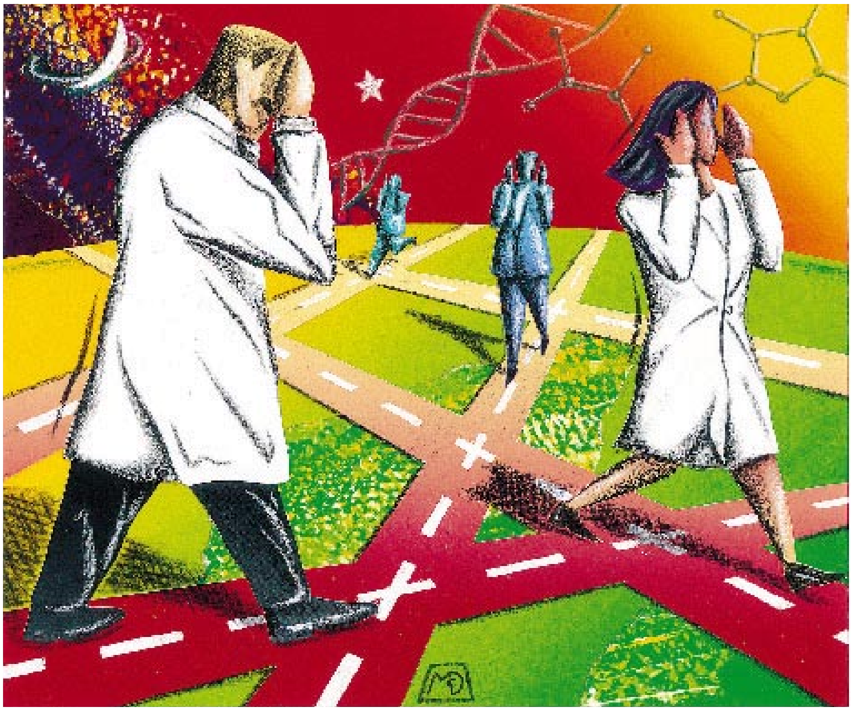
commentary

What cannot be said in science

The scientific enterprise is full of experts on specialist areas but woefully short of people with a unified world-view. This state of affairs can only inhibit progress, and could threaten political and financial support for research.

Mott T. Greene



MARK DOBSON

Scientists have an understandable modesty about publicly discussing areas of research other than their own. But this reticence has had the unforeseen consequence that generalization and synthesis, essential parts of the advance of science, are very much neglected. Scientists are trapped in their own specialisms, leaving others, often poorly qualified, to represent to the public the larger architecture and interconnections of modern scientific theories. Although the capacity to convey to society a compelling vision of the whole of science may not be necessary in the day-to-day progress of investigation, it is crucial in maintaining cultural, political and financial support for science.

Scientific education has become so specialized that scientific literacy is little more advanced among scientists than it is among non-scientists. Undergraduates who have completed courses on cell biology and evolution are unable to discuss broad issues in evolutionary theory, let alone Earth history or cosmogony, in any greater depth than can their non-scientist peers. Physics students don't know how a protein differs from a nucleic acid; chemistry students don't know the age of the Earth; geology students cannot give a simple account of metabolism or say why the sky is blue.

This is not to say that science students cannot understand several fields of science and their connections. But a generalized curiosity has not been encouraged or reinforced in basic science training for almost a century. The robust pride that one's knowledge of science is narrow and deep is almost universal among specialists and is powerfully reinforced by three related phenomena.

Narrow depth of knowledge

First, there is no provision in undergraduate curricula for broad acquaintance with several sciences. The norm in the United States, as in many other places, is self-selection of a single science in the first year. Basic science education is like basic training in the military — directed to tactics rather than to strategy and designed to teach recruits as quickly as possible to use the latest weapons, so that they can be sent to the research front at the earliest opportunity.

Second, there is the problem of the impenetrability of specialist discourse — not only to non-scientists, but to highly trained scientists in different specialisms. Journals such as *Nature* were created to provide rapid publication of results of such importance that they ought to be communicated beyond the boundaries of individual fields. This function will be frustrated if articles are written in language understood by no-one outside the authors' fields of expertise.

Third, and perhaps more subtle, is the general and strong sense among scientists that, because the advance of science depends on the accumulation of knowledge rather than of opinion, they are not permitted to speak about scientific subjects in public other than those in which they are expert. When Erwin Schrödinger published *What is Life?* in 1944 (ref. 1), he began with an apology: "A scientist is supposed to have a complete and thorough knowledge, at first hand, of some subjects, and therefore is usually expected not to write on any topic of which he is not a master." Freeman Dyson extended this apology in *Origins of Life* more than 40 years later[2], only to discover that many biologists had not yet forgiven Schrödinger and now were annoyed at him as well.

At less exalted levels of discourse, the imperative to segregate oneself within one's specialism is strong enough to impede the development of interdisciplinary undergraduate science courses even at the first-year level. Most doctoral-level scientists think themselves unqualified to teach a first-year undergraduate course even in a closely allied discipline.

Specialization is not itself the problem. As the volume of knowledge increases, the proportion of the total comprehended by an individual must diminish. Yet specialization has had unanticipated and even paradoxical consequences.

The paradox is that specialization, however necessary, is not all of science: generalization and synthesis are parts of it as well. Yet generalization and synthesis, even as long as 50 years ago, were well on their way to disappearing altogether from the careers of scientists. I am not referring to textbooks, review articles or the occasional popular lecture, but the deliberate attempt to summarize how the work of one's field fits into the larger framework of scientific advance. Scientists used to do this regularly. That they no longer see synthesis as even a remotely plausible activity is a measure of how completely 'what goes without saying' can pass within a generation or so into 'what cannot be said'.

Stigma of popularization

The loss of a view of the whole travels in harness with a contempt for generalization — invariably stigmatized as 'popularization' or 'speculation' — and with an irritation directed at those who claim that historically things were different, and even better.

This problem is not resolved even if one takes the position that scientific work is 'self-integrating' — that it is structured so it can function well even if no-one is in charge of the overall picture. There is still a need for some compelling vision of the whole of science and of its worth to animate those other 'necessary regulators of scientific advance': elected officials who vote on whether to support science, and their constituents.

On the other hand, the unhappiness of many scientists with the picture of the whole presented by historians and others who study science could be simply a negative reaction to seeing the state of science as a whole when it is reasonably well represented, rather than a well-informed reaction to a supposed misrepresentation. One must wonder at the criteria by which scientists can determine the accuracy of any historical representation, given that most have declared themselves ineligible to comment on issues outside their own part of the research front.

But I have never met a scientist, however specialized, who felt she or he could not discuss what science is, or the scientific viewpoint, or the scientific method, or the difference between science and superstition. So an interesting phenomenon has evolved in this century — people who freely admit not knowing much detail about matters beyond their own field of science, and who would never speak about any other aspect of science in particular, but are perfectly comfortable speaking about science in general. How did this happen?

Birth of subdisciplines

The answer lies not in the fact of specialization, but in the way it comes about. Take a group of investigators working on a problem. They decide that new data of a different kind are needed to solve it. In attempting to obtain this information, they stumble across a genuinely interesting but separate problem. Hence a specialism is born of a failed excursion in which the line of investigation is complex enough to require recruitment of graduate students to finish the job. Students are recruited by presenting the problem as a hot, new, cutting-edge field.

When the students arrive, they hear about 'the problem', which their advisers divide into parts so that no student sees the whole picture. The students graduate, decide that they need an annual meeting (as each of them knows only part of the problem), their own specialist journal, their own students and their own funding — and the sub-field is permanently established.

The first breakaway group was trained in one field, but has helped almost inadvertently to create another. When its members retire and write their memoirs, they recall that the field they helped to start came from another field of which they still feel themselves to be members, and they think of themselves as having helped to solve the original problem. But the students of this first group are the first generation of the new field. And when *they* retire and write their memoirs, they recall their good fortune at entering the field "just as their science assumed its modern form". They think of themselves as having supplanted the field of their advisers, in the sense that, even though the field still exists, they see it as behind the current research front. They assume a historical discontinuity — a case of replacement, not of descent.

Hence there are two historical views of the same situation, one recording an evolutionary development and the other a revolutionary rupture. The shift in mental geometry is critical — the first view implies a structure with return as one plausible course of action. But the second view contains no such possibility. This is exactly the moment where 'what goes without saying' turns into 'what cannot be said' — and it happens very, very fast.

There are dozens of examples, but two will suffice. First is the case of cladistic systematists and biogeographers, who see themselves as having supplanted other approaches to systematics and biogeography at the research front. Their technique of exhaustive characterization and splitting based on structural divergence works well with living organisms. But applied to palaeontology, the technique has made it impossible to infer evolutionary descent of very similar but distinct organisms in conformably bedded strata. Cladists are so concerned with the classification of organisms that they have forgotten to tell their students (or were never told themselves) that the point of having a fossil record is to document evolutionary modification and descent. A system of classifying fossils that makes statements about descent impossible is useless for evolutionary biology. It will split biology and palaeontology apart and leave them with two different concepts of a species.

My second example is numerical modelling in the Earth sciences — another new approach that sees itself crowding out older ones at the research front. In constructing numerical models, it is necessary to use real data as a check on the model's adequacy, then to use more and more real data to tweak the model a little better. But there is a danger of using all the data in the world to make the model, to the point that there is nothing left for the model to interpret. Further, there is the danger of accepting existing constraints in model-building as legitimate constraints on theory (taking seriously global climate models with no clouds, for instance).

In both cases, the main aim of the exercise has become to have the 'best model' — of the classification of organisms or some dynamic system in the Earth — without regard to the consequences for the unity of theory or the adequacy of explanations provided for the problems these fields were founded to solve.

But isn't this just how science works? Yes, if 'works' is defined as the current internal standard of self-advance by continued unbalanced specialization and the indefinite postponement of synthesis.

This state of affairs comes about not when scientists are behaving badly, but when they are behaving well; not when they fail to meet the challenges they were trained to face, but when they do meet them. Yet this pattern will not advance scientific literacy, but must further corrode it. It creates cohorts of young scientists who do not perform the integrating function and don't know anyone who does or ever did do it.

There no longer exists a reward structure encouraging scientists in mid-career to pause, reflect and produce careful narratives of continuity of descent, linking new developments to the great structure of science that precedes them, and impartially sorting claims of novelty. No such structure has existed for almost a century.

Historians of science are thus the unintentional residuary legatees of the process of scientific integration and the 'view of the whole'. I should point out, as I am a historian of science, that historians of science could never take over the function of producing general scientific literacy. There are only a few thousand of us in the world, and only a very few study contemporary science in a way requiring complex mastery of the ideas of several fields. There are notable exceptions, for example Steven G. Brush's history of modern planetary physics[3], but these are not directed at producing general literacy among nonspecialists.

The point of generalization

In the seventeenth century, people used all the science they knew to explain the operations of the world, and then 'plugged the remaining holes with God'. Now there is enough science for a world-view with widely placed small holes. Yet most people, including increasing numbers of natural scientists, have a world-view with large and rather closely placed holes that they are content to fill either with blithe ignorance or with supernatural explanations for phenomena already well understood in physical terms.

So today's scientists are in command of only a small part of what is known, and there are no educational or career structures that mandate, suggest or reward the synthesis of results into a unified world picture. If this trend continues, one can imagine a world dominated by the results and artefacts of natural science, but in which no-one has a scientific world-view. This outcome, not as bizarre or unlikely as it may appear at first, would be remarkable, not least for the danger it would pose to the continued survival of the scientific enterprise. □

Mott T. Greene is at the University of Puget Sound, Tacoma, Washington 98416, USA.
e-mail: greene@ups.edu

1. Schrödinger, E. *What is Life?* (Cambridge Univ. Press, 1944).
2. Dyson, F. *Origins of Life* (Cambridge Univ. Press, 1985).
3. Brush, S. *A History of Modern Planetary Physics*. 3 vols (Cambridge Univ. Press, 1996).