

Social Media, Peer Review, and Responsible Conduct of Research (RCR) in Chemistry: Trends, Pitfalls, and Promises

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Over the last two decades, various themes inherent in the responsible conduct of research (RCR) in chemistry have been brought to light through prominent cases of research misconduct. This article will describe a few of these cases especially through the lens of social media such as blogs and Twitter. A case will be made that these wholly novel modalities of online discussion are now complementing, and in some cases even circumventing some of the limitations of traditional peer review in chemistry. We present in detail our evaluation of three recent cases of RCR along with several other social media illustrations. These cases have been selected to be representative and showcase several of the most prominent issues at the intersection of traditional and social-media based peer review. In each case, basic details are presented along with a brief discussion of the underlying issues—readers interested in deeper analysis of each subject are referred to a collection of relevant articles and websites. This perspective focuses on the most important RCR issues that have arisen in the past decade, a time which we believe coincides with the serious participation of the scientific community in general, and the chemistry community in particular, in social media-based, citizen-enabled peer-review. A discussion of important trends in RCR in the age of social media, outstanding developments in this area, and questions of enduring interest for the near future concludes the article.

Keywords: blogs, falsification, misconduct, questionable conduct, responsible conduct of research (RCR), scientific journals, social media

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Editor's Note

Every step of scientific research is highly interactive. And the most highly interactive facet of science begins when the research results are made public, either in a publication [from the Latin word “publicare,” public], a lecture, a press release, a presentation before a government regulatory agency, or a report on the internet. With information release comes information receipt, review and analysis by others, even additional research. The scientific process churns and churns, especially for research results that have meaning and interest to others. Social media have transformed these processes, providing instantaneous exchange of information and ideas. The consequences of such speed, openness, and expanding virtual communities of science are extraordinary—on research and the practice of research. In this special issue of Accountability in Research, we are favored to publish an article by Ashutosh (Ash) Jogalekar, a Ph.D. medicinal chemist who has been an active blogger as The Curious Wavefunction (<http://wavefunction.fieldofscience.com/>) and a contributor to the blog of Scientific American. Ash's assignment, wonderfully achieved, is to discuss the role of social media as it relates to ethics and responsible conduct of research.

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INTRODUCTION

Public discussions of responsible conduct of research (RCR) have recently played out in multiple guises in the general media as well as in the world of academic chemical research. Various high profile cases dealing with scientific misconduct—for example fabrication, falsification, and plagiarism—have been highlighted in large numbers following the proliferation of online social media like blogs, Twitter, and other websites devoted to the analysis of published scientific literature. This discussion has included an analysis of both the human and the technical aspects of chemical research. Important news-making episodes have been cataloged during the last decade, but as of yet, there does not seem to be a single publication gathering many of these episodes together, classifying them, discussing their salient features, and identifying common themes between them as well as their possible unique relevance to chemistry. The purpose of this article is to provide a flavor of RCR viewed through the lens of social media developments during the last decade. While the case studies we discuss are specific to chemistry, we also make an effort to highlight how they raise questions that are far more general and relevant to the role of social media in scientific research.

Although various media forms have examined RCR during the last two decades, one of these forms is entirely novel and stands out from the rest: Internet blogs and social media. The criticism and analysis of RCR in chemistry through such online open forums was nonexistent just a few years ago. The significant rise of this new mode of communication was enabled by the explosive growth of the World Wide Web and the proliferation of inexpensive, Internet-friendly computing resources including free, ready-to-use blogging platforms (Wordpress, Blogger, etc.). As of 2014, several blogs written by graduate students, professional scientists, science writers, and journalists have come into their own as serious, vibrant, and popular platforms for discussing both technical and human issues in chemistry. As will be gleaned from the discussion below, these online forums have emerged as a striking alternative—and some may say, second—tier of literature review, although sometimes as salient or more so than peer review. In addition social-media forums have served as a marketplace of ideas regarding other facets of chemical research—for instance the history, philosophy and sociology of chemistry—that do not traditionally appear in journals and newsmagazines. As such these new forms of media are both enhancing and expanding the debate about critical issues in chemistry as well as raising new questions about the validity, usability, and archivability of this debate.

Stated upfront is the background and motivation of the author of this article: I am a chemist and drug discovery scientist with ten years of research experience in both academia and industry who has written a blog, “The Curious Wavefunction” (Jogalekar, 2014a), mainly focused on the history and philosophy of science since 2004, a year which roughly coincides with the explosive growth of chemistry-related blogs and related online venues. As such I have had a robust social media presence during this time and have observed—and often participated in—debates about the role of social media in chemistry peer review. I thus consider myself to be fairly well placed to summarize the developments delineated in this article. I also note that, while I’ve written a blog under the *nom de plume* of “The Curious Wavefunction,” I have, at the same time, always provided my full name and contact information. This is not always the case with other bloggers and certainly not the case with commenters from the global community. Two particularly noteworthy blogs of the latter kind are “Chemjobber” (Anonymous, 2014a), a site widely recognized for its longtime coverage of the chemistry job market, and “Just Like Cooking” (see Arr Oh, 2014), which focuses mainly on the chemical literature and on social issues in chemistry. Nonetheless, as can be seen from the analysis below, some of these sources have become as integral a part of the online chemistry ecosystem as those with verified identities.

The analyses of recent issues in chemical RCR in this report are not meant to be comprehensive nor judgmental of the merits of any alleged wrongdoing or irresponsible conduct; rather, the purpose of this analysis is to sample a few

recent case studies showcasing the major issues in the field and illustrating the role of blogs and other new social media in debating, highlighting, and even seeking resolutions to some of these issues. This synopsis also makes no claim to providing definitive suggestions regarding either the adjudication or future prevention of any of these issues. In fact, our principal goal is to raise appropriate questions which we believe will engage the chemical community for the foreseeable future and serve as the basis for informed discussion.

CASE STUDIES

The “Arsenic Life” Controversy: Citizen-Based Peer Review and Differing Standards of Scientific Evidence

Case summary: Because of its potential implications for the very definition of life, the field of astrobiology is one that engages both the research community’s attention and the public’s imagination. This case study deals with the announcement of the discovery of a bacterium that purportedly replaces the essential life element phosphorus with arsenic for its survival; if true the finding would be paradigm-changing. However, multiple problems with both the study and its revelation through press conference became apparent almost immediately after the announcement. The initial criticism was followed by a novel experiment on a blog seeking to duplicate the results; this experiment along with several others found fundamental flaws with the findings. The case illustrates both the perils and benefits of Internet-based, citizen-enabled peer review as well as citizen science. It also raises interesting questions about the modalities of peer review in the age of interdisciplinary research and the great potential of citizen-based analysis in favorably augmenting these modalities.

On December 2, 2010, a team of scientists funded by NASA and led by Felisa-Wolfe Simon made an announcement about a discovery with significant potential implications for our definition and understanding of life. Wolfe-Simon and her associates claimed that they had found a bacterium named GFAJ-1 in Mono Lake, California that seemed to substitute arsenic for phosphorus in the structure and workings of its major biomolecules and life processes. Since phosphorus is one of life’s essential elements (along with carbon, hydrogen, oxygen, nitrogen, and sulfur), the substitution of phosphorus by arsenic in critical biological molecules and processes would indeed lead to a new paradigm for life.

What was intriguing about this purported discovery was that it was announced at a press conference organized by NASA before the embargo on the formal paper in *Science* magazine lifted and the article appeared online (Wolfe-Simon et al., 2010). This highly public announcement made before the

official publication and broadcast live from NASA (NASA Television, 2010) not only heightened expectations about the nature and significance of the findings but also signaled the press conference as an accepted means of formally communicating science. As we will see later, the decision on the part of the authors to announce their results via press conference created significant roadblocks in their own abilities to respond properly to debate and questions about the article. It is interesting to consider, however, that the format of press releases of research, an activity that is much older than the rise of the Internet, may be considered an early form of social media. Also in this context one cannot help but contemplate the infamous cold fusion controversy, another case of distinctly flawed scientific work in which the results were announced prominently and officially by the principal participants through press conference before the formal publication appeared.

There was almost immediate, widespread criticism of the Wolfe-Simon et al. findings when their details were revealed. While journalists and writers in major, worldwide influential and traditional news sources like the *New York Times* (Overbye, 2010) and *The Wall Street Journal* (Hotz, 2010) were optimistic about the discovery, multiple articles written by a wide variety of scientific investigators, science writers, and journalists voiced major concerns. Among all these critiques, a detailed analysis written by University of British Columbia microbiologist Rosie Redfield stood out. In a single post (Redfield, 2010) written on the same day that the article and the public announcement appeared, Redfield pointed out what she thought were multiple issues with the article, including lack of proper controls, paucity of data, and a lack of confirmatory experiments, especially ones deemed to be important in describing a discovery as novel and potentially important as “arsenic life”; in voicing these concerns about inadequate confirmation, Redfield and others were citing the late astronomer and writer Carl Sagan’s admonition that “extraordinary claims require extraordinary evidence” (Sagan, 2013). Among other sources, Redfield’s criticism was followed by an article in *Slate* on December 7 by noted science writer Carl Zimmer who compiled a list of objections to the article from thirteen respected scientists through email (Zimmer, 2010): the reactions of these researchers ranged from measured skepticism to outright dismissal of the results.

By this time, while there had been major objections to the article raised by a number of prominent scientists and sources, there was no peer-reviewed publication documenting these objections, partly for the obvious reason that it was too early for a proper response. When questioned by Zimmer and others regarding the critiques, not only the authors, but also NASA declined to respond to any kind of social media, press conference, or news source critiques, noting that they would prefer to address only criticism published in the formal literature (Anonymous, 2010). However, this refusal struck many as being inconsistent with the initial announcement of the findings which had in fact been made

through news sources and press conference before the formal study appeared; *Wired* magazine called this espousal of only traditional sources of communication by the authors and the federal agency a “call to pre-Enlightenment thinking” (Dobbs, 2010). The “arsenic life” case thus again raises questions about the proper way to reply to criticism in an age when valid and even overwhelming criticism can arise from any number of nontraditional sources. Part of the problem is simply speed. Traditional modes of research dialogue with their considered reviewing and editing processes can never compete with the quick calls to action and publishing orchestrated by social-media sources, often during days or even hours. Whatever the pros and cons of criticism on these sources, such sources will always pose a compelling question to researchers publishing potentially controversial work regarding the best and fastest way to respond to criticism.

Coupled to this quick response time on blogs and online media was another new development engendered by microbiologist Rosie Redfield. Redfield decided to follow up her initial criticism with a novel open science experiment in which, instead of performing experiments in her lab which would only be published in a scientific journal, she would instead post the results of those experiments on her blog in real time (Redfield, 2012). Over the next year, Redfield carefully cataloged her protocols, controls, and results, and her new mode of doing research was widely publicized in both formal and popular sources. Her findings and opinions were posted on the blog (Redfield, 2011a,b), and comments were allowed and encouraged. In the later part of her work, Redfield also collaborated with Princeton University professor Leonid Kruglyak. On January 30, 2012, Redfield and her co-authors submitted the article documenting the experiments to *Science*. The article was also simultaneously uploaded to the open access preprint server arXiv for dissemination; arXiv is an extensive, open, and free online repository of electronic preprints, mainly in mathematical fields like theoretical physics and astronomy but recently expanded to include other disciplines like theoretical biology (Reaves et al., 2012b). It is worth noting that in recent years arXiv has emerged as a potential venue for publishing research before it navigates the peer review process of formal journals, and the quick availability of this form of publication is likely going to increase its use in the future; in fact many journals have loosened their restrictions on the Ingelfinger Rule—which prohibits publishing in the open press prior to publication in a journal—in the context of open-access sources like arXiv and figshare (Borgman, 2007).

As criticism of Wolfe-Simon et al.’s work steadily mounted, independent studies failing to validate the original claim of arsenic substituting phosphorus and forming a part of the Mono Lake bacterium’s vital biochemical machinery started appearing in the scientific literature. A particularly notable study was by Fekry et al. (Fekry et al., 2011) who performed kinetic measurements on model nucleotide phosphate diesters and their arsenate counterparts and

found that the arsenate diesters hydrolyzed faster than their phosphate counterparts by a staggering factor of 10^{17} . Other articles appearing later dealt with various aspects of the work, including critical experiments to locate arsenic that had not been done in the original study, biological growth experiments on bacteria in the presence or absence of phosphorus and arsenic and studies on the stability and affinity of arsenic-containing biomolecules for each other (Elias et al., 2012; Xu et al., 2012). Redfield's own article appeared in *Science* on July 27, 2012, almost 18 months after the initial press announcement by Wolfe-Simon et al. (Reaves et al., 2012a; Wolfe-Simon et al., 2010). The same issue of the magazine that published Redfield's article also presented a study by Erb et al. (Erb et al., 2012). The two main conclusions of both articles were that GFAJ-1 contained no detectable arsenate and that, when presented with both arsenate and phosphate, it would selectively survive on the phosphate but display resistance to the arsenate.

Aside from the questions regarding open review and experimentation on social-media and other sites discussed above, the “arsenic life” case also brings up another important and intriguing issue in the annals of peer review, one that was discussed most prominently in a recent perspective by Benner et al. (Benner et al., 2013). It has been a tacit expectation throughout the history of peer review that, when an article has been submitted to a journal, the journal editors will select reviewers who are experts in the particular fields of inquiry addressed in the article and thus well-qualified to judge the quality and validity of the material. One reason multiple reviewers are selected is precisely so that their strengths and knowledge can complement each other so that no aspect of a study goes unaddressed. This feature of peer review is especially important in an age of highly interdisciplinary research where articles sent especially to general science publications like *Science* and *Nature* can showcase the intersection of several fields of science, thus making the proper representation of experts in each of those fields as reviewers especially critical.

In their article Benner et al. (2013) ask whether this system of expert representation of reviewers might have broken down in case of the “arsenic life” article. Based on its content, the scientific fields brought to bear upon the problem in the article can be classified, at the very minimum, into chemistry, biology, and geology. Benner et al. note that of these three classes of scientists, chemists seem to have been the most skeptical about the claims in the article. This is because many chemists in the field (including the author of this article) were well aware of a classic article published in 1987 by Harvard chemist Frank Westheimer—who passed away in 2007—speculating on why nature chose phosphates rather than related chemical functional groups like sulfates or arsenates to be part of her major biomolecular machinery (Westheimer, 1987). While arsenic and phosphate are in the same column of the periodic table, as Westheimer explained, arsenic esters are far more susceptible to hydrolytic decomposition compared to phosphate esters; in support

of this assertion he pointed out two references—including one going back to 1870—documenting the minutes-long hydrolysis rates of arsenate triesters and diesters compared to their phosphate counterparts. These observations seemed to be clearly at odds with those reported in the “arsenic life” study.

Chemists who were familiar with Westheimer’s article were thus “primed” to immediately question the validity of the phenomenon at the heart of the “arsenic life” study—namely, the stability of arsenates in the Mono Lake bacterium’s biomolecular makeup and therefore the probability of phosphorus being substituted by arsenic. Scientists from other fields, being relatively unfamiliar with the Westheimer article and unaccustomed to thinking about arsenate ester stability, were probably not questioning this central tenet of the article, and certainly not with as much scrutiny: For instance, geologists are used to seeing different elements being interchanged in minerals without loss of structural integrity, so they might not have had the same objections as chemists did to phosphorus being interchanged with arsenic. Thus as Benner et al. (2013) point out, in this case different communities of scientists might have had different standards of proof for examining the claims in the article, not due to sloppy reviewing standards in general, but simply due to different fields of expertise, each of which operates on the basis of differential field-specific criteria for judging the validity of specific findings. This observation thus underscores the great importance of journals selecting reviewers who are both experts as well as ones whose skills complement each other. Interestingly in this context, open peer review may compellingly be said to be the very broadest form of peer review in terms of diversity, with scientists and readers from every conceivable field having access to a article’s content.

The Hexacyclinol Incident: A Prime Instance of Blog-Enabled Peer Review

Case summary: The total synthesis of complex organic molecules, especially those derived from nature, has long been an activity of great conceptual as well as practical importance in chemistry; many of today’s important drugs, agricultural compounds, dyes, and polymers are the beneficiaries of total synthesis. This particular case study centers on the reported total synthesis of a molecule called hexacyclinol with interesting medicinal properties. As can be seen from the discussion below, the article reporting this synthesis suffered from multiple and fundamental flaws—both technical and otherwise—almost all of which were rapidly identified by bloggers and commentators on social media sites before the apparatus of formal, academic peer review brought its machinery to bear on the study. The case is as good an example as we know of citizen-based review akin to peer review of the chemical literature.

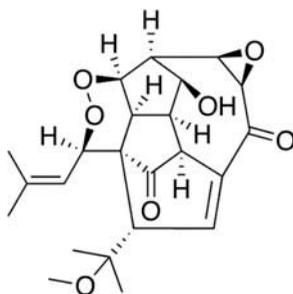


Figure 1: The original structure of hexacyclinol as reported by Gräfe and purportedly synthesized by La Clair (Image: Wikipedia Commons).

In February 2006, the journal *Angewandte Chemie* published an article on the total synthesis of a natural product called hexacyclinol isolated from a fungus that displayed some interesting medicinal and antiproliferative properties (La Clair, 2006). The sole author on the article was James La Clair from the Xenobe Research institute in San Diego, California, a private institution which, according to information on its website, is run only by La Clair. Hexacyclinol (Fig. 1) had been isolated from a dead Siberian log by the late chemist Udo Gräfe's group in 2002 and its structure had been assigned using nuclear magnetic resonance (NMR) and mass spectrometric methods (Schlegel et al., 2002). La Clair's reported synthesis was the first total synthesis of this complex natural product. In March 2006, he presented this work at the American Chemical Society (ACS) National Meeting in Atlanta, Georgia (American Chemical Society, 2006).

Problems with the study were pointed out in June 2006 by Scott Rychnovsky of the University of California, Irvine (Rychnovsky, 2006). The major point of contention was regarding the inconsistency of the NMR spectrum of the molecule with the reputed structure. Rychnovsky had applied quantum chemistry-based computational methods to predict carbon-13 (^{13}C) NMR spectra of related complex natural product molecules, and the agreement with the observed spectra had been excellent. In case of hexacyclinol, however, the ^{13}C chemical shifts of the calculated spectrum for the structure deviated significantly from the spectrum reported for the natural product by Gräfe and presumably confirmed by La Clair. Rychnovsky thus expressed skepticism that the compound synthesized by La Clair was actually hexacyclinol; instead, based on his knowledge of oxygenated natural products, Rychnovsky proposed that the structure of hexacyclinol—or at least the compound prepared by La Clair—could be better assigned as a byproduct of another natural product called panepophenanthrin, which is isolated from the same parent fungus as hexacyclinol and differs from the purported structure of hexacyclinol by a single methanol moiety.

Thus the implication was that the original publication had assigned the incorrect structure based on the acquired NMR spectrum; conversely, if La Clair had in fact synthesized this incorrect structure, it could not have given rise to the original NMR spectrum but to that for panepophenanthrin.

The ensuing discussion on the hexacyclinol synthesis was emblematic of the role that blogs and other elements of social media have played in recent years in the field of RCR in chemistry. Problems with the La Clair synthesis came to the attention of a wider audience when the original report and the response by Rychnovsky became the topic of discussion in a blog post written by Dylan Stiles, a graduate student at Stanford University (“Tenderbutton,” 2006). The post was written on June 3, 2006, only two days after Rychnovsky’s article appeared in the online edition of *Organic Letters*, and was in fact prompted by an anonymous commenter on the blog. The comments section of Stiles’s post raised questions about several other aspects of the article aside from the discrepancies in the NMR spectrum, such as the feasibility of a single-author article describing such a complex synthesis, the difficulty of attempting some highly risky reactions during late stages of the synthesis, and the manner in which the NMR spectrum had been acquired and processed. These comments were also reiterated on other blogs including that of well-known pharmaceutical chemist and blogger Derek Lowe.

Among the RCR issues raised by these comments were the following ones. (1) The sole authorship of an article documenting a 37-step synthesis of a complex natural product was considered unusual, if not nearly impossible, in an era when total synthesis was often performed by multimember teams. (2) The overall yield for the synthesis was 3.7 g, an inordinately large amount for such a long synthesis. (3) The source of the NMR spectrum was also unconventional. La Clair noted that the spectra were contracted out to an organization named Bionic Brothers GmbH in Germany. The contribution of this organization was only recognized on the article by the statement, “J.J.L.C. acknowledges the assistance of five technicians.” (4) What was perplexing was another statement in the article saying that the baseline peak for deuterated chloroform in the spectrum—a standard NMR solvent—was manually added by the operator and that “this was done incorrectly at $\delta = 7.50$ ppm and against the request of the author” (the standard location for this peak is at $\delta = 7.24$ ppm). It was not clear why a spectrum with the correct solvent could not have been obtained and why the peak was added incorrectly in spite of requests by the principal author.

Further support for Rychnovsky’s analysis of the structure appeared in July 2006 when Boston University chemist John Porco, Rychnovsky, and their teams co-authored an article on the total synthesis of Rychnovsky’s panepophenanthrin-based alternative structure of hexacyclinol (vida supra) in *Angewandte Chemie* (Porco et al., 2006). While the proposed identity of the compound as a byproduct of panepophenanthrin could not be validated, comparison of the experimental NMR spectrum of this product with Rychnovsky’s

previously calculated spectrum made it clear that this rather than La Clair's original structure fit the data much better.

At this point, more traditional news outlets began widely reporting on the hexacyclinol story. In its July 31, 2006 issue, *Chemical and Engineering News (C&EN)* featured a news report on the debate and acknowledged the role of blogs in bringing the issue to the forefront of discussion (Halford, 2006). The article also reported La Clair acknowledging the problems with the structure and raising the question of two different chemical structures having very similar or identical NMR spectra. The eminent Harvard chemist E. J. Corey commented on the self-correcting process in science with his statement, "Occasionally, blatantly wrong science is published, and to the credit of synthetic chemistry, the corrections usually come quickly and cleanly" (Halford, 2006). This was certainly true of the hexacyclinol story, in the sense that the time from original publication to serious refutation had been only five months. In August 2006, *Nature* magazine also ran a story on the controversy, noting, "because La Clair is unaffiliated with an institution other than the privately funded Xenobe Institute, there is no obvious body to investigate what happened" (Marris, 2006).

While there was no formal response from *Angewandte Chemie* regarding the article, support for Rychnovsky's revision of the hexacyclinol structure kept emerging in the chemical literature. In February 2009, more than three years after the original article, Saielli and Bagno published a detailed analysis of computed NMR spectra for both La Clair's original structure and the Rychnovsky/Porco revised structure; part of the purpose of the study was to interrogate La Clair's question about whether the two visually very different structures of "hexacyclinol" (Fig. 2.) might have the same spectrum (Saielli and Bagno, 2009). The article differed from Rychnovsky's calculations in both

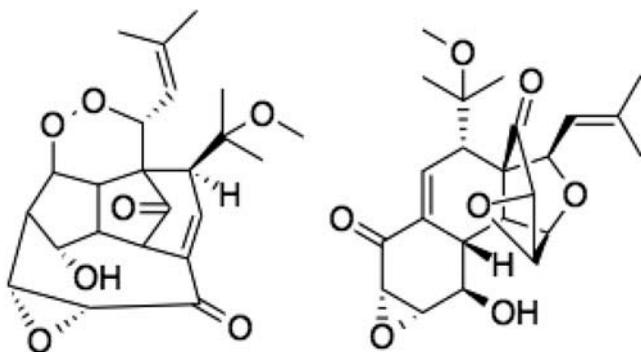


Figure 2: Comparison of the purported structure of hexacyclinol synthesized by La Clair (left) and panepophenanthrin (right). In terms of elemental constitution, the structure on the right differs from the structure on the left by an extra methanol moiety. However, even a casual inspection reveals many structural differences. The numbers indicate corresponding atoms.

using a higher level of quantum chemical theory and in simulating ^1H NMR spectra in addition to ^{13}C peaks. The conclusion of the study was similar to the Rychnovsky report: the spectra for the two structures were significantly different, and the ^1H and ^{13}C peaks for the Rychnovsky/Porco compound agreed much better with the experimental data.

On November 13, 2012, six years after the first serious doubts appeared in online forums, *Angewandte Chemie* formally retracted the La Clair hexacyclinol article with the following statement:

The retraction has been agreed due to lack of sufficient supporting information. In particular, the lack of experimental procedures and characterization data for the synthetic intermediates as well as copies of salient NMR spectra prevents validation of the synthetic claims. The author acknowledges this shortcoming and its potential impact on the community. (Lee et al., 2012)

In the opinion of this author, the retraction, while accurately describing some of the prominent problems with the submission, does not encompass the full range of criticism voiced in various social media and science news channels and discussed above. It also does not contain any explanation regarding the highly belated nature of the retraction, coming as it did much later after even formal peer-reviewed articles controverted the claims of the paper at a fundamental level. This dissatisfaction with the retraction notice was again noted on a few social media sites, including the author's own (Jogalekar, 2014b).

The hexacyclinol incident lies at the intersection of a number of issues discussed in this article. Of all those issues the question of peer review looms largest. The article clearly had multiple problems, both technical (reaction details, NMR discrepancies, yields) as well as methodological (single-author submission, dubious outsourced handling of spectral details), and yet it navigated the peer review process at both the refereeing and the editorial levels. It is also not clear why, despite widespread criticism in multiple forums, it took six years for the journal to retract the article. The traditionally confidential process of peer review makes this potentially immensely illuminating set of underlying motivations behind the publication of the article inaccessible. On the other hand, the case provides a striking, almost exemplary instance of online media quickly and comprehensively assessing the flaws with published research. The wisdom of crowds was on particularly prominent display here as multiple bloggers, authors, and commenters offered their own opinions on problems with the original article so that, within a relatively short period of time, blog posts and comments sections were populated with a comprehensive distillation of pointed criticism. There are few other cases dealing with the literature of mainstream chemistry where social media conducted its own version of peer review in such a timely and efficient manner; but, with time, this is not likely to be the last such case.

Another point driven home by the story involves the complexities of data submission and formats. In a communication with *C&EN*, La Clair himself noted that he had started offering to upload original free induction decay (FID) files with his articles (Drahl and Halford, 2009). It could be conjectured that if one knows the structure of a purported molecule, it is much easier to doctor an NMR spectrum than a FID, which inherently contains more random noise. However, mandates for submitting FIDs do not seem to be a standard part of manuscript submission guidelines; for instance, the *Journal of the American Chemical Society* (Editors, 2014c), *Organic Letters* (Editors, 2014a), and *The Journal of Organic Chemistry* (Editors, 2014a) all recommend submitting such data but regard it as optional. What seems indisputable, however, is that the more the spectral and characterization data that are provided, the harder it would be to fabricate these multiple pieces of data and the easier it would be for reviewers, editors, or the community at large to spot inconsistencies; at the very least, when multiple forms of data are provided (actual spectra, FIDs, etc.) reviewers and authors can cross-correlate them with each other to confirm the claimed results.

The latter possibility brings up an interesting point: In the current milieu of peer review, while data is often reported, tools that can enable reviewers to process that data and recreate at least some of the crucial results are not. In this particular case, for instance, a reviewer with access to a good quantum mechanics-based chemical shift calculation computer program could likely have repeated Rychnovsky's comparison of calculated and experimental chemical shifts; one is tempted to think that such an alert reviewer would then have detected the discrepancy at the preliminary stages. Unfortunately, many such methods are only part of privately sold—and often expensive—software and are not openly available. Cases such as that of the hexacyclinol episode thus offer a good argument for the open availability of data processing tools that can allow reviewers to perform independent checks on reported results.

The hexacyclinol story also highlights the need for institutional oversight of research conduct. As the comment from the *Nature* report pointed out, there was no obvious body that could investigate the details of the study and the article submission. It is not clear what agency funded La Clair's original hexacyclinol work—the source of funding is not stated in the article—although the website of the Xenobe Institute does state its programs as being at least partially federally funded. If federally funded, the research would technically fall under the jurisdiction of the Office of Research Investigation, which until now does not seem to have offered any comment on the incident. However, it is clear that in the absence of oversight by an independent institution, investigation of research misconduct becomes difficult and the community is the poorer for its lack of understanding of such episodes. In such cases, one might argue that reviewers and editors have an even greater responsibility to ensure the accuracy and transparency of the examined work.

The Breslow Case: Review by Twitter and an Evaluation of “Self-Plagiarism” Standards of Scientific Evidence

Case summary: This case study deals with an issue in peer-reviewed research that is both little discussed and rather nebulous in its definition and reach. This issue concerns “self-plagiarism,” or the extensive reproduction of material from one’s own past research. The case is notable because it is, to this author’s knowledge, the first instance of an editor of a major peer-reviewed journal pointing out self-plagiarism in an article by a noted authority in chemistry on the social media site Twitter instead of the pages of a traditional journal. This unprecedented example of peer-review by social media raises many important questions about the role of the global community in assessing chemical research, the permanence of analysis on the Internet, and the very issue of self-plagiarism that was the focus of the debate.

On March 25, 2012, Ronald Breslow—former ACS president, University Professor at Columbia University, senior statesman of chemistry, and recipient of many awards including the National Medal of Science—published a Perspective in the *Journal of American Chemical Society* titled “Evidence for the Likely Origin of Homochirality in Amino Acids, Sugars, and Nucleosides on Prebiotic Earth” (Breslow, 2012a). The perspective summarized research dealing with the question of why some of the key biological molecules sustaining life—such as sugars and amino acids—exhibit homochirality; that is, they exist as only single enantiomers (D in case of major sugars; L in case of major amino acids). This question constitutes one of the outstanding conundrums in understanding the origins of life. The work described in the report was one part of a diverse, large, and highly decorated research program carried out by Breslow and his group for almost half a century (Breslow and Levine, 2006; Breslow and Zhan-Ling, 2009, 2010). Much of the research on which the perspective was based had been conducted during the past fifteen years or so by the Breslow lab, though similar work had been done in a few other laboratories over the years, for instance that of Donna Blackmond at the Scripps Research Institute. The major plausible explanation advanced by this research and summarized in the perspective was the role of homochiral α -methyl amino acids—some of which are found in chiral excess in meteorites—in possibly encouraging the formation and subsequent propagation of the corresponding homochiral natural amino acid stereoisomers.

The *JACS* perspective contained two unusual features that brought it to the attention of the chemistry community at large. One was a speculation at the end of the piece that there may be life forms built out of enantiomeric versions of sugars and amino acids on other planets and that these life forms may exist in the form of dinosaurs; such speculation is usually not found in articles published in serious, top-tier, chemistry-only journals like *JACS*. The exact same speculation had also been stated in a previous 2011 Breslow article published

in *Tetrahedron Letters* (Breslow, 2011b). Further giving voice to the speculation was an April 11 ACS press release (later redacted) on the article, which chose to focus mainly on this last paragraph of the article rather than on the bulk of the scientific argument. Several bloggers and commenters criticized both what they thought was a fanciful speculative assertion in a serious journal and the media's selective focus on this assertion at the expense of the preponderance of the research described in the article (Bracher, 2012). The debate thus centered upon a longstanding issue—the role of the media and public relations arms of scientific societies in highlighting contemporary scientific research in a responsible and reasonable manner.

However, a second aspect of the perspective soon gained far more attention. The chief editor of the respected journal *Nature Chemistry*, Stuart Cantrill, noticed and highlighted a number of paragraphs in the perspective that seemed to be virtually identical to those published in two previous Breslow publications, one from the *Tetrahedron Letters*, cited above (Breslow 2011b), and another from the *Israeli Journal of Chemistry* (Breslow, 2011a), both published in 2011. At least half of the material in the *JACS* publication was identical to that from the previous two articles. The most striking aspect of Cantrill's critique was the fact that he chose to publicize the similarities between the articles, not in a peer-reviewed journal, but on the social media site Twitter. As of this date, Cantrill's Twitter stream remains the only source where an explicit comparison of the three articles exists (Cantrill, 2012a,b). This episode thus features a prominent editor of a leading scientific journal using social media sites to disseminate thoughts and critiques on a peer-reviewed article from a prominent journal and researcher. Publishing on blogs or on Twitter has some clear advantages and disadvantages. Among the advantages are wide, free, completely open-access dissemination, speed, and the ability to bypass the middleman, namely editors and reviewers, as well as production delays and even logging into institutionally-funded websites. The disadvantages are the lack of oversight and some degree of vetting of the published material as well as possible advocacy-oriented discrimination. Since social media sites will continue to offer free and uninhibited opportunities for criticizing peer-reviewed research as well as other modes of scientific communication including magazine articles and scientific meeting presentations in the foreseeable future, we believe that examples like Cantrill's analysis will become more common and will undoubtedly spark debate and discussion regarding such new modalities of peer review.

Cantrill's Twitter critique along with extensive discussion on chemistry blogs [including the official blog of *Nature News* (Bracher, 2012)] and websites led to charges of "self-plagiarism" being leveled against Breslow. Self-plagiarism is, of course, quite different from the much more serious charge of plagiarism and in fact raises a sound question regarding definition, since at first glance it might seem that it would be impossible to truly

“plagiarize” material from one’s own work; nevertheless, as discussed below, it is a legitimate phrase defined by several prominent journals. A related charge was the possible infringement of copyright by massive duplication of work that has been legally protected by copyright by the journals which originally published Breslow’s earliest communications. In an interview with *Nature News*, Breslow asked the community to distinguish a perspective or review from an original research article and stated that since a review by definition considers previous work, one cannot completely avoid mentioning material from earlier articles. Breslow also said that he had been careful to avoid copyright or self-plagiarism by “making enough changes” to ensure that his words were different enough from those in the previous articles. While Cantrill’s detailed highlighting of the extensive similarities between the articles seemed to undermine this claim, it also posed the logical question whether Breslow’s words were in fact different enough to warrant his assertion of having made “enough changes.” More formally, it leads to the question of what legal or scientific criteria exist for labeling changes in text from one publication to another as being “enough.” Lastly, given Breslow’s achievements and illustrious status in the community, publishing one more article was surely not going to increase his visibility but, as it turns out, could seriously damage his reputation.

In order to seek clarification if not a resolution to these issues, it is instructive to peruse the relevant journals’ publication codes of conduct. The American Chemical Society document titled *Ethical Guidelines to Publication of Chemical Research* which explicitly addresses the issue of self-plagiarism (Editors of the Publications Division of the American Chemical Society, 2014) borrows its language from similar guidelines published by the Society for Industrial and Applied Mathematics. In contrast, the *Guidelines to Authors* document from *Tetrahedron Letters* seems to contain no mention of self-plagiarism (Editors, 2014b).

The body of the ACS guidelines reads as follows:

Authors should not engage in self-plagiarism (also known as duplicate publication)—unacceptably close replication of the author’s own previously published text or results without acknowledgement of the source. *ACS applies a “reasonable person” standard when deciding whether a submission constitutes self-plagiarism/duplicate publication.* If one or two identical sentences previously published by an author appear in a subsequent work by the same author, this is unlikely to be regarded as duplicate publication. Material quoted verbatim from the author’s previously published work must be placed in quotation marks. In contrast, it is unacceptable for an author to include significant verbatim or near-verbatim portions of his/her own work, or to depict his/her previously published results or methodology as new, without acknowledging the source. (*Italics ours*) (Editors of the Publications Division of the American Chemical Society, 2014).

These guidelines raise questions of value for any of us who seek to publish review articles based on past research articles, either our own or others’. In the

context of the Breslow case, however, it is thus important to look at the charges of self-plagiarism through the lens of the “reasonable person” standard cited by the ACS above. Since this standard is not precisely defined, its applicability may appear rather nebulous. However, the subsequent statement provides a clue; the standard is likely to hold in the case of one or two identical sentences borrowed from a previous publication. Thus if one were to simply gauge the Breslow *JACS* perspective using a ruler applying a “number of sentences” metric, the material would clearly conform to charges of self-plagiarism as defined in the ACS document. The duplicated material also does not conform to the rule about being enclosed in quotation marks. It is worth noting, however, that while the perspective may fail the “reasonable person” test, it does satisfy at least one other condition—proper citation of sources—laid out in the ACS guidelines; the article certainly cited the previous two articles from *Tetrahedron Letters* and the *Israel Journal of Chemistry*.

On May 12, *JACS* formally withdrew the article, citing a request by Breslow himself that underscored the validity of the science and acknowledged the similarity with the previous material (Breslow, 2012b). The April 11 ACS press release was also simultaneously retracted. On April 30, in an article in *Chemical & Engineering News*, former *C&EN* chief editor Rudy Baum disclosed a note that Breslow had sent him. In the note, Breslow admitted that he “fell in love with his [own] words” and said that he understood why so much repetition of previously existing material necessitated retraction.

The Breslow case raises several interesting questions relating to peer review, copyright, and disincentives for self-plagiarism, many of which will endure into the future even as free online peer-review lacking editorial oversight becomes commonplace. A clear question is, at what point does duplication of language, facts, and statements from one’s own previous publications cross the line into self-plagiarism? A similar question could be asked about the larger and much more serious issue of plagiarism in general. It is clear that the ACS applies a reasonable person standard to judge self-plagiarism. Other leading journals seem to have similar guidelines. For instance, *Nature* judges instances of both plagiarism and self-plagiarism “on their own merits.” *Science* magazine applies a similar standard.

Concomitant with the question about defining self-plagiarism is the one about detecting it. A variety of publishers are now members of CrossCheck (Anonymous, 2014b), an online automated software platform for detecting plagiarism through comparisons with existing research reports. The database of reports is periodically updated and comparisons are made with full-text version of the articles to ensure accuracy. As of 2014, 4,800 participating publishers and societies are members of CrossCheck. Interestingly, the ACS is one of these members. In case of the present article, it would seem that the manuscript had presumably not been interrogated with CrossCheck before it was approved for publication.

This discussion of detecting plagiarism cannot help but tread into a thread central to our discussion—that of nonconventional, online sources of peer-review. As mentioned before, the Breslow article was being discussed on online blogs and websites much before any formal comment regarding its content appeared on the ACS website; the most notable instance of this discussion was the explicit disclosure of similarities between the published article and the two previous articles by *Nature Chemistry* chief editor Stuart Cantrill. It is also worth noting that this informal peer-review process was liberating and democratizing, in the sense that, firstly, anyone could have duplicated Cantrill's efforts in principle and, secondly, even an individual as undoubtedly accomplished and well known as Breslow was not free from its purview. One wonders whether in the future, the extensive level of scrutiny and publicity that valid social media-based analysis brings can provide an extra incentive for authors, even ones as distinguished as Breslow, to exercise greater care in publication.

The fact that Twitter remains the only source where Cantrill's comparison exists also raises interesting questions about the future of referencing and record keeping in chemistry. Unlike a reference to a traditional journal article which is formally recorded in archival material and is searchable through multiple chemistry-related tools like the Chemical Abstracts Service and SciFinder, a Twitter reference exists as a soft link on the Internet. In fact, the reference section of this very article, with its abundance of online links to information not available elsewhere, showcases new questions about the nature of record keeping. Such links are subject to change or even deletion and, therefore, may not serve as references with the kind of permanence assigned to an article in the *Journal of the American Chemical Society*. Nonetheless, as Cantrill's critique demonstrates, it is going to likely be increasingly prevalent for important elements of scientific debate regarding chemical research to exist only or predominately and then perhaps only fleetingly (in terms of months or years rather than centuries) on the Internet. How to store, archive, and codify such online links on social media into a permanent format is one of the questions the scientific community will have to grapple with in the future. One solution implemented by a consortium of universities is perma.cc (Anonymous, 2014c), a service that forges permanent links to archived copies of webpages. Similar solutions will undoubtedly become popular with the proliferation of references to online sources.

Abbreviated Additional Cases

For reasons of space, we have not been able to chronicle other prominent cases in the present category, but we do briefly mention two others presenting their own unique insights; interested readers can consult the relevant references and links below:

1. On July 21, 2009, an article appeared in the *Journal of the American Chemical Society* detailing the use of sodium hydride as an oxidizing agent for the conversion of alcohols to ketones. This observation was at odds with the well-known nature of sodium hydride as a reducing agent, a property that has been known and described in college-level organic chemistry textbooks for decades. Not only did the article raise immediate concerns, but it also led to a novel experiment by chemist Paul Docherty in which he carried out the protocols described in the article in his own laboratory, carefully documented the results and communicated them online on his blog in real time (Docherty, 2009; Hadington, 2014). Docherty was essentially doing peer review by live blogging. Within 24 hours, five other chemists had tried to perform the same reaction. Based on the observation of partial and inconsistent oxidation of selected substrates, a consensus was quickly reached that whatever was causing the oxidation had to be something other than the sodium hydride, perhaps oxygen from air or a trace contaminant. On December 23, 2009, *JACS* retracted the article “for scientific reasons” (Wang et al., 2011).
2. On June 19, 2013, an article on the synthesis of nanorods with tunable angles between them was published in the ACS journal *Nano Letters* (Anumolu et al., 2013). On August 13, 2013, Mitch Andre Garcia, then a graduate student at the University of California, Berkeley, wrote a post on his blog pointing out some serious issues with the images in the article, most prominently the fact that the immediate background surrounding the nanorod images seemed to be very different from the general background of the transmission electron microscopy (TEM) technique used to obtain the images (Mitch, 2013). Others like St. Louis University chemist Paul Bracher followed up with their own analyses (Bracher, 2013). These observations pointed to the possibility of the images being photoshopped and overlaid on top of the background. On August 14, 2013, only two days after Garcia’s post appeared, the journal withdrew the article, citing “concerns over the integrity of the data” (Anumolu et al., 2013).

DISCUSSION AND CONCLUSION

During the last decade, blogs and other kinds of social media have spurred vigorous online discussion and debate of RCR issues in chemical research. These forms of inquiry now complement traditional peer review and have grown by leaps and bounds during a startlingly short period of time. This growth has paralleled the general growth of the online chemistry community; chemistry-related blogs are now hosted not just by individual scientists and writers but also by respected scientific publications like *Chemical & Engineering News* in the form of their “Safety-Zone” blog (Kemsley, 2014), *Discover* (Anonymous (Seriously Science), 2014), *Wired* (2014f), *Nature* (2014d),

and *Scientific American* (2014b). Almost all of these blogs offer comment sections, and one of the central and enduring threads that has emerged from the cases discussed is the indispensable role of both certified and anonymous commenters in contributing to citizen-enabled peer review, a role that has interestingly been also recognized by open access journals like *PLoS* (2014e) and *eLife* (2014c). The number of chemists active on sites like Twitter is also now significant; a recent listing by *Nature Chemistry* editor, Stuart Cantrill, on his blog counted at least a hundred academic and industrial chemists, chemistry writers, editors and journalists, and laymen seriously interested in chemistry (Cantrill, 2014), and there are undoubtedly many more. In general, this participation of chemists and the ensuing impact of online forums on peer review has emerged during the last decade or so because of the increased use and availability of the Internet and platforms for commenting and blogging, but it has also undoubtedly been engendered by the attractive opportunities that social media sites provide for obtaining and disseminating information and opinion, for fostering a sense of community and for enabling the wisdom of crowds to bear on published research.

Two striking themes emerge from every single one of the cases of online peer review discussed above: first, that in almost every one of them the online forums and their authors were remarkably quick in pointing out problems with the content, and, second, the fact that the discussion was completely open and allowed commenters of all backgrounds and motivations to contribute a diversity of criticism that is not apparent or even possible in more limited, formal peer review. Most importantly, as evidenced by the retraction of all of the above articles from the relevant journals, it is clear that the online criticism of their content was accurate and valid. Thus, while one can argue about the various pros and cons of largely unmoderated debate, there is little doubt that the debate was warranted in terms of the target of the criticism.

Also in this context, one would be remiss in not mentioning that the utility of blogs in discussing the potential reasons why an article is retracted parallels that of the prominent website Retraction Watch (Oransky, 2014), a blog set up in August 2010 to bring public and media attention to retracted articles. As should be evident from the retraction notices of some of the articles discussed above, the motivation for Retraction Watch was to try to detail the actual reasons that articles are retracted, a service that retraction notices in journals usually do not provide. During its five-year tenure, Retraction Watch has become highly successful and has been responsible for detailing mistakes and malfeasance in hundreds of articles from almost every field of science that were withdrawn from formal journals. We would like to point out that, in the world of chemical research, blogs and social media have achieved the same goal and, as documented above, at least some of this discussion emerged before Retraction Watch was inaugurated.

In principle, one of the significant impacts of the influence of social media in exposing gaps in traditional peer review would be to compel journals to reconsider their formal process of vetting for articles. Unfortunately, most of the deliberations regarding formal review are confidential—precisely a fact that has inspired “activist” discussions on blogs—so it is not possible to quantify what the impact of the above cases on the formal review process has been except to speculate that the widespread discussion of obvious errors on social media might have potentially expedited the retraction of the relevant articles. However, one cannot emphasize enough that this is not an “us vs. them” dichotomy. The prominent role that social media played in so many high-profile cases of peer review is not meant to point to a possible replacement for the traditional protocols: rather, it is meant as a message to the entire community—researchers, authors, editors, publishers, reviewers, bloggers, and journalists—to make the protocols more transparent, productive, and ultimately beneficial to all. To this end, it is worth contemplating the kind of policies that the community can adopt in order for the system to function as smoothly as possible.

This article has already mentioned arXiv, the online repository of articles where authors can submit articles before they are officially submitted to journals; this prepublication process thus opens up articles to constructive criticism and debate. Interestingly, arXiv has become a staple of mathematicians, physicists, and computer scientists, but not of chemists and biologists, in spite of the fact that the website does accept chemistry and biology submissions. Biologists now in fact have their own arXiv-like repository named bioRxiv (2014a), but chemists do not seem to have one yet. This phenomenon leads us to ask why chemists may be more reluctant to publish on arXiv compared to their fellow physical scientists. One reason may be the proprietary and practical nature of much of chemical research which lends itself to being patented and commercialized for monetary gain. Another reason may simply be that chemistry journals may forbid authors from prepublication on third party sources. A third reason may be cultural; perhaps the community of chemists is not as receptive yet to open publishing as that of theoretical physicists. Whatever the reasons, it is clear that having articles prepublished on a site like arXiv would give authors a chance to constructively engage with their target audience and address errors and shortcoming which anyone is free to point out.

A related issue concerns alerting authors to potential highlighting of their published material on blogs and other websites. Currently, there is no mechanism for an author to be alerted to discussions of his or her article on social media. Any knowledge of such discussion is purely accidental, usually acquired when more formal news sources pick up the debate or more often simply by word of mouth. It would be a salubrious development for the entire community to perhaps build a central repository which would be known to all authors and which could duplicate the discussions of their research the moment they

appear on blogs and other sites. In a typical scenario, a blogger would critique a particular article and create a link to his or her post on this central website. Other bloggers and journalists who pen their own critiques could do the same. This would give the authors of the article under discussion an opportunity to view all criticisms of their material in one central location, and then perhaps to respond to individual bloggers or write a response on their own website. The key goal would be to involve the entire community—authors, editors, and the target audience—in the process of criticism right from the beginning, so that the discussion does not become disjointed and is not seen purely in terms of an outsider group attacking the status quo. A central repository of the kind envisaged above would be inclusive rather than divisive and would seek to reach a common understanding of what went wrong with a particular piece of research, along with potential suggestions for improvement or lessons learned. The entire community would benefit this way.

While this discussion has focused on the great utility of blogs in shedding light on chemical error, flawed design, analyses, and falsification, it behooves us to consider the possible pitfalls of open and online peer review. Interestingly, these pitfalls are not too different from those faced by online scrutiny of issues in other fields like politics or sports; in some sense they are an inevitable consequence of the rise of the Internet and in fact the information age. It is clear that online forums provide a mouthpiece to those who are quick to judge and who may not have scrutinized all of the existing evidence carefully. This can lead not only to inaccurate and uninformed judgments, but also to potential defamation of authors and researchers. To this author's knowledge, there has not been a single formal defamation suit arising from such criticism to date, but one must still be mindful that, through their comments, they are potentially openly tarring the reputation of *bona fide* scientists who may have had long and distinguished scientific careers and who are publishing in respected peer-reviewed journals.

The easy accessibility of comments sections of blogs can also lead to exploitation by those who wish to willfully tar someone's reputation because of a personal grudge or vendetta. In addition, it is quite easy for online comment sections to turn into echo chambers where similar opinions are reaffirmed and contrary opinions are suppressed. Special problems arise when such debates descend into vitriol or *ad hominem* attacks; in fact, several recent studies have demonstrated that hostile blog comment sections not only deter reasonable commenters with valuable opinions from commenting for fear of getting embroiled in a bitter and unproductive debate, but they also negatively shape these potential commenters' perception of the original post (Weigel, 2013). The ensuing lack of balance and original opinion benefits no one.

This armchair access to social media, the potential permanence of content on it, and the possibility of echo chambers potentially turning a scientific debate unproductive thus impart an added degree of responsibility to bloggers

and commenters. Mindful of the pitfalls of open commentary, bloggers must strive to make sure that the comment sections of their posts are free of *ad hominem* attacks, self-reinforcing viewpoints, and what is commonly called “trolling” (this behavior can range from collective harassment and/or abuse of commenters with contrary opinions to continued repetition of viewpoints to engaging in completely unrelated discussions). Opinion when stated should clearly be identified as such. It is clear that with the power to offer criticism at the click of a button comes the responsibility to make sure that such criticism is reasoned and backed by facts.

While it is not easy to quantify how well prominent chemistry sites have succeeded in this regard, it is this author’s considered opinion—considered carefully over ten years of observation and participation in such forums and reinforced by many cases of successful social media-based peer review such as those noted above—that the best blogs and social media sites have achieved remarkable success in maximizing their signal-to-noise ratio and in fostering productive debate. In fact, an October 2013 editorial from the journal *ACS Nano* which asked bloggers to exercise caution in leveling accusations and criticism was nevertheless unable to cite a single instance of a case where a blogger had wrongfully accused a researcher of fraud or misconduct (Parak et al., 2013). Comment sections of the most commonly read blogs are almost uniformly civil and present a diversity of useful opinions. Personal communication with prominent bloggers in the field has revealed that they have had to spend a minimum amount of time deleting inappropriate comments or policing their comments section in general. In other words, the best blogs and online forums seem to attract the best commenters.

But the bigger argument here should not be lost on us. Occasionally, a blogger may make a mistake in accusing a researcher of misconduct, but the fact that such mistakes can be made is no reason for us to not use blogs as instruments of constructive criticism and progress in chemical peer review. This basic belief was reflected in an editorial in *Nature* which acknowledged the diversity of voices in postpublication peer review and noted, “It is better to ask that debate be civil, responsible and courteous, than that it not appear online at all” (Editor, 2013).

In addition, the online chemistry community has until now been remarkably discerning in identifying and citing high-quality discourse, so it is not unreasonable to imagine this community criticizing examples of misguided accusations with the same spirit that it nurtures valid ones. This “like attracts like” phenomenon, in which good discussion forums are self-selected and their results reinforced, is gratifyingly not limited to blogs. Its widespread prevalence is in fact a reassuring reminder to those who are concerned about the quality of open-access forums disintegrating in the hands of trolls and troublemakers. A soaring example is the profoundly successful online encyclopedia, Wikipedia. As an encyclopedia that was open to editing by anyone,

observers initially worried that the quality of its content might massively suffer. Today, however, not only is Wikipedia one of the most accessed sites on the Internet, but it is also one of the most accurate. For instance, as long ago as 2005, *Nature* conducted a study that found that the average accuracy and quality of articles on Wikipedia is comparable to those in the centuries-old *Encyclopedia Britannica* (Giles, 2005). And just this month, the (open-access) journal *PLOS ONE* published a study comparing the accuracy of information on drugs between Wikipedia and respected textbooks of pharmacology (Kräenbring et al., 2014); the report found that, by and large, Wikipedia was at least as accurate as these frequently prescribed textbooks for undergraduate medical education. Wikipedia, therefore, is an appropriate metric for judging the quality of online discourse, and by that measure, the best blogs have succeeded remarkably well. Imperial College computational chemist and blogger Henry Rzepa affirmed this value of online chemical discourse when he commented on it in the wake of the sodium hydride story:

There seems little doubt that the very best blogs can provide a level of critical scientific commentary which in many cases surpasses the more traditional “QA” mechanisms such as journal peer review. In the latter, a small number of possible experts in the topic being reviewed will probably respond in that time-stressed manner which can often result in flaws or gaps in a scientific argument being overlooked. Blogs provide a creative new alternative to this scientific process, and can on occasion actually contribute to the collaborative ways of working that are nowadays so essential. (Rzepa, 2014)

On balance, therefore, the discussion in this article of the role of social media in peer review fills us with optimism and hope. The existence and vigorous participation of these forums in analyzing, challenging, and enhancing dialogue about the chemical literature and the human elements in research raise interesting questions with which the chemical community will have to grapple for the foreseeable future. Given the nature of transformational change over generations, it is also reasonable to predict that the younger generation which has grown up in the milieu of the breakthrough technology of the Internet will adapt and respond much more quickly to the changing norms of research and review discussed above. However, we have scant doubt that future analysis and debate regarding these norms will enrich and fortify what has been, is, and will always remain, a thriving marketplace of ideas. We can all await that salubrious development.

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