What kind of a life for a scientific journal?

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Abstract In June 2015, Thomson Reuters informed our publisher Springer that the Journal of Cell Communication and Signaling, the official journal of the International CCN Society, « had been selected for coverage in Thomson Reuter's products and services. Beginning with V. 1 (1) 2007, this publication would be indexed and abstracted in Science Citation Index Expanded (also known as SciSearch), Journal Citation Reports/Science Edition, Biological Abstracts and BIOSIS Previews ». In this fall editorial I briefly revisit a few milestones of the JCCS life since it was first created in 1988, with the deep and genuine willingness to help in the dissemination, in the highly competitive world of publishing, of the best quality science regarding the roles of CCN proteins in signaling.

What force is driving us to editorialship ?

For many years, I have been deeply committed to sharing knowledge, either through continual efforts to write books and articles in scientific journals, by active participation in conferences, be they specialized or open to the general public, or the organization of international scientific meetings such as the International Workshop on the CCN family of genes whose 8th edition will be held in Nice, November 2015.

As the Editor -in -Chief of Methods in Molecular and Cell Biology (MCCB), that I created in 1989 with Wiley, I quickly

learned that attracting a sufficient number of good publications to produce a satisfactory number of issues each year, was not an easy task at all, in spite of the strong support of very motivated colleagues who joined the MMCB editorial board.

At a time when the number of scientific journals was steadily increasing, the decision to create a new journal could only be justified if it served as a medium for essential information that was not adequately published elsewhere.

Methods and Experimental Design were most of the time included within manuscripts and described with « materials » as a technical section allowing assessment of the results.

Except for truly fundamental and original new methods that would appear in full in a few journals, detailed experimental procedures were not always easy to find.

As the author of A Practical Guide to Molecular Cloning, I had realized that there was a need for an efficient vehicle to disseminate methods that might be of general interest to biologists working at a molecular level.

Along this line, I proposed two new sections for MMCB : « Commentaries on protocols » and « Nuts and Bolts ». These sections, which I have also introduced into JCCS, were aimed to stimulate discussions on theoretical topics and to promote sharing of information regarding the quality of commercial reagents.

Although a satisfactory pace of original manuscripts published was reached in a short period of time, we faced an internal editorial reorganization at Wiley-Liss that was asking MMCB to drift towards a review-type of journal. After debating the issue with the editorial board, I decided along with most members of the board to relinquish my responsibilities of Editor-in-Chief, at the end of 1993.

This first experience with journal publishing was a great source of profound joy and excitement.

It is important to note that most of the reviewing process is performed for free by members of the scientific community who spend valuable time on their task to read and critically analyse the content of manuscripts. As recently quoted by

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« the Scientist »¹ this situation raises a very serious problem as to the time that researchers spend on manuscript reviewing, training young students, participating in scientific life inside and outside their own institution is rarely considered at the time of a job application or for promotion.

The birth of JCCS

At the turn of the 21st century, work performed on the emerging family of CCN proteins pointed them as critical players in the control of intercellular communication and the integration of signals from the surrounding environment.

In addition, as to their involvement in channel signaling, CCN matricellular proteins proved to functionally interact with several kinds of bioregulators in the extracellular matrix, at the membrane or within the nucleus.

At that time, it came to my attention that publications addressing intercellular signaling and cross-signaling of cells with their microenvironment were scattered in various journal dealing with transduction signals or more general biological aspects.

To fill this gap, I proposed to BiomedCentral to create a journal that I designated « Cell Communication and Signaling ».

After BioMed accepted my proposal, including the name that I proposed for the URL « biosignaling.com », I became Editor-in-Chief and raised an editorial board composed of distinguished scientists who specialized in CCN and other signaling-related fields.

On this occasion, I explained in my first editorial (Perbal 2003) the goals of this new journal which, at the time was among the first « open access » journals.

Having clearly expressed in 2002 our belief that open access journals were offering several advantages² and stated that our goal was to promote free access to knowledge, the whole board agreed with me that we could not accept the commercial modifications proposed by BMC in 2006 that significantly increased the financial publication charge paid by the authors.

After the board and myself resigned in 2007, we were offered by P. Butler at Springer to continue the publication of a journal on the same topic, but with a slightly different name to avoid confusion, and to clearly distinguish our agreement with Springer from our previous relation with BMC.

¹ With Science Publishing ? [http://www.the-scientist.com/?articles.view/articleNo/32378/title/Whither-Science-Publishing/-](http://www.the-scientist.com/?articles.view/articleNo/32378/title/Whither-Science-Publishing/)

² « Open access policy changes the way in which articles are published. First, all articles become freely and universally accessible online, and so an author's work can be read by anyone at no cost.... authors are assured that their work is disseminated to the widest possible audience, given that there are no barriers to access their work... the information available to researchers will not be limited by what their libraries can afford, and the widespread availability of articles will enhance literature searching »

This deontologic position was satisfactory to all of the previous board members who happily accepted in 2008 to join the newly created Journal of Cell Communication and Signaling (JCCS), the official journal of the International CCN Society.

It was agreed that JCCS would be both printed in the classical way and available as open access on line.

After my resignation and creation of JCCS, BioMed decided in 2008 to reuse the CCS acronym that I had created, and run the same type of scientific publication with a new editorial board.

This was done without informing me and without asking me permission to use the CCS acronym.

From a legal standpoint, and even though I had agreed to transfer my copyright for the financial aspects related to CCS, it appears that I still legally own the moral rights for the use of this acronym which was and remains my intellectual property.³ This situation should not be mistaken with that of a trademark where generic names cannot be used.

Since BMC now belongs to Springer, a legal action would make no sense.

In any case, the publication of CCS by BMC raised confusion among potential authors who did not quite understand the rationale for having two different journals with closely similar names.

Obviously, our efforts to maintain a high standard of publication in our journal have now been recognized by the reviewers at Reuters Thomson who recently considered that JCCS qualified for an impact factor.

What does an impact factor provide ?

In order to better understand what is the point to obtaining an impact factor we must turn to the basic aspects of publishing in scientific fields and ask a few key questions :

- 1) what are the aims of publishing ?
- 2) how are articles selected to be published in journals ?
- 3) what are the criteria used to evaluate the scientific quality of journals ?
- 4) who is using the ranking and for what purpose ?

Producing a thorough review and a complete response to these questions would go far beyond the scope of this editorial. However, I will try to briefly discuss these various aspects and provide our readers with my personal views, those of an

³ Art. L.112-4 of the code de la Propriété Intellectuelle stipulates that : «The title of a work of the mind, when it is original, is protected as the work itself. » No one if the work is no longer protected in accordance with Articles L. 123-1 to L. 123-3, can use this title to distinguish a work of this kind under conditions likely to cause confusion. »

average scientist, that I believe are shared by many other colleagues.

The aims of science publishing

Publishing the results of experiments and conclusions that can be reached from critical interpretation is a necessary step in the communication that is required for scientific progress. Not only the data that are shared constitute a platform useful to other colleagues who wish to pursue work in the same or closely related areas of research, but it is also an accepted idea that validation of data by other approaches is required.

Publishing is useful only if it allows the readers to replicate pieces of important data, not only to confirm their credibility, but also to use these as a solid foundation to further experimental advances.

Breakthrough discoveries that question well-established dogma also need to be validated and extended.

Thus, science progresses stepwise.

Scientific journals fulfill several important missions, among which communication and archiving of knowledge participate in the constitution of a universal community of knowledge.

At the earliest age in scientific publication, journals were run by universities, and scientific or medical societies such as, the American Medical Association (AMA), the American Society for Microbiology (ASM), the American Association for Cancer Research (AACR), Cambridge University Press, Oxford University Press⁴

Publication costs were covered mainly by registrations of institutions, personal memberships and funding from universities.

Libraries which were well funded provided access to a wide range of publications.

There were already many scientific journals but manuscripts published in good journals were often more thorough and comprehensive. Very often the publications were milestones in a field. More time was spent to make sure that the body of results was robust.

As a young graduate student, I was told by my mentor Dr. Guy Hervé that I should never retract the content of the manuscripts I would publish. Not that he suggested to blindly ignore the evolution of scientific fields. He would simply mean that data included in the manuscripts should be rock solid. It was at a time when we had to repeat all experiments three times to ensure that they were reproducible.⁵ Believe me science was fun....

⁴ see https://en.wikipedia.org/wiki/List_of_university_presses.

⁵ It does not come to a surprise that my first manuscript was a full story of 18 printed pages in the *Journal of Molecular Biology* (ref Biosynthesis of *Escherichia coli* aspartate transcarbamylase. I. Parameters of gene expression and sequential biosynthesis of the subunits. Perbal and Hervé 1972)

The scientific output over the past years has increased at an amazing pace. In life Sciences, which is the topic covered in this editorial, molecular and cellular biology were boosted by the advent of new cloning and sequencing technologies. As a direct result, the number of publications have « exploded » and technology transfer to industry has rapidly become a must in numerous research areas.

At the same time, the unfortunate shrinking of research funding has affected the vast majority of laboratories who rely on grants to perform their research and access publications.

In spite of these restrictions, the number of publications has significantly increased and created needs for new communication media.

Publishers who understood that their support become essential, seized the commercial opportunity and launched quite a large series of new journals over the past decade.

As of today, publishing in science relies on a group of major publishers.⁶

Selection of manuscripts

Not all articles have the same impact on science, not all articles bring the same amount of new information, and not all articles report the results of critically designed experiments that allow one to pave the road to new concepts or applications.

Since biologists cannot read the thousands of manuscripts that are published each year in hundreds of journals dealing with their topics of interest, they have to rely on a system expected to provide a fair evaluation of the content of manuscripts, with an evaluation of their potential impact.

There is no easy solution to this problem.

In the art world, critics publish reviews after they have attended a painting exhibition, the premiere of a play, the release of a movie etc.

People who read these critics may be inclined to rely on them, or simply decide to make their own opinion and « attend the show ».

The same situation holds for science and even though we need to rely on an evaluation system, I believe that nothing can replace the personal evaluation of a published work by researchers specialized in the field themselves.

All researchers know that the publishing of manuscripts is based on a peer review process.

This is one of the forces and also a weakness of the system.

First of all, a critical objective and thoughtful evaluation of an experimental work requires that the reviewer is competent

⁶ These include Reed-Elsevier, Springer, Wiley-Blackwell, Taylor & Francis, and Sage. This situation is the subject of many debates which go beyond the scope of this editorial (<http://www.vocativ.com/culture/science/five-corporations-control-academic-publishing/>).

in the field and can spend the time to carefully read the manuscript, and write a constructive review⁷.

The review process has been highly criticized over the years. Excessive situations have been reported in many different media. For example, graduate students being used as an alternate for reviewing in place of the Head or the Principal investigator who was contacted for review. There were reports of manuscripts being rejected for insufficient quality when they were the exact same copy of a manuscript previously published in the same journal by a renowned scientist...

Flaws in the peer-review process may let weak manuscripts get published and raise confusion in the field of interest.⁸

How about computer-generated manuscripts being accepted for publication in journals that claim to enforce peer-review⁹?

Of course these are disturbing exceptions. So we hope.

Overall the system is fair, but caution is required in some publishing areas.

Criteria being used to evaluate the scientific quality of journals

This issue is a very controversial one because of the nature of metrics which are presently used to evaluate the overall impact of a journal.

For many years, the impact factor (IF) published in the Journal of Citation Reports by Thomson Reuters has been considered as a gold standard for « ranking, evaluating, categorizing and comparing journals »¹⁰.

The journals with the highest impact factors are usually considered by the scientific community as good journals.

Briefly, the IF is the ratio obtained after « dividing the number of current year citations to the source items published in that journal during the previous 2 years ».¹¹

The two values which are critical in this estimation are a) the number of current year citations : Journals indexed by Thomson Scientific are screened. Citations in other Journals will not be taken into account ; b) the number of source items : in other words, the article types that Thomson Reuters regards as « citable elements ».

⁷ For an overview of peer reviewing see <http://www.qualityresearchinternational.com/glossary/peerreview.htm>.

⁸ See for example « Flaws in the peer-reviewing process : a critical look at a recent paper studying the role of CCN3 in renal cell carcinoma » (Perbal 2012)

⁹ Open Access Publisher Accepts Nonsense Manuscript for Dollars <http://scholarlykitchen.sspnet.org/2009/06/10/nonsense-for-dollars> - Another Predatory Publisher Accepts a Computer-Generated Paper <http://scholarlyoa.com/2015/01/12/another-predatory-publisher-accepts-a-computer-generated-paper/>

¹⁰ Web of Science. The Thomson Reuters impact factor - <http://wokinfo.com/essays/impact-factor/>

¹¹ Ibid

A mode of calculation based upon the number of citable counts may indeed lead to the kind of weird situation described for Physical Review letters.¹²

The concept of « citable element » that has been strongly criticized for its lack of transparency, is now clarified by Reuters Thomson.

The « number of citations » is also a big source of misevaluation. Everyone in the scientific community very well knows that citations are often re-used by young as well as senior investigators who did not even read the papers that they quote. This becomes obvious when a mistake that appeared in the title, author name, or page numbers of a reference, is propagated over and over in the literature...

Some open access journals publish a listing of the highly accessed or highly downloaded papers, without knowing whether they have indeed been read !

It is also well known that people tend to cite papers of colleagues who are 'living' within the « same circle ». I was once told that this practice is primarily due to the reduction of space that is left in some journals for bibliography.

Primarily...

In any case, institutions take advantage of this stated fact when it comes to university rankings.

Although in most cases, high quality journals will end up publishing high quality manuscripts, not all manuscripts in a journal may have the exact same impact. Therefore, it is important to realize that the impact of a journal refers to the journal, not to individual publications. Conversely, high impact factors may be generated by a low number of highly cited papers. For example, Nature reported that their 2004 IF was mainly generated by 25% of their articles.¹³ The great majority of their 2004 papers received fewer than 20 citations.¹⁴

Due to the considerable economical benefits that can be drawn from a reliable publishing of journals ranking, several other metrics have been proposed.

They include : Source normalized impact per paper, the impact per publication, and SCImago journal rank, Scopus journal impact and ranking, 5-year journal impact factor, Eigenfactor, Goggle scholar metrics and more.¹⁵

A load of other discussion, addresses the significance of the various metrics used in addition to the IF. Readers interested in these aspects should consult « The Metric Tide », a report published by the « Higher Educational Funding Council for England ».¹⁶

¹² Editorial: Is PRL Too Large to Have an "Impact"? <http://journals.aps.org/prl/edannounce/PhysRevLett.102.060001>

¹³ Not-so-deep impact Nature Vol 435, Issue no. 7045, 23 June 2005 - http://www.sportsci.org/jour/05/Not_so_deep_impact.pdf

¹⁴ Ibid

¹⁵ see http://www.fzt.haw-hamburg.de/pers/Scholz/aast_AAST_M_JournalMetrics_14-12-06.pdf - <http://www.springer.com/gp/authors-editors/journal-author/journal-author-helpdesk/impact-factor/18684>

¹⁶ <http://www.hefce.ac.uk/pubs/rereports/Year/2015/metrictide/Title,104463,en.html>

What are the various impact factors used for ?

This is indeed a critical question, as most of the criticisms which have been raised against impact factors find their roots in the usage of these metrics to evaluate the individual achievements of researchers, instead of being used for what they were originally created.

In a review intended to ask what journal indicators and metric indicators were measuring,¹⁷ R. Van Noorden pointed out that the Reuters Thomson's IF invented by Garfield¹⁸ is not good at measuring an individual's performance, and should never be used by institutions to evaluate their performance.

As a matter of fact, and as recalled by Thomson Reuters,¹⁹ the journal IF was not meant to « measure the quality of an individual article in the journal-since it is not based on the citation of individual publications-but it does correlate to the reputation of the journal in its field ».

The journal IF (JIF) was meant i) to help librarians choose journals of wide audience appeal, and ii) to be used as a tool to identify leading journals, topics and institutions in specific regions and areas of science.

That statement being clearly expressed, one can wonder why Institutions use the JIF as an indicator of an individual's scientific achievement at the time of its evaluation for promotion, hiring or funding.

This statement applies to all of the metrics available today.

Having sat on university committees in charge of career evaluations, I always rose against those who considered as a priority to search for JIFs of published papers, instead of examining the real scientific activity and the scholarly input of the candidates, including time spent in training of students, participation in scientific orientation committees, and so on.

Why was it so ?

I believe that the very problem stems from the difficulty to objectively evaluate the scientific production of individuals. In most of the cases, the evaluation of a candidate begins by examining the number of papers that he/she produced and his/her position amongst the list of co-authors signing the paper. The IF of the journal is also considered to be a key metric.

When it is a single author and their mentor who sign the manuscript, the situation is clear.

Serious difficulties arise when one needs to assess the participation of a researcher who is working as a member of a team where other colleagues actively participate in the publication of a sound paper.

How to properly acknowledge the participation of each collaborator ?

In some cases two authors have equally contributed to the work.

Who should sign the manuscript first ?

Even though a small asterisk or any other typographic sign indicates that each of them produced an equivalent input to the work, everyone knows that the first name comes first in the citation, especially when the name of authors ends up being cited as « first author et al. ».

When CCN leaders agreed to adopt a new nomenclature for the *cyr61*, *ctgf*, *nov*, and *wisp* genes that would not convey any functional misconception I proposed publishing a joint manuscript with co-author names listed in alphabetical order. Nothing wrong with that. Nobody ever complained, until one colleague pointed out in a conversation that the « benefit » of this paper is always attributed to the first author whose name appears as « alone, et al. » when the paper is cited. I took it as a trait of humor of course.

In my own laboratory, I had to argue for hours to convince a postdoc that the name of the graduate student who performed most of the critical work would appear first on a manuscript although their mentor actively participated in the experimental training and also helped them.

I truly believe that it was a fair decision, even though I made someone angry with me forever.

The pressures associated with authorship become an extremely important and concerning problem when promotion, recognition, and funding are at stake.

Too often, poorly designed evaluation criteria are “dominating minds, distorting behaviour and determining careers” (Lawrence 2007).

To overcome these problems, graduate students and PIs aim to publish as much as possible, in journals with the highest IF whenever possible. Some prominent journals with a high IF, complained that this situation ends up overloading the editorial board with manuscripts that are not among the best ones at all...

The hunt for an IF may also be detrimental to the quality of publications and result in behaviors that are not acceptable in the world of scientific research where results must be trusted.

When I was a young student, I dreamt of a publication system in which papers would only be a multi-collaborative mature work solving an important set of questions and permitting real significant progress. One manuscript being read by a very wide audience instead of hundreds of papers read by specialized groups...

The vast majority of our papers today solve only a tiny parts of huge questions.

The lack of a real assessment of an individual's scientific production that is the cause for the publication hunt – publish or perish- does not permit such a dream to come true.

How nice would it be to read manuscripts that are chapters of the « book of scientific progress » that tell you about « great achievements » and present « realistic perspectives »...

¹⁷ Metrics: A profusion of measures. <http://www.nature.com/news/2010/100616/full/465864a.html>

¹⁸ References op. cit.

¹⁹ Thomson Reuters Statement Regarding the San Francisco Declaration on Research Assessment- http://researchanalytics.thomsonreuters.com/statement_re_sfdra/

Have you noticed how many times we end manuscripts by quoting the fantastic applications that should result from our publications ?

What can we expect for the future of JCCS ?

As the editor in Chief of JCCS I must ensure that our journal survives and that all the efforts that we have invested since our very early days in the publication of cell communication and signaling, the starting point of our present activity, are not wasted with a snap of the finger...

Why is the hope to finally get a JIF driving us, when we know how it can be misused ?

The answers are straightforward.

As a new journal, in an as yet unexplored field that is now expanding at a quite reasonable pace, we face a catch 22 situation.

Without an IF, many researchers do not send us their manuscript and turn to other journals.

Whatever meaning is given to the JIFs, this situation interferes with our goals.

We recently experienced that kind of situation when a group, who originally considered JCCS as a media of choice, later informed us that their institution would not permit them to publish in a journal with no IF. One former editor of JCCS even informed us that she would not send the group's manuscripts to a journal which had no IF and would rather choose a competitor journal... which in the end was the choice she made !

In spite of our considerable efforts to maintain a good quality of scientific publication in a field where we are present, not only as pioneer editors but also as researchers and for some of us as leaders in a promising new field of cellular biology, getting colleagues to send us their manuscript is a « tour de force ». As previously said in these columns (see for example the 2009 JCCS editorial), we need colleagues showing support.

I would like to take this opportunity to address my sincere and deep thanks to all those who trusted us enough to send in their manuscripts and to all the reviewers and editors for helping JCCS to survive the struggle.

In December 2012, a group of leading scientists, editors and publishers convened at the annual meeting of the American Society for Cell Biology to initiate a reflection on the original aims and increasing misuse of impact factors.

The San Francisco Declaration on Research Assessment (DORA)²⁰ that was issued from these discussions, claimed

once more that JIFs were neither meant to assess the scientific performances of individuals nor the quality of higher learning institutions.

I find it amazing that there are scientists who took advantage of the IF system to advance their own career had the gumption to endorse the DORA when they themselves overused the JIF to evaluate the achievements of graduate students to promote colleagues and to counterselect job applicants. Is there then a message in this ?

Using the JIFs as an indicator to cut funding sources or closing job opportunities is unacceptable.

At the same time, other complaints regarding publishing in science were expressed,²¹ with no much change resulting from these movements.

In September 2014, the scientific community was truly afflicted by « The tragic case of Stefan Grimm, whose suicide (in September 2014) led Imperial College to launch a review of its use of performance metrics, a jolting reminder that what's at stake in these debates is more than just the design of effective management systems. Metrics hold real power: they are constitutive of values, identities and livelihoods. »²²

We truly need ways to objectively assess scientific achievements.

The changes that are expected to emerge in the near future as a response to an increasing unease of the scientific community, will help all of us to find the best appropriate ways to evaluate all aspects of an individual's achievements, and will help journal editors to participate in the dissemination of high standard scientific information.

Acknowledgments Once more, my deepest thanks go to Herman Yeger and Annick for their encouragement, suggestions and critical review of this editorial.

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²¹ see for example : Academic spring: how an angry maths blog sparked a scientific revolution. http://www.theguardian.com/science/2012/apr/09/frustrated-blogpost-boycott-scientific-journals?CMP=share_btn_link; Harvard University says it can't afford journal publishers' prices http://www.theguardian.com/science/2012/apr/24/harvard-university-journal-publishers-prices?CMP=share_btn_link

²² J. Wilsdon in the foreword of the 2015 metric tide executive summary, Ibid

²⁰ Dora <http://www.ascb.org/dora/wp-content/uploads/2015/07/SFDeclarationFINAL.pdf>