book reviews

Quality quantified

Social Stratification in Science. By Jonathan R. Cole and Stephen Cole. Pp. xiv+283. (University of Chicago: Chicago and London, 1973). £5.65.

SINCE 1967 the Coles have been addressing themselves to a set of interesting problems concerning the internal social organisation of science, using as principal tool the *Science Citation Index*. The number of citations received is taken to be a measure of the quality of scientific work. Those who disdainfully dismiss this way of measuring quality should read the discussion of its reliability and validity in chapter 2 of the book. Despite the admitted drawbacks—notably the fact that important discoveries may become accepted to the extent that the original papers stop being cited—it emerges from the scrutiny as a reasonable indicator.

The Coles' major concern is the degree to which the reward system of science operates by universalistic criteria. How far do inequalities in the rewards received reflect quality of work, rather than particular attributes like sex or race or being related to the right people? Is the homage which conventional rhetoric pays to universalistic ideals better matched in practice by science than by other social institutions? The answer is reassuring. Science—which here means mostly American physics—emerges in a favourable light. For receipt of honorific awards, for visibility and for appointments in prestigious university departments, quality of research is found to be a key determinant.

Women are not much discriminated against—once they are over the Ph.D. hurdle. The same limitation effectively removes from the field of study the question of discrimination against blacks, because they receive so few Ph.Ds (less than one per cent of those given in science). The restricted scope of the Coles' study is exposed by their revealing use of the expression "social origins" to refer to the rank of the scientist's doctoral department (page 117). It is only for those who have got inside the system that its inequalities can be seen to be equitably distributed.

Is there a serious departure from universalism in what R. K. Merton has called "the Matthew effect" ("For unto every one that hath shall be given"—Gospel according to St Matthew)? The Coles test the consequences of such an assumption for the diffusion of knowledge and conclude that the effect is rarely important. Controlling for assessed quality at time 2, the assessed quality of papers at time 1 is only slightly influenced by the reputation already achieved by their authors.

The Coles also test what they call "the Ortega hypothesis", after an assertion by Ortega y Gasset that the many scientists who are of no more than mediocre quality nevertheless contribute in a major way to scientific progress. The work that physicists use in their best papers, as indicated by the references they cite, is itself found to be produced largely by elite physicists. This does not support the hypothesis and leads to the suggestion, which may antagonise some readers, that the number of active physicists could be cut substantially without crippling progress.

Science as studied by the Coles is defined in a narrow way. Only the publication of research papers counts; even review articles are excluded. Teaching students or administering research establishments or advising governments is "unproductive"; so is improving the productivity of industry. The "quality" of a company or government agency is determined by its commitment to basic research (page 43). Social responsibility in science is outside this impregnable circle of definitions—so far outside that the idea that "rather than society influencing science, science influences society" is described as one that "some of the more polemical historians of science have gone so far as to argue" (page 14).

The most immediate danger arising from work like that described in this book is that statistical measures adequate for fair-sized samples may be misapplied to individual cases. Within a few pages of a warning about this (page 31), the authors themselves are teetering on the brink. In general, however, they remain clearly aware of the limitations of their approach and they use their tools with care as well as ingenuity. They have done a useful exercise in the art of the possible. Like it or not, citation counts will doubtless continue to be used as well as misused, and the Coles' work itself is likely to be plentifully cited. The smokescreen of sociological jargon in their book is refreshingly thin and the style is lucid, considering the high density of correlation and regression coefficients.

F. R. JEVONS

Ignorance of interferon

Interferon: Theory and Applications. By V. D. Solov'ev and T. A. Bektemirov. Translated from Russian by Basil Haigh. Pp. xvii+304. (Plenum: London and New York, 1974.) $30.

THERE have been several international meetings on interferon during the past few years, but unfortunately Russian workers have not been able to attend any of them—often having to withdraw at the last moment. Since the Soviet Union has also had few visitors from the West, this has led to the isolation of the Russian workers and the effects of this isolation are only too apparent in this present book.

The book claims, in its forword, "to give a systematic account of the existing information on interferon obtained both from the extensive literature, and from the authors' own observations made over a period of several years..." The book falls sadly short in its first aim—that is to give a systematic account of the existing information on interferon. First of all, it is out of date. This is obvious as soon as one looks at the extensive list of references given at the end of the volume. The first hundred and ninety refer to Russian papers published up to 1967, the next four hundred to papers by non-Russian authors (only three being later than 1967), while the final hundred are made up of more recent Russian publications and Western papers, but only twenty odd are dated 1970 or later. This is presumably because the Russian text was published in 1970, but it is a pity that the text was not updated before translation. The book shows its age in other ways too—the account of interferon purification is nearly ten years old and data presented on the molecular weight and amino acid composition of interferon is quite incorrect. The translational-inhibiting protein theory of Marcus and Salb is restated although it was abandoned years ago. Isaacs's theory that interferon induction is due to 'foreign' nucleic acid is described, although Isaacs himself later withdrew it. Nagano's inhibitory factor is described as an interferon, despite Fantes's (1966) careful analysis of the results, showing that the inhibitory factor is almost certainly a polysaccharide. There is no account of the interferon standardisation meeting in London in 1969, nor of the development of