

CURRENT SCIENCE

Volume 82 Number 6

25 March 2002

EDITORIAL

Credit and discredit

A wonderfully provocative commentary appears in a recent issue of *Nature* (2001, 413, 835). Writing about the allocation, or more correctly the misallocation, of credit, the Cambridge geneticist Peter Lawrence drew attention to the fact that it may indeed be time 'to bring justice to the allocation of credit'. Lawrence seemed incensed by the prospect of a brave new world of science where 'scientists are ranked like tennis players, measured by their numbers of papers, impact factors of the journals concerned, their position in the author list and the number of citations their papers receive'. To climb up the ranking list requires strategic accumulation of credit and Lawrence argues 'that a common way to build rank is to annex credit from junior colleagues'. Lawrence suggests that granting agencies 'ensure that those they pay to run research groups put caring for their groups first and swanning around the world or running companies second. They, as well as prize committees and those assessing job applicants, must cease rewarding those who misappropriate credit'. Lawrence's sharply-worded essay raises many contentious issues, problems of authorship (whose name must appear first in the bylines of scientific papers), the clever use of the conference circuit to build up a few stars, the treatment of research students by supervisors, 'competition within and between groups' and the increasingly damaging impact of the impact-factor measurement. While Lawrence's litany of complaints addresses the contemporary practice of science in Western laboratories with a focus on biomedical research, many of his concerns might indeed be viewed in a wider context.

Modern research has come a long way since the times of Newton and Faraday. The lone researcher toiling away in isolation is practically extinct. Research groups in many disciplines of science are large and the practice of science has become immensely competitive. In academic settings, groups have a pyramidal structure with the 'principal investigator' presiding over a team of graduate students working for PhD degrees and post-doctoral fellows, who lie at the bottom-most rung of the academic ladder. Many areas of research require large-scale-collaborative effort and cooperation amongst members of a research group or even between groups. The sharing of 'credit' necessarily becomes a complex

and sometimes unpleasant issue. Conventionally, the scientific success of group leaders is recognized by grants, awards and lecture invitations; much less credit seems to be distributed to other members of a group. But, in most instances successful attacks on scientific problems requires years of effort, steady direction by groups leaders and appreciable inputs into the training of apprentice scientists; at the end of the day consistent performance of laboratories over long periods of time can be traced back to their heads, who most often get the credit that is their due. There are inevitably and deplorably, cases of exploitation, where the origin of ideas and innovative technique are conveniently forgotten, especially when the advances emanate from the least well-positioned members of a research group. Lawrence uses an evocative metaphor: 'Students are like boosters on space rockets, they accelerate their supervisors into a higher career orbit, and, when their fuel is spent, fall to the ground as burnt-out shells'. Here Lawrence exaggerates, his perceptions clouded by images from some of the most ruthlessly competitive and immensely 'successful' research groups, which are sometimes held up as the standard bearers of the biomedical research enterprise. By and large, the majority of academic research groups still have reasonably healthy mentor-student relationships, with most supervisors forging a life-long bond with the students who pass through their laboratories. In a comment on Lawrence's essay, submitted shortly before he died, Max Perutz recalls that he did not author some papers with his collaborators when his contribution was not critical; but he says 'I had my reward in their lasting respect and affection, and it did not damage my scientific career' (*Nature*, 2002, 415, 819). While most group leaders may not follow Perutz's path, the accepted norms for scientific authorship require at least some measure of contribution from all listed authors, most importantly from the head of a group.

Is Lawrence voicing a predominantly Western concern or is there need to look at our practices in India too? Using the history of the discovery of HIV, the virus that causes AIDS, he argues that there are instances when 'scientific, legal and governmental systems not only failed to curtail but actually rewarded

unethical behaviour'. Our scientific system is particularly vulnerable, when group heads indulge in ethically questionable practices. The structures of our institutions are built to favour those who hold responsible positions, irrespective of their conduct. There are few mechanisms, even in the best of places, to redress difficulties faced by beginning researchers, who find it difficult to adjust to the sometimes unreasonable demands of over-ambitious supervisors. The cancer of the impact factor measurement has spread so widely in our academic evaluation system that we are now confronted in lectures by projections of publication lists, with journal impact factors conveniently displayed. We might do well to read Lawrence's lament: 'We should stop measuring success by where scientists publish and use different criteria, such as whether work has turned out to be original, illuminating and correct'.

Lawrence's central thesis that 'the misallocation of credit is endemic to science' is overstated. There have been a few celebrated examples of egregious error and he cites the case of Selman Waksman, who received the 1952 Nobel prize for Medicine for his discovery of streptomycin. The antibiotic was really the product of

work done exclusively by a student, Albert Schatz, but the professor quickly created a myth which had little place for a co-discoverer. And, as Lawrence notes 'the scientific community . . . helps to ensure that credit always flows up the ladder of rank'. But, we must also see the other side of the coin. Science progresses by the smooth linkage of generations, the torch passing from mentors to students in unobtrusive fashion. The relationships are not always smooth, a feature not uncommon in all human affairs. There are celebrated instances where students taste the highest scientific success, their mentors fading into the background.

Lawrence has, justifiably, stirred a hornets' nest. With crass competitiveness and increasing commercialism fuelling intellectual dishonesty and promoting exploitative practices in research laboratories, the scientific community needs to discourage many 'disturbing trends and practices' that Lawrence has highlighted. Left unchecked, these trends will bring discredit to science.

P. Balaram

