Rank injustice

The misallocation of credit is endemic in science.

Peter A. Lawrence

What does rank really mean? Last year's Wimbledon men's tennis champion, Goran Ivanisevic, was ranked 125th in the world at the start of the competition.

What has rank to do with the process of creative discovery in science? Very little. What has rank to do with the politics of science and the allocation of credit for discoveries? Almost everything.

Imagine a time, perhaps not so far off, when scientists are ranked like tennis players, measured by their number of papers, impact factors of the journals concerned, their position in the author list and the number of citations their papers receive — put these numbers into a computer and watch it generate your publicly available ranking as number 2,340 in the world! Indeed, a tendency to rank like this already exists, causing biomedical scientists to focus more on their careers and less on understanding nature and disease.

Here I argue that a common way to build rank is to annex credit from junior colleagues. To stop this I would like to see granting agencies meet, agree on and publicize principles of how the contribution and responsibility of those scientists they support should be indicated in the list of authors of papers. These agencies should also 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. 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.

The history of the discovery of HIV (the AIDS virus) and its aftermath, described compellingly by the journalist John Crewdson[1], graphically illustrates how scientific, legal and government systems not only failed to curtail but actually rewarded unethical behaviour.

Problems in the ranks

For a week or two after arriving at the MRC (Medical Research Council) Laboratory of Molecular Biology in Cambridge, UK, in 1962, a young graduate student, Mark Bretscher, addressed Francis Crick as "Dr Crick". Crick told him to "stop that nonsense" and to call him by his first name. In the MRC, Crick explained, distinctions based on rank reduced communication and were inimical to progress.

The 1952 Nobel prize for medicine or physiology was awarded to Selman Waksman, primarily for the discovery of streptomycin. Yet it was his graduate student, Albert Schatz, who discovered the antibiotic while working alone in an isolated basement laboratory. Waksman did not once visit this laboratory during what Schatz described[2] as "just four months of work, day and night". Schatz even established in court that he was the joint discoverer of the antibiotic, yet Waksman created the myth that he alone deserved credit, a myth widely accepted by contemporary scientists ("the higher their status, the more likely they were to side with Waksman...without apparently acquainting themselves with the details of the case")[3].

More recently, Nobel committees have tried hard to look beyond rank and publications to assign credit properly. A good example is the 1984 Nobel prize awarded to Georges Köhler and César Milstein for their collaborative discovery of monoclonal antibodies — Köhler was a postdoc at the time. Unfortunately, many other prize giving bodies do not take the same care.

Crewdson, in a painstakingly researched history, describes the isolation of HTLV-1 (human T-cell leukaemia virus), crediting it mainly to Bernie Poiesz and Frank Ruscetti, who were postdocs in Robert Gallo's lab. Later, the credit for this discovery seemed to become Gallo's alone. Looking back, Poiesz reflected: "There are many different reasons why people associate Bob Gallo's name as the discoverer of HTLV, I can't change how people perceive it, or how people presented it to the media. The only thing I can do is do my work. I spent many many nights in that laboratory. The moment of discovery was mine"[1,4].

Every scientist knows what Poiesz means by the "moment of discovery": it happens when one person alone, or a group of people together, find something or come to understand something for the first time. It cannot be taken away or transferred, and is indelible, being clearly recalled by each person involved because such moments are so significant — and so rare — in any scientist's life.

In contrast to the moment of discovery itself, the history around it can be so easily rewritten and credit so easily transferred — especially from juniors to seniors. The moment of discovery of LAV (later named HIV) was traced by Crewdson to two scientists, Françoise Barré (now Barré-Sinoussi) and Jean-Claude Chermann, working with Luc Montagnier. "When we started," says Chermann, "it was the virus of Barré, Chermann and Montagnier. Then Montagnier, Chermann and Barré. In 1985 it was the team of Pasteur. In 1986, it was Montagnier". Many colleagues view Barré-Sinoussi as the scientist who deserves the most credit for discovering the AIDS virus, yet it is she who has received the least[1].

Credit due

The scientific community supports the natural tendency of the experienced to take advantage of the inexperienced, and helps to ensure that credit always flows up the ladder of rank. Most of us know examples of how, over time, the contributions of younger

colleagues have become extinguished. There are some celebrated cases (for example, Hilde Mangold[5] and Candace Pert[6]) and countless uncelebrated ones. Although it is usually true that some credit properly belongs to others who were not present at the moment of discovery, it is too often the senior absentees who manage to claim all of it.

The practice of science has changed; nowadays, younger scientists (graduate students and even postdocs) are given little independence, and work under the control of a principal investigator (PI), whose role is to decide the overall field of research and to obtain grants, and who is given the credit for the discoveries of his or her underlings. I write "is given" because, although the PI does not always grasp the credit, any misappropriation tends to be encouraged by several practices.

First, we cannot remember too many names, so in our memories and conversations we confirm and reconfirm the senior author of a paper as the one who wrote it, or made the discovery described, even if this is not the case.

Second, the conference circuit is designed to build up a few stars. Presentation and publicity has come to count for more than discovery and publication. I know many good scientists who refrain from travelling so that they can concentrate on teaching and working in their labs — and whose reputations suffer because they put their primary responsibilities first.

The etiquette of conference lectures is revealing. A talk summarizing the work of a group is usually given by the PI, who mentions results simply as found "in the lab". The truth would be more like: "done by someone in my group, I may or may not have suggested it — in any case I would like you, the audience, to take it as mine". At the end of the talk, the PI thanks many people, often from over several years. The motivation may be honest, but the effect is that nobody remembers any name except that of the speaker.

Third, the exponential rise in the secondary literature allows PIs to write numerous reviews of their field, giving their own perspective of discoveries and keeping their names in the limelight. Because journals usually limit the number of citations in primary and secondary articles, authors have to refer to other reviews, reinforcing a few 'star' names, fixing them in the memory and cementing them as 'the' leading experts. PIs can even leave much of the actual writing to their juniors, co-authoring reviews to ensure that the credit goes to them.

Fourth, the treatment, fate and attitude of graduate students helps the process. The productive engine of larger groups has become mainly graduate students, yet the chief beneficiary is the PI —

> There's no remedy; 'tis the curse of service, Preferment goes by letter and affection.
>
> **Iago in *Othello***

because students, unlike postdocs, usually do not go elsewhere to become competitors, and students' publications are too few for their names to be remembered. 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.

Students may be treated as technicians and given laborious projects, providing little time to innovate, explore and reflect. Control by the PI is reinforced by the ever-present awareness that, one day, the student will need a reference on which so much will depend. Even minor problems may cause that reference to lack the all-too-necessary enthusiasm — making it hard to get a postdoctoral position and funding. Also, competition within and between groups can make a student's time stressful and unpleasant[7], causing many to leave research. It is interesting that a larger proportion of women than men drop out of research after finishing their PhDs[8]. In my opinion, this is not a sign of gender discrimination, but is because more women than men find aggression and competition distasteful.

Modern science is very fashion-conscious. Groups are getting larger, yet the available amount of imagination per PI cannot have improved — the result is that several graduate students in different labs can be given similar projects. Consequently, two or more papers containing similar data and conclusions are sometimes published at the same time; other related projects may become unpublishable. This situation is wasteful as well as devastating for those students who are 'scooped'.

It would be easy to claim that we established scientists are entirely to blame for exploiting the young. But many young people have a timid and careerist attitude to research. Most of all, they want a PhD and to minimize risk by taking up a 'safe' project. But safety is elusive, because others may choose the same project for the same reasons. These careerists are eager to leave the bench almost before they have become proficient, to work by proxy through the next batch of junior scientists to perpetuate the system.

Rank and file

Everyone has their views on the contentious topic of authorship. Mine are that the person who is most responsible for the scientific findings and conclusions should be the first author, write the paper, settle any differences and take responsibility for its contents (good and bad). Yet in reality, the PI takes the lead (and his/her name occupies the final position in the author list), rarely having done the experiments or sometimes even written the paper — providing many opportunities for muddles and worse. Gallo, for example, spent much of the mid-1980s travelling, yet managed to author up to 90 papers per year!

In general, PIs accept rewards that stem from papers authored in this way, but shrug off responsibility if it turns out that the work has been sloppy or even fraudulent. Hence the position of an author's name has come to signal more about his or her rank than individual responsibility for the paper's contents.

Ranking impact

The impact-factor measurement was introduced to reveal average numbers of quotations for papers. It has evolved to become an end in itself — the driving force for scientists to improve their reputation or get a position, and causes damaging competition between journals.

In the past 10 years there has been a big increase in medically related papers in top biology journals. I believe that some of these papers are chosen for their beneficial effects on the impact factor, rather than for their scientific quality. Papers in large fields are favoured at the expense of those in smaller ones, for the same reason.

More specialized journals are publishing reviews; again, the motivation is to improve the impact factor as, on average, reviews are cited more often than research papers. Many journals publish editorials, minireviews and so on; surprisingly, these generate references that count towards the impact factor without these articles themselves counting in the same equation[9]!

Many scientists are saddened and frustrated by the trends and practices I have described, but they also know that research can be creative, rewarding and worthwhile. Can these people work together to see that those who make discoveries are justly credited? Perhaps they can ask those who appoint scientists, as well as those who award grants and prizes, to bring justice to the allocation of credit. ■

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