IS THIS PROFOUND AND SIGNIFICANT?

Bruce Yardley

I recently had the pleasure of travelling around a wide range of university departments in Europe and North America, courtesy of the Mineralogical Society of America Distinguished Lecturer programme, and a very interesting and enjoyable experience it was. One feature of academic departments today is that, wherever you go, you find really interesting work being done by enthusiastic people who are in touch with the frontiers of their field. The days are long gone (if they ever existed, I am relying on rumours I heard from greybeards back in my youth) when the only people worth talking to, if you were a world leader in your field, were a few colleagues in elite institutions. Of course this means that more and more researchers are carrying out investigations with an underlying purpose and curiosity that goes beyond just doing more of what they have always done. This has to be a good thing, especially where the training of young researchers is concerned, but there is one interesting result that we have to live with: it has now become a requirement of funding agencies and journal editors alike that everything we do must be clearly identified as of profound and lasting significance.

It has now become a requirement of funding agencies and journal editors alike that everything we do must be clearly identified as of profound and lasting significance.

Choosing to embark only on research that will be of profound significance sounds fine (who would vote the other way?), except that none of us (I think) really knows in advance whether or not the work we are starting is going to make a fundamental contribution to human understanding. As Susan Stipp has emphasised in her editorial in this issue, truly original research is often the research which ends up going in completely unexpected directions. Which brings me to a dilemma for the 21st century scientist: Should we try to ensure that everything we do has underlying significance and search for deep meanings in all our data, to ensure that we don't become complacent and repetitive? Or should we concentrate on doing what we enjoy and are good at, for "the pleasure of finding things out," and then if need be, dress it up to look like it is potentially profound and relevant to get it past our peers?

At the beginning of the 20th century, most scientists seem to have been naturally modest people, but by the end, even those who had much to be modest about had learnt to push their work forward as though it held the key to the universe. Einstein's revolutionary paper in March 1905 was entitled "On a heuristic viewpoint concerning the production and transformation of light"; today, perhaps it would have to be called "Energy comes in quanta: Say goodbye to waves" before any potential reviewers or readers would sit up and take notice. It is easy to poke fun at over-the-top attempts to claim a major advance by people who have just rediscovered the wheel, but I am not sure we should do so too severely, because these are also people who have enthusiasm and drive, which means that sometimes they really do break new ground. Cynics don't waste time pursuing lost causes, but they never actually make new breakthroughs either. On the other hand, overly enthusiastic claims for one's work are usually only a mild embarrassment in later life, not career stoppers (although there have been some exceptions!). The fact is that some people feel comfortable making brash claims for the importance of what they do, while others remain reserved and self-effacing. Despite the international nature of science, a certain amount of national stereotyping is still possible here, though I have noticed that if an Englishman modestly suggests that his latest work really says nothing very new, in recent years his fellow-countrymen have become rather too keen to simply agree.

What is more invidious, and arguably a greater hindrance to the progress of science, than claiming profound significance for the mundane is when authors of papers claim that their studies provide proof for the established hypothesis of the day. A paper saying that the majority view is correct is relatively easy to get past referees, so what sometimes happens is that quite mundane observations are cited as providing "tests" or "proofs" of currently popular hypotheses. This makes the study appear more profound and significant, but also boosts the status of the hypothesis, perhaps undeservedly. A test is possible when a particular hypothesis predicts one result while alternative hypotheses predict different answers. By this definition, many observations that are supposed to verify the veracity of a hypothesis are simply consistent with it, but might also be consistent with all other hypotheses as well. For example, an isochore constructed for a selected fluid inclusion from a metamorphic rock may pass through the *P–T* conditions at which the host mineral is supposed to have formed. However the isochore also passes through a swath of other *P–T* conditions, while if other nearby fluid inclusions are considered, there may be little *P–T* space remaining outside the wedge of possible conditions. But if some of the data give the answer we expect, it is amazing how often these data are emphasised. What a sad reflection it is on how we sometimes use our sophisticated modern facilities if we only accept their evidence when it agrees with our pre-existing (and therefore ill-informed) prejudices!

If we can agree that such an attitude is wrong, should we reject the pressure to show that all our work is of wider significance beyond its immediate subject? One approach would be to go back to making the observations and reporting the results for the love of it, not worrying about whether or not they address burning issues of general interest. That might seem a very pure and objective way of doing science, but it also runs the risk of enshrining mediocrity by removing the incentive to move on. We don't give medals to people because they have reached the average age for researchers and have published an average number of papers that have received an average number of citations; we give them to people who do exceptional work.

Instead, I put in a plea to keep faith with the brash and arrogant who are trying to move in new directions, but let's not make it easy for them to fake it. Reviewers should be ruthless in pointing out whether papers solve problems or simply add to an already massive pile of inconclusive data. If the necessary measurements to resolve the problem were not made, the work was not done adequately. If the required measurements are beyond our current technologies, why waste time trying to solve the problem now? But when there are good data helping us to understand better how the world works, we need researchers/authors who have been trained to think about the potential implications of their work and are prepared to push them at the community. Otherwise, no one will notice. So by all means pursue your science for the pleasure of discovery, but unless you have a natural flair for your subject that allows you to see the value of your work and its wider significance as a matter of instinct, then having to demonstrate these things in order to get funded or published may be good discipline, and not such a bad thing after all.

Bruce Yardley
University of Leeds