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Cite and oversight

The research publication is the currency of our scientific lives, and the value of our careers is measured with what we publish. It is not enough to document what we have done, it is also important to know whether we have made a meaning ful difference, hence the desire to measure our worth and value. Enter citation analysis and impact factors.

I have been interested in this issue for along time, but the recent stimulation came from reading the recent article in *Drug Discovery Today* by Raymond C. Rowe entitled *Publish or perish* [1]. This response to Rowe's article represents one individual's opinion on a controversial issue. It is not a meta-analysis or a review, therefore, there will not be a surfeit of references; the citations will be idiosyncratic to suit my purpose. I am following in the footsteps of many, only honest enough to admit it. If at the end of it all, I have provoked a response, I will rest content.

The research publication that plays such an inordinate part of our professional lives was largely the creation of a single individual, Henry Oldenberg. His tale has often been told [2–4], therefore, a simple summary will suffice here. Born in Bremen between 1617 and 1620, he settled in England after 1653, becoming friendly with those who were trying to propagate a new way of

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looking at knowledge and learning. The proponents of this 'new learning' created the Royal Society and Oldenberg became its secretary. One of the tenets of the new way was that knowledge was a public good to be shared by those who contributed to its production. Medieval secrecy gave way to open discussion and dissemination. Individuals were not working in private and in secrecy but were contributing as a community to the creation of an edifice of knowledge. Thus, information that was gathered by a given individual needed to be disseminated and shared. This new approach demanded that information be widely disseminated and the form in which this occurred was a letter. Oldenberg, as the Secretary of the Royal Society, took the new form of communication – the letter – codified it and transformed it into the research article, as we know it today. Clearly, this laid the beginnings of the assessment of contributions to knowledge through publication of letters and their acceptance and recognition by a community of scholars. As time went on, much of the paraphernalia of the modern research journal came into being. Oldenberg played a crucial role in all of this. What he set in place has continued to this day, with minor changes.

The new learning was a creation of 'dead white European males' of the 17th Century [5] and few perhaps at the time recognized that within a few centuries that approach would become truly international and legitimized as the way of contributing to knowledge. But the instruments of expansion of one generation become the vested interest of the next. Whereas failures can be written off or ignored, success demands accountability; so began the publication game. The early versions merely counted; one who published more was seen as being better than one who did not. That was not enough. After all, did it matter that one published? What if, like the sound of falling trees in a forest, no one heard? Enter citation analysis and impact factors.

The intentions were quite honourable [7]. In all the arguments and controversies about this issue, it is important to note that Garfield and others have been scrupulous in pointing out the caveats and pleading for proper usage. If the scientific community has erred it is entirely their responsibility. The impact factor is the ratio of citations to papers published in a given journal to the total number of publications in that journal over a given time. At a simplistic level, the notion makes sense. If a given journal scrupulously publishes only those articles that are of superb quality and those articles are avidly read and properly referred to by the community of scholars, then that journal would have a tremendous impact. Conversely, a journal that accepts everything that is submitted to it might find that most articles remain unread and uncited and would have no impact at all. Scientists would like their observations to count, therefore, they would assiduously seek out journals with better editorial policies and higher impact factors. This seems to be intuitive and obvious, however, a lot of this founders on the bedrock of reality. The arguments that have raged on this issue are well documented and I am not going to cite them all. An easy way to enter the controversy is for readers to refer to a series of articles that appeared in *Trends in Biochemical Sciences* in 1989 [8,9].

One of the problems with citation analysis and impact factors relates to the numerator: the total number of citations to a given paper or papers in a given journal. This assumes that scientists would be scrupulous in giving credit where credit is due and only cite those papers that have meaning to their own paper. How true is this? The evidence is mixed [10–12].

If the critics are right and the problem is real, are there any solutions? I am going to propose several solutions, in descending order of outrageousness. (1) Return to the ideology of the New Learning, which, in a sense, began it all. If scholars are building the edifice of science brick by brick, why should they not remain anonymous? It is the product alone that matters. There will be no takers for that option. (2) Argue that modern science can rarely be done without institutional support. Therefore, the name of the institutions alone should appear on the paper with the individual contributors in the acknowledgements. I seriously doubt if this option would find much favour either. (3) Take citations seriously and annotate them. Certain journals do, but the practice is not widespread. That way referees can crosscheck whether the citations are real, spurious or unwarranted. However, before one contemplates any such solutions, I would suggest that more experiments be done.

Rather than rely on the testimony of experts and metricians of one kind or another, I propose that we decide on our own, based on simple experiments. In a Baconian spirit, we can call these Experimenta Reflecta or Illuminata:

***Experiment 1*:** With each of your publications, turn to the reference list, go through each of the references that you have cited and ask the following questions: Why did I cite this paper? Is the person a potential referee? Or even on the editorial board? Did I just happen to have it in my files? Did I find it from the reference list of some other paper? Have I actually read this? Am I citing it to show how current I am in my literature search? Did I just pull it off the web? Is it because it is in English and I cannot read other languages? Is it from a reputed journal and therefore adds credibility to my claims? You can repeat this experiment several times to get more quantitative data.

Experiment 2: Select one to five papers that have cited you. Now take a deep

breath and read the paper carefully. Ask yourself why was your paper cited? Was it for the sorts of reasons that you found for yourself? Would it have made any material difference to the publication at hand if the author had failed to cite your paper? Be brutally honest with yourself. Remember that you do not have to publish the results or even tell anybody about them.

These experiments should help you decide for yourself the meaning of it all.

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Maurice Wilkins: the third man of the double helix

'DNA, you know, is Midas' gold. Everybody who touches it goes mad.'

The words used by Maurice Wilkins, joint winner of the Nobel Prize in 1962 for his role in the elucidation of the structure of DNA, capture some of the strong emotions felt by those closest to what was perhaps the defining moment in contemporary biology – the solution of the double-helical structure of DNA. The success of Watson and Crick through model building is widely known but this accomplishment inevitably overshadows the key contributions made by Wilkins and Rosalind Franklin, the capable research student Wilkins hired. The contribution of Wilkins was to demonstrate how X-ray diffraction, a technique commonly used to determine the structure of inorganic molecules, could be used to solve the structure of DNA. Without this step, the structure of DNA would not have been as immediately forthcoming.

What is life?

Wilkins was born in 1916 in Pongaroa, New Zealand, a small town situated in the south of the North Island. His parents came from Ireland and his father was a doctor in the school medical service. When Wilkins was six, the family moved to England. He went on to study physics at St John's College, Cambridge (UK), and as a research assistant to John Randall at the University of Birmingham (UK) investigated the luminescence of solids. After completing his PhD thesis in 1940 on the theory of phosphorescence and the stability of trapped electrons in phosphors, Wilkins went on to study the separation of uranium isotopes. This undertaking was to continue at the University of California, Berkeley (USA), where he joined the Manhattan atom bomb project. His work on this project, and the eventual dropping of the bombs on Hiroshima and Nagasaki in 1945, left an indelible mark on Wilkins' life and career, leading him to question the moral implications of scientific research that could produce such devastating destruction.

Like many scientists of his generation, Wilkins' career path was to be transformed by reading Schrödinger's highly influential book entitled *What is Life?* [1]. Although the originality of this book has sometimes been questioned, it undeniably captured, in an accessible style, the way in which physical methods could have a major impact on biological problems. Like many seminal works, *What is Life?* came along at the right time and opened up exciting new research possibilities at the interface of two scientific disciplines: physics and

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