



Sisyphus desperately seeking publisher

ANTOINETTE MOLINIÉ¹ and GEOFFREY BODENHAUSEN^{2,3*} 

¹Université de Paris-Ouest Nanterre La Défense, Maison de l'Archéologie et de l'Ethnologie (MAE), 21 Allée de l'Université, 92023 Nanterre Cedex, France

²Département de Chimie, Ecole Normale Supérieure, PSL Research University, UPMC Univ Paris 06, CNRS, Laboratoire des Biomolécules (LBM), 24 rue Lhomond, 75005 Paris, France

³Sorbonne Universités, UPMC Univ Paris 06, Ecole Normale Supérieure, CNRS, Laboratoire des Biomolécules (LBM), Paris, France

*Corresponding author (Email, Geoffrey.Bodenhausen@ens.fr)

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As a punishment for his trickery, King Sisyphus was made to endlessly roll a huge boulder up a steep hill. The maddening nature of the punishment was reserved for King Sisyphus due to his hubristic belief that his cleverness surpassed that of Zeus himself. Today's scientists also pay a heavy price for their hubris and narcissism. They try to trick the editors of a few 'top' journals by peppering their papers with glitter and 'bling-bling', making overblown promises, and giving minimal credit to their predecessors. The editors wield their Olympian authority by making today's scientists endlessly push their weighty boulders up steep hills. By bowing to this implacable ritual, we scientists confer undue power to a handful of popular but irresponsible journals.

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1. The ordeal of publishing

Much has been written about pernicious effects of bibliometrics, impact factors, and *h*-indices (Molinié and Bodenhausen 2009; Ernst 2009). Science used to be driven by curiosity, and sometimes by a noble desire to be useful to future generations. Today, science appears to be driven by a hubristic craving to earn recognition and become famous. Fame, so it appears, comes from publishing in famous journals. Journals derive their status from their impact factors. Although these numbers are largely devoid of any statistical significance, they remain essentially unchallenged. Journals and magazines such as *Nature*, *Cell*, *Science*, *Journal of the American Chemical Society* and *Angewandte Chemie* exert an inordinate power, for they decide what is worthy of publication. These journals and their editorial staff are empowered by two converging factors: (1) the editors' wish to defend a dominant position, and (2) the authors' unbridled thirst for celebrity. While the system unquestionably benefits the major publishing companies and their profits, it damages the standards of research by favouring spectacular work purported to be 'of broad and general interest' that may not fulfil the highest intellectual standards.

In the first 30 years of our activity, roughly from 1975 to 2005, we occasionally experienced the pain of seeing some of our papers rejected, but at least in retrospect, these early negative decisions seem to be fully justified. However, in the last 10 years or so, we have been faced with a new phenomenon: papers are now frequently rejected, not because of some critical comments of referees, but rather by arbitrary decisions of editors whose 'front offices' do not even bother to consult any referees. The editors seem to be guided by their judgment about fashion in science, and favour subjects that are expected to become 'hot'. They anticipate that the papers they select will be frequently cited in the next few years, as if they were buying shares on the stock market. The editors generally do not seem to be aware of their heavy responsibility as trendsetters. Since many colleagues and friends must have suffered similar experiences, we should like to give a few excerpts of some of our worst misadventures, in the manner of a 'best of' collection.

We do not question the value of anonymous peer review: many hard-working – and unpaid – referees often continue to offer remarkably detailed and competent opinions. Many of our papers would have been burdened by errors and infelicities, had they not been extensively rewritten in

response to critical comments of selfless anonymous referees.

If we did not suffer too often from rejections in our early years, this was probably because we recognized the limitations of our work. We used to submit many of our papers to Wallace Brey, editor of the low-profile no-nonsense *Journal of Magnetic Resonance*. We used to write, ‘Dear Wallace, please consider the enclosed manuscript....’ This would almost invariably trigger a kind reply, ‘Dear Geoffrey, I’m delighted....’, accepting our work, often without any peer review. In those days, the fact that an editor made such a judgment all by himself was not only deemed acceptable but highly desirable. In the days of postal submissions, airmail between Europe and USA would typically require about 10 to 15 days each way, so consultations were mercifully skipped when the editor believed them to be superfluous. Academic Press has since been bought up by Elsevier, and the benevolent Wallace Brey has long been replaced. Even ‘technical’ low-profile journals like the *Journal of Magnetic Resonance* are now being run according to tough business rules. That is hardly surprising, since publishing has become a highly lucrative business. Many editors – even the fine scientists among them – blindly identify with the business objectives of their publishers. Is this a new variant of the Stockholm syndrome?

In the last decade, roughly since 2005, it has become increasingly difficult to publish our papers. Granting agencies, ministries, deans, directors and presidents obviously want us to publish in high-impact journals. For a senior investigator, it is difficult to ignore such pressures ‘from above’. Furthermore, our students and post-doctoral fellows are pushing ‘from below’ to submit their work to ‘top’ journals. Understandably, they wish to be recognized for their work, knowing full well that, whether we like it or not, faculty appointments are often guided by impact factors. Today, the members of appointment committees rarely bother to read the candidates’ papers. It is so much more convenient to look up their impact factors! Any secretary can take decisions on appointments. In the hope of attracting attention, our students and post-doctoral fellows often volunteer to go through the harrowing experience of multiple submissions. Some of our papers have been submitted in vain to *Nature*, *Nature Chemistry*, *Nature Methods*, *Science*, *Proceedings of the National Academy of Sciences*, *Journal of the American Chemical Society*, *Angewandte Chemie*, etc. In some cases, our modern descendants of Sisyphus (figure 1) pushed their boulder uphill no less than eight times! A few selected examples are cited in the [Appendix](#).

2. ‘Top’ journals and magazines

It is tempting to blame the editors of ‘top’ journals like *Nature*, *Science*, etc., their plethora editorial teams, their ‘artificial intelligence’ programs, and the greed of profit-hungry publishing companies like Elsevier, Springer and

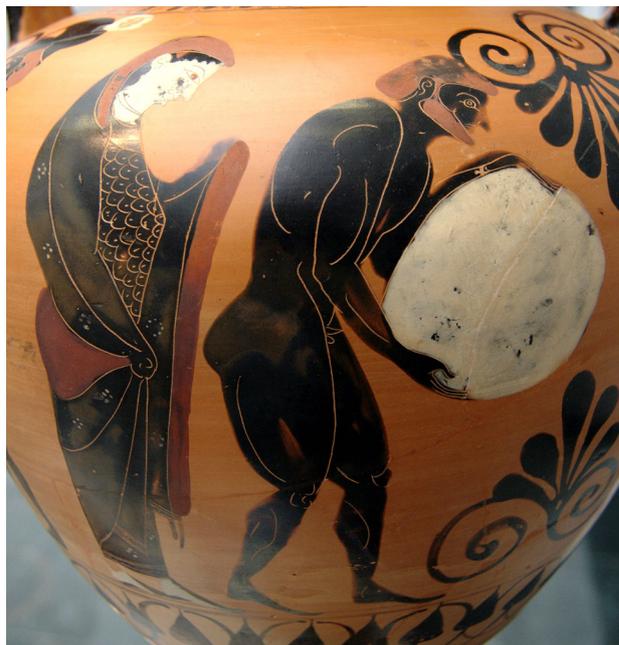


Figure 1. Persephone supervising Sisyphus in the Underworld,¹ Attica black-figure amphora, ca. 530 BC, Staatliche Antikensammlungen (inv. 1494).

Wiley. Some ‘learned societies’, like the American Chemical Society and the Royal Society of Chemistry, tend to be even more aggressive. Why do a handful of journals wield so much power? In our opinion, it is essentially *our* responsibility, i.e. the fault lies with the authors who have conferred this power to the editors, in part because of our unbridled narcissism, in part because we deferentially try to fulfil the wishes of our granting agencies, ministries, deans, directors, and presidents, and in part because we do not want to disappoint our younger co-workers.

In our opinion, the exorbitant power of publishers flows naturally from the tendency of modern capitalism to transform all commodities into merchandise. The results of scientific research, papers and patents are treated like any other merchandise. As a result, the richest investors accumulate fantastic wealth,² far more than they will ever be able to

¹ P Grimal, *Dictionnaire de la mythologie grecque et romaine*, SPADEC, 1969. Wikipedia offers a short version of the wonderful story of King Sisyphus, which belongs to Greece’s most precious contributions to humanity’s heritage (<https://en.wikipedia.org/wiki/Sisyphus>). To the best of our knowledge, India’s mythology does not offer an exact equivalent, although the myth of Trishanku shares a few common features (<https://en.wikipedia.org/wiki/Trishanku>).

² 99%, Pierre Laurent, *Le Cherche Midi*, Paris, 2016. Not surprisingly, the precise numbers depend on the sources. Although the bank Crédit Suisse can hardly be suspected of left-wing sympathies, their analysts estimate that the richest 8.4 % own 83.3 % (see *Global Wealth Databook* 2013.) See also http://www.lemonde.fr/economie/article/2015/01/19/les-1-les-plus-riches-possederont-bientot-la-moitie-de-la-richesse-mondiale_4558585_3234.html#AI33lYwHsKYBylJ.99.

invest wisely. They turn to bankers for advice, but of course the bankers have no clue either, and they turn to specialized agents, who have nothing better to offer than their infamous ‘ratings’. Hence investment is funnelled into activities that are highly rated, regardless of other criteria, such as if further investment in established activities really makes sense, or if investment in other areas might open more promising perspectives. The whole process is accompanied by a pervasive sense of mediocrity and a frightening drop in intellectual standards.

Whether ‘ratings’ concern equities on the stock market, raw materials, soccer stars or post-doctoral fellows, *c’est tout du même tonneau*: all ratings flow from the same insane logic. That the price of oil barrels and soccer stars depends on their scarcity may be a good thing. But should the ‘market value’ of a post-doc, and his or her chances of landing a decent job, be determined by similar ratings? Should faculty positions be given to post-doctoral fellows because they have published in ‘top’ journals?

Our first attempt to publish in *Science* was inspired by the frantic ambition of the National High Magnetic Field Laboratory (NHMFL) (now known as MagLab) in Tallahassee to gain recognition as one of the world’s top laboratories. We made a special effort to write a story about Xenon-131 NMR in gas phase, a story that every layman could understand. The paper was promptly rejected by *Science*, but ended up, much to our surprise, in *Physical Review Letters*. We foolishly neglected to rewrite the paper in a more scholarly style, as would have been appropriate for such a respectable journal (Meersmann *et al.* 1998). Many years later, another attempt to publish in *Science* was likewise inspired by our petty desire to please our authorities by obtaining some recognition. Its catchy title, ‘Unknown Spatiotemporal Variations...’, does not really describe our idea, but the editor took so much delight in his own invention that he virtually pushed it down our throats (Pelupessy *et al.* 2009). To be truthful, this is not the best of our work, but it was instructive to see the awe and respect our paper enthused in our granting agencies, ministries, deans, directors and presidents.

Trying to please the editors of ‘top’ journals is fraught with dangers. The more the glitter and ‘bling-bling’, the better. It is tempting to add some overblown promises and, at the expense of gentlemanly behaviour, give minimal credit to our predecessors. In one recent paper, we teamed up with a few younger investigators who possess remarkable marketing talents. We were promptly rewarded by the editors of *Proceedings of the National Academy of Sciences of the USA* (Gajan *et al.* 2014). When we tried a few months later to submit a sound but humble paper to the same journal despite our limited marketing skills, we promptly failed.

The mere attempt to please the editors of bling-bling magazines can have severe side effects. In particular, senior investigators should be concerned with the *impact on the*

morale of their PhD students, post-doctoral fellows and permanent staff. To push a boulder up the hill again and again, only to be confronted with a cascade of rejections, can be a disheartening experience for all but the most resolute. Is this ‘collateral damage’ acceptable?

3. Justified rejections

We have at times grossly overestimated the importance of our work. Thus, when Phil Bolton and I were both post-doctoral fellows at University of California San Diego, we submitted a highly specialized paper on phosphorus-31 NMR of cyclic nucleotides to the *Journal of the American Chemical Society*. The referees promptly put us right. The paper ended up in *Journal of Magnetic Resonance*, which is a technical no-nonsense journal that is quite appropriate for a technical no-nonsense paper (Bodenhausen and Bolton 1980). This kind of experience helps to shape one’s judgment. I like to share my correspondence with various editors with my students at the Ecole Normale Supérieure, to show them how they can try to plead their case.

4. Surprising rejections

Our paper on the well-known ‘product operator formalism’, which has become one of the pillars of modern NMR, was rejected by *Journal of Chemical Physics* with a scathing recommendation to rewrite it ‘in frankly pedagogical vein’. The ill-fated paper was finally published in *Progress in NMR Spectroscopy* (Sorensen *et al.* 1983) and has been cited over 1000 times since its publication in 1983. Presumably the reviewers of *Journal of Chemical Physics* failed to perceive that our paper would turn out to be ‘of broad interest’.

5. Surprising acceptances

Buoying on the popularity of our paper on product operators (Sorensen *et al.* 1983), we wrote a rather weird paper that attempts to achieve the same thing using only graphical tools, without any mathematics or trigonometry. It is not clear why the editor of *Angewandte Chemie* was willing to publish this bizarre work (Eggenberger and Bodenhausen 1990). Many years later, the same journal decided to publish an even weaker paper that basically confirms that spinning about the magic angle is better than spinning about other angles. The editors may have been deceived by the irresistible combination of the words ‘truly’ and ‘magic’ in our crafty title (Antonijevic and Bodenhausen 2005). Another rather odd paper was initially rejected by *Physical Review*, but accepted to our surprise by *Journal of Chemical Physics* (Ermakov and Bodenhausen 1995). Admittedly, to

paraphrase Erwin Hahn, it might have been possible to make the Messiah come back with our pretentious title, ‘Controlled Violation of Adiabaticity and Antiparallel Doubly Resonant Irradiation’. Difficult to offer a better concentrate of bling-bling!

6. Epic struggles

Shortly before completing his PhD in 1991, Lyndon Emsley struggled with a clinical MRI machine, for the radiologists at the hospital in Lausanne had only granted him limited access in the middle of the night, while Siemens refused to provide support to modify the pulse programs. After considerable efforts, a paper was submitted to *Magnetic Resonance in Medicine* (Emsley and Bodenhausen 1989). Doddrell *et al.* challenged our views by publishing a ‘Note’, to which we responded by publishing a ‘Comment on the note...’ (Emsley and Bodenhausen 1991). For a moment, we thought that we were going to crash and burn.... Actually, this was to be our only experience with a frank and honest public debate. Neither the editor of *Magnetic Resonance in Medicine* nor the referees interfered in any way. A pity that such debates are no longer fashionable, except perhaps in blogs and similar media.

More recently, we developed a trick to screen putative drug molecules by assessing their affinity for a target protein. The idea relies on the contrast between free and bound ligands. Such a contrast may occur for parameters like chemical shifts, diffusion coefficients, relaxation times, etc. We discovered that the contrast could be dramatically improved if one observes the lifetimes of long-lived states (LLS) associated with ligands. Although this idea was both original and useful (a rare combination!), our paper went through a cascade of unsuccessful submissions to *Nature Methods*, *Proceedings of the National Academy of Sciences of the USA* and *Nature Chemistry*. After pushing the boulder up the hill four times, it was finally published in *Journal of the American Chemical Society* (Salvi *et al.* 2012). The thankless role of Sisyphus was played by a young ‘corresponding author’ who had recently obtained his PhD degree. This is a surely traumatic way to start one’s career as a scientist.

Fast proton exchange is a fascinating challenge that has attracted attention of a few great minds like S Meiboom, Al Redfield and Richard Ernst. In our lab in Paris, Philippe Pelupessy invented a novel approach that is described in some detail in *Journal of Magnetic Resonance* (Kateb *et al.* 2007). Initially, we believed that it was of sufficiently broad interest to justify publication in *Journal of the American Chemical Society*. However, the editor-in-chief rejected our paper 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* [our italics]. (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 abroad general interest journal like Science, Nature, PNAS, JACS that is recent.’

In other words: if you want to publish in *Journal of the American Chemical Society*, make sure to insert lots of references to *Science*, *Nature*, *PNAS*, etc. Whether those references are actually meaningful is immaterial. It is true that no significant progress had been made in the area of fast proton exchange for about 30 years. It was precisely because we made a significant ‘conceptual advance’ that we have been able to accomplish a leap by several orders of magnitude towards faster rate constants.

Rightly or wrongly, we felt that our work on drug screening using a combination of long-lived states and hyperpolarization represented another breakthrough. We sobered up when we encountered seemingly insurmountable resistance, our paper being flatly turned down by *Nature Chemical Biology*, *Nature Methods*, *Journal of the American Chemical Society*, *ChemBioChem* (all without consulting any referees), than by *PNAS* (who consulted two referees), before being finally accepted by *ChemMedChem* (Buratto *et al.* 2014). The boulder had to be pushed up the hill a mere six times. A trifle! We have reproduced some extracts of the irksome correspondence relating to this ill-fated paper in the [Appendix](#). Again, this is hardly a desirable way to start one’s career as corresponding author.

7. A glimmer of hope

Our discussion is not intended to discourage younger generations of graduate students and post-doctoral fellows who are rightly concerned about their professional perspectives. Many respectable institutions are beginning to realize that the impact factors of journals in which the applicants’ papers have been published do not provide a reliable measure of their intellectual and organizational abilities, not to mention their talents as teachers and team leaders. Appointment committees are focusing less and less on these whimsical metrics, in favour of a more careful evaluation conducted ‘as of old’ by asking a panel to interview the candidates in depth, by attending their lectures, and in favourable cases by actually reading the abstracts and conclusions of some of their papers.

It is not yet possible to figure out if a paper that has been downloaded has actually been read, scrolled and annotated. (Some start-up specializing on massive surveillance will no doubt soon resolve this deficiency.) Thus, if we are

compelled by institutional policies to assess the impact of our papers, the only metrics that we can offer is the number of citations and the number of downloads. In our case, our most cited papers – bearing in mind that we distrust this metric – were printed in not-so-well-known journals such as *Biochemical and Biophysical Research Communications* (impact factor IF = 2.466 in 2016; our ‘most popular paper’ (MPP) was cited 2591 times by August 2017), the *Journal of Magnetic Resonance* (IF = 2.432; MPP = 1257), and *Chemical Physics Letters* (IF = 1.815; MPP = 2180). Perhaps more to the point, our only paper published in *Science* (IF = 37.205) was barely cited (MPP = 53), as were those in *Physical Review Letters* (IF = 8.462; MPP = 34). This shows that our most cited papers were *not* published in so-called ‘top’ journals. Could it be that their editors failed to recognize ‘papers of broad interest’ that they cherish so much? Clearly, one can enjoy a rewarding career and build a reasonable reputation without paying much attention to impact factors.

8. Conclusions

The rush to a handful of ‘top’ magazines and their promise of celebrity leads to tremendous collateral damages. Several alternatives exist, such as *Public Library of Science* (PLOS), which includes *PLOS Biology*, *PLOS Medicine*, *PLOS Computational Biology*, *PLOS Genetics*, *PLOS Pathogens and PLOS Clinical Trials*, and the fully electronic *PLoS ONE*, as well as various other ‘open access’ journals where the authors (rather than public libraries) must pay to get published. (Some of these journals were initiated at École Polytechnique Fédérale de Lausanne and at the University of Zurich; see www.sciencematters.io). There are also state-sponsored repositories like arXiv.org in the US, or the French Centre pour la Communication Scientifique Directe (CCSD) that runs an open archive called Hyper Articles en Ligne (HAL), where authors can deposit scholarly documents from all academic fields, albeit without undergoing a salutary peer-review process. Despite all these efforts, the ‘top’ journals continue to wield their seemingly unlimited power, supported not so much by their objective strength (Verma 2015),³ but rather by our narcissism, our need to please granting agencies, ministries, deans, directors and presidents, and of course our wish to support our students and post-doctoral fellows. In our opinion, the best way to resist these pressures is to revert to publishing in technically sound low-profile journals, and to argue forcefully against the infectious poison of bibliometrics in editorial boards and committees that confer prizes and academic appointments.

³ The editor-in-chief of PNAS reminds us that only 0.5% of 38 million items have been cited more than 200 times from 1900 to 2005.

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Appendix: Our struggle to publish a paper on drug screening

Our paper on ‘Drug Screening Boosted by Hyperpolarized Long-Lived States in NMR’ (Buratto *et al.* 2014) was published after pushing the boulder up the hill no less than six times. We reproduce a few excerpts that give some insight into the policies of the journals, while deleting comments that specifically refer to the merits of our paper (or lack thereof):

- (1) *Nature Chemical Biology* (Submitted on 23-01-2014, rejected after 4 days on 27-01-2014).

‘... *Nature Chemical Biology* is not a ‘methods’ journal. As such, we particularly seek papers in this field to meaningfully complement or outpace existing methods, ideally by showing their application to gain new *mechanistic* insights into a system of interest. ...’
[our italics]

- (2) *Nature Methods* (Submitted on 30-01-2014, rejected after 8 days on 07-02-2014).

‘... It is *Nature Methods*’ policy to decline a substantial proportion of manuscripts without sending them to referees, so that they may be sent elsewhere without delay. Decisions of this kind are made by the editorial staff when it appears that papers are unlikely to succeed in the competition for limited space. - Among the considerations that arise at this stage are a manuscript’s probable interest, level of methodological development and immediate practical relevance to a *general readership*. ...’

- (3) *Proceedings of the National Academy of Sciences of the USA* (Submitted on 13-03-2014, rejected after review on 03-04-2014)

‘... We receive more than 16,000 submissions each year and currently accept less than 20%. [...] After considering the reviews and re-reading the manuscript,

the editor concurs with the negative reviewers that the paper is *better suited for a more specialized journal*. ...'

- (4) *Journal of the American Chemical Society* (Submitted on 14-04-2014, rejected after 8 days on 22-04-2014)

'... We anticipate that, absent a more compelling biological application that clearly demonstrates the advance of the reported methodology (e.g., versus previous approaches [...]), the submission will have difficulty receiving support from Reviewers for consideration at JACS. [...] I would potentially be willing to consider resubmission of a manuscript to JACS that applies the method to *several proteins* or demonstrates actual *HTS* [high throughput screening] implementation for *novel ligand discovery*. ...'

- (5) *ChemBioChem* (Submitted on 20-05-2014, rejected the next day on 21-05-2014)

'... Our policy is to publish contributions of current general interest or of great significance to a *broad readership* in the field of chemical biology. ...'

- (6) *ChemMedChem* (Submitted on 23-05-2014, accepted after 35 days on 27-06-2014)

'... We are pleased to inform you that the above-mentioned manuscript has been accepted for publication in *ChemMedChem*. ...'

References

- Antonijevic S and Bodenhausen G 2005 High-resolution NMR in solids by truly magic-angle spinning. *Angew. Chem. Int. Ed.* **44** 2935–2938
- Bodenhausen G and Bolton PH 1980 Elimination of flip-angle effects in two-dimensional NMR spectroscopy. Application to cyclic nucleotides. *J. Magn. Reson.* **39** 399–412
- Buratto R, Bornet A, Milani J, Mammoli D, Vuichoud B, Salvi N, Singh M, Laguerre A, Passemard S, Gerber-Lemaire S, Jannin S and Bodenhausen G 2014 Drug screening boosted by hyperpolarized long-lived states in NMR. *ChemMedChem* **9** 2509–2514
- Eggenberger U and Bodenhausen G 1990 Modern NMR pulse experiments: A graphical description of the evolution of spin systems. *Angew. Chem. Int. Ed. Engl.* **29** 374–383
- Emsley L and Bodenhausen G 1989 Self-refocusing 270° Gaussian pulses for slice selection without gradient reversal in magnetic resonance imaging. *Magn. Reson. Med.* **10** 273–281
- Emsley L and Bodenhausen G 1991 On the use of a slice-selective 270° self-refocusing Gaussian pulse for magnetic resonance imaging: Comments on the note by D. M. Doddrell et al. *Magn. Reson. Med.* **19** 461–463
- Ermakov VL and Bodenhausen G 1995 Coherence transfer in three-level systems: Controlled violation of adiabaticity and antiparallel doubly resonant irradiation. *J. Chem. Phys.* **103** 136–143
- Ernst RR 2009 The follies of citation indices and academic ranking lists – A brief commentary to 'Bibliometrics as Weapons of Mass Citation'. *Chimia* **64** 90
- Gajan D, Bornet A, Vuichoud B, Milani J, Melzi R, van Kalkeren HA, Veyre L and Thieuleux C 2014 Hybrid polarizing solids for pure hyperpolarized liquids through dissolution dynamic nuclear polarization. *Proc. Nat. Acad. Sci. USA* **111** 14693–14697
- Kateb F, Pelupessy P and Bodenhausen G 2007 Measuring fast hydrogen exchange rates by NMR spectroscopy. *J. Magn. Reson.* **184** 108–113
- Meersmann T, Smith SA and Bodenhausen G 1998 Multiple-quantum filtered xenon-131 NMR as a surface probe. *Phys. Rev. Lett.* **80** 1398–1401
- Molinié A and Bodenhausen G 2009 Bibliometrics as weapons of mass citation. *Chimia* **64** 78–89
- Pelupessy P, Rennella E and Bodenhausen G 2009 High-resolution NMR in magnetic fields with unknown spatiotemporal variations. *Science* **324** 1693–1697
- Salvi N, Buratto R, Bornet A, Ulzega S, Rentero Rebollo I, Angelini A, Heinis C and Bodenhausen G 2012 Boosting sensitivity of ligand-protein screening by NMR of long-lived states. *J. Am. Chem. Soc.* **134** 11076–11079
- Sorensen OW, Eich GW, Levitt MH, Bodenhausen G and Ernst RR 1983 Product operator formalism for the description of NMR pulse experiments. *Progr. NMR Spectrosc.* **16** 163–192
- Verma I 2015 Impact, not impact factor. *Proc. Nat. Acad. Sci. USA* **112** 7875–7876

Corresponding editor: B JAGADEESHWAR RAO