and bureaucratic". And he offers an intriguing proposition: that the Strategic Defense Initiative, widely opposed by scientists as militarily provocative and technologically not feasible, reflects sound political instincts that transcend scientific considerations. Weinberg concludes that "[this administration's] decisions on science matters and on broader policy having scientific components . . . have not been notably worse than in previous administrations where the science advisor had more bureaucratic status. . . ".

On the other hand, Frank Press, president of the National Academy of Sciences and science adviser to President Carter, expresses hope that the "younger breed of presidential candidates" will see that they need science advice close at hand "because of the way society is going". He proposes elevating the job in the White House hierarchy, perhaps to cabinet status without portfolio. And he wants to resurrect PSAC. But Press concedes the ultimate political reality: "You can't force it on the president".

The same point is stressed by Solomon Buchsbaum, of AT&T Bell Laboratories, who has served throughout the Reagan administration as chairman of the White House Science Council, the slimmed-down descendant of PSAC. Noting that "There is much moaning these days about the absence of a 'truly presidential' science advisory mechanism, that is, the absence of a PSAC", Buchsbaum petulantly observes, "the critics expect miracles".

Many who are sceptical about the value of science advice in the White House point out that, in Washington today, science advice, solicited and otherwise, gushes from innumerable organizations, including the National Academy of Sciences, the Congressional Office of Technology Assessment, professional societies, congressional witnesses and so forth. H. Guyford Stever, who served as President Ford's science adviser, estimates that the science advising industry has a "collective budget of a good fraction of a billion dollars", and adds, "I do not see that there are important ideas in science and engineering that do not get the attention of the top decision-makers in our country".

The strongest case for the importance of presidential science advice is offered in the volume's only contribution by a recipient of such advice, Gerald Ford, who signed into law the restoration of the White House science office. In justification, he explains: "Presidents don't like surprises". That may be the strongest argument for putting scientific skill directly in the service of the presidency. □

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Assessment of the assessors

Philip Gummett

Evaluating Applied Research: Lessons from Japan. By John Irvine. *Frances Pinter/Columbia University Press: 1988. Pp. 103. £27.50, $45.*

THE word 'evaluation' has come a long way from its origins as a term used in the insurance business of the eighteenth century. It now increasingly refers to the latest fashion in science policy, namely, the attempt by governments and other bodies to appraise the quality and benefits of scientific and technological research and development. The search is on for quick, cheap ways of making such appraisals, preferably through the development of sets of 'performance indicators' such as numbers of papers published and subsequent citations, patents awarded or some measure of rate of return on the initial investment. As these approaches spread throughout government, industry and the universities, more and more scientists are becoming aware of them, and of the fact that speed and cheapness in their use can be attained only at the price of sensitivity in their application.

Much of the early work concentrated on evaluation of basic research, mainly using bibliometric techniques. Attention has moved more recently to applied research, defined in this book as "original investigation undertaken in order to acquire new knowledge which is directed towards practical aims or objectives". This definition includes the concept of 'strategic' research, where there are likely to be practical applications but they cannot yet be clearly specified. Obviously, counts of papers and citations will be less valuable for evaluating applied research than for basic, and assessment of the value of the work for eventual practical application will be more important.

The shift in attention towards applied research has been a notable feature of Japanese work on evaluation. This is partly because of the Nakasone government's concern to get better value for public money, but it also owes much to the changing circumstances of Japan's science and technology. The rapid expansion of government-funded research of the 1970s has given way to more modest growth. After catching up with the West in many aspects of technology, Japan now feels obliged to pioneer key areas of science and technology. Government laboratories are becoming less concerned with specific applied research in support of industrial ventures, and instead are concentrating more on longer-term strategic research. Finally, there is a growing awareness that the traditional research management procedures, based on the search for consensus, may not be appropriate where relatively radical decisions have to be made, such as closing an institute.

All this has created a need for a better basis for making today's tough decisions over resource allocation, and for ways of minimizing risks as the emphasis shifts from an almost exclusive concern with product and process development towards the less-charted upstream areas of applied research. Accordingly, systematic external reviews have, for the first time, been introduced to Japanese government laboratories and a number of reports have been produced offering guidelines for research evaluation.

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Pointing forward — Japan's Maglev train.

John Irvine offers an overview of Japanese practice in these respects. *Evaluating Applied Research* is basically a report prepared for the British government's Department of Trade and Industry. It is therefore written in report style, complete with "Executive" summary (the superfluous adjective is such a ubiquitous feature of report-writing that it too must be seen as a sign of the times). The book emerged from a five-week study, involving interviews at over 20 Japanese organizations. The main substantive chapters are devoted to the evaluation procedures of the Administrative Inspection Bureau (the specialist evaluation arm of the Management and Coordination Agency of the Prime Minister's Office), the Agency of Industrial Science and Technology in the Ministry of International Trade and Industry, and the Science and Technology Agency. It is a pity that the Defence Agency was not included because, even though still a small spender on research and development by Japanese standards, it is growing very fast, particularly in the more upstream areas.

The systematic evaluation of the 140 government laboratories and national

research institutes is being carried out by a division of 14 staff within the Administrative Inspection Bureau. At eight to ten evaluations per year, it is clearly going to take them some time to complete the first round of appraisals. Interestingly, there is no requirement for the inspectors to have a science degree. They operate by collecting written material from the institute, and then conducting a site visit, which is followed by hearings with external experts or users of the institute. It is already clear that they are willing to recommend closure of programmes of research.

As well as the infrequent attention of the inspectors, institutes and programmes are increasingly subject to review by their parent agency. The Japanese have recognized the need to build up the skills needed for this task; interestingly, they differ from Western countries in making little use of external evaluators from consultancy firms or research institutes. They also differ from at least some Western countries in having no hang-ups about the role of the state in the support of industrially relevant research and development. Projects in the Agency of Industrial Science and Technology have a planned duration of ten years; they are chosen in part because they are expected to have an industrial impact yet cannot be undertaken by private firms because of the risk involved; and the Japanese do not, as in Britain, apply an 'additionality' test (which constrains the state to fund only work that would not otherwise have been undertaken by firms), but instead use government funds to coordinate and target research of the highest priority that would in any case be carried out by companies.

Increasingly, Japanese projects have evaluation criteria built into them from the outset. In this respect they follow standard planning practice of identifying goals and specifying how one will tell whether they are being achieved. Considerable work goes into *ex ante* evaluation of the potential importance of candidate research programmes. Special efforts have been made in recent years to introduce effective mid-term reviews, which enable programmes to be changed and resources concentrated on the more promising areas in the light of experience gained earlier on. Quantitative performance indicators are used, particularly patents and publications, and Irvine detected some evidence that the Japanese government is tempted by evaluation techniques that lend themselves to simple quantification. However, calculations of rates of return are attempted only rarely for government-funded programmes. Instead, emphasis is put upon whether or not projects are achieving previously defined technical and other strategic goals (which might include building a national capability in a particular area of technology). Care is always taken to discuss the results of the appraisal with the appraisees, not least because of a desire to generate a commitment among researchers themselves to the idea that assessment will not only improve efficiency but is also in their interests.

There is a danger that the evaluator may inherit the mantle of Oscar Wilde's cynic, who knew the price of everything and the value of nothing. To gain the benefits of evaluation while avoiding the pitfalls is a task of growing importance, to which this book makes a valuable contribution. □

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Rhine maidens

Steve Blinkhorn

Forbidden Knowledge: The Paranormal Paradox. By Bob Couttie. *Lutterworth: 1988. Pp.155. £9.95.*

THE lunar branch of marine biology is not a thriving field of research. Before 1969 it might have been possible to hope for a renaissance, but it is to the eternal credit of science that no one any more thinks that there are mermaids on the Moon — not even *very small* mermaids.

Sadly, the lesson has not been learned by parapsychologists. In the nineteenth century, belief in a parallel reality, a different order of causality, led to reports of paranormal manifestations on a scale and intensity which demanded attention. Tables flying through the air unaided by forces known to the heirs of Newton; ectoplasm in grocers' quantities; thought transference, spirit manifestations and hauntings of an intensity commensurate with the existence of an independent *élan vital*.

Curiously, the paranormal has become decidedly less spectacular. At best these days we have the spoon-bending antics of Yuri Geller, and laboratory effects which strain for statistical significance by dint of mind-numbing repetition of trials. Parapsychology has retreated to claiming, in effect, that on the Moon are mermaids of microscopic proportions.

Forbidden Knowledge examines the current state of parapsychology from the point of view of an experienced practitioner of long-distance magic. Here is a book that deserved to have been written at greater length. As it is, it effectively dishes parapsychology, to the extent of providing a cookbook of methods for replicating spoon-bending and spirit-communicating stunts. But Couttie's prose style stumbles over itself in haste, and the argument suffers from superficial reference to research rather than detailed discussion. Having made a special study of the field, the author's talent for communicating his knowledge is stretched.

So, as a book *Forbidden Knowledge* fails to satisfy. Nonetheless, it is welcome. The fundamental flaw in popular accounts favourable to the paranormal is accurately identified: only positive results find ready publication. One could add that selective publication subverts all classical hypothesis-testing on a statistical model and that even the best-designed experiments can admit of more than one alternative hypothesis given rejection of the null hypothesis. Hypotheses involving extra-sensory perception, thought transference and the like are difficult to frame in the first place, and the usual statement that a set of results could only have happened by chance at some extreme level of improbability glides over the range of uncontrolled factors which could provide a mundane explanation. Close inspection typically reveals just such uncontrolled factors, and Couttie neatly disposes of a number of highly publicized instances of paranormal phenomena which were greeted with breathless enthusiasm, but turn out to have been examples of the conjuror's art. So much for the impressive significance levels frequently cited in ESP laboratory reports.

However, if you want to find out how to bend spoons *à la* Geller, summon up spirits *à la* Doris Stokes or just read the truth about some past cases of haunting, *Forbidden Knowledge* is in a class of its own. Lunar mermaids will hate it — but that's their problem. □

Fairy tales — pictures of the Cottingley fairies, published in Strand Magazine *in 1917, caused a storm of controversy. Only many years later did the girls admit the pictures were faked. The picture is taken from* The Reality of the Paranormal *by Arthur Ellison, published by Harrap price £12.95.*

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