doi:10.2533/chimia.2010.90 Chimia 64 (2010) 90–90

The Follies of Citation Indices and Academic Ranking Lists
A Brief Commentary to *'Bibliometrics as Weapons of Mass Citation'*

Richard R. Ernst*

The account by Antoinette Molinié and Geoffrey Bodenhausen on 'Bibliometrics as Weapons of Mass Citation' presents a lucid indictment on the current misuse of citation numbers and of science rankings. In the face of ratings and rankings by merely counting citations like nit-picking, the outcry of two concerned researchers necessitates no corollary or further supporting arguments. The present hype of bibliometry made it plainly obvious that judging the quality of science publications and science projects by bibliometric measures alone is inadequate, and reflects the inadequacy of science management regimes staffed by non-scientific administrators or by pseudo-scientists who failed to develop their own personal judgment.

Today, an erroneous conviction prevails that institutions and individuals of 'value' can be measured ultimately in terms of a single number that may form part of a competitive 'ranking list'! Only nobodies and nameless institutions never ever appear in a ranking! Today, an uncountable number of granting and promotional decisions are taken based on such superficial and misleading lists. – The absurdity of such a craze may best be enlightened by a comparison with non-scientific fields: Who would ever select top musical performers just by the number of references in newspapers, irrespective whether the reviews are favourable or not? Who would ever qualify renowned painters based on the number of 'quotes' in the form of plagiary borrowings by less creative artists or by plain copyists? Who knows, soon also Nobel Laureates in literature will be selected based on citation indices! – Fortunately, very fortunately, most of the great human minds of the past had not yet to worry about the mediocrity of rating agencies. Otherwise, human history would have taken a different course; and many of the greatest human achievements would never have been made. Our pride of being the most creative species ever living on earth would then be plainly ridiculous.

The only question that remains to be answered, after having read the pertinent account by Molinié and Bodenhausen, is how can we stop this degrading bureaucratic regime of ranking and citation agencies and their mindless fan community? – In the following, I would like to propose a number of remedies to save the dignity and creativity of scientists and researchers.

i) Let us formulate a creed of scientists and researchers of all kind: Never ever use, quote, or even consult science citation indices! Let us appeal to the pride and honesty of researchers to derive their judgments exclusively by careful studies of the literature and other scientific evidence. It is better refuse to comply with requests than to base your judgment on numeric bibliometric indicators! Let us incorporate this creed into our teaching, discrediting 'number games' as incompatible with our goals of objectivity, credibility, fairness, and social responsibility, as researchers.

ii) Let us establish, on the Internet, a generally accessible Webpage to list agencies, journals, and individuals who regularly use and misuse bibliometric measures in their judgements. Let us encourage researchers to add their critical commentaries to this database to identify notorious violators of the above creed. We may call this database 'Bibliometric Discredibility Pillory' or BDP. It could be that an enthusiastic bibliometrics fan might even be inclined to apply the standard bibliometric evaluation tools to this database to establish a 'Bibliometric Discredibility Index' or BDI to identify the worst offenders of academic credibility.

iii) Let us discredit specifically rating agencies and their managers that have established and regularly publish science citation indices and university ranking lists; agencies that enrich themselves on the account of science quality, and cause more harm than good. Let us urge funding agencies to never ever support projects that intend to further extend bibliometrics based on merely counting citations.

It is only by this kind of active resistance to the follies of bibliometrics that our scientific self-respect and credibility can be saved. We should liberate our minds again to enable true creativity in view of long-term social benefits. We certainly do not want to convert our precious universities into bureaucratic training centres for mindless citation hunters! Our institutions shall remain for ever unbiased resorts of limitless human dignity and foresight.

We are deeply convinced that human ingenuity and creativity are beyond all conceivable quantitative measures. We know that human beings are singular in their qualities (and their deficiencies). In order to apply justice to them, we have to respect them as individuals, each with his own particular gifts. Let us try to understand researchers and their creative output, but not attempt to compare or rank them! Whenever ill-conceived bibliometric measures are being applied, it means that non-quantifiable extraordinary achievements are cropped such that they become commensurable with the mediocrity of routine research. In this way, science loses all its outstanding features that could justify also outstanding supporting efforts. Bibliometrics may indeed turn out to become the ultimate tombstone of veritable science.

And as an ultimate plea, the personal wish of the author remains to send all bibliometrics and its diligent servants to the darkest omnivoric black hole that is known in the entire universe, in order to liberate academia forever from this pestilence. – And there is indeed an alternative: Very simply, start reading papers instead of merely rating them by counting citations!

Received: December 21, 2009

*Correspondence: Prof. Dr. R. R. Ernst
Laboratorium für Physikalische Chemie
ETH Zürich
Wolfgang-Pauli-Str. 10
CH-8093 Zürich
E-mail: Richard.Ernst@nmr.phys.chem.ethz.ch

doi:10.2533/chimia.2010.78 Chimia 64 (2010) 78–89

Bibliometrics as Weapons of Mass Citation

La bibliométrie comme arme de citation massive

Antoinette Molinié*[a] and Geoffrey Bodenhausen*[bc]

Abstract: The allocation of resources for research is increasingly based on so-called 'bibliometrics'. Scientists are now deemed to be successful on the sole condition that their work be abundantly cited. This world-wide trend appears to enjoy support not only by granting agencies (whose task is obviously simplified by extensive recourse to bibliometrics), but also by the scientists themselves (who seem to enjoy their status of celebrities). This trend appears to be fraught with dangers, particularly in the area of social sciences, where bibliometrics are less developed, and where monographs (which are not taken into account in citation indexes) are often more important than articles published in journals. We argue in favour of a return to the values of 'real science', in analogy to the much-promised return to a 'real economy'. While economists may strive towards a more objective evaluation of the prospects of a company, a market, or an industrial sector, we scientists can only base our appraisal on a responsible practice of peer review. Since we fear that decision-takers of granting agencies such as the FNRS, CTI, EPFL, ETHZ, ANR, CNRS, NIH, NSF, DOE,[1] *etc.* will be too busy to read our humble paper in *Chimia,* we appeal to scientists of all countries and disciplines to unite against the tyranny of bibliometrics.

Keywords: Bibliometrics · Citation indices · Eigenfactors · *h*-Factors · Fun factors · Impact factors · Science policy · Teaching factors

Introduction

Much has been written about the current economic crisis. Subprime loans were granted to insolvent borrowers by irresponsible financial institutions. Shares and other 'financial products' were over-packaged, over-rated and over-sold. Very few economists anticipated how fantastic expectations fuelled by false promises would cause the world to shake on its foundations. We perceive similarities between the overblown ambitions in the financial sector and those in scientific research. In the realm of modern science, false promises and 'hype' have become widespread. Nobody seems to care if we can live up to our promises. Just as the 'value' of financial products is assessed by irresponsible ranking agencies, the value of scientific research is assessed by ill-conceived parameters such as citation indices, *h*-factors, and worst of all, impact factors of journals. It can be argued that citation indices give a similar measure of scientific excellence as the Dow Jones index does of the economy. We shall illustrate some of the most perverse effects of bibliometrics-driven science policy.

The *Science Citation Index*, which has appeared in the form of bulky volumes since the sixties, offered the possibility to find out if (and by whom) a particular paper was cited. Many scientists used this *Index*, rightly or wrongly, to find out if their work had been followed up by others. But modern bibliometrics have become truly fashionable since 1997, when the *Institute for Scientific Information* (ISI), now part of Thomson Reuters, made it possible to look up the complete citation record of any scientist, friend or foe, in a matter of seconds. In this manner, one can readily 'quantify' their reputation, or so it seems. Granting agencies clearly appreciate these metrics, which greatly simplify their arduous job.

But the overwhelming fashion of bibliometrics is largely due to the narcissistic mind of the scientists themselves. Their fascination for citation indexes often drives them beyond reason. Their obsession with their egos is much more perverse than the laziness of granting agencies. In some respects, scientists remind us of body-builders who, under the pretext of 'working out' in gyms and other *salons de musculation*, seem fascinated by their own bodies, reflected in floor-to-ceiling mirrors. Perhaps a 'narcissistic scientist' is a tautology. *Wir alle brauchen ab und zu eine Streicheleinheit* (MMu[2]). Fascination with one's self is one of the driving forces of research. But if mirrors indeed constitute a necessity of the trade, one may at least hope that they provide a reasonably undistorted image.

Le narcissisme des chercheurs est d'autant plus tragique qu'il se pratique sans miroir. En effet, malgré la multiplication des réunions scientifiques, malgré les scoops d'une presse ignorante, malgré l'entrée des 'nouveaux philosophes' dans la cour des people*, jamais le savant n'a été aussi seul. Si Léonard de Vinci est mort dans les bras du roi François 1er qu'il fascinait, aujourd'hui le président de la République française ne cache pas son mépris pour la communauté scientifique de son pays.*[3] *Et le narcissisme des médiocres s'accompagne de l'auto-flagellation des grands. Le miroir bibliométrique serait-il le puits sans fond de la science moderne?*

*Correspondence: Dr. A. Molinié[a], Prof. G. Bodenhausen[bc]
[a]Maison de l'Archéologie et de l'Ethnologie (MAE)
Université de Paris-Ouest Nanterre La Défense
21 Allée de l'Université
92023 Nanterre Cedex, France
E-mail: antoinette.molinie@mae.u-paris10.fr
[b]Département de Chimie
associé au CNRS, Ecole Normale Supérieure
24 rue Lhomond
75231 Paris Cedex 05, France
[c]Institut des Sciences et Ingénierie Chimiques
Ecole Polytechnique Fédérale de Lausanne
CH-1015 Lausanne
E-mail: Geoffrey.Bodenhausen@epfl.ch

What is it that Makes us Tick?

Truly creative researchers do not seem to care for citation indices. Our ministries of research, offices of higher education, granting agencies and other *tutelles,*[4] as the French call them affectionately, try to encourage the emergence of new ideas, create more attractive conditions, offer rewards, *etc*. But none of their officers, no matter how hard they try, seem to understand what makes us tick. There seems to be a divorce between the science policy makers and those who are being evaluated.

After all, generations of scientists have somehow managed to keep their momentum without citation indices. A clock keeps ticking away as long as its spring is wound up from time to time. So what does it need to keep a scientist ticking? For some of us, watching the steady progress of our *h*-factor may help. But the motivation of most scientists is more complex. Truly inspired scientists seem unable to explain their own creativity. They do not seem to know how to wind up their own springs. They simply *are* creative, pretty much regardless of the force fields to which they are exposed, regardless of income, rewards and recognition.

To make progress, science relies on a mix of ingredients, ranging from romantic strokes of genius to boring footwork. There are indeed those rare insights, such as the celebrated ideas of Galileo, Darwin, and Pasteur. But there are scores of lesser scientists who have painstakingly charted the motions of planets, established a tribal kinship genealogy, determined reaction enthalpies, decrypted an archive in Middle English, substituted methyl groups, recorded spectral signatures, carried out site-specific mutations, polymerase chain reactions, *etc*. At times, much of this humble footwork may seem pointless. Yet, without accumulating a sound bedrock of observations, science would not be able to move forward as it does. Since *h*-factors and the like tend to focus on flashy work, the fashion of bibliometrics may be a *coup de grâce* for humble science.

Citation Indices: A Few Case Studies

Bibliometrics are based on the idea that frequently cited papers must be innovative, creative and influential. This basic assumption turns out to be a fallacy in many cases. We shall take a few examples picked from the area of magnetic resonance. Similar examples can easily be found in any other area, ranging from synthetic chemistry to elementary particle physics.

i) NOE (nuclear Overhauser effect). Albert Oberhauser's name is well known to a broad community of scientists, ranging from solid-state physicists to organic chemists. It has been argued that his name, which is 'embedded' in the ubiquitous abbreviation 'NOE' is mentioned more often than any other scientist's. Having been overlooked for years by the Nobel Prize Committee, Albert Overhauser was recently awarded a Russell Varian Prize on the occasion of a EUROMAR conference held in Göteborg in 2009. He gave a stunning lecture about the intellectual environment at Berkeley and Urbana Champaign in the early fifties. Younger scientists might enjoy his historical account in the *Encyclopedia of Magnetic Resonance.*[5] Overhauser's seminal paper, which appeared in *Physical Review* in 1953, is modestly entitled 'Polarization of nuclei in metals'.[6] Had it been submitted 50 years later, it would probably have been turned down by the referees unless it had been 'hyped up' to something like 'Thousand-fold enhancement of NMR signal intensity' or 'A paradoxical violation of the second law of thermodynamics: nuclear cooling through heating of the electron spin reservoir'. Perhaps the lack of hype is in part responsible for the low visibility of the paper: it has gathered a mere 530 citations in 56 years. By modern standards, this must be regarded as a flop! Perhaps the failure of Overhauser's original paper is due to the success of CS's brilliant experimental verification[7] (*NC* = number of citations[8] = 105), to IS's clever extension to molecules in solution[9] (*NC* = 2365), or to the publication of an excellent monograph[10] (*NC* unknown, in accordance with ISI rules).

ii) HSQC (heteronuclear single-quantum correlation spectroscopy). It seems obvious that the number of citations is often *not* proportional to the importance of the paper under scrutiny. At the risk of appearing a trifle self-centred, let us illustrate this point by a paper that GB 'co-authored', modestly entitled 'Natural Abundance Nitrogen-15 NMR …',[11] but later re-dubbed by AB as HSQC. This fancy acronym seems to match today's unquenchable thirst for hype and exotica. In fact, HSQC is nothing more than a double transfer of magnetization, for example from protons to nitrogen-15 nuclei and back. Back in 1975, the very idea that magnetization could be transferred in liquids was unknown, and it required RE's considerable skill to devise a revolutionary experiment.[12] This brilliant invention was hardly recognized, since it gathered a mere 373 citations. A much better reception (*NC* = 1442) was given to GMo's and RF's paper entitled 'Insensitive nuclei enhanced by polarization transfer', better known as INEPT,[13] perhaps in part because of its witty acronym. The idea of stringing together two consecutive steps of magnetization transfer was pioneered by LM,[14] who gathered no less than 759 citations for a method that became known under the acronym of HMQC. The cherry on the cake is the amazing impact of DR's and GB's work,[11] who were 'awarded' no less than 1776 citations for HSQC, which is really nothing more than two INEPT sequences put back to back. The idea may be useful, but can hardly be called creative!

iii) DQF-COSY (double-quantum filtered correlation spectroscopy) is the acronym of a clever idea due to UP, OS and RE[15] (*NC* = 1603), developed independently by AS and RF[16] (*NC* = 387). The most famous paper on the subject[17] (*NC* = 2367) describes a simple application to a small protein that gave rise to some nice graphical illustrations. From GB's personal perspective, its success is mostly a reminder of a failure. At MR's request, he made a round-trip from the centre of Zurich to the Hönggerberg, in the hope that the experiment might be improved, but the discussion did not lead anywhere. GB did not realize until much later that RE and KW had kindly put his name on the manuscript. He only discovered 'his' work several years after it had appeared in print in a rather obscure journal. Today, this work appears as GB's most-cited paper, provided one refines the search to include 'biophysics' (*Monsieur Jourdain apprend à sa surprise qu'il fait de la biophysique!*) It is the 3rd most popular paper on KW's list, and the 4th on RE's list. Obviously, their Nobel prizes were based on more profound contributions.

iv) NOESY (nuclear Overhauser effect spectroscopy) may be regarded as one of the most influential inventions in magnetic resonance.[18–20] AK has given a historical account of the emergence of this revolutionary concept.[21] The idea was well ahead of its time, and it took many years to be 'picked up' by the scientific community. The citations of the three seminal papers only took off after a long period of gestation (BM) (Fig. 1). This phenomenon is interesting in view of the definition of the impact factors (*IF*) of journals (see Annex 1): from the point of view of the journals, NOESY was a complete failure!

Scientists and their Beloved *h*-Factors

The most recent 'measure' of an individual's fame is his so-called *h*-factor (see Annex 2). Although some scientists view these factors with a healthy dose of scepticism, most of them love to monitor

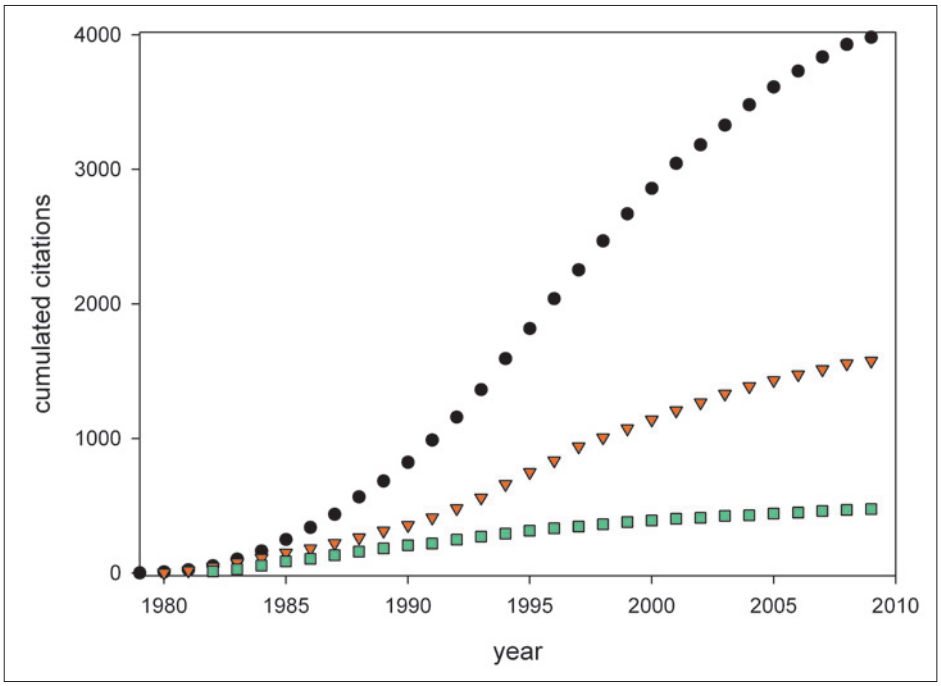

Fig. 1. Cumulative numbers of citations for the three seminal papers on nuclear Overhauser effect spectroscopy (NOESY). The initial slope and sigmoid shape (reminiscent of second-order effects) imply that these classic papers did not significantly contribute to the impact factors of the journals. Black circles represent the work of Jeener, Meier, Bachmann, and Ernst,[18] red triangles the paper of Kumar, Ernst and Wüthrich,[19] and green squares the work of Kumar, Wagner, Ernst and Wüthrich.[20] Figure kindly drafted by PM.

the steady progress of their own *h*-factors (which can never go down, unlike a golfer's handicap). The *h*-fashion has been spreading across the world even faster than the A(H1N1) influenza virus. Provided you have authorized access (*http://apps.isiknowledge.com/*), you can check anybody's *h*-index by ordering his or her list of publications in decreasing order of popularity, *i.e.,* the most-cited first. One must of course narrow down the search to avoid adding up the indices of people who carry the same name. It suffices to check that the *h*th paper has been cited at least *h* times (see Annex 2). By looking up your own '*h*-factor', you can quantify your 'weight' as easily as you can step on the scales in your bathroom!

Of course, such a 'beauty contest' can only be meaningful if the number of citations is a sound measure of quality, originality, *etc.* The case studies discussed above invite us to be cautious. On the occasion of the 25th anniversary of the National Latsis Prize in Bern on January 25th 2009, FM offered some provocative comments: 'Judging the ability of a scientist by his *h*-factor amounts to choosing wine according to the price of the bottle, Swiss cheese by measuring the size of its holes, and chocolate by its sugar content.'

Celebrities (and would-be celebs) who are obsessed with their own *h*-factors are likely to forget their core business. Suppose that professor X approaches the age of 60 and has an *h*-factor of, say, 49. If he is driven by ambition, his overriding objective should be to make sure that his 50th most-cited paper be cited at least 50 times. So he should invite his friends to cite him more often, or (a safer expedient) cite himself in a review in *Chimia*. Once professor X has thus managed to push his *h*-factor up to 50, he can switch his attention to his 51st most-cited paper. Is there any harm in this childish game? Yes: there is a perverse effect, as noted by HH: why should professor X bother to publish a paper in a novel area? Would such a paper stand any chance of being cited 51 times before professor X's retirement? Publishing in a novel area is surely not worth the effort! As a consequence, X will persevere in polishing his tombstone, even if his pet subject has gone stale years ago.

More Case Studies

Let's consider a few cases for the sake of illustration. Albert Overhauser's h = 32, since his 32nd paper has been cited 38 times, but his 33rd only 32 times. In the (unlikely) case that he should wish to push his index up to h = 33, he should attract more attention to his work on electron-phason scattering effects.[22] Likewise, GB should no longer waste his time on cross correlation effects in the presence of spin locking[23] (his 46th paper having been cited 51 times, despite its forbidding title), nor need he pay further attention to Bloch-Siegert effects[24] (his 47th paper having been cited 49 times), nor should he worry about the fate of longitudinal two spin order[25] (his 48th paper having been cited 49 times). On the other hand, GB would be well advised to renew his interest in multiple-quantum effects associated with double resonance[26] (his 49th paper having been cited a mere 48 times, evidence of poor *h*-management). Such an opportunistic policy leads to weird zigzag trajectories through disconnected themes.

Des armes de citation massive

Les chercheurs en sciences sociales attendent peu des indices de citations. Prenons l'exemple de Claude Lévi-Strauss, l'un des fondateurs de l'anthropologie sociale. Son rayonnement intellectuel dépasse de loin les frontières de sa discipline. Il peut être considéré comme l'un des penseurs les plus prestigieux du XXème siècle, peut-être comparable à Sigmund Freud ou Albert Einstein. La plupart de ses travaux ont été publiés sous forme de livres, lesquels ne sont jamais pris en compte par la base des données de l'ISI. Seuls quelques 25 articles qui ont été publiés dans L'Homme, Les Temps Modernes, Esprit, Diogènes, Sinn und Form, the International Social Science Journal, *et* Current Anthropology *sont mentionnés par ISI. Le nombre de citations de son travail ne donne guère la mesure de son extraordinaire influence: le facteur h de Claude Lévi-Strauss ne dépasse pas h = 1!*

Plus modestement, la liste ISI de AM ne mentionne que 4 articles parus dans the Journal of the Royal Anthropological Institute, L'Homme, et Diogènes, *avec un facteur h = 0. Ses 97 autres travaux, la plupart publiés en français ou en espagnol, n'ont pas été jugés dignes d'être mentionnés dans les registres bibliométriques.*

Il peut sembler que les disciplines des sciences humaines disposent d'outils convenables, soit trois banques de données produites par Thomson, le Science Citation Index (SCI), *le* Social Science Citation Index (SSCI) *et le* Arts and Humanities Citation Index (AHCI). *Cependant les disciplines des sciences humaines diffèrent de celles des «sciences dures» non seulement par leurs méthodes, leurs objets d'étude et les liens qu'elles entretiennent avec le contexte social, mais encore par leur mode de communication scientifique. Un rapport de Science-Metrix, une organisation canadienne spécialisée en évaluation scientifique dans les pays anglo-saxons et scandinaves, intitulé* 'The Use of Bibliometrics in the Social Sciences and the Humanities' *montre que la communication scientifique est beaucoup plus hétérogène dans ces disciplines*

que dans celles des sciences dures.[27] *Les livres ont un impact beaucoup plus important en sciences sociales: ils représentent en effet 40% des citations. Ces proportions varient significativement selon les disciplines, de l'économie au droit ou à l'histoire. C'est ainsi que le recours à la bibliométrie a une utilité variable selon les disciplines.*[28]

Une des caractéristiques du Social Science Citation Index (SSCI) *et du* Arts and Humanities Citation Index (AHCI) *de Thomson ISI est la surreprésentation de la production scientifique rédigée en anglais. Ce biais peut être estimé à une surreprésentation de 20 à 25% de l'anglais dans ces banques de données.*[29] *Andersen 2000 citant Andersen 1996*[30] *suggère que 60% des articles répertoriés dans le SSCI ont des auteurs ayant une adresse aux États-Unis et 20% au Royaume-Uni. Puisque Thomson ISI sélectionne les revues selon le nombre de citations qu'elles reçoivent, les habitudes de citation des différentes communautés linguistiques jouent un rôle important dans la constitution même des banques de données. Par exemple, les sociologues américains et anglais citent dans 99% des cas des articles écrits en anglais, bien que ces articles ne constituent qu'approximativement 70% de la littérature internationale en sociologie,*[31] *ce qui explique en grande partie la surreprésentation anglo-saxonne dans les banques de données de Thomson ISI.*

Si on compare la liste des périodiques en sciences sociales de l'UNESCO à celle de l'ISI, on remarque des divergences assez importantes. La liste de l'UNESCO contient environ 2.5 fois plus de revues scientifiques que celle du SSCI. La couverture du SSCI contiendrait 60% de revues américaines, alors que la proportion mondiale qu'elles occupent ne serait que de 17% selon la liste compilée par l'UNESCO. En fait, il est apparu qu'il y a plus de revues américaines couvertes dans le SSCI qu'il n'y en a de recensées dans la liste de l'UNESCO.

On ne peut ignorer la hiérarchie implicite qui règne dans les «sciences molles», certaines étant considérées comme plus «dures» que d'autres. C'est ainsi que l'économie occupe le sommet, probablement parce qu'elle prétend à des méthodes quantitatives que les autres disciplines ne peuvent revendiquer, mais aussi parce qu'elle est réputée utile à la société. Les revues en sciences économiques ont la part belle dans les indexations: une excellente publication en histoire de l'art sera moins citée qu'une œuvre médiocre en économie.

Par ailleurs, les chercheurs en sciences sociales sont particulièrement conscients du réductionnisme qu'implique la quantification, du fait même de la complexité de leurs objets d'étude et de l'importance des contextes de ces derniers: leur thème de recherche souffrant souvent d'une quantification naïve, ils sont d'autant plus conscients quand celle-ci frappe leur production. De plus, l'internationalisation de leur savoir à travers les revues est beaucoup moins avancée que celle des sciences dures. Le Web of Science *(*WOS*) a été souvent critiqué pour son inadéquation aux sciences humaines:*

«Il recense quelque 8700 revues internationales (or, on estime à environ 20000 le nombre de revues scientifiques dans le monde), mais seulement 1000 pour les sciences humaines et sociales (SHS), dont la quasi-totalité sont d'origine anglo-saxonne. A titre d'exemple, les revues SHS soutenues par le CNRS ne figurent pas dans le WOS. Le CNRS a tenté de négocier en 2004 des améliorations auprès de Thomson/ISI qui prendraient en compte les besoins et les spécificités européennes, mais il n'y a pas eu d'avancée. Un rapport remis en 2003 par Philippe Jeannin au ministère de la recherche et des nouvelles technologies sur l'évaluation de la recherche en SHS confirme l'absence de couverture des revues françaises dans ces domaines. En vue de mieux analyser la production scientifique européenne dans le WOS, la European Science Foundation (ESF) *s'est lancée dans l'évaluation des revues en SHS et a publié en juin 2007 une liste initiale sous le nom de* European Reference Index for the Humanities (ERIH)*, qui les classe par rang A, B, ou C. Cette liste exclut certains domaines des sciences sociales, comme la géographie, qui feront l'objet d'un autre classement. L'ESF statue sur l'intégration des revues dans cette liste de référence, ce qui explique pourquoi les revues soutenues par le CNRS n'y figurent pas toutes. Mais l'ERIH n'a pas vocation à être un outil bibliométrique et ne se pose pas comme une alternative au WOS. L'Observatoire des Sciences et des Techniques (OST), qui a précisément pour mission de concevoir et de produire des indicateurs sur la recherche et le développement, annonce qu'il va travailler sur les indices d'impact à partir des revues recensées par l'ESF. La base de données 'Scopus' (Elsevier), pourrait constituer une alternative au monopole de Thomson. En effet, Scopus indexe environ 17000 titres dont 2850 en SHS, soit le double du WOS, et ne se limite pas aux revues anglo-saxonnes. La répartition géographique des titres est de 25% pour le Royaume-Uni (4157 revues), 25% seulement pour le reste de l'Europe/Moyen-Orient/Afrique, 37% pour l'Amérique du Nord, 12% pour Asie/Pacifique, et 1% pour l'Amérique du Sud. La couverture est donc beaucoup plus large que celle du WOS.»*[32]

Les citations des œuvres en sciences sociales sont particulièrement vulnérables à des pratiques peu éthiques: il est facile de piller un article non indexé car publié dans une langue minoritaire telle que le français, ou bien de piétiner les pelouses d'une discipline proche à laquelle le lecteur n'a pas accès: c'est ainsi que tel ethnologue consacrera quelques chapitres à l'histoire de la région où il travaille et importera en contrebande les informations d'un historien non cité. Le jury chargé d'évaluer son travail de recherche pourra s'extasier devant la pluridisciplinarité (sacro-sainte qualité en sciences sociales) du chercheur. Partageant avec les sciences dures les pratiques de citations de courtoisie, les oublis opportuns et les renvois d'ascenseurs citationnaires, les sciences sociales ont trouvé une particularité dans la coquetterie de la citation inattendue, osée et exotique. Celle-ci peut faire l'objet d'une toquade qui multipliera ainsi la parole de médiocres salonnards. C'est ainsi que l'on peut expliquer le succès de la 'French Theory' *dans les* transatlantic cultural studies*, par une distorsion de la citation. On voit bien à quel point les sciences sociales sont vulnérables à des phénomènes de mode. La multiplication des citations des œuvres post-modernes nord-américaines, avec leur exigence bien-pensante de donner la parole aux peuples étudiés et de mettre celle-ci au même plan que celle du scientifique, a produit une «inflation citante» et des monstres d'évaluation. Certains auteurs critiqués par cette école ont bénéficié d'un grand nombre de citations: c'est ainsi que des post-modernes médiocres (est-ce un pléonasme?) ont célébré des pré-modernes médiocres (jusqu'alors non cités) dont le seul tort avait été de relever une ethnographie classique dans laquelle l'indigène avait la place honorable d'informateur plutôt que de victime héroïque.*

Un autre facteur de distorsion des citations en sciences sociales provient de leur sensibilité médiatique. Il y a eu en France dans les années 70 une mode de l'ethnologie qui a propulsé les marchands d'exotisme. Puis, à mesure que la planète se rétrécissait dans sa diversité, ce fut le tour des historiens. Aujourd'hui, grâce à la crise, c'est l'économie qui a la cote. De façon générale, les sciences humaines, dès lors qu'elles traitent de problèmes sociaux, ne peuvent être insensibles à la société qui les finance, et les citations de leur production reflètent ces relations complexes.

La communication scientifique utilise désormais les nouvelles technologies de la communication et de l'information (NTCI), ce qui a pour effet une réforme profonde des modes de diffusion de l'information scientifique. La plupart des travaux scientifiques sont numérisés et mis en

ligne, soit par les chercheurs eux-mêmes, soit par les éditeurs commerciaux, sur des sites d'archives institutionnelles, sur des sites publics, ou encore sur des pages personnelles. Les NTCI modifient donc les comportements des chercheurs qui ont désormais les moyens d'augmenter la visibilité de leurs travaux à l'échelle mondiale et d'accroître leur «potentiel de citabilité» par la mise en ligne. Cette évolution marque un tournant important à la fois dans les relations avec les éditeurs scientifiques commerciaux et dans les pratiques de travail. En effet, le développement des NTCI peut nous faire entrer dans une ère nouvelle, une ère du partage et du libre accès. Mais cette évolution est contrée par la résistance des acteurs financiers, comme en témoigne la création toute récente par l'Association des éditeurs américains de l'organisation Anti-Open Access Partnership for Research Integrity in Science & Medicine (PRISM). *La politique de la citation en sciences sociales pose ainsi des problèmes spécifiques du fait de l'interaction toute particulière entre la production scientifique et la société qui gère cette production. Mais les sciences de la vie sont aussi en interaction étroite avec la société qui commercialise leurs découvertes. La particularité des sciences sociales viendrait plutôt de la facilité avec laquelle on peut instrumentaliser les citations à des fins idéologiques ou commerciales, de par le caractère non expérimental de leur méthode.*

«Depuis début 2006, le CNRS a entamé une grande campagne d'information allant dans ce sens. La European Strategy Forum on Research Infrastructures (ESFRI) *réfléchit sur de nouvelles formes d'évaluation des SHS dans la perspective du 7ème programme-cadre de recherche et de développement (PCRD) de l'Union européenne. La Fondation Européenne pour la Science (ESF) a entrepris le recensement et la validation de listes de revues pour chaque domaine des sciences humaines et sociales, avec un classement selon leur réputation».*

Tous ces efforts sont probablement méritants, mais il reste que l'évaluation des recherches en sciences humaines demeure peu perméable à la bibliométrie. En cela ces disciplines constituent le miroir grossissant des problèmes plus généraux que pose à la science son évaluation bibliométrique.

Il est clair que les sciences humaines attendent les analyses multi-factorielles qui pourraient donner une approximation à leur évaluation. Quoi qu'il en soit, il serait dommageable de leur imposer les mêmes critères qu'aux sciences expérimentales. La bibliométrie en sciences humaines pourrait alors se présenter comme un avortement. La peinture de la renaissance italienne aurait-elle supporté une cotation en bourse?

The Madness of Impact Factors of Journals

The impact factor (*IF*) of a journal is supposed to give a measure of its inherent 'quality' (see Annex 1). The higher the *IF*, the better the journal is believed to be. For the sake of illustration, consider the impact factors for 2008 of a few journals in our respective fields: *L'Homme* (the journal is indexed but its *IF* is not listed), *Journal of the Royal Anthropological Society* (*idem*), *Nature* (*IF* = 31.434), *Science* (28.103), *Angewandte Chemie International Edition* (10.879), *Proceedings of the National Academy of Sciences* (9.380), the *Journal of the American Chemical Society* (8.091), *Physical Review Letters* (7.180), *Progress in Nuclear Magnetic Resonance Spectroscopy* (6.162), *Physical Chemistry and Chemical Physics* (4.170), *ChemPhys Chem* (3.636), the *Journal of Chemical Physics* (3.149), *Chemical Physics Letters* (2.169), the *Journal of Magnetic Resonance* (2.438), and *Chimia* (1.283). Impact factors tend to vary wildly in short timespans. Consider by way of example some recent ups and downs in physical chemistry (Fig. 2).

What do these numbers mean? To be honest, very little. The calculation of the *IF* is only based on the preceding two years. For example, the 2008 impact factor of a journal is defined as $IF = A/B$, where A is the number of times that articles published in the journal in 2006 and 2007 were cited during 2008, B being the total number of 'citable items' published in 2006 and 2007 in the same journal (see Annex 1).

Suppose we wish to boost the *IF* of our favourite journal in 2012. That implies that: i) we should publish a paper in 2010 (preferably early in the year, as explained below), ii) some colleagues, friends or competitors should discover our paper before the ink has had time to dry, iii) these colleagues must find an interesting application of our method, or invent an improvement of said method, iv) carry out novel experiments, v) write up their observations, vi) submit their paper to a journal of their choice, vii) wait for the referee reports, viii) incorporate suggestions for improvements, and ix) have the paper accepted and appear in print before the end of 2011! Depending on the date of publication of the original paper (which could be anywhere between January and December 2010), our colleagues have only 12 to 24 months to complete the entire process! This is clearly pure folly. Very few scientists can produce a serious paper in such a short period. (Incidentally, NMR may be an exception, since many techniques can be implemented virtually overnight. But beware if you have to build your own instrument, align your laser beams, pump your vacuum chambers, synthesize your precursors, let alone carry out field work in the depth of the Amazonian forest!).

Impact factors are not only unstable: they are ill-conceived and misleading. Impact factors appear to be even more erratic than stock exchange indices such as the Dow Jones, NASDAQ, FTSE or CAC40.

Why bother to dwell on these obvious shortcomings? Because it turns out to be easier for scientists who publish in journals with high impact factors to obtain grants, to get promoted, and to find jobs for their former students. In some countries, as explained by GO, scientists are evaluated simply by weighting the numbers of their publications with the *IF*s of the journals in which they have appeared. By this standard, a single paper in *Science* would be 'worth' more than 11 papers in the *Journal of Magnetic Resonance*.

In France, according to the unwieldy instructions to the experts of the *Agence*

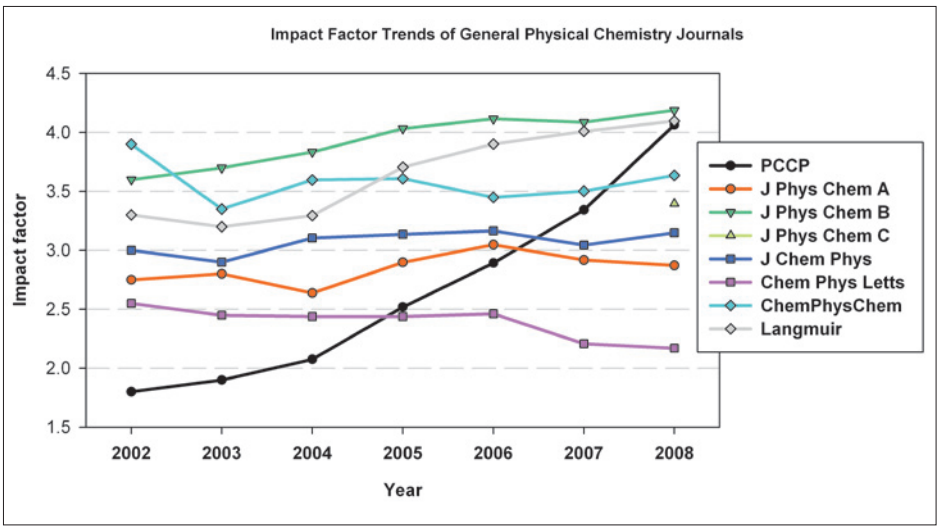

Fig. 2. Trends of impact factors of journals in physical chemistry.[33]

pour l'Evaluation de la Recherche Scientifique (AERES): *'une [seule] publication dans* Nature, Cell *ou* Science *[en quatre ans] va suffire à cataloguer le chercheur ou enseignant-chercheur comme publiant.'*[34]

Editors and Science Policy

Good journals used to pay a great deal of attention to the quality of their production. Nowadays, they expect the authors to submit a finished product. Along with typesetters, copy editors have been largely phased out, so that the publishers can focus on their core business of making profits.

Wielding their *IF*s as Greek gods their bronze axes, editors-in-chief of major journals are now endowed with inordinate power. Their Editorial Boards have little say. Their staff terrorizes the community by taking far-reaching decisions about papers that are rejected straight away, and about papers which in their opinion deserve to be sent out to review. Publications must be perceived as relevant to a 'broad audience', which is of course at the discretion of the editors. Editors have a central function as gatekeepers of the system. They can shape research much more than any funding agency! It can be argued (KG) that if you would replace all associate editors of JACS, chemistry as a science would look completely different in 10 years from now.

On 24th July 2006, we received a revealing letter from the Editor-in-Chief of one of the most highly regarded journals in the field of chemistry (kindness forbids us to betray his identity). He rejected one of our papers without sending it out to review on the following grounds:

"(i) A major requirement of a Communication [in our journal] is timeliness and urgency. How can your manuscript be timely if most of your references are 30 years old and the most recent one is from 2001? (ii) Why is this urgent? This is an improvement on an existing technique with little or no conceptual advance that I can see. (iii) If this really is of broad general interest why are your references to broad general interest venues like PNAS and JACS 30 years old? There is not a single reference to a broad general interest journal like Science, Nature, PNAS, JACS that is recent."

You can trust us that, ever since, we have taken the precaution to pepper our works with recent references to Science, Nature, PNAS, JACS, *etc.* Thus embellished, our paper ended up in *ChemBioChem*[35] and was cited four times in its first year.

Comment analyser la notion d'urgence dans la production scientifique? D'après l'exemple ci-dessus il semblerait qu'une publication est réputée 'urgente' à publier quand elle fait référence à un article récent donc, suivant cette logique, ayant été lui-même considéré comme 'urgent'. La production de la science irait ainsi d'urgence en urgence, comme le service des premiers secours dans un hôpital. Il s'agirait ainsi de parer sans cesse au plus pressé sans jamais se plonger dans une réflexion à long terme. Cette conception correspond d'ailleurs aux modes d'attribution des crédits de recherche: ceux du CNRS sont passés de l'attribution aux laboratoires à l'allocation de moyens par l'ANR sur des critères d''urgence' qui sont mal définis. Ceux-ci sont tellement flous que l'ANR a été contrainte à multiplier les projets 'blancs' dans ses appels d'offres. En linguistique, plutôt qu'un commentaire sur un article 'urgent' dans une revue des urgences, n'est-il pas plus 'urgent' de publier la grammaire d'une langue en voie de disparition? Il est certain qu'une telle publication n'augmentera pas le facteur h de son auteur puisque son objet sort du néant et se présente comme un témoignage d'une mort annoncée.

Par ailleurs les revues de sciences humaines sont sujettes à l'intervention d'une idéologie politiquement correcte. Ainsi, la rédactrice en chef de la prestigieuse revue Journal of the Royal Anthropological Institute *a voulu imposer des modifications à l'auteur d'un article portant sur des rituels espagnols opposants Juifs et Chrétiens: selon elle, certaines données ethnographiques et commentaires indigènes des rituels étudiés étaient suspects d'antisémitisme et l'auteur devait donner en note un témoignage d'antiracisme. Comme si l'auteur d'un article sur l'anthropophagie devait jurer ne jamais avoir consommé de la chair humaine!*

It is sometimes believed that high-impact journals have high standards of peer review. This perception is a fallacy. If asked to review a paper, we do not pay more attention when the request comes from *Science* than from the *Journal of Magnetic Resonance*. On the contrary! Since the likelihood that a paper actually will be accepted in *Science* appears slim, it is all too tempting for a referee to deliver a superficial review. Worse, the 'generalist' editors of non-specialist journals do not know whom to ask. As a result, it is not rare to read papers in *Science* that are as muddled in their argumentation as spectacular in their claims. Such papers would never be accepted by the *Journal of Magnetic Resonance*! In fact, many articles that are published in *Science* are very ephemeral, while more fundamental long-lasting papers can only be found in the specialized literature. *Science* has great stories on exoplanets, global warming, and influenza epidemics. This may explain its popularity. As one of our most esteemed colleagues put it, magazines such as *Science* are fit to be left on the coffee table in the common room, along with *Time Magazine, l'Hebdo, Spiegel,* and perhaps *Gala.*

A Dance amongst the Wolves

Most scientists are content with publishing papers in a variety of specialized journals. Senior readers of *Chimia* know how the process works, but let's give a brief summary for the benefit of younger students. We can distinguish a number of steps in this tribal dance: i) Having collected sufficient experimental evidence or constructed a sound body of theory, one may decide that time has come to publish a new paper. ii) Equally respectable (and more profitable in terms of *h*-factors), one may decide to write an overview of some area that is close to one's heart. iii) You then submit the paper to a journal of your choice (nowadays *via* a website), considering its reputation, its peer review policy, its impact factor, the career ambitions of your students and post-docs, *etc*. iv) The editors forward your paper to a few more or less well-chosen experts (known as 'referees'). v) After a few weeks, you receive the written opinions of the anonymous experts, often with excruciatingly detailed criticism, plus a more or less standard letter of the Editor who decides to publish or let you perish. vi) You try to overhaul your paper to placate the scepticism of the experts, and write an accompanying letter in the hope that you can convince the Editor to let you live a bit longer. If you fail to convince him, go back to iii) and start all over again. vii) If the Editor can be swayed, you'll receive so-called galley proofs (sometimes riddled with misunderstandings about mathematical or chemical formulae) a few weeks later. viii) You fight a battle against the printer (or against the computer program that replaced him) to get everything right. ix) Your paper appears on the Web (sometimes even before you have submitted your corrections!). x) Your paper appears in hard copy for the benefit of a few remaining old-fashioned libraries. In fact, the only evidence that a hard copy actually exists lies in the page numbering.

Perhaps some readers of *Chimia* have shared our experience: driven by ambition, by the desire to be recognized, by the wish to promote the careers of one's graduate students, one is tempted to submit a paper to the 'best' possible journal, *i.e.,* to the journal with the highest impact factor. Having been taken by surprise by a favourable (perhaps erroneous?) decision by the editors of *Science,*[36] we have become bold enough to submit a recent paper on MRI in inhomogeneous fields to *Nature*. Upon receiving a curt refusal, we

tried *Science*, then *Nature Methods*, than the *Proceedings of the National Academy of Sciences (PNAS)*, all in vain. Finally, we settled for our old friend, the *Journal of Magnetic Resonance (JMR)*. To overcome these humiliating defeats, we tried to boost our morale by remembering that JMR has published some nice pieces, such as the first paper on 'Fourier transforms in MRI'[37] (*NC* = 553) and my favourite work on 'Coherence transfer pathways'[38] (*NC* = 1173). Clearly, the 'big' journals do not have a monopoly. Impact factors give undue attention to generalist journals, while pure science, necessarily focused, only appears in specialized journals. *Les revues spécialisées sont mal cotées par la bourse des revues!*

A Threatened Species: Monographs

The noble art of book writing, which is much more prevalent in the realm of social sciences than in the so-called 'hard' sciences, is threatened by the current obsession with citations. RE's, GB's and AW's magnum opus,[39] which sold over 11000 copies in four languages between 1987 and 2009, may be considered a success. Yet it did not contribute a speck to the *h*-factors of its co-authors. In fact, it has been argued (MMo) that writing a book no longer pays off, since an author who summarizes his work in a monograph damages his own *h*-factor. Perhaps the end of book-writing is in sight?

Ceci est particulièrement dommageable pour une discipline comme l'anthropologie. En effet une monographie sur une ethnie, un village ou un groupement d'individus est la voie royale de cette discipline. La découverte d'une culture, d'une langue ou d'un rituel peut difficilement s'exprimer en un seul article. Prenons par exemple les résultats d'une enquête de terrain sur telle ou telle divinité. Celle-ci est à la fois une image vivante dont il faut exposer les procédés d'anthropomorphisation et débusquer les substituts; un symbole régional dont il faut montrer les ressorts politiques; une figure de royauté dont il faut démarquer le territoire; une marque sur le cycle rituel de la région; l'objet d'une initiation à l'âge d'homme par les porteurs de son image; le vecteur d'une agentivité sur le réel qu'il faut rechercher dans l'énoncé de ses vertus, dans ses apparitions, dans ses miracles, dans son invention…. Chacun de ces aspects peut évidemment faire l'objet d'un article et l'ensemble pourra favoriser le facteur h de l'ethnologue concerné. Mais ne serait-ce pas plus cohérent, plus intéressant et, finalement, plus scientifique, de publier l'ensemble de ce travail dans un livre?

La civilisation du chiffre: pourquoi la fièvre bibliométrique?

La hausse rapide de la fièvre bibliométrique mérite sinon une explication, du moins la recherche d'indices contribuant à son analyse. Il est vrai que l'infantile narcissisme du scientifique se satisfait de la hausse de son facteur h, comme de la puissance du moteur de sa voiture ou de sa vigueur sexuelle. Cet obscur goût de soi s'accompagne d'une certaine lâcheté: la bibliométrie est un refuge pour le scientifique évaluateur qui craint ses collègues et les institutions. Dans la jungle académique du pouvoir, comment tracer avec exactitude les jeux d'alliance, les réseaux, les clientèles? Plutôt que d'avancer un jugement indépendant qui peut vous créer des ennemis, il est moins risqué de faire œuvre de comptable.

Cependant il est facile d'imputer les carences de la bibliométrie à des troubles du caractère et il est plus juste d'en chercher des raisons dans la société qui produit cette science formatée. Il semble que la recherche de facteurs d'impact ailleurs que dans la qualité intrinsèque d'un travail scientifique vienne d'abord d'une perte de repères. De même qu'il y avait en Bourse des valeurs sûres, des «actions de père de famille», il y avait jusqu'à présent en science des thèmes porteurs, en particulier dans les humanités. On distinguait les bons sujets des mauvais et l'on classait ceux-ci dans des disciplines plus ou moins figées. Un certain nombre de critères étaient requis pour faire de la bonne science, tels que la qualité de la documentation, la rigueur du raisonnement ou l'élégance de la présentation. Ces critères n'étaient évidemment pas tous adéquats, mais ils avaient le mérite d'exister et d'emporter une certaine unanimité. Aujourd'hui beaucoup d'entre eux sont considérés, à tort ou à raison, comme conformistes. Mais comment les remplacer? Le facteur h fournit non un critère interne à la science et à sa qualité mais un repère externe. Comme si on jugeait une œuvre d'art à son succès de diffusion en son temps: à ce titre le Requiem de Mozart, les paysages de Van Gogh ou les poèmes de Verlaine ont souffert de leur temps d'un facteur h déficient. Il ne faut pas se cacher qu'il est difficile d'énoncer des critères mesurant la qualité d'un travail scientifique, et le facteur h est un recours à la facilité, le fruit d'une paresse intellectuelle qui, de plus, a bonne conscience.

En effet, cette perte de repères s'accompagne dans l'évaluation scientifique d'une contagion par l'égalitarisme bien pensant présenté comme de la démocratie. L'un des dictats de la bibliométrie: nous devons tous être jugés à la même aune. Même si la mesure est médiocre, elle vaudrait mieux qu'un soupçon de favoritisme ou de fantaisie. L'allégorie de la Science ressemble désormais à celle de la Justice: les yeux bandés, elle s'avance en présentant la balance impitoyable mais égalitaire du facteur h. Ce qu'il y a dans les plateaux de la balance importe peu: c'est la mesure qui doit être «juste», c'est-à-dire exactement la même pour tous. Cette représentation dévoyée de la Démocratie pollue actuellement les évaluations des chercheurs, les attributions de poste, les distributions de crédits. Elle conduit trop souvent à des choix de médiocrité sous couvert d'égalité des chances. Cette question est particulièrement délicate dans les sciences non expérimentales et singulièrement dans les sciences humaines pour lesquelles les estimations objectives peuvent être empreintes d'idéologie : telle conclusion de l'un vaut bien celle de l'autre puisqu'il n'y a pas de démonstration mathématique. Face à l'impossibilité d'établir un jugement qui pourrait faire courir à l'objectivité un danger majeur, on a recours à la métrique de la bibliométrie qui, comme nous l'avons vu, s'avère particulièrement inadéquate à ces disciplines.

Finalement les deux tendances que nous venons d'examiner, d'une part la perte de repères et d'autre part le besoin impérieux d'un égalitarisme qui ne se trouve évidemment ni dans la Nature ni dans l'intelligence, convergent vers une troisième perversion qui est probablement la plus grave: le besoin de recourir à une entité supérieure qui juge à votre place. Quoi de plus rassurant qu'une divinité comptable? On n'est plus à la merci d'un collègue irascible mais confiant dans la toute-puissance du chiffre. On voit tout ce que cette conception a de dangereux pour la vie politique en général. La Justice aux yeux bandés peut faire place imperceptiblement à un Big Brother comptable, caché derrière le caractère imperturbable de la statistique. Il décerne des notes aux revues, mesure les bibliographies individuelles, débusque les minorités de publications visibles ou invisibles, mais surtout inhibe toute réflexion. Tout cela au nom sacré de l'égalité des chances.

Are there any Alternatives to Bibliometrics?

Since 'any number beats no number' (YG), it will be difficult to overcome the era of fake indices without replacing them by other indices. Yet AF's complaint is unambiguous: "The terrible legacy of *IF* is that it is being used to evaluate scientists, rather than journals, which has become of increasing concern to many of us. Judgement of individuals is, of course, best done by in-depth analysis by expert scholars in the subject area." Yet another call for a return to the *status quo ante*! AF also argues

in favour of 'eigenfactors' (*EF*s), apparently without the slightest concern for their definition, thus providing evidence that the standards of his beloved PNAS are not as rigorous as he claims.

YG favours leaving the business of evaluation to professionals:

"La plupart des critiques se résument à faire ressortir les «limites» des classements sans jamais se poser clairement la question préalable de leurs fondements épistémologiques: les indicateurs ont-il bien la signification qu'on leur attribue?" (…) "*l'absence de balises et de réflexions méthodologiques sérieuses donne lieu à ce qu'il faut bien appeler des utilisations anarchiques, pour ne pas dire sauvages, de la bibliométrie.*"[40]

He reminds us that useful indicators should be defined in adequacy to the subject (people, journals, and institutions should not be judged by the same metrics), the measurement should be homogeneous and should reflect the inertia that is characteristic of journals and institutions. The worst infractions against these rules have been made by the so-called Shanghai classification of universities, which mixes many unrelated and unstable parameters. The damages that have been inflicted on European universities by this ill-conceived indicator dwarf the effects of weapons of mass destruction.

How are your *TF*s and *FF*s Doing?

Most scientists are unlikely to dedicate sufficient time to acquire a truly professional grasp of indices. We favour a more provocative and playful response. There are plenty of stimuli to which we scientists eagerly respond. Let us start with the *teaching factor* (*TF*). Paying attention to your *TF* is much more fun than watching your *h* factor! Indeed, many of us are sensitive to the rewards of teaching: i) the pleasure of being challenged by a demanding student who, after an ill-prepared lecture, complains that your half-baked theoretical explanation is incomprehensible (*e.g.* ring-current contributions to chemical shifts), ii) the satisfaction of hearing a student in an oral examination clearly expose some challenging piece of theory (*e.g.* Overhauser effects), when you remember having given a muddled explanation on the blackboard a few weeks earlier, iii) the pleasure of hearing a smooth seminar given by a self-confident graduate student who was riddled by doubts when starting his PhD, iv) the joy of attending the public PhD defence of a student who is radiant with pride of his achievements, v) the promotion of one's former students to a tenured professorship, vi) a prize, grant or project of international scope given to one's former students. (One should of course always resist the temptation to take credit, as AE pointed out after receiving a gold medal at the *Académie des Sciences* in Paris: '*Man soll keinen Anspruch erheben.*') Such are the rewards of teaching, which cannot be measured by any *h*-factors and the like.

Research can also be a source of pleasure. This aspect is best expressed in terms of a *fun factor* (*FF*). Consider i) the pleasure of giving a revolutionary talk to a raving audience in a beautiful seaside resort, ii) reading a review that starts with '*This is a well-written paper…*' knowing that the writing was one's chief (and perhaps only?) contribution. But there are more secretive pleasures, such as iii) the stare of an envious colleague, iv) writing a nasty review on a paper from a competing laboratory, *etc.* All of this easily beats the fascination for *h*-factors!

It will be challenging to convince granting agencies to respond to yet-to-be-defined factors. Imagine writing in a proposal that your *TF* = 8 and your *FF* = 9! Pure fantasy? Not necessarily. In the last few years, GB has consistently included in his proposals an overview of the current jobs held by former PhD students and post-docs. A poor man's *TF*?

Des réformes mal engagées

La focalisation sur la seule visibilité de la recherche à travers des facteurs h conduit au désintérêt pour les structures de production de la science. En France, le devenir du CNRS est au cœur de la reproduction de la science. Faut-il consolider le CNRS? Faut-il au contraire le démanteler? Dans son plaidoyer pour la sauvegarde du CNRS, Albert Fert, lauréat Nobel de physique 2007, attire notre attention sur les dangers de la disparition du CNRS.[41] *Il s'insurge contre "une réforme du système de recherche... dictée par des motivations idéologiques." En effet, la vraie recherche échappe à toute volonté étatique, surtout si celle-si s'habille des atours de la bibliométrie.*

*Les grandes découvertes, surtout celles qui débouchent sur des applications utiles, ne se font pas par décret ni par facteur h. Tout au plus peut-on espérer créer un champ de force favorable à l'éclosion d'idées nouvelles. Or, sur ce point, la France a frappé un grand coup par l'invention du CNRS, structure unique au monde. Certains pensent que l'Agence Nationale de la Recherche (ANR) pourra prendre un jour la relève du CNRS. Mais les premiers balbutiements de l'ANR sont assez inquiétants, et pas seulement par leur dérive bibliométrique: on n'est guère rassuré par le manque d'expérience de ses responsables, la pesanteur administrative derrière laquelle ils se réfugient, leur zèle à réinventer la roue en ignorant les modèles étrangers, et les lacunes quant aux modes d'évaluation qui iraient au-delà des simples mesures bibliométriques. Il ne suffit pas, comme le fait l'ANR, d'injecter des moyens au coup par coup, dans quelques projets choisis. Il faut assurer la continuité et la stabilité des équipes, l'encadrement des jeunes, et la relève de leurs dirigeants. Tout cela relève d'une dynamique subtile, qu'on ne saurait décréter d'en haut (*top down*), mais qui exige une écoute attentive des besoins des laboratoires (*bottom up*). Le Comité National du CNRS a su, tant bien que mal, assumer cette fonction depuis des décennies.*

Les grandes découvertes ne viennent plus aujourd'hui d'un cerveau génial inspiré dans sa tour d'ivoire par le miroir de son facteur h. Elles nécessitent un terreau qui peut, certes, contenir quelques mauvaises herbes, mais qui permet la floraison de belles plantes. Un tel terreau existe aux USA dont les universités sont adaptées pour recevoir les fonds de la National Science Foundation (NSF), *des* National Institutes of Health (NIH), *du* Department of Energy (DOE), *et d'autres agences de moyens. En France, ce sont les équipes de recherche associées au CNRS qui jouent ce rôle de capteurs de fonds. Faire disparaître le CNRS reviendrait à vouloir faire de l'agriculture hors sol. Les produits de celle-ci, on le sait, sont insipides. En revanche une réforme concertée des trois piliers de la recherche française, CNRS, Université et ANR est la seule manière de donner un sens à l'ANR dont les perversions déjà visibles proviennent en partie de la faiblesse des établissements qu'elle a pour mission de soutenir.*

Publish but Nevertheless Perish

La logique de publication en sciences sociale d'un grand centre de recherche comme le CNRS est perverse: i) On paye des scientifiques pour faire de la recherche; ii) on ne prévoit pas de fonds pour publier les résultats, ni pour les traduire en anglais afin de prévenir le pillage intellectuel; iii) on reproche au chercheur la faiblesse de ses publications dont le financement n'a pas été prévu. C'est ainsi que SP, chercheur au CNRS, a bénéficié de subsides pour plusieurs missions en Indonésie étudier une société pratiquement inconnue. Après des années d'enquêtes difficiles, elle a écrit une monographie remarquable, et elle a obtenu d'une fondation une aide à la publication. Elle a attendu cinq ans pour que l'éditeur publie enfin son livre, c'est-à-dire les résultats d'une recherche qui avait coûté au CNRS des années de salaire et des frais de mission. De futurs évaluateurs se pencheront peut-être sur son dossier pour

lui reprocher la faiblesse de son indice de publication. Celui-ci de toute évidence ne sera en rien représentatif de la qualité de ses recherches. Il ne suffit pas d'écrire les résultats d'une étude. Encore faut-il trouver un éditeur qui investisse des fonds pour les publier! Pour avoir un indice h correct un chercheur en sciences sociales doit se doubler d'un businessman.

Dans ces disciplines, l'évaluation par la vitesse de publication est d'autant plus absurde qu'un auteur doit parfois attendre un ou deux ans avant que son article, une fois accepté par une revue, ne soit publié, parfois le temps qu'un rival publie un article sur le même sujet dans un autre journal. On a vu des revues pratiquer volontairement la rétention d'un article pour permettre à tel ou tel collègue de publier le sien sur le même sujet! Font aussi l'objet de trafics indignes les comptes-rendus de livres qui sont déterminants pour le facteur h d'un auteur. C'est ainsi que tel collègue acceptera de recenser un ouvrage qui fait concurrence au sien, puis oubliera d'écrire ce compte-rendu, le temps que son livre à lui déploie sa force de citation. Le livre dont il s'est engagé à rendre compte est ainsi pris en otage.

Inflationary Publication Rates

In principle, the fashion of *h*-factors should favour publishing a small number of good papers, rather than endless series of minor works. Perhaps the only positive aspect of the *h*-fashion is that it might slow down the tremendous inflation of the number of published papers. By combining 'copy-and-paste' methods with the power of 'search engines', it has become far too easy to write! Some readers may have overheard a Senior Common Room exchange between two Oxbridge dons: "I've just published a new book", says one of them. "Jolly good", comes the reply, "under what title have you published it this time?"

Referring to the good old times when the increasingly fat volumes of *Physical Review* were still physically stacked on library shelves, AA observed that the speed of propagation *v* of the front page would soon exceed the speed of light *c*. The principles of relativity were not at stake, said AA reassuringly, since the information content was approaching zero.

To fight this tendency, EH suggested that each scientist should be issued a non-renewable chequebook at the beginning of his career, so that it would be impossible to publish more than, say, 20 papers in the life of a scientist. Actually, notwithstanding his own rule, EH wrote no less than 95 papers! But these include time-reversal by spin echoes[42] (*NC* = 2860), cross-polarization[43] (*NC* = 1369), and self-induced transparency[44] (*NC* = 1004). Allowing for a hundred trials does not seem excessive to achieve three 'hits' of such magnitude.

In the vein of EH's prohibition, one might also consider imposing a limitation on the creation of new journals. Nobody questions the respectability of *Angewandte Chemie.* But was PG well-inspired when he created *Chemistry a European Journal*, *Chemistry an Asian Journal, ChemPhys Chem, ChemBioChem, ChemMedChem, etc.*?

Academic Appointments

Two bright graduate students have recently completed their PhDs at EPFL. Although they have very different personalities, interests and areas of expertise, it would be hard to say that one is 'better' than the other. Each of them co-authored about eight papers during four years of research. SCa has collected 97 citations, while RS has to be content with a mere *NC* = 8. Does this mean that SCa will find it easier to get a job? Or will prospective employers realize that SCa's subject area is more fashionable, while RS's subject may be more challenging and less popular? One shudders at the implications!

Appointments of assistant professors, *maîtres de conference,* and other faculty members shape the future of institutions of higher learning. Many mechanisms have been invented in various countries to fight the more-of-me (MOM) syndrome, old boy networks, and nepotism. None of these safeguards appear to be quite fool proof. When directors of institutes, deans of faculties, and presidents of universities are ill-inspired, they can create havoc. And they often do! Excessive reliance on bibliometrics fuels poor decisions. Instead of reading the candidate's research project, browse through his published papers, and listen attentively to his lecture, it is so much easier to ask a secretary to look up some bibliometric data!

The risks of the current policies have been summed up by GMa:[45] *"Nous arrivons maintenant à la question épineuse: que ferait-on aujourd'hui d'un type comme Einstein? Je réponds de façon assez pessimiste à cette question. Il aurait été envoyé à l'apprentissage plutôt qu'à l'université, en tout cas, il n'aurait jamais obtenu la maturité [équivalent suisse du baccalauréat]. Etudiant universitaire de* bachelor*, il aurait probablement raté nos examens [ceux de l'EPFL]. Même s'il avait atteint le niveau du* master*, il aurait été victime de la maîtrise insuffisante de l'anglais (langue qu'il martyrisa toute sa vie). Après un éventuel doctorat, il aurait difficilement trouvé un emploi public. Jeune professeur assistant, il n'aurait jamais passé la procédure d'évaluation, car ses grands résultats furent validés avec un retard incompatible avec la* tenure track *d'aujourd'hui. Il n'aurait jamais été capable d'obtenir un subside du Fonds National [en Suisse], de donner un cours de base ou de suivre des doctorants. Juif laïque et libre penseur, il aurait été victime du feu croisé d'intégristes et fanatiques de plusieurs religions."* Thus Einstein would probably never be granted tenure in today's system.

Permanent Evaluation

Once upon a time, there were no peer reviews at all. When Einstein received an anonymous review for the first time in his life, he drily replied to the editor: "I submitted this paper to be published, not to be criticized". Today, the quality of peer review is the main (and perhaps only) difference between good and bad journals, between granting agencies that fail and those that sail. Anonymous peer review lies at the heart of modern science. Recently, I was asked by *Science* to review a paper. In a brief spell of arrogance, I wrote "The authors would be well advised to submit their work to a more serious journal less inclined to favour hype". I told the story over breakfast at some meeting, and was impressed by the profound silence around the table, as if my dear colleagues all thought "how dare you!" *Une forme de lèse majesté ?*

Much more than in Einstein's days, we are being evaluated continuously.[46] When we write papers and submit them to journals. When we submit proposals to granting agencies. When we send a sketch of a patent application to an industrial partner. When we encourage young PhDs to apply for grants to pursue post-doctoral work. When we submit abstracts to the organizers of meetings in the hope of being invited for a talk. When our Department or Institute or Faculty or *Unité Mixte de Recherche* (UMR) or *Ecole Doctorale* (ED) is evaluated. If you are a member of *n* such groups and teach *m* courses, you will be evaluated *n* + *m* times. By a professional evaluator. By a computer. By students (how refreshing their comments can be!) By an *ad-hoc* committee whose members have been picked by some Director, Dean, or President. By a committee despatched by the CNRS. By the *Agence pour l'Evaluation de la Recherche Scientifique* (AERES). Whatever the inherent shortcomings of these rituals may be, bibliometrics can only make them worse!

La fascination du pouvoir

We scientists have power! This is particularly obvious in the area of chemistry. The power to heal. The power to avert suffering. The power to improve life. The

power to predict the future. One of my friends (MMu) developed a drug against hepatitis C that is currently being tested in 'phase III'. SCo hopes to cure tuberculosis. CD worked out how to screen drugs against protein targets. PP hopes to make MRI more accessible. Whether these hopes are legitimate, or whether they might end up with disappointments, is not the point here: the key lies in the driving force, the motivation, the libido, in the question 'what makes them tick', what motivates them to return again and again to their laboratories.

Scientists who are passionately involved in their pet areas have little interest in taking over administrative responsibilities. The corollary is that it is difficult to find qualified scientists who are willing to run the show. As a consequence, colleagues who have never experienced the thrill of a real discovery tend to rule the waves of science policy.[47]

Par contraste avec les sciences dures, la majorité des sciences molles ont de moins en moins de pouvoir, dans la mesure où le savoir 'inutile' a de moins en moins de prestige, contrairement à ce qui était le cas dans les salons bourgeois du XIX^{ème} siècle.

Par ailleurs les scientifiques des disciplines 'molles' n'ont pas accès aux médias. Quand de rares radios ou télévisions leur ouvrent leurs portes, c'est à des pseudo-scientifiques qu'elles s'adressent. C'est le cas des 'nouveaux philosophes' comme BHL: ils se servent de leur pouvoir symbolique pour défendre des 'causes justes', ces dernières venant légitimer un savoir qu'ils ne possèdent pas. Le raisonnement pervers est le suivant: BHL défend des causes justes et nobles, donc son savoir est juste et noble. C'est le cas de CC qui abreuve de clichés bien-pensants sur les sauvages l'Université Populaire que lui a confiée le musée du Quai Branly. Pourquoi ne pas avoir accordé cette mission aux nombreux anthropologues qui cherchent à se rendre utiles? La dénonciation par EJ de l'imposture que constituent les déclarations de cette dame 'people' sur les Dogons ne peut avoir d'impact sur le public car les ethnologues qui s'en prennent aux mythes dont notre société a besoin sont privés de pouvoir. Il faut bien avouer d'ailleurs que, quand ils sont compétents, ils ne le cherchent guère. Les scientifiques de haut niveau recherchent plus le pouvoir symbolique que le pouvoir politique, qui est trop souvent abandonné aux médiocres.

Recovery in Sight? *La sortie du tunnel?*

There is a glimmer of hope. If our *tutelles* were really serious about *h*-factors and the like, they should leave it to computers to take *all* decisions about science policy. One corollary would be that they would make themselves superfluous. It is well known that no body of administrators will ever agree to be phased out. Let's hope that they will find some convoluted way to justify their continued existence, and that the procedures they will invent can attenuate the impact of impact factors!

The most obvious suggestion (CG) is to revert to the *status quo ante*. After all, science has relied on peer review for decades without being contaminated by any bibliometrics. Reverting to the old times is a bit like asking people who are addicted to watching the so-called 'news' on TV to revert to reading newspapers. This advice may be taken in individual cases, but is unlikely to work for an entire community. One should constantly torpedo bibliometrics by forcefully speaking out against them in appointment committees (BM).[48] We should constantly challenge the arrogance of journal editors and make every effort to ridicule their beloved impact factors. Another way of restoring the balance is to pay more attention to teaching factors and fun factors.

La bibliométrie donne incontestablement une indication sur l'activité d'un chercheur et il ne faut pas la refuser par principe. C'est plutôt l'obsession métrique qu'il faut combattre car elle crée les distorsions d'évaluation que nous avons exposées. Mais surtout elle peut, par le mirage comptable qu'elle produit, cacher les vraies valeurs du travail scientifique: le talent de chercheur et d'enseignant, et surtout son engagement personnel qui est fait à la fois de prise de risque, d'implication personnelle et de responsabilité collective. Ces valeurs ne peuvent ressortir que d'un examen qualitatif de l'activité scientifique.

Acknowledgements and Initials:

We are indebted to (in alphabetic order): AA = Anatole Abragam; AB = Ad Bax; AE = Albert Eschenmoser; AF = Alan Fersht; AK = Anil Kumar; AM = Antoinette Molinié; AO = Albert Overhauser; AS = AJ Shaka; AW = Alexander Wokaun; BHL = Bernard Henri-Lévy; BM = Beat Meier; CC = Catherine Clément; CD = Claudio Dalvit; CG = Christian Griesinger; CS = Charles Slichter; DR = David Ruben; EH = Erwin Hahn; EJ = Eric Jolly; FM = Frédéric Merkt; RF = Ray Freeman; GB = Geoffrey Bodenhausen; GMo = Gareth Morris; GMa = Giorgio Magaritondo; GO = Gottfried Otting; HH = Hashim El-Hashimi; IS = Ionel Solomon; KG = Karl Gademann; KW = Kurt Wüthrich; LM = Luciano Müller; MMo = Manfred Morari; MMu = Manfred Mutter; MR = Mark Rance; OS = Ole Sorensen; PG = Peter Gölitz; PM = Pascal Miéville; PP = Philippe Pelupessy; RE = Richard Ernst; RS = Riddhiman Sarkar; SCo = Stuart Cole; SCa = Simone Cavadini; SP = Simonne Pauwels; UP = Umberto Piantini; YG = Yves Gingras.

Abbreviations used in the Text:

AERES = *Agence [française] pour l'Evaluation de la Recherche Scientifique*; AHCI = Arts and Humanities Citation Index; ANR = *Agence Nationale [française] pour la Recherch*e; CNRS = *Centre National [français] pour la Recherche Scientifique*; CTI = *Commission [suisse] pour la Technologie et l'Innovation*; DOE = [US] Department of Energy; DQF-COSY = double-quantum filtered correlation spectroscopy; *EF* = Eigenfactor; EPFL = *Ecole Polytechnique Fédérale de Lausanne*; ERIH = European Reference Index for the Humanities ; ESF = European Science Foundation; ESFRI = European Strategy Forum on Research Infrastructures; ETHZ = Eidgenössische Technische Hochschule Zürich; *FF* = Fun Factor; FNRS = *Fonds National Suisse de la Recherche Scientifique*; HMQC = Heteronuclear multiple-quantum correlation spectroscopy; HSQC = Heteronuclear single-quantum correlation spectroscopy; *IF* = Impact Factor; INEPT = Insensitive nuclei enhanced by polarisation transfer; ISI = Institute for Scientific Information; JACS = Journal of the American Chemical Society; JMR = Journal of Magnetic Resonance; *NC* = Number of citations; NOE = Nuclear Overhauser effect; NSF = [US] National Science Foundation; *NTCI = Nouvelles technologies de la communication et de l'information; OST = Observatoire des Sciences et des Techniques; PCRD = programme-cadre de recherche et de développement [de l'Union Européenne];* PNAS = Proceedings of the National Academy of Sciences [USA]; PRISM = Partnership for Research Integrity in Science & Medicine; *SCI* = Science Citation Index; *SE* = Streicheleinheit; SHS = *Sciences humaines et sociales*; SSCI = Social Science Citation Index; *TF* = Teaching Factor; WOS = Web of Science.

ANNEX 1

'The *impact factor*, often abbreviated *IF*, is a measure of the citations to science and social science (sic) journals. It is frequently used as a proxy for the relative importance of a journal within its field. The impact factor was devised by Eugene Garfield, the founder of the Institute for Scientific Information (ISI), now part of Thomson, a large worldwide US-based publisher. Impact factors are calculated each year by Thomson Scientific for those journals which it indexes, and the factors and indices are published in the *Journal Citation Reports*. The publication of each year covered occurs in the summer of the following year. The impact factor of a journal is calculated based on a rolling two-year period. It can be viewed as the average number of citations in a year given to those papers in a journal that were published during the two preceding years. For example, the 2003 impact factor of a journal would be calculated as follows: if A = the number of times articles published in 2001 and 2002 were cited (by indexed journals) during 2003, and B = the total number of 'citable items' published in 2001 and 2002. ('Citable items' are usually articles, reviews, proceedings, or notes; not editorials or Letters-to-the-Editor), then the 2003 impact

factor is determined to be *IF = A/B*. (Note that 2003 Impact factors are actually published in 2004; it cannot be calculated until all of the 2003 publications had been received by the indexing agency.) A convenient way of thinking about it is that a journal that is cited once per year, on average, for each article published has an IF of 1 in the expression above. There are some nuances to this: ISI excludes certain article types (so-called 'front-matter' such as news items, correspondence, and errata) from the denominator. Thomson Scientific does not have a fixed rule for which types of articles are considered 'citable' and which front-matter.' (source: Wikipedia, the free encyclopedia.)

ANNEX 2

'The *h*-index is an index that attempts to measure both the scientific productivity and the apparent scientific impact of a scientist. The index is based on the set of the scientist's most cited papers and the number of citations that they have received in other people's publications. The index can also be applied to the productivity and impact of a group of scientists, such as a department or university or country. The index was suggested by Jorge E. Hirsch, a physicist at UCSD, as a tool for determining theoretical physicists' relative quality and is sometimes called the *Hirsch index* or *Hirsch number*. Hirsch suggested that, for physicists, a value for *h* of about 10–12 might be a useful guideline for tenure decisions at major research universities. A value of about 18 could mean a full professorship, 15–20 could mean a fellowship in the American Physical Society, and 45 or higher could mean membership in the United States National Academy of Sciences (*sic*). The *h*-index was intended to address the main disadvantages of other bibliometric indicators, such as total number of papers or total number of citations. Total number of papers does not account for the quality of scientific publications, while total number of citations can be disproportionately affected by participation in a single publication of major influence. The *h*-index is intended to measure simultaneously the quality and sustainability of scientific output, as well as, to some extent, the diversity of scientific research. The *h*-index is much less affected by methodological papers proposing successful new techniques, methods or approximations, which can be extremely highly cited. For example, one of the most cited condensed matter theorists, John P. Perdew, has been very successful in devising new approximations within the widely used density functional theory. He has published three papers cited more than 5000 times and two cited more than 4000 times. Several thousand papers utilizing the density functional theory are published every year, most of them citing at least one paper of J.P. Perdew. His total citation count is close to 39 000, while his *h*-index is large, 51, but not unique. In contrast, the condensed-matter theorist with the highest *h*-index (94), Marvin L. Cohen, has a lower citation count of 35 000. One can argue that in this case the *h*-index reflects the broader impact of Cohen's papers in solid-state physics due to his larger number of highly-cited papers.' (source: Wikipedia, the free encyclopedia.)

Received: November 14, 2009

---

[1] See abbreviations at the end of the article.

[2] The initials are given in the section 'Acknowledgements and Initials'.

[3] *Ce n'est pas le moindre mérite de l'association 'Sauvons la recherche' que d'avoir fourni un condensé du point de vue du président de la République française sur les chercheurs. Ceux-ci refuseraient d'être évalués, se contenteraient d'un confort douillet («Il y a de la lumière, c'est chauffé») et seraient largement improductifs, sauf peut-être les mathématiciens et les physiciens qui sont «l'arbre qui cache la forêt.» voir («Réponse à la provocation présidentielle du 22 janvier **2009**»; voir http://www.sauvonslarecherche.fr/spip.php?article2373.*

[4] *On notera tout le mépris que ce mot exprime: on «met sous tutelle» un vieillard gâteux, un dément et un laboratoire de recherche…*

[5] A. W. Overhauser, 'Encyclopedia of Magnetic Resonance', Eds. D. M. Grant, R. K. Harris, vol. I, 513–516, **1996**.

[6] A. W. Overhauser, 'Polarization of nuclei in metals', *Phys. Rev.* **1953**, *92*, 411.

[7] T. R. Carver, C. P. Slichter, 'Polarization of nuclear spins in metals', *Phys. Rev.* **1953**, *92*, 212.

[8] All numbers of citation (abbreviated 'NC' in this paper) refer to the interval between the date of publication and August 2009.

[9] I. Solomon, 'Relaxation processes in a system of 2 spins', *Phys. Rev.* **1955**, *99*, 559.

[10] D. Neuhaus, M. P. Williamson, 'The Nuclear Overhauser Effect in Structural and Conformational Analysis', 2nd ed., Wiley, New York, **2000**.

[11] G. Bodenhausen, D. J. Ruben, 'Natural abundance nitrogen-15 NMR by enhanced heteronuclear spectroscopy', *Chem. Phys. Lett.* **1980**, *69*, 185.

[12] A. A. Maudsley, R. R. Ernst, 'Indirect detection of magnetic resonance by heteronuclear 2-dimensional spectroscopy', *Chem. Phys. Lett.* **1977**, *50*, 368.

[13] G. A. Morris, R. Freeman, 'Enhancement of nuclear magnetic resonance signals by polarization transfer', *J. Am. Chem. Soc.* **1979**, *101*, 760.

[14] L. Müller, 'Sensitivity enhanced detection of weak nuclei using heteronuclear multiple quantum coherence', *J. Am. Chem. Soc.* **1979**, *101*, 4481.

[15] U. Piantini, O. W. Sorensen, R. R. Ernst, 'Multiple quantum filters for elucidating NMR coupling networks', *J. Am. Chem. Soc.* **1982**, *104*, 6800.

[16] A. J. Shaka, R. Freeman, 'Simplification of NMR spectra by filtration through multiple quantum coherence', *J. Magn. Reson.* **1983**, *51*, 169.

[17] M. Rance, O. W. Sorensen, G. Bodenhausen, G. Wagner, R. R. Ernst, K. Wüthrich, 'Improved spectral resolution in COSY ^{1}H NMR spectra of proteins via double quantum filtering', *Biochem. Biophys. Res. Commun.* **1983**, *117*, 479.

[18] J. Jeener, B. H. Meier, P. Bachmann, R. R. Ernst, 'Investigation of exchange processes by 2-dimensional NMR spectroscopy', *J. Chem. Phys.* **1979**, *71*, 4546.

[19] A. Kumar, R. R. Ernst, K. Wüthrich, 'A two-dimensional nuclear Overhauser enhancement (2D NOE) experiment for the elucidation of complete proton-proton cross-relaxation networks in biological macromolecules', *Biochem. Biophys. Res. Commun.* **1980**, *95*, 1.

[20] A. Kumar, G. Wagner, R. R. Ernst, K. Wüthrich, 'Buildup rates of the nuclear Overhauser effect measured by two-dimensional proton magnetic resonance spectroscopy – implications for studies of protein conformation', *J. Am. Chem. Soc.* **1981**, *103*, 3654.

[21] A. Kumar, 'Two-dimensional nuclear Overhauser effect experiment in a protein: the first NOESY (1979-89)', *Magn. Reson. Chem.* **2003**, *41*, 526.

[22] M. F. Bishop, A. W. Overhauser, 'Theory of electron-phason scattering and the low-temperature resistivity of potassium', *Phys. Rev. B* **1981**, *23*, 3638.

[23] I. Burghardt, R. Konrat, G. Bodenhausen, 'Measurement of cross-correlation of fluctuations of dipolar couplings and anisotropic chemical shifts by selective spin-locking', *Mol. Phys.* **1992**, *75*, 467.

[24] L. Emsley, G. Bodenhausen, 'Phase shifts induced by transient Bloch-Siegert effects in NMR', *Chem. Phys. Lett.* **1990**, *168*, 297.

[25] G. Bodenhausen, G. Wagner, M. Rance, O. W. Sorensen, K. Wüthrich, R. R. Ernst, 'Longitudinal two-spin order in 2D exchange spectroscopy (NOESY)', *J. Magn. Reson.* **1984**, *59*, 542.

[26] L. Emsley, I. Burghardt, G. Bodenhausen, 'Doubly selective inversion in NMR and multiple quantum effects in coupled spin systems', *J. Magn. Reson.* **1990**, *90*, 214, corrigendum: *ibid.* **1991**, *94*, 448.

[27] *L'utilisation de la bibliométrie dans les sciences sociales et les humanités* ['The Use of Bibliometrics in the Social Sciences and the Humanities'], Science-Metrix, *Rapport final préparé pour le Conseil de recherches en sciences humaines du Canada (CRSH)* [Final report prepared for the Canadian Research Council in Human Sciences (CRSH)]: *http://www.science-metrix.com*. E. Archambault, E. Gagne (dir.), **2004**.

[28] D. Hicks, 'The difficulty of achieving full coverage of international social science literature and the bibliometric consequences', *Scientometrics* **1999**, *44*, 193.

[29] *http://www.science-metrix.com/pdf/Science-Metrix_Utilisation_bibliometrie_SSH.pdf.*

[30] Acta Sociologica pa den internationale aréna, Dansk Sociologi, **1996**, *7*, 72; H. Andersen, 'Influence and reputation in the social sciences - how much do researchers agree?', *Journal of Documentation* **2000**, *56*, 674.

[31] *http://www.science-metrix.com/pdf/Science-Metrix_Utilisation_bibliometrie_SSH.pdf*

[32] *Les citations de ce chapitre proviennent de 'Révolution numérique et évaluation bibliométrique dans les sciences humaines et sociales', Ch. Kosmopoulos et D. Pumain, Revue européenne des sciences sociales, (Cahiers Vilfredo Pareto), 2008, XLVI, n°141, 73. Pour la version en anglais voir « Citation, Citation, Citation: Bibliometrics, the web and the Social Sciences and Humanities », Cybergeo, Science et Toile, article 411, mis en ligne le 17 décembre 2007, modifié le 01 juillet 2009. URL: http://www.cybergeo.eu/index15463.html*.

[33] One can easily guess which publisher sent this material as unsolicited advertisement by electronic mail.

[34] *Agence pour l'Evaluation de la Recherche Scientifique (AERES), in 'Critères d'identification des chercheurs et enseignants-chercheurs «publiants»' (octobre 2008), chapitre 'Quelques remarques permettant de tenir compte du domaine et du contexte', p. 3: "Dans le secteur des Sciences de la Vie, de la Santé et de l'Environnement, l'AERES est sensible à la notion de prise de risque dans la recherche, et analysera chaque cas ne répondant pas aux critères. Ainsi, alors que trois publications dans des journaux d'impact moyen suffisent pour une activité standard [sic], une publication dans Nature, Cell ou Science [en quatre ans] va suffire à cataloguer le chercher ou enseignant-chercheur comme publiant. De même, la place en premier auteur dans un article d'audience importante (J. Biol. Chem., Nature, Science, EMBO J., …) peut suffire à classer le chercheur ou l'enseignant-chercheur comme publiant."*

[35] T. Segawa, F. Kateb, L. Duma, P. Pelupessy, G. Bodenhausen, 'Exchange rate constants of invisible protons in proteins determined by NMR spectroscopy', *ChemBioChem* **2008**, *9*, 537.
[36] P. Pelupessy, E. Rennella, G. Bodenhausen, 'High resolution NMR in magnetic fields with unknown spatiotemporal variations', *Science* **2009**, *324*, 1693.
[37] A. Kumar, D. Welti, R. R. Ernst, 'NMR Fourier zeugmatography', *J. Magn. Reson.* **1975**, *18*, 69.
[38] G. Bodenhausen, H. Kogler, R. R. Ernst, 'Selection of coherence transfer pathways in NMR pulse experiments', *J. Magn. Reson.* **1984**, *58*, 370.
[39] R. R. Ernst, G. Bodenhausen, A. Wokaun, 'Principles of Nuclear Magnetic Resonance in One and Two Dimensions', Oxford University Press, Oxford, **1987**.
[40] Y. Gingras, *'La fièvre de l'évaluation de la recherche. Du mauvais usage de faux indicateurs'*, Centre interuniversitaire sur la science et la technologie, Université de Montréal, **2008**.
[41] Propos recueillis par Pierre Le Hir, *Le Monde*, jeudi 25 octobre **2007**.
[42] E. L. Hahn, 'Spin echoes', *Phys. Rev.* **1950**, *80*, 580.
[43] S. R. Hartmann, E. L. Hahn, 'Nuclear double resonance in rotating frame', *Phys. Rev.* **1962**, *128*, 2042.
[44] S. R. Hartmann, E. L. Hahn, 'Self-induced transparency', *Phys. Rev.* **1969**, *183*, 457.
[45] G. Magaritondo*, in* Polyrama, EPFL, No 122, juin, **2005**.
[46] Y. C. Zarka, '*Cités. L'idéologie de l'évaluation. La grande imposture'*, Presses Universitaires de France, Paris, **2009**.
[47] *C'est avec une insoutenable légèreté que Valérie Pécresse, actuelle ministre de la recherche en France, lors d'une interview accordé récemment à Paris Match, exprime son ambition: "Laisser une trace. Une loi sur l'université, il y en a une tous les vingt-cinq ans. Pourquoi voulez-vous que je fasse de la politique si ce n'est pas pour me lancer dans de grands défis? Pourquoi voulez-vous que je sacrifie une partie de ma vie de famille, mes week-ends, mes loisirs, pour de petits désirs, de petites réformes, de petits combats?" (Paris Match, mars **2009**). Maurice Grimaud (1913-2009), ancien préfet de police de Paris, avait plus le sens de l'humour: "Pour devenir ce que, dans les nécrologies, on appelle un grand commis de l'Etat, il ne suffit pas d'être bien élevé, encore faut-il être incompétent" (Le Monde, 23 juillet **2009**).*
[48] Though one runs the risk that one is no longer invited to serve on any committees.