forting background of assumptions held in common with their readers. But they should at least try to be clear, at whatever cost to their credibility.

Dr Sheldrake writes as Mrs Bloom daydreamed, with no one theme rigorously explored before it sets off another which before resolution gives way to something else again. It would be unkind to suggest that this is a device for escaping from difficulties, but even readers who are wholly unsympathetic might welcome a clearer view of the author's position. For example, in discussing the limitations of morphic resonance he assumes that while past events can be effective now, future events cannot. While conceding that it is logically conceivable that they might be, he excludes the future on the ground of simplicity and remarks severely that "only if there were persuasive empirical evidence for a physical influence from future morphic events would it become necessary to take this possibility seriously". Apart from the ambiguity of introducing *physical* influences into a discussion of extra-physical phenomena, one is left wondering why the same severity is not applied to the past.

Indeed, Dr Sheldrake does believe that his ideas are capable of receiving support from experiment, but his proposals for experiments are curiously tentative and unsatisfactory. Thus

> ... if thousands of rats were trained to perform a new task in a laboratory in London, similar rats should learn to carry out the same task more quickly in laboratories everywhere else. If the speed of learning of rats in another laboratory, say in New York, were to be measured before and after the rats in London were trained, the rats tested on the second occasion should learn more quickly than those tested on the first.

Well, yes, but so they should without the London intervention, and any quantitative predictions in the operation of this hypothetical principle are so wholly arbitrary that the design of such experiments would be difficult indeed. Dr Sheldrake concedes this in his rather casual suggestions for a handful of investigations in each of which he describes a possible result *supporting* formative causation, but the opposite result is inconclusive. It would be a help if he could offer us predictions the failure of which would end the matter.

Anyone tempted to take formative causation or morphic resonance seriously should ask themselves why. A world haunted by messages from the past, some, like those from morphic units of extinct species, destined to vibrate eternally and in vain while seeking a morphic germ with which to resonate, may have a poetic appeal. Unfortunately it may also appeal to a perverse fear of scientific understanding. Dr Sheldrake explains early in the book that while some outstanding biological problems are difficult, others are, in principle, insoluble — for example, those associated with evolution and with the origin of life. Neither, as it happens, is suitable for the operation of formative causation, since they are creative and unique rather than repetitive. But by the end of his exposition one reader had the distinct impression that intrinsic insolubility had its own attractions for him and that the hypothesis of formative causation was his contribution to a happy state of confusion. □

D.R. Newth is Regius Professor of Zoology at the University of Glasgow.

IMAGE UNAVAILABLE FOR COPYRIGHT REASONS

Lindy Guinness

Rupert Sheldrake — contribution to a happy state of confusion?

Selling newspapers and selling science

Eric Ashby

Reflections on Science and the Media. By June Goodfield. Pp.128. ISBN 0-87168-252-4. (American Association for the Advancement of Science: 1981.) $9.

"JULES de Goncourt once wrote that . . . the newspaper bore the same relationship to a book as a whore did to a decent woman". A verdict, as June Goodfield says, "too harsh by far"; but as a caricature of the way some of the mass media deal with science it has an uncomfortable relevance. A news item is a one-night affair unless it attracts enough readers or the TV ratings to justify a follow up. As soon as the issue no longer stimulates the reader or the viewer it is dropped. Never mind that the issue is important, still unresolved and ought to be kept before the public: out it goes to make room for something more newsworthy. Of course there are honourable exceptions to this generalization, but as a rule news keeps no better than fish; indeed less well than fish, for fish can be put in cold store and still eaten with relish. News that has been put in store is worthless.

This is one of the reasons for the mutual suspicion between scientists and journalists. They work on different time-scales. The scientist who publishes half-baked findings loses the respect of his peers. The journalist who fails to publish his findings promptly, however half-baked they are, loses his job. And on the journey from the laboratory to the news-stand the information may get horribly distorted. The interview between journalist and scientist may have lasted an hour; the journalist has to chip away the reservations and complexities so that he can fit the story into half a column; the sub-editor (the worst culprit of all) slices chunks out of the journalist's copy and adds a sensational headline which must often sicken the journalist as well as infuriate the scientist who in good faith has explained his work. And the outcome: a wider alienation between science and the media.

This didn't matter in the days before the public became the patrons of science. It would not matter today if everyone read the *New Scientist* or the science correspondents in the quality newspapers, or even watched science programmes on the BBC. But they don't, and they pay taxes some of which support science. What should they be told about science?

In the United States the presentation of science on TV is much less satisfactory than it is in Britain, and science in the mass-media newspapers is no better reported than it is in Britain. The only science that is "sold" to the public is likely to be scandalous (for example the thalidomide affair) or open to ominous speculations (genetic engineering) or fashionable (pollution, in the 1970s). What is urgently needed is a much better public understanding of what science is about, how it is done and what consequences it has for society. These are difficult matters to put across and there's no money to reward those who try to do it.

It was, therefore, an excellent idea for the American Association for the Advancement of Science to commission an essay on science and the media, and a brilliant choice to offer the commission to June Goodfield, who already has an international reputation for her interpretation of science to the so-called lay reader. With first hand experience in the two professions of journalism and science and a sympathetic understanding of the constraints under which both these professions work, she has offered a clear — and in places refreshingly provocative — analysis of the "uneasy relationship" (as she calls it) between the two professions.

She rejects the view (still held, alas! by some scientists) that the great bingo-playing, football pools, top-of-the-pops majority don't need to be told about

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science and wouldn't understand it anyway. They do understand the science stories that do get into the mass media: if this were not so, the science stories would not appear there at all. But this does not mean that science reporting must be sensational. Members of Mechanics' Institutes in the nineteenth century responded with enthusiasm to the journalistic essays of Faraday. Readers of the tabloid press were enthusiastic about science reporting by J.B.S. Haldane in the 1920s. So, there is a strong case for science journalism at this very popular level to be accurate, enlightening and responsible. What constraints stand in the way?

For the mass circulation newspapers the constraints are that the news item must arouse immediate interest, it must be grasped at first reading, and it must have an element of novelty. (How close to the bone was the analogy of Jules de Goncourt!) If papers don't sell they go out of business. Released from these three constraints they would not sell.

For television news, as contrasted with sustained scientific programmes such as *The Ascent of Man* and *Life on Earth,* there are similar constraints. A Select Committee report (on hazardous wastes, for instance) is published on a Tuesday. It is, let us say, 150 pages long. On Tuesday evening it will have to be reported "on the box", with a verdict on its merits or (preferably) its deficiencies, mustered that afternoon over the telephone. In Britain some of the sustained programmes are splendid. This is possible (as June Goodfield explains) because the producer is "totally responsible for the content and impact of the work". He doesn't have someone else in the hierarchy breathing down his neck all the time. In America it is otherwise; there "the long arm of sponsorship" reaches down even to the details of the production. Even public television, where there have been some successes, seems constrained to plug the wonders of science rather than the experience of doing science and the effects science is having on the way people live.

The scientist, too, works under constraints that block his contacts with the public. His peers, higher in the pecking order of distinction, are breathing down his neck: too much (or, just as bad, too successful) popularization, and the whisper "not altogether *sound,* you know" echoes in the academic groves. News is enlivened by human interest, so the media like a little autobiography to be brought into a science story (hence the success of James Watson's *The Double Helix*). But it is the aim of scientists at work to be self-effacing; they never publish their false starts, their failures, their own reactions to the research they do: all these are filtered out before the paper is sent to the learned journal to be published. The chief constraint is the necessity imposed by the conventions of scientific research — not to reach premature conclusions, not to advance to a position from which you may one day have to retreat, and never, never, in reporting research, to extrapolate beyond your data to broad speculations. If your research does have social consequences, discussion of these has to be put into different journals addressed to a different readership. This is one of the difficulties science reporters have to contend with.

June Goodfield illustrates the difficulties in the way of bringing journalists and scientists on to the same wavelength by four examples. There was the scramble of reporters to make a sensation out of the affair of Summerlin's painted mouse. In their haste to get a story out, all but one of them got the story wrong. Only one journalist, Gail McBride of the *Journal of the American Medical Association,* took the trouble to understand the whole story, and (of course) by the time she published, four months after the event, it was no longer news. Then there was the worldwide, and at times hysterical, publicity following the Asilomar conference on recombinant DNA. There was an even more hysterical exhibition over Rorvik's false claim about a cloned man, exposing the venality of a publisher who contrived to create a best-seller out of the phoney book. And there was the example of investigative journalism at its best: the sustained campaign to expose the cover-up over thalidomide.

With great skill June Goodfield attempts a summing-up. Both scientist and journalist begin on common ground: it is their business to discover and to publish the truth. Neither profession has an explicit code of ethics, nor a controlling body which (like the General Medical Council) can deprive a practitioner of his right to practise. In science, peer opinion provides a crude, but on the whole effective, mode of control. Shoddy work is not condemned in bitchy articles, such as Leavis wrote about Snow; it simply sinks without trace.

In journalism the implicit code of ethics is weakened by a pervading hypocrisy. It is a scoop to publish a secret government document illegally obtained; it is also a scoop to expose someone outside the profession who has obtained a secret document for his own ends. A contractor who bribes a politician is fair game for publicity, but not a journalist who bribes a witness. June Goodfield wants journalists to tighten their code of ethics but she would like them also to assume some responsibility to act as critics of the ethical implications of science. The need for this arises because scientists do not have a cloud of critics hovering round them, as novelists and dramatists have, and they deliberately dissociate their work from its ethical overtones.

As for the scientists, she wants them to take the trouble to sympathize with the constraints under which journalists have to work, to be less arrogant and more co-operative, to realize that journalists are an essential channel of communication between themselves and the public on whom they depend for a livelihood. She wants devices for disseminating information in suitable form to the media. She wants the interpretation and communication of science to be taken more seriously as disciplines in universities, so that a student could find training for science-writing.

Her main suggestion is that there should be critics in science who play roles like those played by critics in music, literature and the arts, critics "in the accepted, old-fashioned meaning of the term". This is the one point in June Goodfield's essay with which I disagree; for two reasons. First, the achievements of science are published normally as papers in journals, not as books. (For books there is a mild critical apparatus already, e.g. in the review columns of *Nature* and other science magazines.) Second, the *scientific* content of research papers is effectively criticized by the use made of the papers by other scientists. Research of high quality is quoted. Research of poor quality is disregarded. So I see no merit in having professional science critics. If all critics were like Walter Lippmann or Edmund Wilson or Donald Tovey, that would be fine. But critics now abound like carrion flies, swarming over the work of people engaged in the desperately difficult professions of literature and art, dismissing, with a patronizing drawl, such as one hears on some radio programmes, creative work they would be quite incapable of doing themselves. God preserve us from critics like that in science. Nor will they be needed while there are scientists about who have June Goodfield's brilliant capacity to work at the interface between science and the humanities. □

Lord Ashby is Chancellor of Queen's University, Belfast, and a Fellow of Clare College, Cambridge.

The measure of man

P.B. Medawar

A History of the Study of Human Growth. By J.M. Tanner. Pp.499. ISBN 0-521-22488-8. (Cambridge University Press: 1981.) £30, $69.

PROFESSOR Tanner has made important contributions to the practice and methodology of anthropometry; one thinks especially of his studies of secular changes in human growth rates and in landmarks such as age of onset of sexual maturity in the two sexes. He is well known also for his advocacy of the cohort method in the analysis of human growth (the "longitudinal" method as opposed to the more familiar "cross-sectional" method

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