The system is currently being evaluated by a randomized controlled trial of computer system records versus standard hospital records in respect of efficiency in recording and retrieval of information, the time it takes to use the system, the administrative effort, and the cost.

Patients attending hypertension clinics are highly selected, the group hopes to develop a modified version of the system for use in general practice, with a view to aiding management and study of hypertensive patients in the population at large.

Requests for reprints should be sent to Dr. L. J. Beilin.

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Key Symptoms Pre-printed on Initial Input Form

Angina	True vertigo	Migraine
Intermittent claudication	Transient loss of consciousness	Headache
Dyspnoea on exertion	Transient loss of vision	Depressive illness
Paroxysmal nocturnal dyspnoea	Transient paresis (less than 24 hours)	Nocturia
Ankle oedema	Cerebrovascular accident (more than 24 hours)	Dysuria
		Haematuria

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For Debate

Referees and Research Administrators: Barriers to Scientific Research?

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To a considerable extent the course of medical research in this or any other country is determined by the policies of the major grant-giving councils, trusts, and foundations. In theory this policy is publicly expressed in annual reports. In practice the only way of knowing whether the policy is actually being followed is to examine the detailed processes by which grant applications are accepted or rejected. In most cases the procedures of the major grant-giving bodies make such examination impossible. This paper presents evidence that such secrecy can seriously distort the true picture of potential research activity.

Secrecy

Almost invariably grant applications are sent out to referees, whose identities are not disclosed. This process offers the first opportunity for secret policy making. There are wide and legitimate differences of opinion about the value of a piece of research work. The greater the potential significance

of the work the more spectacular may be the differences. History has repeatedly shown that new research which is genuinely important is often initially vehemently rejected by established scientific opinion. It is therefore possible to destroy an application by deliberately sending it to eminent referees who are known to be opposed to the work. This enables administrators to claim that while it is the firm policy of the council or trust to support research in the area execution of the policy has proved impossible because of an absence of work of sufficient calibre.

The expert referees' reports are normally considered in conjunction with the application by a committee of eminent medical men who may or may not have expertise in the area. The applicant is given no chance to comment on referees' reports which, as will be shown later, may be wrong in factual detail, based on misunderstanding, or very occasionally even frankly malicious. The prosecution therefore has the last word, a situation unheard of in any procedure whose aim is to get at the truth.

Finally the applicant is informed either that the grant is to be given or that the application has failed. Usually in neither case are the referees' reports shown to the applicant. It is hard to see how this secrecy can further scientific progress. The successful applicant is denied access to the comments of supposedly serious and experienced referees; these comments should be helpful to him in carrying out his project. The unsuccessful scientist is unable to know whether the application was rejected for valid reasons or not. He cannot find out

whether his work must be drastically revised or whether the objections are only trivial.

The system as it now operates is almost completely closed. Real power is potentially in the hands of the administrators who can manipulate the refereeing process to sink a good application or encourage a poor one. Whether this manipulation actually goes on is unknown because of the secrecy. What fragments of information we do have, however, indicate that there are very serious reasons to question whether secrecy is in the best interests of medical science.

Referees

This sort of vague accusation is repeatedly made by disgruntled scientists but rarely carries weight because detailed evidence is not provided. I am prepared to provide such evidence. In 1971 I applied to a major international grant-giving body for two research grants. The first project was an investigation of the actions of ovine prolactin on fluid and electrolyte balance in sheep. The second aimed to test the effects of prolactin and other hormones on smooth muscle from veins, ureters, gastrointestinal tract, and uterus. As usual the application was sent to a referee. By accident I later found out his identity. As would be expected of the organization concerned he was a world authority on the physiology of pregnancy and reproduction. Unusually I was sent the referee's report. Some of his comments were valid and helpful. Others are given below.

PROLACTIN AND FLUID AND ELECTROLYTES

REFeree: The errors of water space measurement in a ruminant like the sheep are known to be so great that they would certainly swamp the effect of a five-day hormone treatment.

REPLY: We are attempting to measure 12 parameters: methods for the first 10 are highly accurate and reproducible. I would agree that one cannot be certain whether changes in body water spaces will be significant but that, surely, is the point of doing an experiment! Science would be very easy if one could know experimental results in advance. (The experiments later showed large and statistically highly significant changes in total body water content.)

REFeree: Assuming that changes could be shown, how would one interpret these in reference to pregnancy in a species which does not have the hormone and which does not get oedema anyhow?

REPLY: The sheep does have pituitary prolactin and prolactin of ovine origin will be used in the experiments. Understanding of the changes produced by the hormone in the relatively simple experimental situation is essential for an appreciation of the complex changes which take place in pregnancy. I am particularly puzzled by the statement "a species which does not get oedema anyhow" since nowhere in the application did I mention oedema and nowhere did I suggest that a prime purpose of the investigation was an elucidation of the mechanism of oedema. (We later proved that prolactin could cause severe oedema in sheep.)

PROLACTIN AND SMOOTH MUSCLE

REFeree: The ureters are, in fact, not relaxed in pregnancy; evidence on this matter is now unequivocal. Nor is there any reason to believe that veins and ureters behave in the same way—indeed a great deal of work has been done on venous distensibility by people interested in the effects of the pill. One is left therefore with the Csapo theory of myometrial block which already is being tackled vigorously.

REPLY: I am astonished by your referee's statement that the ureters are not relaxed in pregnancy. (At this point I quoted extensively from recent papers and indicated that the referee seemed not to understand the law of Laplace.) Your referee's next comments appear to be a series of non sequiturs. Nowhere did

I suggest that veins and ureters behave in the same way. If I did believe that completely there would be little point in studying both. The purpose of the investigation is to look at the behaviour of both under similar conditions of hormonal stimulation and to find out where they are similar and where they are different. I do not see what relevance the otherwise true statement that "a great deal of work has been done on venous distensibility by people interested in the effects of the pill" has.

The last sentence "One is left therefore with the Csapo theory of myometrial block which is already being tackled vigorously" simply leaves me bewildered. Your referee in the previous two sentences has been talking about ureters and veins and this third sentence is written with the air of being an entirely logical consequence of what has gone before. But of course there is no connexion. The myometrium is very different from both veins and ureters and in none of his recent work has Csapo made the suggestion that it is progesterone which is responsible for reduction of extrauterine smooth muscle activity in pregnancy. The effective in vitro inhibitory concentration of progesterone as far as veins and ureters are concerned is of the order of 500 times the naturally occurring plasma concentration at the end of pregnancy and with these structures the concept of a high local concentration of progesterone cannot be invoked as it can in the case of the myometrium. Apart from such factual considerations the philosophy of the referee's last sentence would seem to be unscientific to say the very least. He seems to be implying that because one old theory which even its most passionate advocates would admit has not been entirely successful is still under vigorous investigation then no other hypotheses should be investigated.

If my application and the referee's report had been considered by a committee without my comments being available to them there can be little doubt that it would have been rejected. The referee's reputation and his unequivocal and unchallenged condemnation of my work would have damned me. In fact, the grant was awarded.

There is another source of information about referees' behaviour—the reports they write on articles submitted to journals. In this case the referees know that their comments are often transmitted to the author and one might therefore expect a high standard. It is unlikely that referees behave worse when reviewing a paper than when reviewing a grant application and I think therefore that this is legitimate evidence to use. My experience is that about one report in five contains a major error. The following examples will illustrate what I mean. In each case the journal was of international standing.

PAPER DESCRIBING EFFECTS OF ALDOSTERONE ON BARORECEPTOR FUNCTION

REFeree: (1) Aldosterone has never been shown to have any direct membrane actions. (2) The physiologic effects of aldosterone usually do not become evident before a lag time of several hours and changes occurring within an hour are unlikely to be due to aldosterone. (3) I cannot explain the findings but I do not feel they represent anything but experimental artifact.

Replies were not actually made as I felt that there was no point in arguing. A form of the paper was later published by the *Lancet*. Potential replies might have been: (1) There is a first time for everything. In fact, a direct membrane action of aldosterone was neither shown nor claimed in the paper. This is a common example of a technique (also seen in the oedema statement in the previous example) whereby a referee makes a statement not made by the applicant and then proceeds to destroy his own statement as though this were a criticism of the applicant. (2) Untrue. (3) A surprising comment from a scientifically trained referee.

PAPER DISCUSSING THE ROLE OF PROLACTIN IN MENTAL ILLNESS

REFeree: The author is not concerned about the lot of very serious work which has already been done in this field with other releasing factors, for example, I would like to mention the studies

on TRH* effect in depressed patients (A. J. Prange *et al.*, *Lancet*, 1972, 2, 999).

REPLY: I should like to chide the assessor gently for apparently not reading long sections of the paper. He claims I am not concerned with much serious work which has been done on other releasing factors such as TRH. In fact TRH was mentioned in 23 paragraphs of the original paper and discussed in detail in several of these. He claims that I am ignorant of the effect of TRH in depressed patients and draws my attention to a reference in the *Lancet*. This reference was discussed in the paper as was the prior reference in the *Lancet* which described the same phenomenon. The implication that TRH is an "other releasing factor" apart from the ones which regulate prolactin secretion suggests that the assessor failed to read the several paragraphs in which I discussed the powerful effect of TRH on prolactin secretion. I am sorry to mention this, but I do feel that authors have the right to expect assessors to read papers in full.

I submit therefore that there is serious objective evidence that some referees, and even some highly respected ones in top academic positions, are at best ignorant and careless and at worst deliberately obstructive. Information about referees' behaviour obtained from experience with journals and certain grant-giving bodies does not and cannot show that referees acting for organizations which do not disclose their reports also sometimes act in irresponsible ways. On the other hand it would be surprising if standards of behaviour were higher in a secret as opposed to an open system. It is therefore possible that in some areas research effort is being channelled along unproductive lines by senior scientists with a vested interest in preventing studies which might lead to reappraisal of their own work. One could be reassured on this point only if referees's reports were sent to applicants before a final decision on the application and only if applicants were finally informed about reasons for rejection.

Open and Closed Systems

One may well ask why such an open system is not now in operation. Formal inquiries to major grant-giving bodies elicit only formal replies to the effect that it is not part of official policy. Informal inquiries provide some revealing answers. The most consistent and serious replies indicate that such an open system would be inefficient and would lead to unnecessary conflicts. What is meant by efficiency? It seems in this context to be the transmission of applications to acceptance or rejection files with the minimum of administrative effort. I can accept that this is a legitimate if unwise policy for the private organizations, but it is difficult to see how it can be justified by Government-financed bodies. It can hardly be efficient to reject good projects because the supposedly expert advice is either bad or obstructive. It can hardly be efficient for research workers to spend in ignorance many unproductive hours wondering why on earth an application

has been rejected and trying to work out how it should be modified to meet the approval of the mandarins.

The "unnecessary conflict" argument leads to the question, "Unnecessary for what?" The only answer can be that the conflict is unnecessary for the smooth working of the establishment machine. If research is to be effectively supported and if humbug and obstructiveness are to be exposed conflict between referees and research workers is absolutely essential.

Science and the Open Society

The closed system is a real bar to rapid progress. There is a common belief that a benevolent dictatorship is the best method for the achievement of efficient advance. Karl Popper's *The Open Society and its Enemies* should have destroyed that fallacy for ever but alas his works go largely unread. He has shown conclusively that only in an open system where ideas are freely discussed and argued about can the best ones be most rapidly exploited. A closed system, operated by faceless men with vested interests in the status quo and subject to no genuinely informed criticism, can only be inefficient in its support of research. In theory science should be the most enlightened and establishment-free area of human endeavour. In practice senior scientists, subject to sanction by neither vote nor financial performance, are in positions of almost unparalleled authority. Experts who have been in any field for twenty years or more often believe that major advances are not possible; if they were possible the experts would, of course, have made them themselves. As a result any project which does seem to offer prospects of a major breakthrough must of necessity be fallacious. All money must be spent on safe minor work which the expert knows is possible because he has done the same sort of thing himself. Max Planck's lament that new ideas gain wide acceptance only when the established authorities die is all too true.

The myth which sustains the scientific establishment—namely, that all scientists are honourable and interested only in scientific progress—must be rejected. Scientists behave no better than businessmen or politicians and the only safeguard against Machiavellian distortion of research effort is complete openness of decision making.

At present I am collecting information about the behaviour of referees and grant-giving bodies and would be delighted to receive evidence from any scientist.

[Dr. D. F. Horrobin has made available to us the names of the organizations and journals discussed in this paper.—ED., *B.M.J.*]

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*Thyrotropin-releasing hormone.