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RESEARCH ARTICLE

The Role of Metadiscourse in Negotiating Trust in the Communication of Science by Blog

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ABSTRACT

Advocates for open systems in science make claims for their efficient collaboration and transparent communication. Although these characteristics are consistent with the traditional norms of science, the implementation of open systems has had mixed effects, particularly on the role of trust. This case study of the published correspondence in research journals suggests that when communication moves from traditional print systems to open on-line systems, two levels of trust arise, one at the discourse level and another at the metadiscourse level. The coincidence and conflation of discourse in these two registers both ameliorate and trouble trust in the communication of science. Taken together, these methodological issues raise doubts about the validity of Wolfe-Simon et al.'s assertion … (Redfield, 2011). I don't know whether the authors are just bad scientists or whether they're unscrupulously pushing NASA's 'There's life in outer space!' agenda (Redfield, 2010).

KEYWORDS: Awareness, Blog, Metadiscourse

Introduction

In December of 2010, Wolfe-Simon and her colleagues published a research paper in *Science* that provoked much criticism from their research community. This criticism was published as printed correspondence in June of 2011. Rosie Redfield's critique (Redfield, 2011) was among this correspondence. This sequence of published research and

published correspondence is typical of the publication of research and is effective if not efficient. In this case, however, the criticism of Wolfe-Simon, *et al.'s* research spilled into the blogosphere when Redfield posted her critique (Redfield, 2010) almost on the heels of the research paper. Redfield's two critiques differ in their timeliness, but they also differ in style. In the printed correspondence (Redfield, 2011) her persona recedes, and her statements seem value neutral; in the blog post (Redfield, 2010) her persona intrudes and her statements seem value laden. Science by blog in this case was efficient if not always civil. Redfield's blog attracted the attention of commenters, other bloggers, and the media, and seemed to generate so much controversy over Wolfe-Simon *et al.'s* research in the public sphere that *The Scientist* included it as one of its 'Top Science Scandals of 2011' (Ghose, 2011).

Science blogs are among the various new on-line technologies for ameliorating, if not correcting, the troubles with conventional systems for doing and communicating science. For the most part these technologies have in common an open character. The potentially universal access of the Internet makes them radically democratic. Their resources can be available and transparent to all users, and their services can be communal so as to support collaboration in both disciplinary and trans-disciplinary enterprises. This open character has import for both the efficiency of the process of communication, but we expect it to affect also the negotiation of trust among its participants.

Open systems in science are a subset of a larger movement towards a paradigm of network collaboration, which includes open source software, the open education movement, crowdsourcing, open access journals, open peer review, and citizen science. Scientists' embrace of such systems is part of a larger optimism. Von Krogh and Spaeth (2007), for example, laud the open source software for five characteristics: impact, theoretical tension, transparency, communal reflexivity, and proximity. Kelty *et al.,* (2008) express hopes in the open education movement for its prospects for making diverse and high-quality teaching and learning materials freely available to everyone. Albors *et al.,* (2008) assert that the tension on the internet between democratizing open systems and the forces of

intellectual property and the profit motive is not necessarily irreconcilable.

In science, the embrace of open systems has been both enduring and transitory. Open access journals like *PLoS One* seem destined to exist side-by-side with the proprietary publication of science. Confidence in open peer review systems, on the other hand, has been equivocal, as exemplified by the experiences of *Nature* (Greaves *et al.*, 2006). Huss et al., (2010) promote what they call community intelligence in the efforts of Gene Wiki to create through collaboration and continuous review by the community an article for every gene in the human genome. Levina (2010) goes so far as to assert that citizen science, as it is practiced by those who share their personal genetic information, is 'an act of citizenship' (p. 7). They temper these hopes with the admission that since wiki users are anonymous, doubts about trust in the accuracy and objectivity in their contributions inevitably arise. Dickinson, *et al.*, (2010) note that trust in citizen science is problematic for the same reason, but they seem confident that more sophisticated and rigorous data collecting protocols and more effective strategies for evaluating observer quality will help.

Blogs are one species of open systems that have provoked particular concerns over trust as they have emerged as an important democratizing component of Web 2.0. Although science blogs comprise only a small percentage of all blogs, they have contributed to the opening up of science to the public. Research on blogs in general is already considerable (Kenix, 2009); research on science blogs is less so.

Since their appearance science blogs have provoked considerable commentary about the wisdom of their use. Traditional perspectives on science have been predictably skeptical; Willard (Bonetta, 2007), for example, asserts categorically that blogging is 'antithetical' to the paradigm of his generation of scientists (p. 444). One of the most comprehensive, if almost entirely laudatory, commentaries on science blogs is Wilkins's (2008), who asserts enthusiastically that scientists should blog both to enhance their practice of science and to promote it to the public. Other commentaries fall more cautiously between Willard and Wilkins. Batts *et al.*, (2008) and

Bonetta (2007) both worry about the quality control of science blogs. Murray (2010) and Butler (2005) both worry about credibility. Bubela *et al.,* (2009) believe that science blogs can vet false claims, but Brunfiel (2009) doubts that blogs can be watchdogs. Such uncertainties over the quality of science blogs is troubling because, as Brunfiel (2009) and Bonetta (2007) both note, journalists and the mass media look to blogs for ideas and story leads. In fact, Batts (2008) and Wilkins (2008) both refer to science blogs as bridges between scientists and the public. Wilkins asserts, '[T]he public should see science during its manufacture' (p. 411). In fact, through at least one radically open science blog, *Open Notebook Science*, the public can see science in the making, both its successful and its 'failed' experiments (Bradley *et al.* 2011).

Research on science blogs, particularly in matters of trust and credibility, is much more scant. One of the earliest attempts to examine a science-related blog is Sundar *et al.,* (2007), who look at the mental-health blogs and raise questions about their credibility. Buis and Carpenter (2009) follow up on Sundar *et al.,* to discover differences in health and medical blog content between credentialed and noncredentialed bloggers. The most recent work on science blogs is Kouper's (2010), who points to some of the troubling aspects of the 'water cooler' quality of science blogs, 'quick personal judgments, insulting and sarcastic remarks, and personal details' (p. 8), and notes that for them to be useful, 'science blogs need to stabilize as a genre' (p. 8). Such a rhetorical dimension of science blogs is in fact the focus of our study.

The goal of this paper is to examine what happens to trust when the communication of science moves from conventional systems to open on-line systems. Do open systems successfully address the problems of trust in conventional systems? Do the open systems create new problems for trust? To answer these questions we present a case study of science communication in chemistry that is Janus-like: it looks back on the old system and looks ahead to the new. To examine the role of trust in this case we will: (1) identify the issues of trust that typically trouble science; (2) locate the points in the publication of science where trust over these issues is relevant; and (3) focus on the specific

textual artifacts where trust is operative and explicit. In examining this case we hope to see whether the notion of trust in science communication systems has evolved as scientific discourse communities embrace web technologies, and if it has, to suggest hypotheses for more comprehensive studies of such systems.

Conceptual Framework and Method of Analysis

Science succeeds because of effective collaboration among scientists. Collaboration works to the extent that scientists trust each other. Most contemporary scientific research projects depend on the efforts of many individuals and different disciplines. A dramatic, if atypical, example is the human genome project. Venter *et al.'s* (2001) research paper in *Science* reporting on the complete encoding of the human genome had 273 authors on its byline. Such science entails a process of discovery that demands more time and effort than any individual is capable of. Moreover, no single individual has the intellectual resources to mount the argued justifications that scientific discovery requires. Thus, the production of science is possible to the extent that scientists trust each other. Hardwig's (1991) conclusion to this line of thinking may seem unsavory to those who are committed to logico-empiricism: '[M]uch of our knowledge rests on trust in the moral character of testifiers.' (p. 708)

If trust is such an important dimension of science communication, what exactly is it based on? The Mertonian norms of science, as augmented by Ziman (2000) and countered by Mitroff (1974), seem to comprehensively answer that question: communalism, universalism, disinterestedness, originality, and skepticism. These norms, Ziman has noted, characterize the public face of scientists. Scientists, in fact, are often held accountable by their peers (for example, in the context of peer review) on the basis of Merton's norms. But given that scientists are human and fallible, their private lives are as likely to be characterized by Mitroff's counter-norms: solitariness, particularism, interestedness, redundancy, and dogmatism.

Merton's norms have been around since 1942, so concerns about their enduring validity require some attention. Mulkay (1969) raised doubts about their validity early on, and subsequently their relevance has come under attack because of alleged changes in the nature of science through privatization and industrialization. Nevertheless, appreciative references to Merton's norms continue to pepper the literature in the sociology of science and elsewhere. Huff (2007) and Enebakk (2007) each trace the historical roots of Merton's norms. Huff claims that the norms represent 'deep structures grounding the ethos of science' (p. 207). Enebakk refers to the norms' 'continued relevance' (p. 235). Anderson, *et al.,* (2010) report on their focus group and survey data that suggest some assent among practicing scientists over the relevance of Merton's norms. Bray (2010) proposes a scheme for extending Merton's norms from the context of research to the ancillary context of academic deanships.

Even among those who continue the critique of Merton's norms there are expressed hopes for salvaging them. Kellogg (2006) describes the differences between science as Merton exemplified it in his defense of the norms and science as it is practiced in 21st century post-academic science. He invokes Ziman's (2000) norms of industrial science (proprietary vs. communal, local vs. universal, authoritarian vs. disinterested, commissioned vs. original, and expert vs. skeptical), but only to supplement, not to supplant Merton's. Bauer (2004) likewise acknowledges that the norms no longer characterize the conduct of late 20th century scientists, and for that reason, 21st century science under the influence of corporations has created 'knowledge monopolies' (p. 643). Nevertheless, he suggests that an antidote for this corruption might be 'something like the Mertonian norms' (p. 645). Cook-Deegan (2007) also acknowledges the legitimacy of proprietary R&D, but asserts that the training of all scientists, whether they occupy the science commons or corporate R&D, must rest on Merton's norms. Perhaps the most trenchant critique of Merton's norms is Barnes's (2007), who notes how the 'intransitivity of sameness' (p. 189) makes the applications of these norms to what he calls techno-science (p. 188) difficult; nonetheless, he concludes:

> [T]here are few better ways of recognizing these, and avoiding the misconceptions they can encourage, than by making use of frameworks and schemata developed from the work of the great classical sociologists, the work which so productively enriched the thought of Robert Merton and among which his own work now indisputably belongs. (p. 190)

Our study attempts to push the application of the norms farther into the digital recesses of 21st century technoscience.

The Mertonian norms and their corresponding counter-norms have particular relevance to the publication of scientific research. Communalism is the ideal that makes the work of scientists freely accessible to their wider scientific community. The publishing and archiving of research reports are tangible outcomes of this ideal. Scientists, within the constraints that might arise from proprietary interests and concerns over national security, are expected to share openly in the research enterprise. But if they are obsessed with historical priority, they may be inclined towards a counter-norm of solitariness (Mitroff, 1974) and be tempted to work in secrecy. Universalism is the ideal that makes age, race, nationality, or gender irrelevant in the practice of science. Scientists, in other words, ideally are expected to evaluate the research, not the researcher. Scientists in the real world, on the other hand, may seem to be working from a counter-norm of particularism (Mitroff, 1974) if as reviewers they judge research with regard to the identity of the researcher rather than the quality of the research itself. Disinterestedness is the ideal that creates a wall between scientists' research interests and their personal beliefs, attitudes, and values. Scientists, then, are expected to be impartial — to maintain a separation between the observed and the observer. In the practice of science, however, the search for profit can trump the search for truth. The interestedness of scientists becomes evident when conflicts of interest are revealed after the fact. Originality is the ideal by which scientists receive credit for producing new knowledge. Scientists, therefore, are worthy of praise for their work but must also acknowledge the help and foundations of others. When scientists in the trenches are struggling for tenure and promotion, the emphasis on citation counts and impact factors may tempt

them to embrace redundancy. Skepticism is the ideal that enjoins scientists to submit their work to the scrutiny of their peers. Under this norm the interpretations that scientists assert for their data must be affirmed by their colleagues. In the real world, however, these colleagues may allow their judgments to become dogmatic and ideological if, for example, they fail to recuse themselves from reviewing research that is outside their area of expertise.

The Mertonian norms, as Sztompka (2007) argues, constitute a basis for trust among scientists, and thus they become helpful for identifying the issues of trust in research publication. In order to precisely locate the occasions where trust is crucial in research publication, we adopt Hummel and Hans's (2001) typology, which posits four functions in the publication of research: registration, certification, archiving, and awareness. Registration represents the researchers' point of entry into the publication cycle. At this moment an author must trust the reputation of the journal, and the editor must trust the identity of the author. The recorded date and time mark the event for the sake of validating the author's historical priority of publication. Traditionally, this act was administrative and more recently has been automated by electronic manuscript management systems.

Certification is the function embodied in the traditional peer review process and the wider approbation of the post peer review as found in the correspondence, comments, and corrections sections of journals. Trust at this point in the publication of research links authors, editors and referees in specific ways as collected by Hames (2007): (1) authors must trust editors to oversee the peer review of their manuscripts; (2) editors must trust referees to be efficient and fair; and (3) referees must trust editors to be grateful and confidential. The closed and usually anonymous nature of traditional blind peer review both ensures and troubles this process.

In the age of print journals the function of archiving was more remote from the daily concerns of scientists. Though they depended on libraries (mostly) to maintain the physical scientific record and its bibliographic data, they trusted professional librarians to do so. The commercialization of research journals has complicated the archiving function in that now the scientific

record is not always accessible without cost. Whether the scientific record resides in bricks and mortar or on a corporate disk drive, the integrity of that record is vulnerable to the vagaries of the publishing process that creates the record in the first place. In particular, any given research community must trust that the archive is safeguarded against corruption by invalidated research — that is, errors and retractions.

As originally conceived, the function of awareness in research publication referred to the process by which the wider scientific community understands and synthesizes newly published research. The component of trust in this function arises between author and reader. Can the reader trust the author to present an argument for the research with accuracy, clarity, and concision? Although the function of awareness must eventually include the general reader, in conventional research publication that job was usually relegated to science writers and public relations professionals.

These four functions help to locate discussions over trust; what remains is a means to identify actual expressions of that trust. Students of human communication note that language is used in two registers: (1) to talk about nature and our experiences of nature, and (2) to talk about the process and the occasion of talking about nature. The latter use of language, metadiscourse, calls attention to the words rather than to the objects that the words refer to. Metadiscourse enables us to talk about our talk and in so doing comment of the presence of the talker and the listener. Such comments can express or imply the talker's attitude towards the listener and might even attempt to exert an influence on the listener to react to the speaker in a certain way. Such expressions Van de Kopple (1985) subsumes under the rubrics 'discourse about discourse or communication about communication' (p. 83). With metadiscourse speakers can convey to listeners their personality, credibility, and attitudes. Such attributes are central to issues of trust among interlocutors, particularly in the context of science, as Crismore and Farnsworh (1989) found in their study of Darwin's use of metadiscourse in his *Origin of Species*.

Metadiscourse, in fact, is no stranger to science. Even research reports break the old prohibition against a first-person

point of view. The introduction and discussion sections, as Williams (1997) points out, now include the familiar 'we hypothesize' and 'we conclude,' but such metadiscourse rarely intrudes on the more overtly objective methods and results sections. Such locutions are innocuous enough, although in the interests of economy of style Williams (1997) has called for restraint in the use of metalanguage in formal writing of any kind.

Metadiscourse is more troubling in the communication of science when the style of writing collides with the epistemology of its context of use. The distinction between discourse and metadiscourse reflects the broader philosophical distinction between the use of a word and the mention of a word as in the sentences, 'John has four letters' and '*John* has four letters.' In the first instance the grammatical subject of the sentence is object language; in the second the subject is metalanguage. In the context of science where the purported focus is on the observed, not the observer, metalanguage can blur the separation between subject and object. Hyland (1998) seems to work from such an assumption about academic metadiscourse when he states, '[T]he suppression of personal agency is often considered to be a means of concealing the constructedness of accounts' (p. 452).

Our study attempts to examine metadiscourse in the publication of research: registration, certification, archiving, and awareness. The goal is to see whether the adherence to or departure from the norms of science are reflected in the content. Van de Kopple's (1985) seven types of metadiscourse give a sense of its breadth: text connectives (*e.g.*, 'first of all…'), code glosses (*e.g.*, 'defined as…'), illocution markers (*e.g.*, 'We hypothesize…'), validity markers (*e.g.*, 'presumably…'), narrators (*e.g.*, 'She claims…'), attitude markers (*e.g.*, 'I am surprised to read…'), and commentary (*e.g.*, 'Consider the case of…'). The last three are of special interest in this analysis because they point to the intersection of subject and object. Crismore and Farnsworth (1989) use the same subset of Van de Kopple's typology because they are associated with ethical and emotional appeals — a consideration of importance to the case study below. In order to operationalize these constructs, Adel

(2006) recombines them and distinguishes between personal and impersonal metadiscourse. Our conception of this term is closer to Hyland and Polly's (2004), who regard all metadiscourse as interpersonal (and therefore rhetorical) such that it simply reflects expressions of an author's attitude, comments about the text, and directions to the reader (p. 167).

Since the literature on metadiscourse suggests degrees of an author's presence in a text, our study will be sensitive to this dimension. For example, Kuhi and Behnam's (2011) definition suggests that metadiscourse is almost accidental, calling it 'implicit and explicit traces of writers' desires for promotion, identity, and power' (p. 131). Dahl's (2004) metadiscourse seems more deliberate if still weak: 'Metadiscourse may be broadly described as overtly expressing the writer's acknowledgement of the reader' (p. 1811). Hyland's (1998) metadiscourse is more active: '[M]etadiscourse focuses our attention on the ways writers project themselves into their work to signal their communicative intentions' (p. 437). Halliday and Hasan's (1976) metadiscourse is the most aggressive: '[It] represents the speaker in his role as intruder' (p. 26). The analysis of metadiscourse below adopts the strategies of de Oliveira and Pagano (2006), who compare the texts of research papers with those of popularizations of science and of Kuhi and Behnam (2011), who examine metadiscourse across intermediate genres of science (e.g., textbooks), to explore the extent to which the author's presence is evident in the use of metadiscourse in post-publication correspondence as it moves from traditional print to more contemporary science blogs.

The Arsenic Case

This case study involves a dispute over the validity of a published research paper that was criticized both in the journal's printed correspondence and in the blogosphere. For that reason it seems apt for examining the differences between the two discourse communities. It begins with the chemical similarities between phosphorus and arsenic. Chemists have long wondered whether living cells might substitute one for the other. Wolfe-Simon *et al.*, (2010) claimed as much in their *Science* paper

'A Bacterium That Can Grow by Using Arsenic Instead of Phosphorus.' Although their conclusion is appropriately hedged, their claims are still extraordinary:

> Our data show evidence for arsenate in macromolecules that normally contain phosphate, most notably nucleic acids and proteins. Exchange of one of the major bio-elements may have profound evolutionary and geochemical significance (Wolfe-Simon, 2010:1163).

Since NASA funded Wolfe-Simon's research, they took advantage of this extraordinary conclusion to promote the agency's agenda by conducting a press conference and by issuing a press release, which stated in part: 'This finding of an alternative biochemistry makeup will alter biology textbooks and expand the scope of the search for life beyond Earth.' This translation from research to public relations caused considerable loss of modesty and moderation. Any skepticism among Wolfe-Simon's colleagues could only have been exacerbated by this change in tone to the extent that *Science* received many critiques of the paper and published eight. For example, Benner (2011) suggests, 'The actual numbers reported by Wolfe-Simon *et al.*, describing the ratio of arsenic to phosphorus in various subcellular fractions do not allow us confidently to rule out an alternative hypothesis.' Cotner and Hall (2011) also suggest another explanation for high levels of arsenic reported in the original paper. Borhani (2011) points to 'data inconsistencies' and explanations that require 'unprecedented mechanisms.' Csabai and Szathmáry (2011) assert that the original claim rests on insufficient data. Oehler's (2011) critique is similar, calling the original results only preliminary.

All of these critiques are expressed with the measured caution that characterizes the style of research reports themselves. Redfield's (2011) remarks are typical: 'Although the researchers meticulously eliminated contamination of the reagents and equipment used in their elemental analyses, they made much less effort to eliminate contamination in their biological samples' and 'Taken together, these methodological issues raise doubts about the validity of Wolfe-Simon *et al.'s* assertion that GFAJ-1 can vary the elemental composition of its

biomolecules by substituting As for P.' The style of this writing should not seem surprising since the critiques themselves were peer reviewed and edited, as their editor (Alberts, 2011) notes in his introduction: 'They have been peer-reviewed and revised according to *Science*'s standard procedure' (p. 1149).

These comments, including Redfield's, first appeared in *Science* Express on May 27, 2011. But Redfield had distributed an earlier draft of her critique on December 4, 2010. *Redfield's Research Blog* (Redfield, 2010) ostensibly and substantively plays a role in the conventional form of peer review. She offers a critique of Wolfe-Simon's research report in *Science* in very pointed ways. In assessing the truthfulness or falsity of the research, Redfield becomes an unappointed peer reviewer. Her comments are more accurately called post-peer review of the sort that are found in the correspondence sections of journals. However, the tone of the remarks in her blog is markedly different from that in her comment to *Science*. She complains: 'Lots of flim-flam, but very little reliable information.' Then she raises the stakes by wondering, 'I don't know whether the authors are just bad scientists or whether they're unscrupulously pushing NASA's 'There's life in outer space!' agenda.' Finally, in her most unguarded moment, she rhetorically asks, '[I]s this a shabby trick to increase their superficial similarity?' (Redfield, 2010)

The primary shift in the commentary on Wolfe-Simon's research from *Science* to Redfield's blog is in the register of its discourse. The comments as peer-reviewed and edited in *Science* maintain the customary boundary between the observer and the observed, the subject and the object. In that context the various commenters, including Redfield, address their remarks to the validity of the research. For example, Anonymous (Redfield, 2010) asked: '[W]hy did they not just determine the molecular weight of the 'arsenic' DNA vs. normal DNA?' (12:10 AM) By contrast, Redfield in her blog mingled comments about the observed with comments about the observers. Such a change arose from Redfield's use of words about nature to her use of words about the words about nature — an example of metadiscourse.

In blogs the function of registration is automated: all posts are stamped with time and author. Trust, however, remains problematic because blogs are easily edited and allow anonymous posts. Such anonymity mirrors that of traditional blind peer review, but blogs lack the editorial oversight of anonymous referees. Many of the anonymous comments on Redfield's blog seem scientifically informed and sufficiently civil, but no one is available to verify the pedigree of such posts. Concerns over anonymous comments, in fact, are explicit in Redfield's blog. For example, at one point an exchange between two commenters included: 'How do you expect anyone to take YOU seriously if you don't even write your name?' (RKA, 12:55 PM, in Redfield, 2010). Some commenters, on the other hand, have taken refuge in their anonymity: 'I'm staying anonymous because I'm a PhD candidate so my career hasn't really started yet, and somehow getting wrapped up in a debate like this isn't something I'm interested in pursuing.' (Anonymous, 7:36 AM, in Redfield, 2010) The personal metadiscourse in these two comments do not point to a successful registration process based on a norm of disinterestedness.

In a research blog, which can be both thoroughly anonymous and radically transparent, the trust in the process of evaluating research certification becomes trickier still. On the one hand the tone of blog posts and comments is marked by considerable candor and self-deprecation. For example, one commenter on Redfield's blog said: 'So I was wrong to claim…' (AMac, 12:18 PM, in Redfield, 2010) For the most part, blog comments appropriately contribute to the discussion over the question at hand: Is this research valid? The same blog comments section, on the other hand, often includes remarks seemingly beside the point that impute motives to the behavior and discourse of others, such as, 'The tone taken by Dr. Wolfe-Simon in the NASA TV interview felt like she was trying to sell me something, rather than explain the data.' (J. Gralnick, 7:50 AM, in Redfield, 2010) At the same time comments in this sort of tone often include critiques of others, like this one critical of Redfield: 'What I can't appreciate is your nasty and condescending tone towards what quite likely was science

honestly conducted, if in some ways imperfect.' (Anonymous, 11:33 AM, in Redfield, 2010).

The metadiscursive validity markers ('I was wrong') and the attitude markers ('sell me something' and 'condescending tone') in these remarks point to two levels of trust in the certification function of the blog: (1) questions whether scientists trust each other to be impartial towards each other's research; and (2) questions whether scientists trust each other to be impartial towards each other's character. Both levels reflect concerns over the norm of disinterestedness.

As noted above, the research and commentary on conventional publication of science reveal an imperfect archive of science. A commenter on Redfield's blog, in fact, suggests that science by blog will ameliorate this vulnerability:

> That attitude has led to the situation we find ourselves in today with all kinds of very bad science sitting out there in the scientific literature. The experts know that the papers are flawed but nobody else does. That's because no expert wants to waste time writing up a critique that has a very slim chance of possibly being published several months from now. Instead, they'll just make sure all their colleagues, students and postdocs know enough to ignore the paper. Blogs are just making this process more public and that's good thing. (Larry Moran, 9:14 AM, in Redfield, 2010).

Unfortunately, the scientific record as it exists in blogs like Redfield's has its own vulnerability. Since blogs offer the ability to revoke comments, efficient corrections and retractions are possible. One of Redfield's commenters, however, feared that their remarks were removed for less beneficient reasons. One commenter remarked, perhaps coyly: '[M]y comment seems to have been removed.' (Matt Young, 1:27 PM, in Redfield, 2010) Another said: 'Redfield has also deleted all of the posts on this page which solidly refute her claims.' (Pierre Pequebot, 3:41 AM, in Redfield, 2010) Redfield reassured her commenters about the integrity of her blog's record: 'The only comments I've deleted were three spam comments with links to commercial sites in Asia.' (Rosie Redfield, 10:44 AM, in Redfield, 2010).

However, without any third-party editorial oversight, when Redfield's audience reads her validity marker ('only comments I deleted'), they must trust her to be disinterested.

Perhaps the function in the publication of science that is most troubled by the use of blogs is awareness. In the world of science by blog, the audiences of colleagues and of the general public become merged. Now trust between author and reader becomes much more fragile. Scientists of a given community circumscribed by conventional peer review can no longer assume any common values or norms when the general public is among their audience. If anything, given the contemporary political climate, they should assume an audience indifferent if not hostile to such values. Many of the commenters on Redfield's blog seem acutely aware of this new rhetorical challenge for those who conduct science by blog. For example, one commenter laments:

> What really, really pisses me off is that science can only lose by this story. Either everything gets cleared up in academic circles and the public never knows how badly they were tricked with this paper, or the backlash against this paper reaches the masses, feeding the anti-science sentiments which are already rather concerning. (btm, 3:07 PM, in Redfield, 2010).

Even a commenter who admits to being among the general readers realizes that they are now privy to what had been an almost private conversation before the age of the Internet:

> As a layman, I have no insight whatsoever into the technical arguments made on either side of this issue... However when I come to read statements like 'a shabby trick,' 'lots of flim-flam,' 'unscrupulously pushing an agenda,' and 'science is a contact sport', I understand 'completely' what's going on. These statements say loud and clear to me that, rather than being some noble search for understanding, science is still just another ego battle, with all the smearing, character assignation and pettiness that that entails. (Jay, 1:46 AM, in Redfield, 2010).

Perhaps the starkest expression of the new vulnerability that besets scientists who blog is this one:

> I followed links here from creationist's websites. They are cheering that Prof. Redfield has exposed science as the dishonest sham that all good Christians always knew it was. They don't understand the science, pro, or con. But they do love 'flim flam,' and 'unscrupulously pushing NASA's ... agenda.' (Gary Hurd, 8:31 AM, in Redfield, 2010).

Inevitably, this heated discussion caught the attention of the popular press. When science writer Carl Zimmer asked Wolfe-Simon to respond to the criticism of her paper in Redfield's blog, she declined with metadiscourse:

> Any discourse will have to be peer-reviewed in the same manner as our paper was, and go through a vetting process so that all discussion is properly moderated. The items you are presenting do not represent the proper way to engage in a scientific discourse and we will not respond in this manner. (Zimmer, 2010).

When Zimmer asked Jonathan Eisen to respond to Wolfe-Simon's defense, he also responded with metadiscourse:

> If they say they will not address the responses except in journals, that is absurd. They carried out science by press release and press conference. Whether they were right or not in their claims, they are now hypocritical if they say that the only response should be in the scientific literature. (Zimmer, 2010).

The metadiscursive attitude markers ('we will not respond' and 'this is absurd') reveal a mutual disdain between two scientists that transports the focus of the conversation from the original question — is Wolfe-Simon's research valid? The radically transparent nature of science by blog pushes the norm of communalism to a point that can have troubling effects on the public's perception of science and scientists.

Conclusion

The background for this paper contains many positivist sensibilities, which the sociology of science has largely discredited. Nevertheless, such sensibilities about the practice of science endure, particularly as they appear in the public face of science. However, this paper shows them to be vitiated in particular ways. The results of this analysis suggest that scientists' trust in Merton's norms may have some practical limits at least in so far as they inform the communication of science by blog. At the discourse level of Redfield's blog is the usual discussion over the factuality of research, occasionally appropriately modulated in spite of the convictions of its interlocutors. In that sense science by blog is commensurate with science by traditional print. At the metadiscourse level, however, the discussion broadens to include the community's struggle to redefine its discourse practices in the face of the brave new world of communication technologies at the same time that a skeptical and often hostile public listens. Researchers like Wolfe-Simon, whom Redfield's blog criticized, wanted to operate out of traditional model of peer review. A wider community of researchers represented on Redfield's blog insisted on norms of a more open system.

In this tension, trust over at least three norms becomes eroded: communalism, universalism, and disinterestedness. The greater collaboration among scientists afforded by blogs embodies the norm of communalism, but the metadiscourse in the blog suggests it has the potential for troubling the politics of science. The radically democratic character of blogs would seem to afford scientists a means of thoroughly exercising the norm of universalism and of thus ignoring irrelevant personal attributes like race and gender when they assess each other's research, but the metadiscourse in the blog suggests that an ethic of expertise remains in place to restrict whose research gets assessed. The completely open communication of blogs would seem to ensure that scientists operate under the norm of disinterestedness, but the metadiscourse in the blog exposes the forces of vested interests that seem to prevent objectivity from trumping all subjective values.

The conclusions of this case study seem congruent with other observations in the literature about science by blog. The metadiscourse in science blogs reveals them to be inherently more dialogical than conventional peer review. Dialogue is at the heart of the norm of communalism; this analysis suggests that Redfield's blog pushes communalism beyond the boundaries of scientific disciplines, a practice that confirms the views of Batts (2008) and Wilkins (2008). Watermeyer (2010), in fact, believes that opening the dialogue among scientists to include lay participation will advance the public's understanding of science. On the other hand this analysis reinforces the observation that despite the radical democratizing force of online technologies many comments in science blogs retain a distinction between expert and lay participants. In Redfield's blog scientific expertise still seemed to trump personal expertise, a characteristic that Shanahan (2010) notes about blogs. More realistically, this analysis repeats the concerns of other scholars who find the public access to scientific discussions problematic. Stodden (2010), for example, notes that the very status of a scientific peer can be blurred by the entry of lay participants into the dialogue over science. The comments in Redfield's blog often overtly refer to the fact that the blog has reduced the discussion of the validity of science to a discussion of the politics of science, what Kouper (2010) calls the 'water cooler quality' (p. 8) of blogs. For this reason our recommendations for those who conduct science by blog are similar to Kouper's: pay more attention to the composition of the audience, work towards stabilizing this genre of communicating science by blog, and take more care with metadiscourse.

This study is only exploratory and thus has hopes of only very limited validity. Although the analysis of Redfield's blog suggests that among the posts of her commenters issues of trust become explicit when they switch from discourse to metadiscourse, such a solitary case study is no basis for more broadly characterizing the discourse of science. In addition, this study is limited by the currently unstable nature of the target genre of communication — science blogs. The validity of analysis here is also compromised somewhat by the acknowledged ambiguous nature of the concept of

metadiscourse, a term central to the method of analysis. Finally, just as anonymous posts trouble the communication of science by blog, so do they trouble the research of the communication of science. Many of the analyses of texts in this case study take into account the identity of their authors; thus, any interpretations of anonymous posts become uncertain.

Future research into the negotiation of trust in the communication of science by blog may be strengthened by a quantitative approach to the analysis of its discourse. What the exploratory analysis in this case study of Redfield's blog has intimated about metadiscourse could be generalized to science blogs as a genre of discourse. Among other intellectual endeavors, science distinguishes itself by attempting to keep separate the observer and the observed. Quantification is a means to police that separation, but such quality control requires trust in those who do the policing. For this reason, in his account of the importance of personal characteristics of scientists in the formation of the authority of science, Shapin (2008) begins: '[W]e cannot understand how various scientific and technological knowledges are made, and made authoritative, without appreciating the roles of familiarity, trust, and the recognition of personal virtues' (p. 2).

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