ARTICLE

The Jealousy of Scientific Men

RECOMMENDED FOR *AP Biology*

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Abstract

A look back at some famous discoveries in biology highlights the competitive nature of science, the trophy being priority by publication.

Key Words: *Scientific competition; priority by publication; Charles Darwin; Alfred Russel Wallace; Gregor Mendel; James Watson; Francis Crick; Aurel Babeş; Lynn Margulis.*

Like it or not, the honor of being considered the discoverer of a new scientific idea is awarded to the scientist who gets the idea published first. The scientist makes a discovery, writes up the findings in a logical format, and submits this article to a scientific journal. The editors at the journal forward the article to several reviewers who are knowledgeable in the field, and these peer reviewers determine whether the methodology, statistical analysis, originality, and importance of the article warrant publication and whether revisions are necessary. If judged worthy, the date of publication marks the "discovery" and the author or authors are awarded priority, which is the honor of being considered the first to have made the discovery.

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Priority by publication date makes science a competitive field. Olympic gold medals are awarded to athletes who adhere to the motto of "Citius, Altius, Fortius," and judges can easily compare speed, height, and strength with measurements. Gold and silver can be separated by a hundredth of a second, but only one competitor gets the gold. In the 15th century, colonies were marked with the flags of imperialist nations. The simple flag marked priority and huge territorial claims. In technology, patents recognize priority of invention, and with patents come exclusivity, financial reward, and sometimes fame. Glory in scientific research is gained through discovery, and priority to that discovery is determined by publication date. Such publication has two beneficiaries: progress in the field of science, and the author. For the author, priority is accompanied by enhanced reputation, public validation of original ideas, promotion, tenure, raises, and prizes, including the Nobel Prize, worth $1.2 million per full share in 2012 (Nobel Foundation, 2012). Many of our students, even at the high school level, feel pressure to get involved with research to fill a box on their resumes, and having their names appended to the list of authors on a paper is the tangible recognition of their efforts. There can be, however, a very human and dark side to all these papers, prizes, and progress, and I will set out a few examples.

In 1827, Charles Darwin felt the thrill of discovery and priority at the age of 18. He had observed motile eggs of the bryozoan *Flustra* under the microscope, an observation that had not been made before (Nichols, 2003). He discussed this with his then mentor, Robert Grant, who chastised him for encroaching on his field of study, and Grant presented the new findings to the Wernerian Society without acknowledging Darwin's contribution. Three days later, Darwin presented the findings to the less august Plinian Society, but he never forgot the sting, as his daughter Henrietta recounts here (Litchfield, 1871), with her shorthand left as is:

> Feb 1871 Just before publication of Man, my Father told me "I have just heard that a German book has come out apparently the very same as mine, "Sittlichkeit & Darwinismus"; whereupon I said "Well, at any rate nobody can say you've plagiarized." "Yes, that is the only bother, that is very disagreeable. Otherwise I never have cared abt the paltry feeling of priority & it doesn't signify a bit its coming out first. It is sure to be not exactly the same." It is a good thing it is coming out when two men hit upon the same idea it is more likely to be true.

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> I then made him repeat what he had told me before, namely his first introduction to the jealousy of scientific men. When he was at Edinburgh he found out that the spermatozoa of Flustra move. He rushed instantly to Grant afterwards Professor at University Coll who was working on the subject to tell him, thinking he wd be delighted with so curious a fact. But was confounded on being told that it was very unfair of him to work at Prof G's subject & in fact that he shd take it ill if my Father published it. This made a deep impression on my Father & he has always expressed the strongest contempt for all such little feelings – so unworthy of searchers after truth.

The plagiarism mentioned in the first paragraph probably refers to Darwin's doubts about whether he acted correctly in dealing with Alfred Russel Wallace in 1858 (Figure 1). In 1842, Darwin had written a short synopsis of his idea that natural selection caused the appearance of design in nature and accounted for descent with modification. Whether out of fear or meticulousness, he did not publish this mechanism at that time. In 1858, he received a letter from Wallace in which Wallace clearly described Darwin's own theory, including the concepts of variation, inheritance, selection, and adaptation. Wallace had sent Darwin the essay from the Malay archipelago, in hopes that Darwin would forward it on to Charles Lyell for publication. Darwin feared that his claim to priority would be lost. Lyell and Robert Hooker arranged to have Wallace's essay and an abstract from Darwin, together with a letter to Asa Gray dated 5 September 1857, which outlined Darwin's mechanism, read at a meeting on 1 July 1858 of the Linnean Society. The last letter was what would establish priority for Darwin. Nevertheless, these papers generated no excitement, and Darwin later concluded, in his autobiography, that "This shows how necessary it is that any new view should be explained at considerable length in order to arouse public attention" (Barlow, 1958). His *On the Origin of Species*, published in November 1859, did just that, but it is clear that Darwin, despite Henrietta's recollection, had felt defensive about his claim. So much so that in 1861, he inserted an appendix into the third edition of *Origin of Species* in which he discussed all possible claims to priority, and why they came up short. Lamarck had descent with modification correct, but not the mechanism. Dr. W. C. Wells in 1813 had natural selection correct, but only applied it narrowly to some human traits. In 1831, Patrick Matthew clearly saw natural selection as the force behind change over time, but he published his idea in an appendix to the book *Naval Timber and Architecture*, which was not read widely by naturalists. Darwin also cites the publications of 20 others that preceded his 1859 opus, but gives reasons for each not having caught the attention or enthusiasm of those pursuing the topic (Costa, 2009).

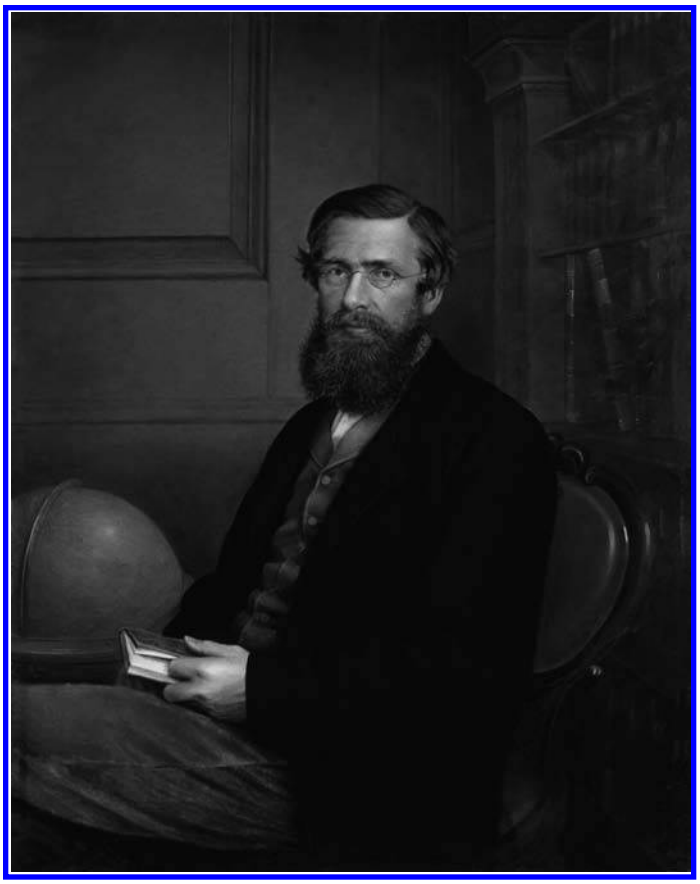

Figure 1. Alfred Russel Wallace. (Painting by Thomas Sims, 1863; © National Portrait Gallery, London.)

Gregor Mendel in his work with peas showed that inheritance was particulate and not a blending of traits (Figure 2). This was a crucial discovery in genetics. Unfortunately, he published his historic paper in the obscure journal *Proceedings of the Natural History Society of Brünn*, in 1866. In 1900, three different scientists, C. E. Correns, E. v. Tschermak, and H. de Vries, did experiments that "rediscovered" Mendel's ideas. De Vries published first that year, without mentioning Mendel's work, which he apparently knew about. Correns alluded repeatedly to Mendel in his paper (Correns, 1900), which starts out (italics original to Correns):

> THE LATEST PUBLICATION OF HUGO DE VRIES: Sur la loi de disjunction des hybrides which through the courtesy of the author reached me yesterday, prompts me to make the following statement: In my hybridization experiments with varieties of maize and peas, I have come to the same results as de Vries, who experimented with varieties of many different kinds of plants, among them two varieties of maize. When I discovered the regularity of the phenomena, and the explanation thereof – to which I shall return presently – the same thing happened to me which now seems to be happening to de Vries: I thought that I had found *something new. But then I convinced myself that the Abbot Gregor Mendel in Brünn, had, during the sixties, not only obtained the same result through extensive experiments with peas, which lasted for many years, as did de Vries and I, but had also given exactly the same explanation, as far as that was possible in 1866.*

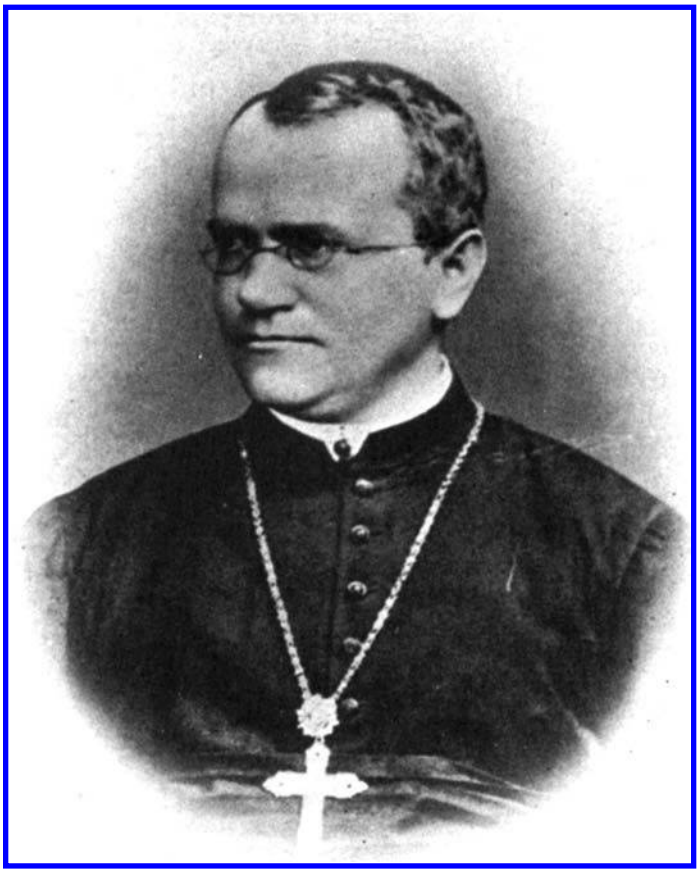

Figure 2. Gregor Mendel (1822–1884).

In a postscript to his paper, Correns seems to take de Vries to task by adding the following, referring to another paper that de Vries published shortly later (italics original to Correns):

> In the meantime de Vries has published in these proceedings (No. 3 of this year) some more details concerning his experiments. There he refers to Mendel's investigations, which *were not even mentioned in the "Comptes rendus."*

Tschermark also fails to mention Mendel in his paper, but also is induced by Correns to add a postscript:

> Correns has just published experiments which also deal with artificial hybridization of different varieties of *Pisum sativum* and observations of the hybrids left to self-fertilization through several generations. They confirm, just as my own, Mendel's teachings. The simultaneous "discovery" of Mendel by Correns, de Vries, and myself appears to me especially gratifying. Even in the second year of experimentation, I too still believed that I had found something new.

The consensus is that de Vries and Correns undertook and understood experiments that confirmed Mendel's laws, but that Tschermark did not understand the significance of his data (Monaghan & Corcos, 1986, 1987).

Failure to receive priority due to publication in a lesser-known journal has a familiar example. In 1923, Georgios Papanikolaou gave a presentation on his ability to detect cancerous cells on examination of cells from a vaginal scraping. He was met with skepticism by physicians, and he did not publish until 1941, in the *American Journal of Obstetrics and Gynecology*, the most cited journal in that field. He had actually been scooped in this discovery, at least as far as publication date. A Romanian doctor, Aurel Babeş, had discovered the same usefulness for vaginal smears, and he had published in 1927 in the *Proceedings of the Bucharest Gynecological Society*. Like Mendel, Babeş had published in a lesser-known journal. If not for that fact, we might be talking about "bab" smears instead of "pap" smears. Aurel Babeş was busy with other projects and never made an issue of priority. As an aside, his uncle, Victor Babeş, demonstrates another benefit of priority. He discovered the protozoan etiology of, and is therefore the eponym for, the disease babesiosis.

James Watson and Francis Crick are the names linked with the structure of DNA because of their publication in *Nature* on 25 April 1953 (Watson & Crick, 1953), but both estimated that Linus Pauling would have the structure of DNA within 6 weeks of his erroneous published structure in February 1953 (Watson et al., 2012). Crick estimated that Rosalind Franklin would have the structure in somewhere between 3 weeks and 3 months from that same time (Sayre, 1975; Figure 3). Watson and Crick, however, published first. The 1962 Nobel Prize in Medicine and Physiology was split among Watson, Crick, and Maurice Wilkins, each receiving a share worth $17,000. Franklin, the source of crucial information and images for Watson and Crick, had died in 1958 and was not eligible for the award, because the rules exclude posthumous awards. Would honest collaboration have brought quicker results with fewer hard feelings? From April 1953 on, there were allegations of misappropriation of the data of Wilkins and Franklin. Many of the scientists involved did not go public with their allegations until after the publication of Watson's book *The Double Helix* in 1968. But an interesting exchange of letters between Watson in the United States and Crick in England later in 1953 demonstrates that Watson knew from the very beginning that there were suspicions about his use of Franklin's data. The following exchange of letters also might indicate the competitiveness even between equal partners in priority, and this exchange incidentally shows the financial predicament of young researchers with families (Ridley, 2009). In 1953, Crick sent a letter to Watson asking if he would mind if Crick went on the BBC to give an interview about their discovery, for as Crick writes:

> It would bring in $50 to $100 which at the moment I could do with.

Watson opposes the interview and writes:

> There are still those who think we pirated data…. If you need the money that bad, go ahead. Needless to say, I should not think any higher of you and shall have good reason to avoid further collaboration with you.

Crick responded in kind:

> As you were so set against it I did not allow BBC to broadcast…although your name is mud in the Crick household because of this…. We live very quietly here mainly because we are so broke.

Crick had some payback when he vehemently opposed publication of Watson's book *The Double Helix*. He wrote to Watson in April 1967 (Crick, 1967):

> My objection, in short, is to the widespread dissemination of a book which grossly invades my privacy, and I have yet to hear an argument which adequately excuses such a violation of friendship. If you publish your book now, in the teeth of my opposition, history will condemn you, for the reasons set out in this letter.

Crick gradually came around to accepting the bestseller as an interesting interpretation of the events for a lay audience, and their friendship endured.

Sometimes, peer-reviewed articles never get published because of the jealousy or the politics of the reviewers or because the article's conclusions threaten an orthodoxy. An example of the latter in biology is that of Lynn Margulis, who presented evidence that mitochondria were former free-living bacteria that developed a symbiotic relationship with another single-celled organism. Her paper on the theory of endosymbiosis was rejected by 15 different journals, and finally accepted by *The Journal of Theoretical Biology* in 1967 (Lane, 2006). Endosymbiosis is now considered a central tenet of biology. The lesson here is not to give up on a good idea. Often, when an idea is truly radical, reviewers will be quite skeptical and, wanting to preserve the integrity of the journal or the consensus orthodoxy, reject it.

Figure 3. Rosalind Elsie Franklin. (Photograph by Elliott and Fry, 1946; © National Portrait Gallery, London.)

Awarding of priority and prestige has been made more difficult over the past 100 years by the number of authors on papers (Greene, 2007). First authorship is more prestigious than being the eighth author on a paper. The assumption is that author listings are ranked by amount of work done. Watson and Crick flipped a coin to determine first author listing in 1953. Science teachers love to see collaboration on projects, and they return for modification student papers that have no references. The best paper, however, would be a sole-authorship paper with absolutely no references. Sole authorship clearly establishes contributions, and legitimately having no references marks the paper as original thinking. Few such papers exist, but one is notable. In 1905, at age 26, Albert Einstein published several remarkable papers, one of which, on special relativity, had him as sole author and no references (Einstein, 1905). The era of one or two authors on a paper may be over because "Big Science" like the ENCODE project and the Large Hadron Collider requires multinational cooperation and resources, with appropriate distribution of the claims of discovery (Maher, 2012).

Science is collaborative and competitive, done by human beings who can be gracious or jealous, noble or puerile. Should publication date be like the flags of territorial claim planted on foreign soils? There are unfortunate consequences to this. How do students think Scott felt when he arrived at the South Pole and found Amundsen's Norwegian flag already set there? Darwin himself wrote to Lyell in 1856 about this: "I rather hate the idea of writing for priority yet I should be vexed if anyone were to publish my doctrines before me" (Darwin, 1887). Besides the loss of public prestige, there is also the loss of self-esteem about one's originality of thinking when someone else comes up with the same idea. Darwin could not have been happy to read Wallace's letter from Ternate in 1858, in which Wallace said to Darwin "that I hoped the idea would be as new to him [Darwin] as it was to me, and that it would supply the missing factor to explain the origin of species" (Wallace, 1905). Darwin's ego and fame survived this mild challenge. There have been many hotter feuds in science: the personal one of Newton versus Leibnitz about priority for calculus; the destructive one of Marsh versus Cope over dinosaur bones; and the devious AC/DC one of Tesla versus Edison. Other potential disagreements never materialized, either because of a self-effacing personality as in Wallace's case, or because one party did not feel there was a race at all, as in Franklin's case (Pauling, 1973).

Competition for priority can lead to student discussion. Is scientific rivalry stimulating or stunting for the progress of science? Do secrecy and use of materials without credit hinder the collaborative process of science? What would be the danger of a scientist putting a discovery on a webpage for public review and critique, in order to make his or her journal article even better than peer reviewers could make it? Would ideas be stolen? Publication as priority may be here to stay, and it surely will generate interesting conversations into the future.

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