#### Booja and Other Non-Auditory Phenomena

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Abstract

How does an article get published? How does a grant get funded? Peer review – A critical review of reviewing.

Forty-five years ago, I gave my very first scientific presentation in this room. There are people here today who were there for that talk on my dissertation research. It is a thrill to have my classmates here – Ruth Bentler, Vic Berrett, Jim Thelin, Aaron Thornton, and Terry Wiley. And my adviser and dear friend – Dave Lilly. For you I will try to make this talk more interesting than the last. That rules out talking about my research. Instead I thought if I talked about the research process I'd have a better chance to engage your interest. I've always thought that learning about the research process makes the research far more interesting. When I took a genetics class it was not mentioned that Mendel's data on tall and short pea plants were statistically improbable but not verifiable because his notes burned in a monastery fire. Nor was it mentioned that Watson and Crick thought up the idea of a double helix over a beer at a pub on the edge of the Cambridge University campus.

The aspect of the research process that I will talk about today is the peer review system. Any of us who have published, reviewed, or edited can relate cases in which the peer review process has succeeded and some where it has failed. You could say of the peer review process what Winston Churchill said about democracy – It's the worst system we have, except for all the others.

My first experience with the peer review system was an opportunity that was given to me by one of my two great mentors in my doctoral program, David Lilly and Arnold Small. Arnold knew that I was interested in loudness adaptation and when he received a request to review an article on the topic for *JASA* he gave me the opportunity prepare a review. The article was written by a senior investigator at a major university who had published previously on the topic. Who was I to judge his work? I read the article and I thought it was scientifically flawed. I was sure there was something wrong with my thinking so I told Arnold that I was concerned that I was missing something important. Arnold assured me that there is no reason why a senior investigator can't submit something bad. He asked for my opinion and my reasons. I don't know if he incorporated my comments into his review, but that was a great learning experience for me.

Peer review of manuscripts for publication has its roots in 17<sup>th</sup> century England. That's when a German theologian and diplomat, Henry Oldenburg, relocated to England and became the founding editor of the *Philosophical Transactions of the Royal Society*. He began sending manuscripts to experts for evaluation before publication and has been credited with the beginning of scientific peer review. That process certainly contributed to the quality of the journal but what they didn't publish is part of my story today. *Philosophical Transactions*, now divided into two publications, one for physical sciences, and one for life sciences, is the oldest scientific publication in existence today.

Peer review is also standard operating procedure for evaluation of grant applications submitted to government agencies, private foundations, and industry and is much more recent than peer review for publications. A sort of reverse peer review system was used by the Rockefeller Foundation beginning in the 1920's. The foundation funded a wide range of research including some of the earliest research on the development of antibiotics. Instead of inviting proposals and subjecting the submissions to peer reviewers, the foundation used a panel of experts to identify leading researchers and then recruited scientists to conduct studies that they wanted. The peer review preceded the design of the project.

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The first government program that awarded research grants to universities was the Office of Naval Research which began in the 1940's and used a system of informal peer review to evaluate grant applications. When the director of the program moved to the National Academy of Sciences, the process became more formalized with reviewers submitting written evaluations. Since then it has become institutionalized in government and private granting agencies. But there can be an inherent conflict of interest in the system. We will see how that plays out in the following examples.

There is no more entertaining story of author-reviewer tension than that of Oliver Heaviside. Heaviside was a British mathematician and physicist who did not attend school after age 16 nor hold a job after age 24. He lived a reclusive lifestyle with family members until his later years when he was alone and was given a small stipend from the Royal Academy out of generosity to an aging scientist whose contributions were only beginning to be understood. There has been speculation about the causes for his reclusive lifestyle and difficulties with personal and professional relationships, including hearing loss stemming from childhood scarlet fever. There is an ironic possibility that his place in this story, and his contributions to audiology may have been substantially impacted by hearing loss. More about his impact on audiology later.

Heaviside published hundreds of papers, mostly in the trade journal *The Electrician*. Remember – scientific peer review started in his home town two centuries earlier. His contemporaries didn't understand the significance of his work, including the editor of *The Electrician*, but his steady stream of articles, for which he was paid a small royalty, filled pages and allowed him to avoid Henry Oldenburg's demanding peer review system. Among his scientific contributions are vector mathematics applied to electrical circuits (later applied to mechanical and acoustic systems), a simplified method for solving differential equations, theory of electromagnetic inductance that led to the development of transatlantic transmission lines, better lightning rods, coining the terms impedance, admittance, and conductance, and the prediction of a layer of the atmosphere that conducts radio signals around the curvature of the earth. The *Heaviside Layer* is best known as a metaphor for heaven in the Andrew Lloyd Webber show *Cats*.

Heaviside's eccentric writing would have never pierced peer review and so thanks to *The Electrician* we are left with gems like the following.

## On the Rise and Progress of Nomenclature

In the beginning was the word. The importance of nomenclature was recognized in the earliest times. One of the first duties that devolved upon Adam on his installation as gardener and keeper of the zoological collection was the naming of the beasts.

Mac, tom, bob, and dick are all good names for units.... I have used tom myself ... to denote  $10^9$  c.g.s. units of self or mutual electromagnetic induction ...

## The Electrician, 1885

Surely he had read *On the Origin of Species* written by his countryman 26 years earlier so he knew that In the Beginning was not the word. But it made a catchy start to the article which would have never survived peer review by the Royal Society.

The following year he published this first definition of Impedance

Let us call the ratio of the impressed force to the current in a line when electrostatic induction is ignorable the Impedance of the line from the verb impede. It seems as good a term as Resistance from resist. (Put an accent on the middle e in impedance).

# The Heaviside concept of impedance is a generalization of Ohm's law, about which he said

Perhaps no scientific law has had so much unscientific discussion, a result to be attributed ... to its remarkable practical importance bringing down from the professors to the multitude ... The professors are all wrong, ... self-confident paradoxers whose peculiar conceit is that their views are necessarily right. Did they only deceive themselves in their delusions little harm would be done, but when they take to writing books for students, then a whole body of blind followers is precipitated into the ditch of mental confusion, from which extrication is so difficult and whose mud sticks for so long.

These things weren't said in correspondence with editors. They were in the articles that he published. Heaviside's writing posed a dilemma for the peer review system. The few scientists who understood his work may have wished that some of his most important writings would appear in the *Philosophical Transactions*. How fortunate we are for *The Electrician*.

Heaviside judged that some of his writing was too technical for *The Electrician* and published a series of papers on electromagnetic induction in *Philosophical Magazine*, established in 1798. It was an important scientific journal that featured contributions from the great English physicists including Faraday, Maxwell, J.J. Thomson, and Joule. It was a step toward the distinguished tiers of science where Heaviside's science belonged but apparently had not adopted the form of peer review established by Oldenberg for *Philosophical Transactions*. After publishing the first 46 parts of "Electromagnetic Induction" there was a change of editor. The new editor asked his students if any of them had read any of the 46 articles and not one had read a single article in the series. Based on this peculiar form of peer review *Philosophical Magazine* declined to published additional articles in the series. Three other journals declined articles on the topic "for reasons best known to themselves." Heaviside responded with a scathing assessment of the peer review system of the time.

Perhaps it was thought that official views were so much more likely to be right that it was safe to decline the discussion of novel views in such striking opposition thereto. There seemed also to be an idea that official views ... should not be controverted or criticized.

Heaviside was pointing out a flaw in the peer review system. A conflict of interest occurs when a manuscript takes issue with research that had been conducted by the reviewer. Publication of a manuscript that challenges the reviewer's findings and conclusions could jeopardize the reviewer's future publications and grant funding, even his or her promotion.

Heaviside's difficulties with the scientific establishment stand in contrast to the career of his uncle, Sir Charles Wheatstone, who helped Heaviside to educate himself after he quit school at age 16, possibly due to hearing loss. Wheatstone is best known for something he didn't do – invent the Wheatstone bridge, actually invented by a co-worker named Christie. The electrical Wheatstone bridge would be the inspiration for the acoustic bridge that is the pre-cursor of diagnostic acoustic immittance instruments we use today. It is because early devices were based on bridge circuits that we still sometimes call our instruments impedance bridges although they haven't utilized bridge circuits for decades.

Wheatstone published an article called "Experiments on Audition" in another of the prestigious scholarly journals of the day, the *Quarterly Journal of Science, Literature, and Art*. In five pages, he described six observations including the occlusion effect, a precursor to bone-conduction hearing aids, binaural loudness summation, aural distortion of the form  $2f_1 - f_2$  now the basis for distortion product otoacoustic emissions, the frequency-dependent effect of middle ear pressure on auditory thresholds, and the frequency response of the external ear. The article presented not a single data point, equation, hypothesis, model, or theory. It is

frequently cited in discussions of the history of research on auditory phenomena. Had it been published in *The Electrician* it would probably be unknown. The contrast between the famous Wheatstone article and hundreds of unknown but scientifically rigorous Heaviside papers may be a failure of the peer review system.

That, of course, brings us to booja. The correct plural form of boojum is boojums not booja but I was afraid if I used boojums you wouldn't know what I was talking about. A boojum is a variety of a snark from Lewis Carroll's epic poem, The Hunting of the Snark, about a bellman, a beaver, a banker, a butcher, a barrister, and a baker who go out snark hunting. Snark hunting is a harmless enough activity, unless you're a snark, or unless your snark is a boojum. As the Bellman told the Baker

But oh, beamish nephew, beware of the day, If your Snark be a Boojum! For then You will softly and suddenly vanish away, And never be met with again!

Great scientists see relationships that don't occur to the mortals among us. David Mermin, a physicist at Cornell University was studying the properties of superfluid helium. At a few thousandths of a degree above absolute zero, liquid helium crawls up the sides of the beaker and spills down the sides until the beaker is empty. Unless a mysterious force is present which makes the superfluidity "softly and suddenly vanish away". Obviously, the mysterious force needed to be called boojum for it made the property of superfluidity of liquid helium go the way of the Baker. Here's what happened to the Baker.

> In the midst of the word he was trying to say, In the midst of his laughter and glee, He had softly and suddenly vanished away— For the Snark *was* a Boojum, you see.

Mermin began a campaign to get the term boojum into the scientific literature. Not in *The Electrician* but in a major scientific journal. The story is an example of the conflict of interest between author and reviewer. But not the usual conflict because in this case the reviewer wanted the article to be published, perhaps even more then the author.

At an international conference Merman spoke about the mysterious force but Boojum was not used in his talk. But when he prepared the paper for the published proceedings he was able to summarize the discussion that followed the presentation, which no one else remembered. In his account of the discussion he used the term boojum. The reviewers and editor let it through and the term boojum was in the scientific literature. That was the first step. Next, he used the term boojum in the main body of a presentation to another symposium. Again, the editor of the proceedings let it go through. But the goal of getting the term into an article in a peer-reviewed journal was still ahead.

Then a miracle happened. He received an article to review from a prestigious journal written by a Nobel Laureate (and sometimes rival) from Bell Laboratories. The article was full of boojums. Mermin proceeded to prepare his review when the bubble burst. The article was fatally flawed. The reviewer was seriously conflicted. Should he be true to his goal of getting boojum into the scientific literature or should he uphold the integrity of science. Sadly, he returned the article with his reasons for rejecting it. There was still hope. The author prepared a revision which was again sent to Mermin for review. The authors had addressed some of the issues but not the one that rendered it unacceptable. The conflict was too steep to overcome. "I let it through. And it looked glorious" said Mermin, rationalizing that it would be interesting reading although it was flawed science. But his breach of scientific ethics backfired when a *The New York Times* article credited his Bell Labs rival as the

originator of the term. After his heroic campaign to get boojum into the scientific literature, his scientific rival got the credit.

But Mermin was successful. If you look up Boojum in Wikipedia you will find

In the physics of superfluidity, a **boojum** is a geometric pattern on the surface of one of the phases of superfluid helium-3, whose motion can result in the decay of a supercurrent.

The decay of a supercurrent - it softly and suddenly vanishes away

Although his was not the typical reviewer conflict of interest, Mermin summed up the problem this way.

Relations between authors and referees are, of course, almost always strained. Authors are convinced that the malicious stupidity of the referee is alone preventing them from laying their discoveries before an admiring world. Referees are convinced that authors are too arrogant and obtuse to recognize blatant fallacies in their own reasoning... All physicists know this, because all physicists are both authors and referees... The ability of one person to hold both views is an example of what Bohr called complementarity.

That last part was tongue-in-cheek. It really isn't what Bohr meant by complementarity but it is exactly what George Orwell called double-think in *1984*, the ability to hold two completely contradictory views simultaneously. Authors think reviewers are imbeciles and reviewers think authors are idiots even when the author and reviewer is the same person.

The more common form of the reviewer conflict of interest is the one Heaviside complained about, when the article challenges the work of the reviewer. The editor wants the opinion of experts with knowledge and experience related to the topic of the submission. So if a rival theory of the universe challenged the theory of relativity it would have likely been sent to Einstein for review. It probably happened many times. I'll give two examples of this type of conflict.

Alfred Russel Wallace developed a theory of evolution at the same time that Darwin was writing *On the Origin of Species*. Wallace sent a manuscript not to a journal but to Darwin asking if he thought it was worthy of publication. Darwin, far from being defensive about his place in history, forwarded it to an editor with high praise. It was the editor, not Darwin, who took the position that Darwin should have priority for the theory. They published Wallace's essay with two unpublished communications by Darwin establishing his priority. Darwin, who had every reason to be conflicted, was willing to cede priority for one of the most important theories in the history of science, to another scientist whose work he admired. Were it not for that editor, Wallace would be famous for the theory of evolution by natural selection, not Darwin.

My second example of referee conflict is an experience that I had with my good friend and former student, Tony Cacace. This story has its origins in the living room of my apartment in Iowa City where Jim Thelin slept during the day and worked on his thesis all night after Sally had moved to Cincinnati where Jim would join her when he completed his thesis. I got home after classes about 5 pm when Jim was just rolling out of bed, and in a stupor said, "You know, that critical band thing is an artifact." The title of my dissertation was Measurement of Critical Masking Bands. Jim had my attention. He said that a number of articles on masking, loudness, and acoustic reflex thresholds, used a bandwidth scale that expanded one end of the scale and compressed the other, producing an appearance of a break in the function that was interpreted as a critical bandwidth in auditory processing. It was the log of a difference rather than the bandwidth measure that was well accepted in engineering – the log of a ratio. Tony didn't need to collect data to prove Jim's point. It could have

been made using published data. But Tony did a very careful experiment replicating previous measurements of the effect of stimulus bandwidth on loudness. The data supported Jim's groggy observation.

We submitted the paper to the *Journal of the Acoustical Society of America*. The associate editor who handled it was W. Dixon Ward, Dix to those of us who were lucky enough to know him. The primary reviewer was a highly respected psychoacoustician who had made significant contributions to the critical band literature. Our article challenged his interpretation. The principle objection raised in the review was that we hadn't collected enough data in the important range of bandwidth values to prove the point. Our conclusion was nothing more than conjecture.

It is unusual, in my experience, for an investigator to collect more data to satisfy reviewers but that is just what Tony did. It would add a year to the publication date. We resubmitted the article with additional data. We thought Jim's point was proven. But the reviewer would not be mollified. I wrote an irate, Heaviside-type letter invoking what Mermin characterized as "the malicious stupidity of the referee… preventing [us] from laying [our] discoveries before an admiring world."

Dix responded.

Dear Bob,

You may be right. It may be unfair to place the burden of proof on the negative, if you regard yourself as the negative... But in the present case, it's not clear who the negative is.

Do you really wish that JASA had published the article without further revision? Do you really think that only those two reviewers would have the reservations they voiced? Let me put it this way: I think that most readers welcome the sight of David coming forth to do battle with Goliath, but they are at least disappointed if David's sling proves to be loaded with marshmallows.

Dix viewed his role as one that should help investigators, especially inexperienced ones, to get their work published in a way that maximizes the value of their work. The article was published.

So the peer review system is not perfect. In fact, it's the worst system we have except for all the others. I will close with a few words of advice for the young investigators in the audience.

When responding to reviews of an article, do everything the reviewers recommend that don't compromise the article. Sometimes that means using the reviewer's word choice rather than your own perfectly good one. If it doesn't matter use the reviewer's. If it means changing a sentence from your perfectly good sentence to the reviewer's perfectly good sentence use the reviewer's. If you make all the positive changes you can you are in a good position to refuse a suggestion that you can't abide.

If you are starting your research career and wish to break into the club that gets research grants, hitch your wagon to a star. A disconcerting finding of a very good analysis of peer review found that the best predictor of a successful project is the track record of the investigator. Grant reviewers and program managers know this. Reviewers are always asked to assess the strength of the research team. Find established researchers to collaborate with. They become part of the research team that is evaluated.

Faculty members – Do what Arnold did. Get your students involved in the peer review process.

These ideas may help you successfully navigate the peer-review process

and your snark won't be a boojum, you see.