-journals-denied-2011-impact-factor/>

⁴ Not only these wrong practices do not deserve to be publicized, but a critical examination of the social context that exacerbates this kind of problems goes far beyond the scope of this communication.

of inadequate referencing that we have witnessed constitute deviations that are unacceptable and should be vigorously fought against.

For example, some authors cite articles that are cited by others without even checking if the credit is appropriate,⁵ others use database to search keywords of abstracts to cite papers which turn out to be totally irrelevant, and some authors cite references of papers that they obviously did not read, etc.

Again these are only a few among hundreds of examples which have previously been heavily discussed in the literature and blamed for spoiling the system.

Moreover, there are additional problems raised by the « tsunami » of open access publications, whose quality is highly questionable and also hijacked and predatory journals. Papers of very questionable quality end up flooding the search engines used by the scientific community.

The race for citations is a source of a very destructive behavior from individuals eager to see their name appearing at the forefront in their field of interest and who do not seem to appreciate that ignoring the codes of ethics and deontology is cutting the branch on which they sit.

A direct and very sad consequence of this insane rush for citation and publishing leads to the growing wave of manuscripts that are being retracted from all scientific journals including those with high impact factors. Indeed high impact journals would seem to preferentially accept those papers which the editors feel would attract the most citations and not necessarily those of the highest quality science.

Although the retraction phenomenon may appear to see the light, it is part of a scientific misconduct that is not new in science and has unfortunately become more and more frequent.

Retractions as consequences of scientific misconduct

Misconduct is a widely spread scientific plague, where retraction is only one aspect.

Back in 1976, a questionnaire conducted by the New Scientist asked a panel of questions regarding bias and malpractice in science (St James-Roberts, 1976a). The questions ranged from knowledge of bias and intention of bias to nature and place of malpractice, the number and qualification of suspects and finally a question regarding the fate that the suspects met. Another poll that was run in 2005 and other surveys of doctoral candidates and professors who were awarded grants established the percentage of scientists who testified having been engaged in fraudulent behavior. Altogether the results indicated that over 90% admitted that they knew about scientific fraud and that 33% were guilty of at least one out of ten misconduct behaviors listed in the questions. Amongst the misconducts that were identified among principal investigators funded by the NIH, 36% were never reported.

Examples of authors, editors and reviewers guilty of “irresponsible and dishonest” research that involved fabrication of scientific data, examples of corrupt peer review practices designed to sabotage academic competitors, and other misadventures were also reported in 2010 (see for example St James-Roberts I. 1976b; Martison et al. 2005; Lorraine 2010; Witkowski 2014).

In spite of scientific misconduct being publicly denounced by whistle blowers in the 1980’s the situation did not really improve as most of them, were often condemned (Rhoades 1995) or forced to close down their blogs (Pain 2014).

Because administrations were very often reluctant to publicize the scientific misconduct committed by their members, several examples of scientific misconducts remained unrevealed, thereby making room for more. Among these are falsification, fabrication of data, plagiarism, embezzlement and bioethical violations,⁶ fabricated results, falsification of notebooks,⁷ self plagiarism,⁸ and erasing names of authors.⁹ For a summary of all things not to be done, see also Rougier and Timmer.¹⁰

Retraction of manuscripts illustrates another face of scientific misconduct.

The recent retraction of a manuscript involving the relationship between HIF1¹¹ and the CCN1 protein in gastric cancer cell invasion¹² was perceived as a tragic event by a few members of the CCN community.¹³ Unfortunately, these events show that the CCN field is not immune to such behavior.

Manuscript retractions have existed for a long time but their numbers started to rise significantly around the year 2000 and led to the launch of the Retraction Watch blog¹⁴ in 2010. In 2015, a total of 1500 papers were reported to be retracted since 2012.¹⁵ The current number for 2016 being more than 650.¹⁶

⁶ J. Neimark (2014) Aeon <https://aeon.co/essays/are-the-retraction-wars-a-sign-that-science-is-broken>; The Editorial Board NY Times (2015) Scientists Who Cheat <https://www.nytimes.com/2015/06/01/opinion/scientists-who-cheat.html>

⁷ see The Office of Research Integrity <https://ori.hhs.gov/sites/default/files/final.pdf> The ORI was created in 1992 following the fusion of OSI (Office of Scientific Integrity) and OSIR (Office of Scientific Integrity Review).

⁸ see for example J Cell Commun Signal. 2013 7:309.

⁹ It is to provide help and support to scientists facing such kinds of misconduct that I founded in 2000 an Association for the Defense of Ethics and Integrity. <http://ethique.integrite.free.fr/index.html>

¹⁰ N. P. Rougier and J. Timmer (2017) Ten Simple Rules for Scientific Fraud & Misconduct. <https://hal.inria.fr/hal-01562601/document>

¹¹ Hypoxia-inducing factor-1 alpha

¹² <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC5207167/>

¹³ Of note, the last author responsible for this publication is a vice-president at Kaohsiung Medical University in Taiwan. He is also the last author of another retracted work published in Nature Cell Biology. <http://retractionwatch.com/2017/01/04/prominent-researcher-taiwan-loses-another-paper-image-duplication/>; <http://retractionwatch.com/2016/11/21/nature-cell-biology-retracts-hotly-debated-cancer-paper/>

¹⁴ <http://retractionwatch.com/>

¹⁵ T. Basu « 64 Scientific Studies retracted by One Publishing Company » Time 18 Aug 2015 <http://time.com/4001464/springer-retraction-studies/>

¹⁶ The scientist issue of December 2016 reports the top 10 retractions of the year. <http://www.the-scientist.com/?articles.view/articleNo/47813/title/Top-10-Retractions-of-2016/>

⁵ Wrong references sometimes end up having a long life ...

Flaws in the reviewing process appear to be a major concern leading to retraction.¹⁷

It is quite disturbing that full versions of some papers labeled as « retracted » on PubMed can still be downloaded from both PubMed Central and the original journal website¹⁸ and are still cited.¹⁹ All retracted papers should be eliminated at once from the databases.

The wave of retraction that we have witnessed for several years is to become an overwhelming destructive weapon with all associated detrimental prejudices profoundly affecting the scientific community.

The reasons for such a drift in the publication standard are multiple.

Among them, flawed reviewing, when there is evidence, represents a major factor. The selection of reviewers is of prime importance. Obviously low quality journals do not attract qualified reviewers. We have experienced directly a case in which the initial review process that pointed out a critical problem led the authors to publish their questionable manuscript in another journal where the reviewers were either incompetent or nonexistent. Sometimes, in higher impact journals experts in the specific field are not necessarily selected as reviewers but rather those in « name » institutions.

In addition, all data submitted for publication (blots, photographs, measurements, etc.) should be accompanied with original documents from where they are extracted. This should limit the multiple utilisation of the same results in different combinations of figures (upside down, recrop, etc.).²⁰

Many of our colleagues agree with us that many articles published in various journals contain erroneous conclusions drawn from weak data. Once published and indexed in the databases, they appear on the surface to be reliable references and then can be used by others resulting in a source of heavy and costly experimentation that will fail.

If the lack of critical professional reviewing is deleterious to the whole system, the lack of control from the principal investigator, financially and scientifically responsible for the management of the study, is also unacceptable. All the co-

authors signing a manuscript share responsibility for the quality of the experiments that are reported. It is well known that some heads of institutes and departments, aiming for the highest number of citations, put their name on all manuscripts without having any idea of what they sign, and often rejecting any kind of responsibility for the content of the fraudulent manuscripts.

Disclosing the names of reviewers was attempted by some journals but turned out to be a policy that most reviewers rejected.

We believe that the names of the reviewers who were involved in the review of retracted papers should be disclosed at least to a third party oversight body. Although we have heard from colleagues that none would agree to perform a review under such conditions, we remain convinced that it would certainly avoid a few problematic situations. The creation of such a third party highly qualified arbitration body could go a long way in ameliorating many of the problems.

A part of responsibility is to be shared by the institutions hosting the researchers. All institutions have to make it clear that responsibility of the principal investigators is legally engaged when they use public financing and publicize scientific misconduct. Research paid by public funds requires public information.

Administrations should be proactive in the elimination of misconduct. Too many institutions prefer to “act internally” which very often means doing nothing, rather than putting under the light the wrong behavior of their researchers who commit misconduct. The problem which has been raised for many years remains actual: in most cases, except for big scandals involving money misuse²¹ little is known about the consequences of the perpetrated misconduct. North American universities and other research institutions were reported to « inadequately respond » to allegations of research misconduct in the 1980's.²² In the United Kingdom, universities receiving grants are requested, since 2013, to publish summaries of their investigation regarding research misconduct. However, A survey performed in 2015 indicated that only a fraction of them publicly disclosed the extent of their investigations into research misconduct (Gibney 2015).

Since many universities get overhead payments on all grants obtained by the PIs for research and publication, they should put into place safeguards that include control of what is published under their name.

Cheating, manipulating data, or misbehaving should not be tolerated.

One cannot accept fake data published in science when we would not accept that a car is sold with a so-called “brake” that does not actually exist.

¹⁷ A total of 43 publications were retracted by BioMed Central (Springer) « following investigation into 50 papers that raised suspicions of fake peer review » (<http://retractionwatch.com/2015/03/26/biomed-central-retracting-43-papers-for-fake-peer-review/>); <http://time.com/4001464/springer-retraction-studies/>) and 64 articles showing compromised reviews were retracted by Springer (<http://time.com/4001464/springer-retraction-studies/>; <https://www.springer.com/gp/about-springer/media/statements/retraction-of-articles-from-springer-journals/735218>)

¹⁸ For example the retracted manuscript by Huang et al. doi:10.1186/s13000-015-0366-1 is still fully available on Diagnostic Pathology and BioMed Central.

¹⁹ see « Top 10 most highly cited retracted papers. » <http://retractionwatch.com/the-retraction-watch-leaderboard/top-10-most-highly-cited-retracted-papers/>

²⁰ As suggested by A. Leask, universities should provide central data bases to store primary data. Such a policy already exists in companies (A. Leask personal communication)

²¹ « Kuo is currently facing allegations that he accepted bribes to add co-authors to his papers » Prominent researcher in Taiwan loses another paper for image duplication <http://retractionwatch.com/2017/01/04/prominent-researcher-taiwan-loses-another-paper-image-duplication/>

²² <https://ori.hhs.gov/print/historical-background>

The next aspect to be considered refers to socio-cultural aspects. As already discussed, all countries do not have the same notion of scientific integrity²³ and do not provide their young students and scientists with the same education and codes of ethics. In some areas, a striking number of retracted papers originate from Asian groups.²⁴ One might consider helping scientifically emerging countries to develop and understand the value of a strong metrix that should be used internally before submitting manuscripts to foreign journals. In this context the temptation for fame might be strong.

Part of the issue may be that hirings and promotions are often based on publication in high impact journals and not necessarily on the quality of the research output.

At the other end, the reviewing process must not be lax and overpermissive.

In the end this is not doing any good for anybody.

Finally, we believe that publishers should certify to the scientific community, which relies on their publications, that the content of what they published is reliable and has been thoroughly checked. Specific measures should be taken by all publishers who should agree on standards to be followed. Those who do not wish to comply with such a procedure would not obtain the label for “reliable publication” and would not be indexed by WoS, Scopus, PubMed or Google.

These indexing institutions should carefully check that, what is sent to them for indexation, comes from a reliable journal applying the standards of the profession.

These measures would counterselect predatory journals and online journals that pop up every day and publish manuscripts that do not comply with any decent kind of reviewing process.

Should Publishers acquire online journals of very variable quality to rapidly find themselves on a slippery slope when they face retraction of so many manuscripts that were published without any control in online journals whose only sole goal was to make money.

Conclusion

In this short overview, which is obviously addressing only a few basic aspects of scientific publication, we have not mentioned the current bias toward publishing in high impact journals that is « imposed » on the scientific community by the financing and evaluation administrations. This aspect was discussed elsewhere (Perbal 2015).

We have stressed that the drift in the citation game, that has been induced by ego, lack of ethics, and financial profit, is

responsible for the madness of speedy uncontrolled publication of articles that are shedding suspicion on a system which has lost its original values.

Even though the system is paved with slippery obstacles and hurdles, room should be made only for those who are really committed to discovery and not simply to achieving fame.

To the one who is passionate and dedicated to discover, it would be inconceivable that a researcher worthy of the name, could commit scientific misconduct and publish fake data.

We are confident that if publishers, institutions, and researchers all unite to work in the same direction, for the benefit of societies and welfare of all individuals, scientific publication will once again become a venerable source of useful information.

In the world of dishonest scientists anxiously seeking fame there is no room for public recognition, support and cheering. The whole educative and research system would certainly collapse and would not be resurrected from the ashes.

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²³ A responsibility index (2009). How to evaluate a nation’s scientific integrity. *Nature* 457: 512

²⁴ A total of 106 tumor biology papers from asian groups and published in *Tumor Biology* between 2012 and 2016 were retracted in 2017. See <https://link.springer.com/article/10.1007/s13277-017-5487-6>