Commentary

Eponyms, priority, and attribution

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Let us join Michael Rosen on his Paladino and ride off with him into the Glomset and there, freed from laboratory cares, consider the vexed (to some) questions of attribution and priority. In flushing the Wolff from his lair, Rosen has stumbled on a Mines field. If we are to be fair to all concerned, the Wolff-Parkinson-White syndrome should be named the Paladino-Kent-Mines-Glomset-Glomset (why give credit to only one Glomset?) — Wolff-Parkinson-White-Holzman-Scherf syndrome. We make this point because it would be equally ridiculous to give the condition a descriptive title — it is a composite, a syndrome and, as such, needs a manageable name by which it may be readily recognised. Furthermore, despite the depth of Rosen’s research, it is not beyond the bounds of possibility that there were other labourers in the electrophysiological vineyard whose contributions have passed unnoticed. “Who knows”, wrote Sir Thomas Browne in Hydriotaphia, “whether the best of men be known, or whether there be not more remarkable persons forgot, than any that stand remembered in the known account of time.” Perhaps Charcot got it right. In 1884 he prompted his protegé, Gilles de la Tourette, to describe nine patients with compulsive tics. One of these was the Marquise de Dampierre who had previously been reported by Jean Marie Gaspard Itard in 1825. The Marquise lived as a recluse, ticking and blaspheming away until her death at the age of 80. We have thus the possibility of three names for the syndrome (the two physicians and the patient) but Charcot preferred the euphonious eponym of Gilles de la Tourette, and so it was.

Remaining in the past, we find that the Stokes-Adams attack which characteristically occurs in patients with heart block was originally (as far as we know) described by Marcus Gerbezius in 1719. He was followed by Giovanni Battista Morgagni (1761) and Thomas Spens (1793) before Robert Adams (1827) and William Stokes (1846) came on the scene, and again, Raymond Vieuussens in 1715 described the collapsing pulse of aortic insufficiency more than a century before Dominic Corrigan himself. Would it be wrong to detect an Anglophone tendency in those days?

A national preference is clearly evident in the case of exophthalmic goitre in so far as this is known as Graves’ disease in English speaking countries, and as von Basedow’s disease in the continent of Europe, despite von Basedow’s description having followed that of Graves after an interval of nearly 40 years. It will come as no surprise, nevertheless, to learn that Robert Graves was a relative latecomer. Caleb Parry had described the disease in 1786, though the case was not published until 1825, three years after his death, but 10 years before Graves’s account. Sir William Osler championed Parry’s priority to such good effect that Parry’s disease entered the literature; however, the competition was too great for it to replace Graves’ disease. However, we would ask you to spare a thought for Giuseppe Flajani, who was responsible for the first published account in 1802. But he really didn’t stand a chance with the description submerged in the third volume of his collected observations.

The eponymous naming of diseases and syndromes should, therefore, never be taken to imply priority of description — that would be far too hazardous an assumption. All too often it is a matter of luck, the laurels being awarded to the person, or author of the description (however incomplete), who the world sees as having been in the right place at the right time to create the necessary impact. Should eponym and priority happen to coincide we have cause for rejoicing. Thus the eponym must, sadly, be regarded as nothing more or less than a convenient shorthand to communication.

When, however, we move from diseases and syndromes (which usually have been sitting in the wings awaiting discovery) to tests, techniques, and the like (which have to be invented) we find ourselves in an infinitely more perilous part of the minefield.

In 1881 Newell Martin of Johns Hopkins University devised an isolated working heart model. Although his system was complex, requiring oxygenation of the perfusing blood by the lungs, it did nevertheless antedate the now standard isolated working rat heart model of Neely et al by some 86 years. Neely et al did not cite Martin’s paper. There was no direct line of descent from Martin to Neely and his group, so no debt was owed to him. In 1967 it was no longer common practice to write fairly exhaustive historical introductions to scientific papers and, presumably, the group did not include a professional medical historian. Martin’s work, advanced though it was, must therefore be relegated to an honourable place in medical history. (A more detailed account of Martin’s work has been given by Opie, though we have identified an earlier paper than the one he cited.)

Langendorff was more fortunate, since his isolated perfused mammalian heart was simple, more suited to metabolic studies, and is still with us, with his name...
attached, as the standard model. Another survivor is the Krebs-Henseleit buffer solution, yet how many of us have read Langendorff's paper or those of Krebs and Henseleit, but alone understood them? We would argue that, since both model and solution have been in use for a sufficiently long time and no one has questioned the originators' priority, there is a strong case for ceasing to cite these references in the bibliography. Other considerations apart, it would probably be the honest thing for most of us to do.

The situation becomes considerably more complex in the case of our next example. The technique of delineating a myocardial infarct by tetrazolium staining for dehydrogenases was originally described by Sandritter and Jestädt in 1958 in the German literature. They used triphenyltetrazolium which forms a red formazan product to improve the delineation. They acknowledged the German workers, and said that their own work "confirms and extends the findings of the European investigators". Which paper is to be cited? Unfortunately, the matter does not end there. In 1983, Mullane et al used Sandritter and Jestädt's technique, but attributed it to Lie et al. A clear case of misattribution? Apparently not, since they had used dogs and Lie et al had also used dogs, whereas Sandritter and Jestädt had used human and guinea pig tissue. This is splitting hairs. Surely it is the first description (and successful use) of the technique that is important, not the subsequent minor modifications and fine tuning? Thereafter significant modifications should be cited as "Smith's technique as modified by Jones". Common sense is obviously necessary, but we should bear in mind that we are biased in favour of the author's viewpoint. However, it sometimes happens that the reviewer is anxious to see his publication, we earnestly hope that controversies over priority will fade into the past, and that it will become progressively easier to detect biases in attribution and misattributions from whatever cause.

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