Bluestone, we are given no information on changing firm structures and how these affect workplace identities and job markets. In short, careful measurement of the characteristics of the workforce can only take us so far and it is sad that nearly all contributors to this book made no effort to go beyond the bounds of their own discipline in seeking to promote greater understanding of the questions they wished to answer.

Noel Whiteside
University of Bristol, UK

American Science Policy since World War II *by Bruce L R Smith*
(The Brookings Institution, Washington DC, 1990), pp. ix + 230, ISBN 0-8157-7998-4, 0-8157-7997-6 (pbk).

A Fragile Power Scientists and the State *by Chandra Mukerji*
(Princeton University Press, Princeton, 1989), pp. xxiii + 253, ISBN 0-691-08538-2.

Back in the 50s, when cinema emerged as an art form, I saw a Japanese film which recounted the same incident first from one person's point of view, then from another's. I was impressed. It struck me as a poignant allegory of the central problem of historiography. It came back to me when I read these two books. Ostensibly they deal with the same subject: science policy in the USA. Both, after taking a brief look at earlier history, concentrate on the period since World War II. The galleries of the names that are mentioned overlap. The two books even quote the same passage from Vannevar Bush's 1945 classic Science: the Endless Frontier — the passage which affirms faith in basic research, arguing that

> a nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade.

There, however, the similarities end. One book is establishment, the other anti-establishment. For Smith, the story concerns attempts by the state to nurture a research system with its parts properly balanced and well articulated with each other. For Mukerji, the state is an intrusion on the proper autonomy of science.

Smith's view is a view from Washington. He writes from the Brookings Institution and formerly worked in the White House Office of Science and Technology Policy. For him, science policy is a matter of watching with tender loving care over a valuable activity, even if non-intervention is the guiding principle. In a revealing analogy, he compares the situation before World War II with an orchestra which had been assembled, "but there was as yet no common score for the musicians and no conductor" (p.4). No wonder balance and co-ordination are necessary!

Smith's book invites comparision with David Dickson's detailed treatment in *The New Politics of Science* (Pantheon, 1984), especially since both books divide the postwar period into three. Smith's periodisation is neater and simpler, possibly at the expense of some loss of fidelity to real life complexities. He distinguishes a first period of consensus lasting until 1965, a period of disarray until 1980 and finally a new and more questionable consensus is the '80s.

With equally disarming simplicity, he announces that science policy is made up of five elements: basic research, applied research, commercialisation, regulatory behaviour and international issues. The book follows these five elements through the three periods. The impression of inclusiveness is, however, not justified, because only the national point of view is dealt with. States in the other sense, that of the sub-units in federal systems, are not considered as science policy agents, and firms are not seen as anything more than pieces in the national game.

Within its self-defined limits, the analysis is rich, authoritative and well written. A point that emerges clearly is how the emphasis on basic science in the postwar years went with a lack of emphasis on commercialisation. Vannevar Bush's affirmation of faith was naturally taken up with enthusiasm by research interests. Meanwhile, commercialisation was taken for granted:

> Commercialisation was to occur almost automatically as a by-product from the government's support of basic research and more applied research and development operations. The incentives of the marketplace would continue to spur innovative activity, and the manufacturing strength of U.S. industry would ensure high-quality products and quick exploitation of market opportunities (p.37).

When the inadequacies of this policy began to show, the emphasis shifted to more relevance and more immediate application, but alas! it turned out that, though the slogans sounded good, nobody really knew how to implement them. Period two was a period of disarray because lack of a proper understanding of the processes involved made a coherent and consistent policy impossible. Policy tacked now this way, now that. Smith has to agree with the conclusion of Harvey Averch that "the U.S. search for an innovation strategy has been marred by faulty design or, more accurately, by no design"(p.136).

The dominant influence of World War II in shaping postwar policy — a feature so salient that Smith and Mukerji agree on it — may well have set a false trail. In the classic wartime cases, such as the atom bomb and radar, research did turn almost automatically into application. For many kinds of industrial innovation, however, the process is vastly more complex, with research accounting for a smaller proportion of the effort and the cost. The use of science in shooting wars is not a good model, as it turns out, for the use of science in trade wars.

Perhaps the atom bombs on Hiroshima and Nagasaki carried a delayed penalty for the victors. There was a conceptual booby trap there: the misleadng half truth that the answers lay in research. Smith concedes that Vannevar Bush's famous clarion call has turned out to be "an absurd oversimplification" (p.72). The traditional emphasis on R&D statistics is a sign of the malaise in science policy. Under that mass of statistics lay concealed from open view the simple truth that research in itself doesn't make money, it costs money. The USA, and with it the countries that look to it as a model for science policy, are still paying the price for that lack of conceptual clarity.

The revived, though less confident, faith in basic science in period three is still just that: it is faith, not reasoning based on evidence. It is a policy adopted for want of any more specific remedy. It makes me think of a doctor who, baffled by the patient's symptoms and at a loss for an adequate diagnosis, prescribes a hefty dose of vitamins.

The reader who turns from Smith's book to Mukerji's is asked to make a Gestalt switch. What previously appeared as a harmoniously balanced vase is now portrayed as two faces, eyeball to eyeball.

The view of science as an autonomous enterprise dedicated to the pursuit of knowledge, without regard to applications to meet the needs and desires of society, is one which has long enjoyed some currency among sociologists. It reached its apogee in W.O. Hagstrom's *The Scientific Community* (Basic Books, 1965): a pretty piece of sociological theory marred only by the many stubborn facts it fails to embrace. Ah well — Merton can't be held responsibile for all that Mertonians do, any more than Marx can for Marxists.

Mukerji, who is Professor of Sociology and Communication at the University of California, San Diego, applies a somewhat similar conceptual framework. Knowledge, for her, is the proper end of science; applied work is inferior, mere "plumbing" (p.24). Mission-oriented agencies of government maintain cohorts of scientists as what she calls an "elite reserve labour force". They do this by offering "soft money", a concept which unfortunately is never adequately defined. The interests of science and of the state are different but they overlap:

> This mix of mutuality and difference in relation to research was at the heart of the soft-money funding system and new to the postwar era (p.50).

Mukerji's underlying theory can be understood only by recognising that it is essentially a normative, not a descriptive statement. It expresses what she thinks ought to be, not necessarily what is. Her main concern is with military applications and there will be many who agree with her that here the "situation has to be (or should be) disturbing" (p.116). But she has to concede that

> during the war, many scientists did not try to make their work serve science; they wanted to end the war with an Allied victory just as much as the military did (p.50).

As a matter of observed fact then, scientists don't always behave as her theory would require. Further, the explanataory structure should hold regardless of the area of application, but in fact it becomes less compelling as one moves from military applications to applications in other areas. She regards it as ironic that

> most American scientists find it an honour to advise the government by serving in the National Academy of Science, testifying before Congress, or being part of a commission studying some problem of another (p.198).

That pinpoints the source of the trouble I have in accepting her view. At bottom, it seems to me, it rests on a simplistic model of the relation between knowledge and the power that comes from applying knowledge. The 'fragile power' of the title, it turns out, would be an apt description of the conceptual framework in which she writes.

This is not to say that there isn't a lot of good reading in her book. Her primary material is taken from oceanography and her text includes some fascinating extracts from interviews and from conversations tape recorded during dives. She provides interesting sociological commentary on some perennial problems such as the nature of modelling and questions of ambiguous priority. It is a pity that she didn't choose to paint on a more restricted canvas. For a general analysis of science-state relations, neither her conceptual framework nor her empirical frame of reference are adequate, but she could have written a really good analysis of the recent history of oceanography. Fortunately, the essentials of that smaller work are contained in the present volume.

As a final reflectoin on these two books by Smith and by Mukerji, it is worth asking whether there is a link between them, apart from the obvious overlap in subject matter. I think there is one that goes deeper — deep enough to find a common cause for the very different difficulties encountered by their

incommensurable analyses. Both are still chipping away at the great question of the relations between knowledge and power: not only political power but also the power to control or modify nature that comes from applying knowledge. The question was posed aphoristically nearly four centuries ago by Francis Bacon: human knowledge and human power meet in one, he asserted, but he could not yet sense the awesome complexity of the confluence. He set out the agenda, but he didn't solve the problem. In the 20th century, the question he crystallised has emerged as surely one of the most momentous facing nations and, for that matter, humankind as a whole. The failure of these two books, intellectually sophisticated though they are, to offer an adequate answer shows, I fear, that we will enter the 21st century with the Baconian agenda still unfinished.

Fred Jevons
Murdoch University

Cognitive Economy: The Economic Dimension of the Theory of Knowledge
by Nicholas Rescher
(University of Pittsburgh Press, Pittsburgh, 1989), pp. x +168, $US29.95, ISBN 0-8229-3617-8.

Rescher credits Charles Peirce with being the first theorist to stress the role of economic considerations in the theory of knowledge. He says this book starts from Peirce's brief discussions and tantalising observations and is "an effort to pick up the torch and pass the flame along". His effort is admirable but success would require that scientists, economists and policy analysts both understand what is in this slim volume and be willing to carry the torch. The majority are, unfortunately, likely to respond with the same resentment and hostility that greeted Fritz Machlup's inclusion of academia in 'the knowledge industry'.

We are confronted with the "unavoidable economic dimension. Economic factors shape and condition our cognitive proceedings in so fundamental a way that they demand explicit attention . . . [O]nly by heeding the concrete processes that engender our knowledge in a way that takes account of their economic dimension can we adequately explain the nature of its operations and properly understand the character of its products" (p. 150).

He is aware of "the inescapable realities of resource limitations" (p. 149). "The limits of science are very real, but they are not inherently intellectual matters of human incapacity or deficient brain power. They are fundamentally economic limits imposed by the technological character of our access to the phenomena of nature. The over-optimistic idea that we can push science ever onward to the solution of all questions that arise shatters in the awkward reality that the price of problem solving inexorably increases to a point beyond the limits of affordability" (p. 150).

Affordability is accentuated by technological escalation with its "massive economic ramifications" (p. 137). "The enormous power, sensitivity, and complexity deployed in present-day experimental science have not been sought for their own sake but rather because the research frontier has moved on into