

Magazine

My word

No man is an island...

Tony Hyman

As much of my work is collaborative, I am very aware of the virtues and difficulties of collaboration in the modern scientific world. Most collaborations are short term; perhaps one scientist has an assay, the other a reagent, and an obvious quick gain will result from joining forces. But greater (though often less concrete) benefits come from longer term collaborations. Your collaborator may be the one person you trust to tell you if your ideas are nonsense, or the person you call for advice when you're barricaded in your office hiding from your students. The hallowed halls of the biology hall of fame ring with the rhythms of famous names in pairs: Watson and Crick, Bishop and Varmus, Brown and Goldstein, Nüsslein-Volhard and Weischaus. The productivity of these pairings make the benefits of collaboration clear. Why, then, are true collaborations between scientists of equal stature so rare?

Laboratories tend to have an island mentality; the group leader sits alone at the top of a pyramid, waiting for a student or postdoc to produce something for them to talk about at the next meeting.

Interactions between these small kingdoms are infrequent, and their rules are set well in advance. "The paper has to be from my lab because the reagents are mine" or "you can be on the paper if you show us how to do your assay" are conditions that are frequently made before any experiments are done. When the paper becomes reality, the lab heads rarely say "that was great, let's go on collaborating". Instead, they often use what they learned in the collaboration to compete with each

other. This view of the world is sour, but it's based on experience.

What are the pressures that induce such behaviour? Of course, it all boils down to credit. Who did the most, where did the idea come from? Is the benefit of collaborating larger than the loss of credit that results? Division of credit is never more fraught than when work is prepared for publication. First you discuss how to eliminate as many authors as possible. Then you argue bitterly over who should be first author, who last. This journal, when starting to take research papers, even proposed that each author should provide a description of their contribution to the research in the paper. (Bloggs did all the work while Smith made the tea, for example).

How far we have come from the original system used by the Royal Society, in which the names of all those associated with the work were listed alphabetically. Perhaps we should give each author a credit percentage, so that assessing the lifetime contribution of an individual just requires you to add up his or her total score. Or we could adopt the patented Hyman two-dimensional system, which allows you to have as many first and last authors as you like, and looks like this:

First authors	Middle authors	Last authors
Green, J. Newman, X.	Bloggs, P. Noggin, Q.	Boss, B. King, R.

The anxiety over apportioning credit arises because individual productivity is the only thing that the system values. Jobs are given and tenure granted for the total output of papers. A side-effect is that the people who used to stimulate everyone in the lab to do groundbreaking experiments, but were too busy talking (and drinking tea) to do any experiments themselves, are a dying breed.

In some US universities, papers done in collaboration are not counted

toward tenure decisions. When I first heard this, I could not believe it. This system seems to me to select for people who like to have a miserable time doing science and prefer to slow down progress, to enhance their own credit. In this respect I feel fortunate to be at EMBL, where, when your contract comes up for renewal, it is a bad sign if you have published no papers in collaboration.

The truth is that no laboratory is an island. Modern biology is a huge enterprise that collects and integrates information from many different systems. It is very doubtful that any of us will be remembered as a scientific giant, in the same category as Einstein or Faraday, yet the selection against collaboration as a criterion for success means that we spend our whole lives worrying about credit with little to show for it in the end. Each lab is judged by "output", instead of by its overall contribution to science, surely a terrible thing.

Although modern biology is an industry, the way we go about it is preindustrial; our labs are a collection of small cottage industries, each of which tries to place an individual stamp on the product, like weavers putting out their own special designs. Limited collaboration is relatively easy — one person may dye the wool while another weaves (though a dyer, even one who develops a new process and makes new colours possible, runs the risk of being labeled as a technician). Getting two weavers together is much more difficult. But in my experience, once the initial difficulties are overcome, long-term collaborations are extremely fruitful for both parties. Two people working together seem to provide more than the sum of two brains, allowing a sharing of knowledge and experience far beyond what's normally possible.

Alan Sawyer read this column for me, so he gets 2% of the credit.

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