

## Manuscript Review

It has been a real pleasure to read the manuscript [REDACTED] and to give a review for both the author and [REDACTED]. The manuscript is extremely well written and the arguments are compelling. I will suggest below some of the changes I believe the manuscript requires before it is published. In general, I believe some work needs to be put into the organization of the paper, the author should think critically about the use of “professionalization”, and the argument concerning the use of the periodical press needs to be more fully teased out. Once these changes are made, I believe the manuscript would be a fitting contribution to [REDACTED].

The manuscript attempts to make three different but interrelated arguments. First, it focuses explicitly on the glacier controversy between Tyndall and Forbes to demonstrate the significance of the debate and how crucial it was for larger debates between the scientific naturalists and the North British physicists. Second – and the most significantly – it demonstrates how Tyndall and his allies used the periodical press to great affect to direct the controversy, undermine Forbes, and persuade the scientific community of their own authority. Forbes, the author argues, relied on an increasingly outmoded strategy of publishing in books and pamphlets. And third, it uses these strategies to highlight the professionalization strategies of the future X-Club, offering a “prelude” or a “prevision” of the strategies the X-Club would use so efficiently in the decades that followed.

All of these arguments have merit, and seem to be correct in general. And they add significantly to scholarship in the field, including work focusing explicitly on the scientific naturalists, the X-Club, and perhaps most importantly, the fascinating work being done on the role of the periodical press in the formation of scientific ideas and opinions. Here are my reservations:

I agree that the X-Club became very good (shrewd?) at using the periodical press to dictate the terms of debates, influence the broader public, and establish their own authority. This manuscript is a good example of that process in action. However, on several occasions the author argues that Forbes did not use the press – “Forbes himself was set on waiting to respond to the criticisms of his theory” (p. 14); “Forbes did not respond directly to any of Tyndall’s, Huxley’s or others’ criticisms in the periodical press” (p. 20); and “It was not the case that Forbes did not respond to Tyndall’s – and Huxley’s – scientific criticisms, but he did so in talks and through his friends, not directly in the periodical press” (p. 28) – but this seems overstated. Forbes had a long history of publishing his results in article form. His *Occasional Papers* is, in fact, a collection of papers published as articles. He seemed to repeat that process during his controversy with Tyndall, using his friends and connections to argue his points and defend his science in the periodical press. Wills published an anonymous letter in the *Saturday Review* as early as 1857. Wills also published a glowing review of Forbes’s *Occasional Papers* in the periodical press. Forbes himself responded to Tyndall in the *Athenaeum* in 1858, reproduced in the *Philosophical Magazine* as “On Some Properties of Ice Near Its Melting Point” in 1858. Even more significantly, Forbes wrote directly to Tyndall in early 1859 specifically to counter Tyndall’s previous publication in the *Phil. Mag.*, a letter which Forbes meant for publication

(and which was printed as ‘Remarks of a paper “On Glaciers and Ice” in the last number of the Philosophical Magazine. In a letter to Prof. Tyndall’, *Phil. Mag.* 17, (1859): p. 197–201). In this published letter, Forbes deftly advertises his forthcoming *Occasional Letters*. Although the author is correct that William Thomson did not directly participate, his brother James did, also publishing in the periodical press, ostensibly on the side of Forbes. Moreover, the other side can also be argued. While Huxley was quick to publish, Tyndall actually questioned such a strategy and waited to really dig into Forbes in his book, *Glaciers of the Alps*. Some of this is noted by the author; some of it is not. And, as I will discuss below, because of the organization, how and when and where things were published, on both sides, is not easy to tease out.

Perhaps it is enough simply to show how well the scientific naturalists used the periodical press – Huxley reviewing himself anonymously is downright mischievous – rather than to put too much weight on Forbes as antiquated and unable to keep up with the changing publication strategies of the day. The author’s arguments would still stand.

I would also suggest that the author think about how he or she is using the term “professionalization.” I hate the word, as I think it hides more than it reveals. But, more significantly, there has been recent work that suggests professionalization is not what is going on here. This scholarship situates the rise of the “professional” scientist much later. Paul Lucier, for instance, has proposed that professional scientists did not exist in nineteenth-century America at all. Rather, a “professional scientist” would have been a contradiction in terms since “professionals” and “scientists” were entirely different and opposed categories. (Paul Lucier, “The Professional and the Scientist in Nineteenth-Century America,” *Isis*, Vol. 100 (2009)) Jim Endersby has made a similar argument on professionalization in Britain (*Imperial Nature: Joseph Hooker and the Practices of Victorian Science* (Chicago: University of Chicago Press, 2008). Perhaps “specialization,” or simply “creation of scientific authority” are more fruitful categories of analysis that better represents what is happening. If the author chooses to stick with professionalization, then this recent literature will need to be engaged. (Footnote 10, where professionalization is discussed, includes only sources that are extremely dated.) My suggestion is that having to engage the literature of professionalization would be really boring, and that the author should sidestep the discussion and just use specialization (or something) and get to the good stuff about scientific and cultural authority.

Lastly, I believe the author should revisit the organization of the paper. By breaking the paper into sections, which are not entirely chronological, the author is able to tell a fascinating story. Admittedly, the narrative arc is wonderful as it is now. However, this necessitates that the author repeat certain aspects of the story, over and over, as they are pertinent to each section. An example is how difficult Rendu’s pamphlet was to obtain, mentioned several times, or the Copley Medal controversy, which pops up numerous times. More problematically, however, is how the lack of chronology makes examining the author’s argument rather difficult. It took me several readings to sparse out when certain articles were published, when the books came out, when letters were written between the main participants, etc., because they appear in the narrative on separate occasions for different reasons. Perhaps no one will need to read the manuscript as closely as reviewers like me, but when specifics of when and where Forbes published becomes critical, the organization and repetitive nature of some of the material made for difficulties.

There is a lot in this paper that merits publication. The manner in which Tyndall and Huxley transformed the debate from a question of viscosity to plagiarism is fascinating. Why Tyndall defended Agassiz – to set a precedent of plagiarism in Forbes's work – was new to me. And, indeed, the early strategies of the still inchoate X-Clubbers and the fascinating role that the periodical press played in the controversy, all suggests the strengths of the manuscript and the reasons to eventually publish it. But, in my estimation, some work needs to be done before that should happen.