
Viewpoint

Rediscovery – a regular occurrence in scientific research

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To rediscover something thinking that it is a “new” discovery (a tautology - discoveries are implicitly *new* findings) should put authors on the line as to whether indeed their findings are genuinely new. The recurrent phrase “*herein we show for the first time*” appears in many papers these days. These words should ring bells with editors and reviewers. A submission has to be *original* work; if not, it should be rejected at triage. If the experimental data have focused on a known phenomenon from a different perspective, a new finding may well accord with earlier work (the original discovery), but in such cases the results are essentially *corroborative*, and their supportive nature should be unambiguously emphasized. This can sometimes be very important since consensus is needed to substantiate an extant hypothesis or extend it, giving us a better understanding of a concept or phenomenon - science relies on team work.

My two main reasons for raising the problem of rediscovery, are first, that I had been informed by a colleague that a recent claim regarding the biology of neovascularization of tumours^{1,2} as a “new” discovery was in fact 40 years out of date, made worse by an editorial in *Nature* paying particular attention the significance of the breakthrough.³ Although I have published elsewhere a criticism of this case,⁴ *Nature’s* editors were not interested in taking any action themselves to correct the situation. My second reason is that I have experienced a similar occurrence when one of my own publications⁵ that gave a completely new concept regarding the problem of short- and long-lived proteins was “rediscovered” by Yewdell and his team at NIH, Bethesda, MD, USA.⁶ Jon Yewdell recognized that they had been eclipsed by my work published 20 years earlier, and wrote a moving article in *Cellular and Molecular Biology* acknowledging my input.⁷

What ways are there of dealing with “rediscoveries”? From the point of view of authors, they could go blindly on believing that they have every right to their claim. However, if they knew it was not new, not acknowledging its provenance shows disregard and disrespect for those who paved the way before them. Most will be found out in due course, but the impact of the issue can diminish.

Let us consider a discovery is much the same as an invention, as may well be the case. Putting it into the public domain would lead to some problems should a patent be sought. It would soon be made candidly clear by the patent officer that the invention (or a sufficiently similar one) had already been granted, and therefore this one has no future, ie it is not new. The message should be the same for scientific

claims, ie check carefully before submission whether you do indeed have something new. In the analogy, it is the patent officer who does the due diligence. For scientific papers that responsibility lies mainly with the reviewers of a submitted paper, even more so than with the editors on whom the latter rely for their detailed knowledge of the specific subject matter.

Other issues can arise as a “(re)discovery” can often appear new to some peer reviewers, grant awarding committee members, and other funding bodies. Thus it could be advantageous in securing considerable finance to support further research. The author might be invited to conferences, awarded prizes and, through the associated kudos, achieve promotion. Should the author(s) continue - wittingly or unwittingly - in this position? We should reflect that, in any case of law, ignorance is no defence, and this should be the same in science whenever someone claims the “right” to a discovery that is not new.

One worrying corollary is that the elapse of time will tend to obscure the original source and the purport of a discovery. Discoveries get pushed aside, ignored and sometime lost. In a rather obtuse analogy - but with a pertinent point to make - consider the cello suites of Bach, which appeared in the late eighteenth century and were known by some cellists of that era. Somehow they went missing until Casals “rediscovered” them well over a century later. Their provenance could have been mistaken or lost altogether; indeed, in other similar situations the “(re)discoverer” has actually laid claim to be the composer. And herein is the travesty, for similar cases have undoubtedly occurred in science. A discoverer of some phenomenon in science might have been dead for many decades, the work lost in the mists of time, or it might have been published in a low-impact journal and not visible. Since we are all concerned with our image and contributions to science, human beings are emotional and seek accolades of recognition. Nevertheless all our contributions should be selfless gifts that add to the sum total of knowledge - the whole ethos of science. However, it is becoming increasingly obvious to me as an editor that the phrase “to show for the first time” should always raise the question - is this really true?

The onus falls on those on the editorial and publishing side of science, who should become increasingly aware of “false” claims. Our peer reviewers, as independent referees and supposedly experts in their own fields, as also members of grant agency reviewing panels, science reporters and journalists, administrators in academic life, should operate in the same way as the patent officer in being thoroughly

astute in recognising and dealing with second eureka's.

Careful training needs to be given in many fields of scientific methodology, notably how to undertake a thoroughly critical review of the literature, how to formulate a hypothesis, how to design and perform experiments, how to handle the data, and finally how to present the findings at seminars and in scientific papers submitted to learned journals, with due reverence to their provenance. The need to take in criticisms and comments of experts before these unfortunate cases of rediscovery are reported is imperative. As an editor, I am disappointed by how authors nowadays seldom acknowledge that colleagues and experts have been asked for their independent criticisms and comments to improve the paper before its submission. Without such recognition, the assumption will be that they did not in fact seek advice, which smacks of unprofessionalism. Once again, better training has to be the answer.

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