Graphitic carbon copies

SIR—David Swinbanks, reporting from Tokyo (*Nature* **348**, 186; 1990), represented both PAN (polyacrylonitrile)-based and pitch-based carbon fibres as having been invented in Japan. While this is clearly correct with regard to pitch-based fibre, I don't believe it to be true for the more important PAN-based fibres.

High-strength graphitic carbon fibre made by progressive oxidation and heat-treatment of polyacrylonitrile was developed by William Watt and his collaborators, L. N. Phillips and W. Johnson, working at the Royal Aircraft Establishment (RAE) in Farnborough, England, in the mid-1960s. According to Sir Alan Cottrell (at that time scientific adivser in the Cabinet Office), Watt was stimulated to this initiative by attending a Royal Society discussion on new materials in 1963 (A. H. Cottrell, *Proc. R. Soc. Lond.* A**319**, 3–4 ; 1970). The RAE work was patented in 1964 and first published in 1966. In 1970, during a Royal Society discussion on strong fibrous solids, Watt gave a detailed account of the carbon fibres invented at the RAE (*Proc. R. Soc. Lond.* A**319**, 5–15 1970;).

The new PAN-based fibre was manufactured in Britain by Courtaulds; US and Japanese manufacturers joined in too and, not for the first time and undoubtedly not for the last time in industrial history, the Japanese gradually cornered a large market share. It took a long time for carbon-fibre-reinforced polymer to be accepted in the marketplace, and there were major upsets on the way such as the Rolls-Royce experience with carbon-fibre-reinforced turbine fan blades. No doubt the Japanese manufacturers felt able to take a longer-term view than the British. None of this deprives Watt and his colleagues of the distinction of having invented and perfected one of the major new materials of the postwar period.

ROBERT W. CAHN
Department of Materials Science,
University of Cambridge,
Pembroke Street,
Cambridge CB2 3QZ, UK

DAVID SWINBANKS REPLIES—PAN carbon fibre was invented by Akio Shindo at the Government Industrial Research Institute on Osaka and he published the work in 1961 (*Studies on Graphite Fibre*, Rep. No. 317, Gov. Ind. Res. Inst., Osaka, December 1961). He was awarded Japanese patents for the material in 1962 (patent number: Sho 37–4405) and 1963 (Sho 38–12375). British and US patents were also awarded to Tokai Denkyoku Seizo KK of Japan before the Royal Aircraft Establishment began its work.

William Watt and his colleagues referenced Shindo's earlier work in a paper in *Nature* in 1987 (*Nature* **213**, 690–691; 1987) and in subsequent correspondence in *Nature* they clarified that PAN carbon fibres were first made by Shindo and that their contribution was to increase the strength of the fibres by applying tension in the fibres during the early stages of preparation (*Nature* **220**, 835; 1968). □

Abstracting art

SIR—Much as I agree with the spirit of Rosner's criticisms (*Nature* **345**, 108; 1990) of science as a product, I think that he misses a major reason for the spread of assertive sentence titles — the need for today's abstracting services (and today's researchers scanning *Current Contents* and the like) to compress as much information into as few words as possible so that the readers can decide whether a particular paper is worth looking at in full.

Consider Rosner's example of DNA. Studies showing that DNA is the genetic material and that it is not could both have the title, say, "A study of DNA and inheritance factors", or even just "DNA and the genetic material". Nor would a list of keywords be of help in distinguishing the two studies. Only by compressing their findings into a short (and, with skill, elegantly pithy) declarative title can the two studies distinguish themselves from one another for the scanners and abstracters of the world. This is a valid intermediate step in connecting the right people with the right research. The title is an identifier, not the work itself. Dissertation-type titles (all three or four lines of them) are not particularly appropriate for research articles, which meet a different need. As a compression, a title is bound to contain more flatly assertive statements than qualifiers ("DNA might be the genetic material"?) Besides, a paper's abstract and summary sections are themselves compressions of the research being reported.

But there is a danger that the drive to be succinct and informative will lead instead to sensationalist titles and the oversimplification, even outright misrepresentation, of the work. "DNA needs SEX." Now that I have your attention, I think that it is this danger that Rosner is really concerned about, and rightly so. As do Infante and Husazagh (*Nature* **346**, 505; 1990), Rosner condemns the erosion of scholarship and the rise of simple-minded science brought on by the hubris of increasing technical sophistication gratifying popular expectations of what 'modern' science should be. These criticisms can be derived from Medawar's 1963 criticism ("Is the Scientific Paper a Fraud?" in *The Art of the Soluble*, Methuen, London) of the obsession of scientific reports with inductive reasoning, that mythical theory-free route to scientific truth. To the detriment of all, induction is being discovered as for the first time, by a generation of naive, but well-funded (and thus arrogant), researchers (who also sit on funding boards) who think they need frame no hypotheses to obtain *the* answer. They are oblivious to the theory-dependency and conceptual ancestry of the research programmes they have been born into.

RICHARD T. O'GRADY
Department of Invertebrate Zoology,
National Museum of Natural History,
Smithsonian Institution
Washington, DC 20560, USA

How to get on

SIR—Selection committees often find if difficult to assess the research ability of those they interview. One way to overcome this problem would be to ask candidates for promotion to say what they consider to be their most important contribution to the literature and to illustrate its usefulness and originality by referring to papers from other laboratories that confirm their own work as well as papers that have used their observations as a step to further knowledge.

A. E. STUART
Ardshiel, Camus Cross,
Isleornsay, Isle of Skye, UK

Right to reply

SIR—The question "Why do simple organisms such as bacteria not develop the same complexity as other (organisms)?" (*Nature* **345**, 707; 1990) is wrongly put: it reveals P. R. Sheldon as a eukaryotic chauvinist. We prokaryotes are not nearly as simple as we may seem to you. We can pack more biochemical versatility into a couple of cubic micrometres than can you in a couple of kilograms of liver, kidneys and other offal. We do not need hearts and lungs, as we breathe by diffusion. With propellers more efficient than yours, we can swim at speeds exceeding a thousand lengths a minute. To fly we don't need wings of bone, muscle and feather: we just get blown around effortlessly, and thereby we can get anywhere. And we can reproduce at rates many orders of magnitude higher than yours, producing offspring with considerably fewer initial contortions and no ultimate pain. Correspondingly we have evolved much, much faster. The trick to all this is, of course, miniaturization unrivalled by even the tiniest midge or the cleverest of human technologies.

So I would rephrase Sheldon's question thus: "Why do organisms such as bacteria not need to develop physical complexity?" The answer would then be obvious.

E. COLI
La Jolla, California, USA