The Wisdom of Crowds, the Madness of Crowds: Rethinking Peer Review in the Web Era

by WILLIAM B. MILLARD, PhD

Special Contributor to
Annals News & Perspective

The case for editorial peer review, in the abstract, appears unassailable: it makes the difference between order and anarchy in the scientific literature. Without expert prepublication refereeing, a flood of sloppy methodology, unreadable or misleading prose, wishful thinking, half-truths, and outright falsehoods would overwhelm the reliable reports on which scientific progress and sound clinical practice depend. It is indispensable for sorting out credible reports from polemics advocating trepanation, astrology, or flat-earthism.

Actual peer review practices, history and scholarship suggest, do not consistently live up to this ideal. "Peer review is supposed to be what determines the quality of science," says *Annals* editor in chief Michael L. Callaham, MD, "and yet we know nothing about it."

Even less is known about the concept of open review, or so-called crowdsourcing. A recent open-review experiment by Shakespeare scholars and the successful collective solution of a mathematical problem[1] attracted mass-media attention to this practice's broader potential. But it may be too early to tell whether it could ever challenge the more traditional model.

In some views, the conventional peer review with reviewer anonymity is conducive to candor; others find it corrosive to accountability. Peer review is either the key to meritocracy, purifying science of commercial, political, and other irrelevant pressures, or the mechanism by which an old boys' network preserves its half-earned authority. Some justify the extraordinary efforts devoted to peer review, often thankless and usually uncompensated, by invoking its role in quality control; others find the practice riddled with incompetence, conflict of interest, interpersonal strife, assorted biases (including pervasive bias toward positive results, along with predictable personal leanings), and occasional intellectual property theft.[2-5]

On one point, the system's defenders and critics agree. It does nothing to prevent fraud, that periodical stain on the research community's reputation.[6]

These problems are often, though not always, intertwined with what Judson[7] called "the contradiction that makes peer review possible at all. . . . that the persons most qualified to judge the worth of a scientist's grant proposal or the merit of a submitted research paper are precisely those who are that scientist's closest competitors."

"It's basically a 200-year-old process that was developed by English [and Dutch] country gentlemen," Dr. Callaham continues, "at a time when there would be maybe 30 or 40 other people in the world [with whom] you could have an intelligent discussion. . . . It really didn't take hold until after World War II; before that, most of the science that you read was not really peer reviewed."

Some trace peer review as far back as Aristotle[8]; institutionalized by the Royal Society's *Philosophical Transactions*,[9] it became standard practice in the postwar era, despite the massive increases in the numbers of scientists and scientific publications during those years and the enduring problem of recruiting capable reviewers. "Everybody uses it and relies on it, and yet nobody had studied it," says Dr. Callaham. "The method that selects science ought to, itself, be scientifically examined and proven."

Moreover, the dramatic expansion of access to scientific articles through the World Wide Web and the examples set by other disciplines, in which preprints are broadly circulated before editorial refereeing—sometimes bypassing that step entirely—pose new challenges to the peer review system. The scalability of electronic communications not only speeds and coordinates editorial communications but makes open review practical, at least in some disciplines.

Advocates of "crowdsourced" or "Web 2.0" review, either before or after publication, claim that such a procedure is preferable on grounds of equity, transparency, and perhaps review quality, as well as more obvious online features such as speed or range of opinion. Defenders of traditional peer review, encountering arguments that it serves only the interests of established publishers and professional societies,[10] may find themselves in a position of relying on a body of evidence that is far from conclusive.

Biagioli,[11] professor of the history of science at Harvard, has linked the rise of peer review by the 18th-century precursors of today's scientific organizations, specifically the Royal Society and the French Académie des Sciences, to 17th-century practices more closely resembling

the imprimaturs conferred by state censors. To some contemporary advocates of open review, the conventional system still bears certain traces of its origins among disciplinary practices in a somewhat sinister sense, the antithesis of a reliable merit-based filter.

With open review arrangements still fairly rare in biomedical research, and crowdsourced review (a process distinct from open expert review) rarer, it may be too early to determine whether the electronic alternatives gaining popularity in other fields offer appropriate advantages for physicians. Yet open review is already incorporated in one form into the procedures of one globally prominent journal and is critical to the mission of that journal's innovative imminent spinoff project. It raises questions that focus attention on the assumptions and uncertainties surrounding a practice considered central to both the conduct of science and the construction of scientific communities.

REVIEWING THE PROCESS OF REVIEW

In the 1980s, peer review became an object of study in its own right. *Annals*, under Dr. Callaham's leadership, in regard to peer review as a critical intellectual problem, as well as a regular component of editorial procedures, has actively participated in such research.[12] It provided training for reviewers and monitored its own reviewers' performance.[13,14] After some 25 years' worth of investigation into a topic notoriously resistant to analysis,[15] says Dr. Callaham, the consensus emerging from "a handful of decent studies, less than half a dozen" is that peer review does help improve articles, but not enormously, and that its gatekeeping effect is overrated.

Even glaring errors in studies frequently slip through. *Annals* is one of several journals that have tested their reviewers[16-19] by circulating fictitious articles with deliberately inserted flaws, Dr. Callaham reports, finding that "basically, peer reviewers did dreadfully . . . they missed at least half of both major and minor errors." Problems include determining whether conclusions follow from results, detecting bias, and citing sources accurately. "These were mostly the bigger, better journals," he adds, "journals that actually cared enough about it to invest the time and trouble to do the study. . . . Their results, which are pretty discouraging, are the best of the best."

The quadrennial International Congresses on Peer Review and Biomedical Publication, inspired and organized by *Journal of the American Medical Association* deputy editor Drummond Rennie, MD, have driven much of the scholarship in this area. At the outset of this project, Dr. Rennie called attention to one fallacy about peer review's success at keeping weak articles out of the discourse: "One trouble is that despite this system, anyone who reads journals widely and critically is forced to realize that there are scarcely any bars to eventual publication. There seems to be no study too fragmented, no hypothesis too trivial, no literature citation too biased or too egotistical, no design too warped, no methodology too bungled, no presentation of results too inaccurate, too obscure, and too contradictory, no analysis too self-serving, no argument too circular, no conclusions too trifling or too unjustified, and no grammar and syntax too offensive for an article to end up in print."[20]

Though the existing review system undoubtedly blocks some of the worst such articles, the ideal system would filter out—or at least drastically improve, through the interactions of editors and authors—the majority, rather than simply shunting them from more prestigious journals to lesser ones. Peer review as usually practiced, one can infer from Dr. Rennie's observation, does not so much perform gate keeping as triage.

In 1998, Dr. Rennie's opening address to the third International Congress expressed the desire for anonymous peer review to join anonymous authorship on the scrap heap of history, replaced by a "fully open" system identifying reviewers, as well as authors, grounded in a belief "that openness strengthens the link between power and accountability."[21] A dedicated theme issue of *JAMA* after the fourth Congress, illustrated with a cartoon of Dr. Rennie as Moses leading colleagues through the desert,[22] included several assessments that conventional peer review was not yielding demonstrably superior scientific results, along with a call for open procedures by Fiona Godlee, BSc, MB BChir, MRCP, of Biomed Central (later editor in chief of the *British Medical Journal* [*BMJ*]) on multiple grounds: ethics, feasibility, lack of adverse effects, and a balance of accountability and credit for reviewers' work.

The *BMJ* initiated a form of open review in 1999,[23] identifying reviewers to authors (though not to readers) at the same time that it published a report[24] indicating that this variable had no effect, positive or negative, on review quality. Richard Smith, MD, MS, the *BMJ*'s editor in chief at the time, argued that the burden of defensibility should rest on conventional anonymous procedures, not on the newer system, and that the likely gains in ethics and civility would outweigh potential losses of young reviewers out of fear that signed reviews might damage their careers. He also conjectured that "we may move to a system where authors and readers can watch the peer review system on the World Wide Web as it happens and contribute their comments. Peer review will become increasingly a scientific discourse rather than a summary judgment."

Dr. Smith, now a board member at the Public Library of Science, has followed research on the subject over the years and grown skeptical toward peer review as an institution.[25] He has written that "Peer review might disappear because its defects are so much clearer than its benefits. It is slow, expensive, profligate of academic time, highly subjective, prone to bias, easily abused, poor at detecting gross defects, and almost useless for detecting fraud."[26] In the absence of conclusive evidence for its value, except to allocate scarce journal space, and in the awareness that digital publishing is not subject to the same scarcity constraints as print, Dr. Smith sees no serious objection to reversing the traditional procedural sequence in which closed review precedes publication. Putting publication first and letting review follow is an intriguing act of faith on several levels: in the purported wisdom of crowds, in potential contributors to choose a publication method that might expose their work's flaws to general scrutiny, in reviewers to balance courtesy with useful correctives, and in readers to find the whole enterprise worthy of attention.

In the fall of 2010, the *BMJ* plans to launch a new online publishing venture, *BMJ Open*, inviting submissions geared toward medical research in any therapeutic area, though excluding clinical case reports, and welcoming both high- and low-impact studies of any size.[27] *BMJ Open* will place peer review documents in public view once articles are accepted, require reviewers to sign their comments, and present all material to anyone with Internet access, free from subscriber pay walls. It will operate alongside the conventional *BMJ*, covering expenses through an author-pays model (waived in cases in which institutional support is unavailable) and publishing work that has not found an outlet elsewhere, including in *BMJ* itself. In an effort to optimize readers' direct access to evidence for independent analysis, it encourages public presentation of raw data sets.

The distinction between open and crowdsourced review is important. *BMJ Open* adheres to the former review model. "Anything published will have been peer reviewed in the 'usual' way," says managing editor Richard Sands, "ie, reviewed by external peer reviewers via an editorial office. So anything accepted for publication will have been through a formal peer review procedure, 'open' to its participants but not the public. If the article is accepted, then the prepublication history (previous versions, peer review comments, and author replies) will be made public, alongside the final typeset and proof-checked manuscript. So we are not crowdsourcing reviews to determine publication."

BMJ deputy editor Trish Groves, MBBS, MRCPsych, notes that other journals, including *PLoS Currents*, use a community peer review process but comments that *Nature*'s 2006 experiment with public review along with standard peer review[28] was "largely unsuccessful." Few authors agreed to participate (only 5% of those invited), numbers of page views and comments were small, and editors likened their efforts to obtain comments to "pulling teeth."

FROM PIONEERS DOWN UNDER TO A RANGE OF OPTIONS

About the same time Dr. Rennie, Dr. Smith and colleagues, and others were calling for revised review processes, the *Medical Journal of Australia* (*MJA*) became one of the first biomedical journals to experiment with a form of dynamic online peer review.[29] With authors' and reviewers' consent, the journal electronically published 56 articles that had already been reviewed and accepted, along with the reviewers' reports and selected e-mail comments from readers. The *MJA*'s Web site thus became a publicly scrutinized space in which authors could reply or revise their articles in response to readers' reactions.

After an open-review stage lasting a median of 10 weeks, articles were copyedited and published in the print journal as before. Majorities of both authors (81%) and reviewers (92%) approached for the project consented to it, and 62% of participating reviewers were willing to sign their reviews; the others chose to retain anonymity, often because of their institutions' preference. Reviewer performance scores did not significantly differ from prestudy scores, though prestudy outlier scores, both high and low, moved closer to the mean. Of 52 open-review comments, largely short and specific, 29% led to authorial changes affecting 7 articles.

These numbers are relatively small, and the research involved was not a random sample; the editor withheld certain articles from the study for various reasons (to link them to editorials, to give all readers simultaneous access, or because resource limits constrained workflow). Nevertheless, these results suggested that open review is palatable to participants, comparable to conventional private procedures in review quality, and occasionally improves articles; the experience set an important precedent. *MJA* deputy editor Bronwyn Gaut, MB BS, DCH, DA, reports that a follow-up study was planned[30] but abandoned for reasons unknown. The journal adopted all-electronic (though not open) review procedures in 2005 and maintains a rapid-publication section[31] for fast-tracked articles.

In subsequent reflections[32] on this and related endeavors, former *MJA* communications development manager Craig Bingham places his journal's initial venture in the context of efforts in multiple fields (from physics and environmental science to psychology and cultural studies) to transform their peer review methods from a black-box process to discussion formats with various levels of openness. Each has its pros and cons, and Bingham's report acknowledges field-specific drawbacks, including, in medical e-journals, clashes with publication policies and publicity embargos.

Some electronic review methods merely replicate existing procedures, accelerating editorial communications without substantively transforming them. Some merge the editorial process with responses that would otherwise appear in post-publication commentary (a form of extended peer review traditionally conducted through letters to editors and subsequent separate studies) so that preliminary data and reports can attract peer contributions and shape the final report.

In Web journals on rhetorical theory[33] and other fields that prize collective experimentation over delineation of individual contributions, distinctions between authorship and dialogue blur entirely; for example, Rhetnet, "a dialogic publishing [ad]venture," according to its Web site, exploring what net publishing might be "in its 'natural' form." This led Bingham to comment that "Rhetnet does not seem to have a peer review process so much as be a peer review process. It is a method quite alien to biomedical journals, but not unlike a scientific meeting or the consensus processes of a working group."

Other disciplines, beginning with high-energy physics, have moved to a self-publication model based on the circulation of electronic preprints (the "e-prints" found on the arXiv.org system developed by physicist Paul Ginsparg at Los Alamos National Laboratory, now hosted at Cornell), again dissolving distinctions among reviewers, authors, and readers. Some claim that these developments, combined with the changing economics of publication, imply that the entire journal format is approaching extinction,[34] but Bingham and others note that the e-print approach might not translate smoothly from the close-knit communities well versed in the abstractions of mathematics and physics to the clinical fields, where prematurely accessible information would find a much wider audience.

Formats used by *MJA*, the Cochrane Collaboration, and other biomedical

enterprises extend the commentary process but neither dispense with structured peer review nor leave the process so open that the end product of a complete published article becomes unrecognizable. Features resembling the arXiv self-publishing model appeared in the E-Biomed proposal by Harold Varmus, MD, in 1999 but disappeared by the time this proposal morphed into PubMed Central, which preserves the roles of traditional publishers and peer reviewers rather than gives the public free access to all pre- and postpublication materials.[35]

In a 2007 blog entry that Dr. Groves cited in a presentation to the Council of Science Editors (an instance affirming the occasional professional value of the Web's volunteer-driven infosphere), freelance editor Matt Hodgkinson[36] offered a typology of review systems along a closed-to-extremely-open continuum: traditional anonymous review; open (named) prepublication review with the option of reader comments; open and permissive review, with author-solicited reviews as in Biomed Central's *Biology Direct*; community review, or true crowdsourcing, as tried briefly by *Nature* but used with more success elsewhere; permissive review with postpublication commentary; and postpublication commentary with no review. The last of these represents the purest expression of faith in unmediated crowdsourcing, as in the general academic site *Philica*, "where ideas are free," as its slogan holds, but also where frank pseudoscience[37] has proliferated.

Medical editors and reviewers agree that different systems suit different fields. "At these Peer Review Congresses," says Dr. Callaham, "there's usually a pretty wide array of disciplines represented, and the math and physics guys always kind of look at us like, 'What's your problem?'" Medicine's slower adoption of open online review puzzles them, yet the distinction may not reflect institutional conservatism so much as the different kinds of complexity and uncertainty encountered in nonclinical and clinical sciences.

"Actually," Dr. Callaham comments, "math is simple compared to real life."

Gregory W. Hendey, MD, professor of clinical emergency medicine at the University of California, San Francisco and a regular reviewer for *Annals*, concurs. "I don't mean to simplify math or physics," he says, "but I think in many basic sciences you can study things in a much more controlled laboratory setting and get black-and-white answers much more easily than you can studying how patients respond in a clinical setting. And if things are more consistent and black and white, you probably could get more consistent comments in an open forum than you could for a medical question. I'm not saying there's not a place for it in medicine; it just seems to me that the disadvantages of a purely open system would greatly outweigh any advantage."

HOW OPEN IS OPEN ENOUGH?

"There are some problems or issues with the current style of the peer review process," Dr. Hendey continues, "but I'm not sure that open peer review fixes any of those. It may address some of the issues, but it may create other problems of its own." One may be to exacerbate an existing problem: finding reviewers with the desired combination of content expertise, methodological knowledge, communication skills, and ability to commit time.

Too few journals, Dr. Hendey notes, take the trouble that *Annals* and others do to orient and train reviewers or to provide dedicated reviewers for statistics and other methodological specialties. In the online environment, he conjectures, "you might get lots of comments from people who like the paper or dislike the paper for whatever reason, but they may not have the background or experience or expertise to really make a valuable critical assessment of the paper. . . . On the plus side, you get lots of opinions; on the minus side, you're not sure how many of those opinions really count."

Quoting his colleague W. Richard Bukata, MD, clinical professor of emergency medicine at the University of Southern California, Dr. Hendey offers a useful metaphor: "Three second graders don't equal one sixth grader."

Crowdsourcing, according to business journalist James Surowiecki, can yield surprisingly accurate estimates of certain kinds of information.[38] Large groups have outperformed their individual members at estimating figures such as the weight of an ox or the number of jellybeans in a jar. What Surowiecki terms "the wisdom of crowds," however, has been less reliable in more complex, ideologically charged, or emotionally weighted types of investigations. Popular assessments of the national distributions of income and wealth, for example, make large errors underestimating inequality, as measured by psychologist Michael Norton and economist Dan Ariely in a much-cited recent article.[39,40] Even Surowiecki concedes that economic bubbles and other herd-instinct phenomena evince the tendency of groups to make disastrous judgments. As Justice Louis Brandeis said, "sunlight is the best disinfectant," but open up a process too far, or in the wrong ways, and one invites a serious sunburn.

No one involved in the peer review debates is seriously proposing moving all the functions of medical editorial review wholly into public space, ie, to the free-fire zone of the Web's comments sections or the original unmoderated Wikipedia, open not only to well-informed laypeople but to trolls, flamers, Astroturfers, the malicious, the misidentified, and the immature of all ages. Peer review migrating to the online environment needs to set a workable definition of a peer community.

In various cases, this might comprise a self-selected crowd of volunteer readers, all subscribers to a specialty journal, its designated reviewers, its editorial board, or a subset vetted for the appropriate expertise and financial disclosures. *BMJ Open* registers its volunteer reviewers, vetting their expertise and requiring them to declare any competing interests. "Any given journal has a crowd of several hundred reviewers; you could turn that into a crowdsourcing model by just allowing any of them to comment on any paper," Dr. Hendey notes. "But then, the more you take an open peer review forum and list qualifications . . . the more you're turning it into a traditional peer review system."

Another convention dismissed in the newer open arrangements is the masking of authors' identities, reviewers' (more

common), or both. The argument for either single or double blinding is that any personal consideration interferes with information being communicated in either direction. Ideally, in this view, an article and the comments on it should rise or fall on pure merit rather than rank, familiarity, or rivalry.

Practical problems immediately arise in both the ethical and operational spheres. Blind reviewing not only is frequently 1 sided and marked by unwarranted harshness but also, in nontrivial numbers of cases, it is easily seen through. Theodore C. Chan, MD, medical director of the emergency departments at both the UCSD-Hillcrest Medical Center and the Thornton Hospital in La Jolla, reviews articles for several journals, including *Annals*, and comments that in his experience, blinding appears an unrealistic goal: "Even if you blind people, sometimes they often know or suspect who the author is, because it's a small circle once you get into very select fields. So I think it's going to be difficult to try to completely erase that."

Tom Reller, vice president for global corporate relations at Elsevier, comments that in the company's focus group research, reviewers believe they can often identify authors through style, subject matter, and self-citation, particularly in "niche areas." More rigorous research[41] has found that blinding failed in 32% of cases, particularly when authors were well known, and had little effect on review quality. Dr. Rennie, holding that only fully open or fully closed review is justifiable and finding that the latter is infeasible, concludes that open systems alone are logical.[21]

David L. Schriger, MD, MPH, professor of emergency medicine at UCLA School of Medicine and an *Annals* deputy editor, observes that failures of blinding are less troublesome in this specialty than in smaller fields. "It's not like there are 20 people in emergency medicine studying some gene, and everybody who studies that gene knows who all the other people are," he says. "There [are] certain papers in emergency medicine where I'm pretty sure who wrote them, but for the most part, because the field is so diffuse—you have people doing research in emergency cardiology and emergency orthopedics and emergency this and emergency that—it's a little bit easier to blind things."

Tensions between openness and anonymity approach an irresolvable state when the discussion turns to the Ingelfinger Rule, the *New England Journal of Medicine*'s (*NEJM*'s) widely emulated policy of refusing to consider articles that have already substantively appeared in un-peer reviewed settings, "to allow time for the independent peer review of scientific findings before public dissemination and to protect the originality of content."[42] Editors Jerome Kassirer, MD, and Marcia Angell, MD, defined Web-accessible prepublication versions of an article or its data as previous publication in a 1995 editorial,[43] with explicit reference to e-print systems such as Los Alamos's arXiv. To date, authors wishing to participate in systems open enough to place data online before review must forgo the opportunity to publish in the *NEJM* and other journals observing the rule.

Not all publications are equally inclined to give "the Finger" to open-review experiments. "The *BMJ* thinks the Ingelfinger rule is outdated, not least because it can penalize authors who have shared their research appropriately to aid scientific discourse," reports Dr. Groves. Her journal, she adds, places no publication ban on research appearing in certain scenarios: results posted on clinical trial registries, presented at scientific meetings, published in non-English languages or for limited audiences, or posted in "systematic reviews and meta-analyses . . . in long, relatively reader-unfriendly versions" (eg, at the Cochrane Library or the UK Health Technology Assessment Agency).

Karen Buckley, media relations manager at the *NEJM*, dissents from Dr. Groves's characterization of the Ingelfinger policy. "It's not accurate to say that we penalize researchers who share their results in public," she states, "because we do not stand in the way—in fact, we encourage researchers to present their findings at a national scientific meeting prior to publication. Many if not most of the studies we publish have been previously presented."

The difference between these journals' stances is thus not so much over the rule's underlying principle—requiring published articles to undergo review—as over the ontological status of Web posts: are they more like conference presentations or like published articles? Are they fluid discursive processes, as Bingham saw Rhetnet, or are they discrete entities needing containment? If the question is even answerable, an answer may not appear until open review systems have established a longer track record. They may open Pandora's boxes of unmerited (even clinically dangerous) circulation of untested ideas, as the Ingelfinger rule aims to prevent, or they may stand the test of time as well as conference presentations have generally done.

The *NEJM*'s review protocols are grounded in the conviction that "the peer review process works best when it is conducted in confidence," as its editors put it in another editorial, spurred by the need to defend private communications against a subpoena from a pharmaceutical company's lawyers.[44] Having guaranteed its reviewers private communications, the journal was unwilling to rescind that promise and send an implicit message of uncertainty to future potential reviewers. The judge applying a balancing test in this case, the International Committee of Medical Journal Editors, and others have agreed.

Dr. Callaham comes down in favor of blinded review, which has consistently been *Annals*' policy, on perceptual grounds. In randomized trials, "was there a difference in quality, or a difference in recommendation for publication? There wasn't," he acknowledges, "but that's not the right question. The right question is, first of all, [whether] there's a difference in the perception of the author as to fairness. . . . It does matter if it's fair or not, but it also matters whether the author thinks it's fair. And if you're not blinded, a certain number of your authors are not going to think that that's really fair." As in many of the communicative practices that bind professional communities, beliefs and mores themselves become variables that affect assessments. Those beliefs, of course, are susceptible to change if open-review processes eventually earn enough credence to shift policies and paradigms in their direction.

CHECKING FOR CONSENSIBILITY, NOT TRUTH

UCLA'S Dr. Schriger places the peer review debates in the context of broader considerations about how units of knowledge are formed, tested, and communicated. Many problems in peer review, he contends, appear because participants expect the wrong things from the process. "Is [peer review] some type of beauty contest or popularity contest of a weird kind," he asks, "where the second most beautiful girl is voting on the first most beautiful girl?" Evaluations of that form within competitive communities are bound to bring irrelevant aspects of these communications to the foreground.

Instead of judging the methodological strengths and internal logic of an article, too many reviewers end up assessing results as if they were truth claims. Their own beliefs and clinical practices invariably make such an ambitious task harder. The primary duty for reviewers, Dr. Schriger believes, is to check whether an article has the elements of what physicist/philosopher John Ziman terms *consensibility*, the possibility that observers can recognize patterns in common, form coherent statements about them, and exchange those statements without ambiguity or undue obscurity. Consensibility is a precondition of those statements' possibly later attaining *consensuality*, which is a scientific community's best approximation of truth about a subject.[45] Rather than asking whether an article is credible, "you could say to someone, 'Is this a complete communication? Does it tell you what a reasonable person would need to know to pass judgment on its merits?' Which is a very different question than asking, 'Is this true?'"

Truth, Dr. Schriger suggests, is better determined by the wider scientific community than by a small number of reviewers. An optimized review system in his view might gauge consensibility criteria such as study design, statistical power, complete data presentation, and clinical implications as aspects of prepublication review; having different reviewers examine methods and content, as *Annals*, *NEJM*, and certain other journals do, strengthens that assessment. Once peers deem an article consensible (that is, publishable), "it's the job of the community to vet it. . . . Prepublication peer review is to make sure that the stuff is worthy of the reader's time. Period. And then postpublication peer review is what happens when people figure out, 'OK, now I've read this thing, and it was complete enough that I can have an opinion about it. Let me air my opinion along with everybody else and see what comes out in the wash.'"

A combination of pre- and postpublication review, with the latter stage invoking whatever wisdom a crowd can muster, strikes Dr. Schriger as a balanced system for putting research communications through a rationally staged sequence of tests. Current practices, he says, leave him sympathetic to the skeptical view that clinical practice is largely "disarticulated from science," that much clinical literature is "published for reasons other than advancing knowledge," and that "peer review is a generally weak force to stop that process."

The real work product of most clinical studies, he says, is "typically a multidimensional data set"—offering the theoretical possibility that an ideal publication would be a dynamic interface that lets the reader structure independent inquiries into the relations among the groups of data generated—but most publications take the form of "a static paper that says 'here's a table of variables A and B, but you can't see A and C, because we didn't make that for you.' If you think about the spectrum of what could be told about something, from the data itself all the way back to just a headline, peer reviewed or non-peer reviewed, there are huge degrees of unexplored possibility." Such limitations preclude reproducibility and thorough analysis, the processes by which a scientific community most effectively tests claims and refines new paradigms when they appear. "When you do get the Kuhnian moment," Dr. Schriger asks, "what does the peer review process do? It slows science down. And that's not necessary; that doesn't have to happen."

In this respect, the open publication formats envisioned by innovators like the *MJA* and *BMJ* editors may contribute strikingly to the advance of science, though prepublication peer review may not be the optimal stage for them. "Peer review is adding another level of complexity compared to just looking at the work itself, but it's the price you pay," Dr. Schriger concludes. It resembles a filter in a radio, calibrated to a certain signal-to-noise ratio, and "most busy clinicians or even clinical academics want that filter set towards filtering out the noise, and they're willing to lose some signal."

As professional structures adapt to search for more of the signals encoded in complex data, yesterday's leaps of faith will presumably yield to tomorrow's evidence-based decisions. Dr. Callaham is cautious not to place too much faith in information technology alone. "I'm as dependent on it and addicted to it [as anyone], having just received my latest iteration of my iPod today," he says, but "the fact is, it makes everything quicker; it doesn't make everything better. At all. And you know, what is good intellectual content, good thinking, good writing—what do they all have in common? They take a lot of time to do well. I don't care how much you're connected to the Internet, or how many people are involved. . . . What clinicians and readers want out of the peer review process is something thoughtful that they can rely upon, and that takes time to produce. The only thing that's keeping peer reviewed publishing from going out of business altogether is the fact that we need a gatekeeper for content, and that's a slow process. We still need editors, thank goodness."

Section editor: Truman J. Milling, Jr, MD
Funding and support: By *Annals* policy, all authors are required to disclose any and all commercial, financial, and other relationships in any way related to the subject of this article that might create any potential conflict of interest. The author has stated that no such relationships exist. See the Manuscript Submission Agreement in this issue for examples of specific conflicts covered by this statement.

doi:10.1016/j.annemergmed.2010.11.012

REFERENCES

1. Cohen P. Scholars test Web alternative to peer review. *New York Times*. August 24, 2010:A1. Available at: http://www.nytimes.com/2010/08/24/arts/24peer.html?sq=scholars%20web&st=cse&

scp=1&pagewanted=all. Accessed September 28, 2010.
2. Resnik DB, Gutierrez-Ford C, Peddada S. Perceptions of ethical problems with scientific journal peer review: an exploratory study. *Sci Eng Ethics*. 2008; 14:305-310. doi:10.1007/s11948-008-9059-4. Available at: http://www.ncbi.nlm.nih.gov/pmc/articles/PMC2642979/. Published March 1, 2008. Accessed September 28, 2010.
3. Bad peer reviewers [editorial]. *Nature*. 2001;413:93.
4. Engber D. Quality control: the case against peer review. *Slate*. April 5, 2005. Available at: http://www.slate.com/id/2116244/. Accessed September 28, 2010.
5. Judson HF. The problems of peer review. In: *The Great Betrayal: Fraud in Science*. New York, NY: Houghton Mifflin; 2004: 244-286.
6. Berger E. Peer review: a castle built on sand or the bedrock of scientific publishing? *Ann Emerg Med*. 2006;47: 157-159. doi:10.1016/j.annemergmed.2005.12.015. Available at: http://www.annemergmed.com/article/S0196-0644%2805%2902102-5/fulltext. Accessed September 28, 2010.
7. Judson HF. Structural transformations of the sciences and the end of peer review. *JAMA*. 1994;272:92-94. Available at: http://www.ama-assn.org/public/peer/7_13_94/pv3112x.htm. Accessed September 29, 2010.
8. Barnes J. Proof and syllogism. In: Berti E, ed. *Aristotle on Science: The Posterior Analytics, Proceedings of the Eighth Symposium Aristotelicum*. Padua, Italy: Editrice Antenore; 1981:17-59.
9. Zuckerman H, Merton RK. Patterns of evaluation in science: institutionalization, structure and functions of the referee system. *Minerva*. 1971;9:66-100.
10. Harnad S. Arnold Relman's *NEJM* editorial about NIH/E-biomed. American Scientist Forum (listserv posting), July 19, 1999. Available at: http://listserver.sigmaxi.org/sc/wa.exe?A2=ind99&L=american-scientist-open-access-forum&D=1&F=l&P=20403. Accessed September 28, 2010.
11. Biagioli M. From book censorship to academic peer review. *Emergences J Study Media Composite Cultures*. 2002; 12:11-45.
12. Callaham ML. Research into peer review and scientific publication: journals look in the mirror. *Ann Emerg Med*. 2002;40: 313-316. Available at: http://www.annemergmed.com/article/S0196-0644%2802%2900045-8/fulltext. Accessed September 28, 2010.
13. Callaham ML, Knopp RK, Gallagher EJ. Effect of written feedback by editors on quality of reviews: two randomized trials. *JAMA*. 2002;287:2781-2783. Available at: http://jama.ama-assn.org/cgi/content/full/287/21/2781. Accessed September 28, 2010.
14. Green SM, Callaham ML. Current status of peer review at *Annals of Emergency Medicine*. *Ann Emerg Med*. 2006;48: 304-308. Available at: http://www.annemergmed.com/article/S0196-0644%2806%2901015-8/fulltext. Accessed September 28, 2010.
15. Jefferson T, Wager E, Davidoff F. Measuring the quality of editorial peer review. *JAMA*. 2002;287:2786-2790. Available at: http://jama.ama-assn.org/cgi/content/full/287/21/2786. Accessed September 28, 2010.
16. Baxt WG, Waeckerle JF, Berlin JA, et al. Who reviews the reviewers? feasibility of using a fictitious manuscript to evaluate peer reviewer performance. *Ann Emerg Med*. 1998;32:310-317. Available at: http://www.annemergmed.com/article/S0196-0644%2898%2970006-X/fulltext. Accessed September 28, 2010.
17. Nylenna M, Riis P, Karlsson Y. Multiple blinded reviews of the same two manuscripts: effects of referee characteristics and publication language. *JAMA*. 1994;272:149-151. Available at: http://jama.ama-assn.org/cgi/content/abstract/272/2/149. Accessed September 29, 2010.
18. Godlee F, Gale CR, Martyn CN. Effect on the quality of peer review of blinding reviewers and asking them to sign their reports: a randomized controlled trial. *JAMA*. 1998;280:237-240. Available at: http://jama.ama-assn.org/cgi/content/full/280/3/237. Accessed September 28, 2010.
19. Schroter S, Black N, Evans S, et al. What errors do peer reviewers detect, and does training improve their ability to detect them? *J R Soc Med*. 2008;101: 507-514. doi:10.1258/jrsm.2008.080062. Available at: http://resources.bmj.com/bmj/about-bmj/about-bmj/evidence-based-publishing/What%20errors%20do%20peer%20reviewers%20detect.pdf. Accessed September 28, 2010.
20. Rennie D. Guarding the guardians: a conference on editorial peer review. *JAMA*. 1986;256:2391-2392.
21. Rennie D. Freedom and responsibility in medical publication: setting the balance right. *JAMA*. 1998;280:300-302.
22. Rennie D, Flanagin A, eds. This week in *JAMA*. Peer Review Congress IV: a *JAMA* theme issue. *JAMA*. 2002;287:2749. Available at: http://jama.ama-assn.org/cgi/content/full/287/21/2749. Accessed September 28, 2010.
23. Smith R. Opening up *BMJ* peer review. *BMJ*. 1999;318:4-5. Available at: http://www.bmj.com/content/318/7175/4.full. Accessed September 28, 2010.
24. van Rooyen S, Godlee F, Evans S, et al. Effect of open peer review on quality of reviews and on reviewers' recommendations: a randomised trial. *BMJ*. 1999;318:23-27. Available at: http://www.bmj.com/content/318/7175/23.full. Accessed September 28, 2010.
25. Smith RW. In search of an optimal peer review system. *J Participat Med*. 2009; 1:e13. Available at: http://www.jopm.org/opinion/2009/10/21/in-search-of-an-optimal-peer-review-system/. Accessed September 29, 2010.
26. Smith R. The future of peer review. In: Godlee F, Jefferson T, eds. *Peer Review in Health Sciences*. 2nd ed. London, England: *BMJ* Books; 2003:329-346. Available at: http://resources.bmj.com/bmj/pdfs/smith.pdf. Accessed September 29, 2010.
27. *BMJ Open*. Available at: http://blogs.bmj.com/bmjopen/. Accessed September 28, 2010.
28. Greaves S, Scott J, Clarke M, et al. Overview: *Nature*'s peer review trial [editorial]. *Nature*. 2006. doi:10.1038/nature05535. Available at: http://www.nature.com/nature/peerreview/debate/nature05535.html. Accessed September 30, 2010.
29. Bingham CM, Higgins G, Coleman R, et al. The *Medical Journal of Australia* Internet peer review study. *Lancet*. 1998;352:441-445. doi:10.1016/S0140-6736(97)11510-0. Available at: http://www.thelancet.com/journals/lancet/article/PIIS0140-6736%2897%2911510-0/fulltext. Accessed September 29, 2010.
30. Bingham C, Van Der Weyden MB. Peer review on the Internet: launching eMJA peer review study 2. *Med J Aust*. 1998; 169:240-241. Available at: http://www.mja.com.au/public/issues/sep7/bingham/bingham.html. Accessed September 29, 2010.
31. Available at: http://www.mja.com.au/public/rop/contents_rop.html. Accessed September 29, 2010.
32. Bingham C. Peer review on the Internet: a better class of conversation. *Lancet*. 1990;351:10-15. (Supplement Internet guide.) Available at: http://www.thelancet.com/journals/lancet/article/PIIS0140-6736(98)90307-5/fulltext. Accessed September 28, 2010.
33. *RhetNet*. Available at: http://wac.colostate.edu/rhetnet/. Accessed September 29, 2010.
34. Odlyzko AM. Tragic loss or good riddance? the impending demise of traditional scholarly journals. *Intern J Human Computer Studies*. 1995;42:71-122. doi:10.3217/jucs-000-00-0003. Available at: http://www.jucs.org/jucs_

0_0/tragic_loss_or_good/Odlyzko_A.html. Accessed September 29, 2010.
35. Kling R, Fortuna J, King A. The remarkable transformation of E-Biomed into PubMed Central. CSI Working Paper No. WP-01-03. Indiana University Center for Social Informatics, 2002. Available at: http://rkcsi.indiana.edu/archive/CSI/WP/wp01-03B.html. Accessed September 27, 2010.
36. Hodgkinson M. Open peer review & community peer review. *Journalology* [blog]. Available at: http://journalology.blogspot.com/2007/06/open-peer-review-community-peer-review.html. Accessed September 30, 2010.
37. Kelly A. The intelligent design of the cosmos. *Philica.com*. 2006, article no. 50. Available at: http://philica.com/display_article.php?article_id=50. Accessed September 30, 2010.
38. Surowiecki J. *The Wisdom of Crowds*. New York, NY: Doubleday; 2004.
39. Norton MI, Ariely D. Building a better America—one wealth quintile at a time. *Perspect Psychol Sci*. In press. Available at: http://www.people.hbs.edu/mnorton/norton%20ariely%20in%20press.pdf. Accessed September 28, 2010.
40. Noah T. Theoretical egalitarians: why income distribution can't be crowd-sourced. *Slate*. September 27, 2010. Available at: http://www.slate.com/id/2268872/. Accessed September 28, 2010.
41. Justice AC, Cho MK, Winker MA, et al. Does masking author identity improve peer review quality? *JAMA*. 1998;280:240-242. Available at: http://jama.ama-assn.org/cgi/content/full/280/3/240. Accessed September 30, 2010.
42. About *NEJM* editorial policies: no prior publication. *N Engl J Med*. Information for authors. Available at: http://www.nejm.org/page/about-nejm/editorial-policies. Accessed September 30, 2010.
43. Kassirer JP, Angell M. The Internet and the *Journal*. *N Engl J Med*. 1995;332:1709-1710. Available at: http://www.nejm.org/doi/full/10.1056/NEJM199506223322509#t=article. Accessed September 30, 2010.
44. Curfman GD, Morrissey S, Annas GJ, et al. Peer review in the balance [editorial]. *N Engl J Med*. 2008;358:2276-2277. Available at: http://www.nejm.org/doi/full/10.1056/NEJMe0803516. Accessed September 28, 2010.
45. Ziman J. *Reliable Knowledge: An Exploration of the Grounds for Belief in Science*. Cambridge, MA: Cambridge University Press; 1978.

More Than Bike Helmets and Car Seats

EDs Step Up Role in Pediatric Injury Prevention

by MARYN MCKENNA

Special Contributor to Annals News & Perspective

Unintentional traumatic injuries are the leading cause of death for children in the United States, sending them to the emergency department (ED) 9.2 million times per year.[1] The more than 12,000 child deaths per year from injury total more than all child deaths from all infectious diseases combined, yet compared with diseases, injuries are not the target of high-profile, broad-based prevention campaigns.

In scattered spots around the United States, hospitals and emergency physicians are taking a second look at those statistics and asking whether EDs can play a role in reducing the likelihood of child injury. They suggest that, at the least, EDs may be able to help reduce the rate of reinjury or help identify families in which a second child is also likely to be injured.

For more than a decade, some EDs have played a preventive role by helping to distribute bike helmets and car seats. Those concerned with the continued high rate of childhood injury say there is more that EDs can and should do. The challenge is identifying which of those 9.2 million visits—which children and which families—would most benefit from additional intervention, as well as figuring out how to create interventions that can be delivered or at least begun in the time-pressed environment of an ED.

Researchers say a renewed focus on prevention could not only protect individual children and their siblings but also illuminate patterns of injury in communities. Specialists say what's needed is both the public health approach of gathering data and a rethinking of the clinical encounter with an individual family.

"Emergency departments have a tremendous obligation to be part of either local or regional and national surveillance activities," said Joseph L. Wright, MD, MPH, who is senior vice president at the Children's National Medical Center and a professor of emergency medicine, pediatrics, and health policy at George Washington University. "Being able to get as much information about the antecedents and circumstances of injuries that present themselves to emergency departments is a fundamental activity. It is the most frustrating circumstance to go into a medical record written by an emergency physician and find nothing more than 'child injured.'"

Throughout more than a decade, Dr. Wright's team at the center has been abstracting data from ED records to identify patterns of injury in children and teens in Washington neighborhoods.[2-6] The effort, which began when the center's charts were still entirely paper based, has helped identify teens involved in neighborhood violence and link them with community programs and personal mentors and also identify parts of the city in which younger children are more likely to be scalded in kitchen accidents.

In California, an effort by the state Emergency Services for Children Program collects 2 streams of electronic data as part of the California emergency medical services (EMS) information system, one from the state's designated trauma centers and another from its local EMS agencies. A recent pass through the trauma data, which includes E-codes, uncovered a significant spike in skateboard injuries (E 885.2) among preteens, rank-