

NUFFIELD PROVINCIAL HOSPITALS TRUST
OCCASIONAL PAPER 3

*A fresh look
at policies for
health service
research and
its relevance to
management*

REPORT OF A NUFFIELD PROVINCIAL HOSPITALS TRUST
WORKING PARTY EDITED BY GORDON McLACHLAN

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MEMBERS OF THE POLICIES FOR RESEARCH AND DEVELOPMENT WORKING PARTY

Professor J. R. ASHFORD, MA, PhD,
Professor of Statistics, University of Exeter
and Managing Director,
Exeter Health Information Services Ltd.

Professor W. W. HOLLAND, MD, FRCGP, FRCP, FFCM,
Professor of Clinical Epidemiology and Social Medicine,
St. Thomas's Hospital Medical School.

Professor E. G. KNOX, MD, FRCP, FFFOM, FFCM,
Professor of Social Medicine,
University of Birmingham.

Professor D. J. NEWELL, MA, PhD,
Professor of Medical Statistics,
University of Newcastle Upon Tyne.

Professor T. P. WHITEHEAD, CBE, MCB, FRCPath, FRCS,
Dean of Medicine and Professor of Clinical Chemistry,
University of Birmingham.

Nuffield Provincial Hospitals Trust
Mr GORDON. McLACHLAN
Dr CALUM PATON

INTRODUCTION

PROFESSOR WALTER HOLLAND

The plight of health services research has not improved since the publication of *Five years after* in 1978 (1). If anything problems have increased. Health services research has a unique role in the planning and operation of health services. As Jennett (2) pointed out in his Rock Carling Lecture, if the potential of new emerging technologies to improve life expectancy and the preventive, curing, and caring services is to be realized, they must be carefully evaluated to establish that claims made of services are fulfilled and at an affordable cost. Despite this challenge, support for health services research has declined in recent years, as has the number of individuals and units working in the field. Health services research is of no value unless it is noted and acted upon by those responsible for service planning. However, the present system for supporting health services research has not succeeded in establishing a co-ordinated approach and despite sanguine hopes, the involvement of the actual customers of health services research—the Regional and District Health Authorities—has diminished. This is in part due to the turmoil in the administration and financing of the NHS—turmoil that has not left the DHSS untouched. Nevertheless, at a time when there is a greater need than ever before for health services research, the mechanism for supporting and initiating research has become more bureaucratic and cumbersome and the impetus for research has faltered.

The advent of the Management Board and the new managerial structure in the NHS offers a new opportunity to ensure effective use of health services research. It is to be hoped that this chance will not be missed. The creation of an adequate management structure within the NHS provides a direct customer for the health services researchers' wares. Modern management should be supported by intelligence provided through sound research and investigation. The health service manager will be in a position to make decisions without being hampered by conflicting opinions. To assist managers, many of whom will not be medically qualified, in this daunting task, it is imperative to strengthen

the links between researchers and managers so that research can be related to practice and the findings used. The proposals and discussion outlined here will it is hoped, help managers to appreciate what research can offer.

Health services research is by nature long-term and multidisciplinary. Over the years, a number of multidisciplinary groups committed to long-term research have been established and there has been a steady development of methodologies. But it is essential to provide a stable and supportive environment, if health services research is to contribute to long-term strategies and be sufficiently flexible to respond quickly to new situations.

In the present climate of constraint which is likely to continue for a number of years and with the planned changes that are bound to have to be made in the types of care provided for patients, it is to be hoped that the management board will see this as an opportunity to determine which of the various options for individual services, procedures etc, are the most appropriate. It is important, and particularly so in a time of constraint, to improve cost effectiveness and only to undertake procedures that have been properly evaluated. This document presents proposals for a new structure for health services research to strengthen its role in meeting the needs for making health services efficient and effective. It is hoped that it will prove of help to the management board in the task that lies ahead of it.

REFERENCES

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DIGEST AND COMMENT

Perspective

The report is directed mainly towards the NHS Management Board. As such the bias is towards research concerned with health services. It is assumed that the DHSS will itself, for its own particular purposes, have its own arrangements for funding and directing research, including its monitoring.

The key policies

1. With the increasing complexity of health issues and the necessity to contain health service expenditure, the need to develop *objectives* by health service *management* has become evident everywhere. Without adequate and appropriately directed *research* this cannot be achieved.
2. It is important to stress that health services research has made—and certainly has the capacity to make in the future—a significant contribution to the effectiveness and improvement of health services in the NHS. Such research should thus be accorded a high priority in the initial policies of the NHS Management Board. The advent of the Board offers an opportunity to review all the arrangements for research, development and evaluation.
3. These arrangements will require adequate funding, competent research management, and intelligence; and involve an integration of the aims and policies of management and the effective application of results.
4. It also means that to avoid confusion, the role of the Board in relation to all other funding organizations, public and private, should be spelled out at an early stage. One of its primary goals should be to link research, management, and implementation. If the DHSS has its own research arm, such a spelling out becomes of paramount importance (see also 8 below).

Some additional desiderata

5. To get some grasp of the overall functions it would be useful at an early stage to identify a substantive programme for organization as well as an approach to research which can be widely appreciated. These will primarily be concerned with assessment, assessment of innovations and research concerned more with the effects of intervention rather than esoteric. To combine practicality with vision presumes an *intelligence* function including among other things the definition of broad priority areas for research objectives. It is fundamental that an intelligence function must be part of management in any organization.
6. The requirements for better information for health services (for all authorities at whatever level) is in present circumstances a major area of research in itself. A satisfactory information base is essential for all research, i.e. for testing the relevance of research, eventually, since such information is necessary for evaluation, policy analysis and resultant action.
7. The **co-ordination** of the work of health service research units (whether as at present or reconstituted) is necessary both for the optimum use of resources and for the efficient creation of further resources through manpower training and deployment. International insight should also receive special attention, for many of the problems with which health services research has to deal are universal in application.
8. At the moment there is over-emphasis on the various slogans dominating *health policy* and the realities behind this must be studied. Managers in the NHS must become much more aware of the advantages of long term enquiry: which is by no means inconsistent with the kind of immediate enquiry often required by them and likely to be in the forefront of their policies, post-Griffiths. This is not to say that all such long-term enquiry should be the responsibility of NHS-based or Departmental-sponsored health services research; some types of long-term research are clearly the responsibility of the universities. The types of long-term research which can be

classified as research for management and therefore the responsibility of the NHS should however be defined and categorized by any 'intelligence' group(s) operating to complement the new research arrangements. This underlines the importance of the consideration under 4 above.

A hard but vital decision . . .

A deliberate policy of investment in new research arrangements (or possibly recasting present investment patterns), as part of the NHS Management set-up, although possibly appearing costly in the short-term, is necessary. It could be infinitesimally minor by comparison with possible savings, as well as through leading to the better use of resources. The Cabinet Office Annual Reviews of Government Funded R&D (1983 and 1984) revealed that the *comparative* amount devoted to the health services sector is pitifully low in all respects. The return from a strong investment in manpower and *management resources* for health services research and development is illustrated by the impressive experience in Scotland which has no parallel in England.

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A new opportunity for coherence?

In the introduction to *Five Years After. A review of health care research management after Rothschild* (1), published by the Trust in 1978, it was noted that the lack of an overall approach to health services research had resulted in much confusion with regard to responsibilities and policies for applied research. Thus '... the problem is [one not just ... whether (the transferred) funds should be within the DHSS OR MRC, but more fundamentally] one of structures and mechanisms to deploy the scientific approach to what is essentially required' (by way of research to improve services) (2). In 1984, the problem remains much the same, indeed in sharpened form. The importance of identifying the elements of a *well-integrated system* for the functions of intelligence gathering, commissioning and funding, in the realm of health service research is still not clearly understood; and there is consequently much frustration, as well as waste of effort and resources, on the part of both researchers and those striving for service efficacy and order. It is conceivable however that the NHS Management Inquiry Report (the Griffiths Report) (3), with its accepted recommendation for a full-time NHS Management Board among the functions of which will be improved management, could provide an opportunity and means at last for starting afresh and forging a coherent and comprehensive policy for health services research. While the 'Griffiths' recommendations are primarily geared to the management of NHS resources, no management can ignore basic research and enquiry: the pre-requisites for information and intelligence, without which no business—public or private—can operate effectively. Certainly the complex business of provision of health services cannot, with its roots in scientific knowledge and its operation in the skilful aggregation of—and moderation between—so many different groups and interests.

This paper discusses a policy and priorities for a new structure for health services research, given new managerial arrangements. Within the fundamental principles which govern research, whatever its objectives, it attempts to exemplify its argument by reference to the role of research in meeting the need for making health services efficient and effective. It thus builds on the previous work of the Trust's Working Group which is part of the Trust's long-established interest and pioneering in the topic of health services research (4).

A key definition

'Intelligence' is understood to mean the extrapolation from information and research of findings which are significant for the organization and running of health services. That is, 'intelligence' is *more* than merely information or research, but implies the constructive use of their insights.

A retrospective review

There has been much criticism (particularly within the past decade) of the arrangements for health services research in Britain. It is of course a specialized area of relatively recent identity, a host to many disciplines. In the UK the rapid growth of public expenditure devoted to health care research in the 1960s (in absolute terms yet not of course as a proportion of NHS spending when compared to other organizations' R&D) ensured an uneven quality of effort and result. This mixture of good, bad, and indifferent revealed by evaluations of the activities has engendered some scepticism if not cynicism about the effectiveness of health services research and therefore of its own cost/benefit ratio, let alone the cost/benefit of implementation of its results. The major problems however can largely be traced to the lack of an identifiable overall policy and framework in which the institutions concerned can pursue research devoted to the complex aims of health policy, and in which research can be effectively planned, co-ordinated and constantly monitored. Ideally such a framework which would encompass a range of bodies concerned in one way or another with the promotion of health, would provide for mechanisms for liaison between major commissioning and funding bodies, together with effective means for dialogues with service authorities, which are the ultimate clients in the application of research.

In the UK, although the State is ultimately by far the major paymaster, the mutations caused by the rapidity of growth in the health services research sector in the 1960s and 70s have been the main factors in disturbing any deliberate rationality in approach. This provides a contrast with the much earlier, very successful development of the MRC: primarily in 'basic' biomedical research, with clinical and more recently 'applied' research coming to the fore in the last decades. The excellent track record of the MRC in its chosen fields needs no stressing. But its most recent move into health services research has largely added to the

confusion of policy-makers. The complexity of the issue is increased through the confusion caused by the Council's fight in the later 1970s to recover full responsibility for all funds for research in the applied field. These had been transferred (notionally) to the DHSS as a result of the Government policy to accept the Rothschild proposal (§48 of CMND 5046) (5): to transfer responsibility for these funds to the DHSS as one of 'the customer Departments to help meet the needs for commissioned research'. This fight was won by the MRC and the responsibility for the funds retransferred to the Council in 1980. However to some extent the problem of health services research—which is often of a quite different character even to the 'applied' research covered by the transferred funds—was further confused by the MRC's undertaking at the same time to increase their annual expenditure on such research, as part of the overall agreement negotiated between the Council and the Health Departments about their respective areas of interest. A recent Annual Report of the MRC hardly gives any substance to the current belief in the range and importance of Health Services Research (6) *to the NHS*. It may be that the apparent lack of enthusiasm for such research is due to the quality of applications. However more plausible is the explanation that the MRC gives its emphasis, on the one hand, to pure, clinical research and, on the other, to general social research with medical content (e.g. the suicidal tendencies of the unemployed) as opposed to *research on the operation of health services*.

On the other hand, there is confusion of aim and purpose in the DHSS concerning its very role in the NHS. If the Department has ever developed an adequate or adequately co-ordinated expertise to allow it to be an effective focus for health services research, or for there to be forged an effective alliance with the MRC (or even for that matter with the ESRC), it is not too evident to the informed observer familiar with the research field. It is also a major criticism that the DHSS in its research role has had no effective liaison with the real customers for service—the Health Authorities. Indeed the 'customer/contractor' principle has been handicapped in practice because there has been no effective dialogue between the principal partners, i.e. the researchers *and* the NHS authorities, about needs. Nor have the Divisions within the DHSS, which are the main commissioning agents, forged the necessarily strong yet flexible links with the researchers at the depth of understanding required for smooth working.

Moreover, it is believed in some quarters that the Chief Scientist's Organization has lacked the necessary capacity for '*intelligence*' in the overall service context, which presumes the means of reviewing service problems both current and prospective as part of the management process, and of studying the success or otherwise of the implementation of results. If there is no provision for this in management, the result is bound to be a severe handicap in the search for appropriate policies both in the short- and long-term and with regard to both strategy and tactics.

In this latter respect there have also been some linguistic difficulties further confusing the concept of 'customers' intentions. Thus there has been significant confusion over varying definitions of different types of research (7). Rothschild and Dainton used different characterizations, respectively, 'basic' research, 'applied' research and 'general' research (Rothschild) and 'basic', 'tactical', and 'strategic' (Dainton). Rothschild's three terms refer respectively to so-called pure or fundamental research, to research whose object is a specific and immediate or rapid application, and to general research which should be funded by 'customers' at the level of about ten per cent (the 'surcharge'). Dainton's first two terms are similar in meaning to Rothschild's, but the 'middleground'—called 'strategic' by Dainton—refers to 'more general scientific effort needed as a foundation for (this) tactical science'. More recently, Mason defines 'strategic' research as 'collateral research required to achieve national strategic objectives that may originate from either of two directions (i) market pull . . . and (ii) technology push . . .'. More recently the Cabinet Office's *Annual Review of Government funded R&D* adopted yet another definition from the Frascati Manual published by the *OECD* in 1980.

The difficulty of differentiating between 'basic', 'applied', and 'general' research and where responsibilities for each should lie, has been compounded by a rather loose (if not universal) assumption that health services research as a whole is 'applied' or 'general' and therefore 'less scientific'. It would be difficult to deny—and this affects attitudes to it, that its prestige, is at best not great, a qualification which is not confined to the UK (8).

Yet despite the many disappointments in achievements there have been successes, and the lessons of these in relation to structure should not be overlooked. At a time when there are growing demands for efficiency and savings as well as effectiveness, health services research is more important than ever.

The first Chief Scientist stressed this as far back as 1962 (9), but the promising moves to develop this and to pinpoint and monitor research priorities starting with the publication *Portfolio for Health* (1971) and *Portfolio for Health 2* (1973) were halted in the 1970s. The questions currently posed about the mechanisms of the Chief Scientist's Organization, as well as its effective organization and the availability and deployment of expertise, have been underlined recently in the Mason Report (10).

To sum up, any review of health services research and its place in medical care particularly points to:

1. the haphazard policies which have plagued the field despite the fact that most of the funds available come from Government;
2. the lack of clarity in the linguistics used;
3. the reduction in the brokerage role of the Chief Scientist's Office since its inception in 1972 in relation to the operating authorities and the potential contractors, and thus the seemingly arbitrary distribution of research funds related to priorities for policy;
4. the seeming failure by reason of the absence of appropriate mechanisms including links to and training for management to utilize the research capability of specific research units;
5. the failure to develop and maintain stable, multidisciplinary research teams which can engage themselves on general as well as applied research (there currently seems for some reason hitherto unspecified to be a policy to make all units specialized);
6. the absence of a two-way mechanism for providing advice from units;
7. the absence of a satisfactory publications policy, which is a *sine qua non* for reviewing research results;
8. the low success-rate in translating the results of research into decisive policies;
9. the failure on the part of the Royal Commission to deal with the issues at any adequate depth and indeed further to compound confusion by suggesting the setting up of a separate 'Institute';
10. the seeming unwillingness to commission evaluative research and the over-emphasis on descriptive studies;
11. the absence of Regional research arrangements, co-ordinated to Regionally-planned research units, reflecting where relevant Regional needs.

These faults seem to point to the need for a New Deal for research and the setting up of the NHS Management Board may provide an opportunity for raising the key issues.

It must be realized that researchers, on the one hand, have very different perceptions from those of administrators, managers, and—most of all—authority members, on the other hand. The latter, in their different ways, tend to play down the usefulness of research. Authority members are generally amateurs, to put it crudely. Managers and administrators often see research as an esoteric diversion from problems 'at the sharp end'. These are cultural reasons for different groups seeing the problem in different ways. There is no conception that health *services* research is as worthwhile as 'medical' research or 'hard' research in general. Yet it may be crucial in its implications for effectiveness of service and therefore efficiency. One of the prime aims of a 'new deal' for research ought to be a redressing of this balance.

Reshaping the arrangements for health services research

A. The challenges

The greatest challenge facing the NHS is to provide a comprehensive service in an age of increasing technological and service opportunities and yet broadly static resources. This calls for policies designed to promote both efficiency and effectiveness, including reviews not only of the current services but also of what will be required prospectively. The achievement of these objectives should be a main priority for health services research.

Applying the lessons of the past, what kind of structure is needed to make the requisite arrangements for such research? The development at last, of a potential 'apex' to the NHS in the shape of the NHS Management Board could conceivably provide the means at the centre, not only for focussing on the priorities for immediate and long-term research to contribute to the improvement of the NHS, but also to raise again how *optimum use could be made of health services research capacity*. There are certain positive advantages in the new arrangements which might give a chance for basic principles to be established *viz*:

1.—the new Management Board is intended to be relatively free from direct political control and separate from the traditional bureaucracy of a Department of State which with its multifarious objectives has caused major problems for research. These have included *vagueness in direction, a concern with accountability, rather than substantive research* (11); *too frequent transfers of bureaucrats to allow scientific management to develop* (12); *insufficient means for close liaison with the operating authorities to pinpoint their problems*.

Recent indications of 'backtracking' on the Griffiths recommendations for the *Management Board* (as opposed to General Managers throughout

the service) are not, however, encouraging. What is more, use of the new arrangements for *greater* arbitrary and superficial political instruction to the service, rather than less, seems to be an emerging problem.

This paper is designed to address the opportunities of an 'honest' rather than 'cynical' implementation of Griffiths.

2.—the major concern of the new Board with efficiency, with value for money and presumably rationalization to make best use of resources, should make for the kind of drive for the effective monitoring of research arrangements which can in the long run be only beneficial to the ideals of health services research with its emphasis on scientific enquiry;

3.—the anticipated increasing involvement by the Board in *the operations* of the NHS should give a special emphasis to the NHS Authorities stake in research. The apparent 'political' difficulties hitherto of relating more closely the function of the CSO to management in the broad sense as long as it is primarily linked to the position of the DHSS, *qua* Department of State, gives some credence to the view it will be necessary for new arrangements to be made with a protocol of joint and several responsibilities to reflect the needs of front-line authorities.

B. The functions to be covered

Any new research arrangements will require to distinguish, clarify, and establish the role and needs of research as well as a number of important functions to promote *viz*:

- (i). *effectiveness and efficiency* in use of research resources which would of course initially be a prime objective;
- (ii). the establishment of *clear priorities for research*, justified in terms of importance to the NHS (and with implications for resource allocation and planning spelled out);
- (iii). the establishment of the *criteria of efficiency and effectiveness* as guiding resource use in the NHS, and a research programme to reflect these criteria. Quality control of existing procedures and forms of treatment, new procedures and amendments of existing practice would provide prime targets for assessment;
- (iv). a particular focus on the assessment of innovations;

(v). the *evaluation* of research, the speedy production of which is a prerequisite both for judicious implementation (including withdrawal of services or amendments of practice where necessary) and for the direction of future research;

(vi). the question of *implementation*—linked with, but as a separate issue to research management which *should* raise the crucial question of the means of co-operation and dialogue with Regions and Districts in the NHS;

(vii). a discussion on the crucial issue of the *role* of research. In particular the need for intelligence, to define general goals and translate them into specific aims, immediately points to the need to develop some mechanism to improve channels for communication with decision-makers. Behavioural change alone is probably not enough. Structural change or rather the creation of structures to define research and intelligence roles at the centre, in the body of the NHS Management Board, in Regions and in Districts is also a prime requirement.

(viii). the necessity of a review of the *needs* for research such as the establishment of methodologies, and ultimately of an increasing body of knowledge.

This immediately raises the fundamental question of a review of the existing organization. Can the CSO, with its present base of responsibility in the DHSS remain primarily responsible for all these functions. After all, to be effective the operation must not only be concerned with the operations of the DHSS *qua* Department of State, but also have a separate role as part of the Management Board's functions in relation to the NHS authorities? It appears that a much sharper definition of objective and function will be necessary since (as indeed was pointed out earlier in *Five Years After*) the variegated interests of the NHS as a whole—along the epidemiological as well as managerial and planning paths—will require to be co-ordinated as part of the new style. The different character of Departmental and Service aims respectively has contributed to much confusion hitherto.

It would therefore seem that, if the opportunity was taken of setting up a flexible mechanism geared essentially as part of the Management Board geared to drawing on scientific expertise but closely related to other health services research and intelligence activities, the potential gain in dispersing current confusion and making for a more integrated research

policy would be outstanding. In this respect a paramount prerequisite for efficiency, economy and savings is a *well-staffed capacity for research operations*, above all: if not actually including, then at least geared to, arrangements for a much needed 'intelligence' for management. This inevitably has a bearing not only on the deployment of research funds but for the commissioning of research and the necessary liaison with appropriate *service organizations*.

Any effective research operation will require development of training in *methodology* for research, as well as adequate arrangements for general *education* in health services research. These two needs have been signally lacking from the present structure, with often low-status and 'temporary' junior researchers providing the core of the team. 'One Professor and a bunch of juniors' may simply be inadequate: looking at *numbers* of researchers is not enough.

C. The organizational perspective

Although its primary aim no doubt will be to promote research concerned with efficiency, perhaps in the immediate term, there are certain perspectives regarding research of which the Management Board cannot lose sight. The *customer/contractor* principle enunciated by Rothschild, while important as an issue of realism to ensure a better pay-off to research funding has possibly been applied too rigidly hitherto under the present arrangements for research commissions. Research should of course be 'relevant' and cost-beneficial but a healthy research capability requires also the funding of *more general enquiries*. The analogy with the Ministry of Defence (which 'had employed the principle of customer/contractor for some time in commissioning weapons and tanks' (13)) assumed by Rothschild is not wholly apt since the military 'customer' can in fact draw on a wide range of industrial research. In any event weapons and tanks are tangible, conceptually simple entities, more susceptible to relatively straightforward customer/contractor realities. Differences between basic and applied research in health services may involve less conceptually simple notions.

Even applied research may not conform to a simple 'customer' mode. There is *always* a need to define differences in implication between 'basic' and 'applied'. The study of health service goals, and of the social and institutional relationships which develop in pursuit of these goals, is

considerably more complex and requires much exhaustive long-term research in the Social Sciences. (In any event Rothschild never rejected the thesis of general studies but the import of his report has often been interpreted restrictively.) It is a too strict application of the customer/contractor principle that is wrong. The practical implication of this is that a better co-ordination of health services research than hitherto should be a major objective. New managerial arrangements for the NHS could provide the opportunities.

Perhaps too, a distinction is required in health services research not so much between 'basic', 'applied', and 'general', as between the *definition* of objectives and the *pursuit* of objectives. On one interpretation, this distinction is similar to that used both by Dainton and Mason (14), 'strategic' and 'applied', but such a characterization could well be misleading. Strategy presumes a definition of objectives which depends on '*intelligence*' just as much as does *tactics*, which are used to pursue objectives.

The real difference which it is important to understand in order to ensure that health services research is not limited to tactical issues is that there must be provision for looking at wider issues as background for strategic considerations.

D. The implications

This means that:

(i). An identified research management group within the Management Board should be created and have an intelligence arm geared *inter alia* to the *definition of objectives*, that is, the translation of general goals into specific aims and thus direction for research service management. Such aims would also include the superficially whimsical, but in effect important task of judging when research is worthwhile and when not. For example, Sir Andrew Watt Kay has pointed out that, in the event, evaluation of therapies may well not be cost effective if results are not rigorously applied (15).

(ii). It is relevant to an understanding of that part of the organization required for intelligence that the task of identifying and choosing between objectives is *not* a purely scientific matter. While careful appraisal can predict likely returns on research, and possible 'benefits' and 'costs' of implementing research results, *social and political choices*

help to decide which 'benefits' are most beneficial. Thus the question of the make-up or connection of such a group as part of the executive of the Board is crucial, and would require much careful attention. Further a methodology for approaching research in 'new areas' is necessary, and would be a major task for any such group. The pursuit of broad questions in important areas, which nevertheless have little likelihood of yielding clear results from research, is necessary in order that options and marginal benefits that may accrue are set out as clearly as possible: in order to understand fully those areas of uncertainty (of which note has to be taken as part of the 'intelligence' appraisal).

(iii). *Some understanding if not actual co-ordination of the work of different health service research units* will be an important function: both for comparison of different objectives, and for co-ordination and clarification of specific and agreed objectives. This would have to be provided for particularly in the mechanism of 'scientific management' which is to be a function of the new Board.

(iv). Knowledge of research in other countries, and the applications of 'intelligence' to its results, is also an important task.

(v). Part of the machinery must be a '*research information clearing house*' which could help to disseminate research results to individual research units (to prevent duplication and/or time-consuming and unnecessary preliminaries to other research) and, more importantly, to practitioners, managers, planners, and authorities—where appropriate—within the NHS.

(vi). With regard to the latter, *communication of research results* has hitherto been a significant stumbling-block. The machinery would have to provide links to Health Authorities, perhaps thereby beginning to give help to the new 'general managers' in the field. The latter will face great difficulties of many sorts, and they will require special briefing with regard to the implementation of research results.

(vii). Without going into the question of their composition in the present arrangements, the Research Liaison Group system fathered by the DHSS does not cover the whole field and the lack of an overall view inevitably causes distortion. This has its effect too on the range and classification of research identified as desirable and hence on the commissioning process. Improved health services research arrangements would *require a policy*

for the re-designation of categories, or areas, of research to cover the field in the pursuit of agreed objectives. Existing areas within DHSS practice are of two types, which may be termed broad and specific (respectively, 'Health and Personal Social Services Research', 'Biomedical Research', 'Social Security Research', and so forth on the one hand; and (within the first of these) client groups such as 'children', 'elderly', 'mental illness', etc. (16)). There is thus a gap which leaves out intermediate categories—which would be useful in helping to co-ordinate research and set priorities. Examples are 'health economics'—or, more specifically, for example, 'studies of medical effectiveness and cost effectiveness', 'health management', etc.

Such categories would help to fill the void between individual specific research projects (e.g. 'drug addiction on Humberside', although that might have applications to a wider role), and broad 'soft social science' research (e.g. in the 'sociology of health') which is theoretical and consequently unlikely to be of great use in the immediate—or even intermediate—policy-making process, but would have to be provided for as part of long-term policy-making.

(viii). The lack of a comprehensive policy covering the field has its results too in scientific management. One current example is in the *inefficiency in the co-ordination of research hitherto*. Some of the effects appear to be minor but they are cumulative. Slowness in consideration of submissions can also have a self-defeating circular effect, since this can result in long delays in approval, which in turn present other problems. If there is significant delay, by the time a project is approved, the research protocol may be out of date, especially in a contemporary, empirical field. Doubt as to the project's likelihood of approval by the DHSS (as opposed to, say, to the MRC if it has been submitted to the Council's Panel) may lead to a researcher being uncertain where to turn, or—worse—to the researcher being shunted back and forth in a frustrating manner. *Delays in the renewal of funding* is also not only frustrating but not infrequently wasteful in the deployment of personnel. A well-co-ordinated comprehensive policy of health services research might pave the way ultimately to the creation of a 'permanent, full-time research' cadre on longer-term contracts or periods of service than is currently the case, for flexibility in research management and brokerage; and a clearer system of reciprocal responsibilities. The *overall* arrangements would have to reflect the

balance of skills in the field in order to play a monitoring, amending, and evaluating role in research, as well as including training publication and implementation protocols.

In short, this poses the requirement for an overall managerial and co-ordinating capacity within an identifiable framework with defined purposes.

E. Determining roles in research

The question of the role of the Chief Scientist's Organization remains crucial to health services research. If it continues as the chief Departmental stimulator monitor of research, not just for the DHSS *qua* Department of State but for the NHS acting for or as part of the NHS Management Board, its brokerage powers would have to be increased substantially: and extend beyond the Department's own 'management functions' to promote strong bonds between researchers and planners/managers in the NHS as a whole, a promotion which one would imagine is likely to be encouraged by the NHS Management Board. The research roles for the Department, the NHS Management Board, the Regions and Districts will have to be clarified as well as those of the MRC and ESRC and indeed of OPCS. Such roles will have to embrace not only clear responsibilities for the implementation of research findings, but also responsibilities for directing research to the appropriate targets. This will require a clear demarcation of responsibility between different tiers of the NHS for different types and areas of research.

Again, at present, Regions and Districts in England especially have little capacity for such research, although an awakening of interest will perhaps be stimulated by the prospective new arrangements. Scotland, which is about the size of the larger English Regions—but, of course, has its own great traditions and experience in health services research—is an example of what is possible (if not immediately practicable in England because of the relative poverty of research resources in most regions). Nevertheless the principles are evident. New roles would not necessarily involve *diktat* from the centre. Indeed, in line with the philosophies of both 'Griffiths' and that of financial accountability, greater Regional involvement in research linked to better implementation, would mean a combination of devolution with accountability for achievement of goals.

At present in reality it seems that the Chief Scientist Organization is

not geared to dealing with local issues including that of management at each level in the NHS, with a resultant weakness of function in the NHS as a whole. The lack of a coherent structure for health services research designed to improve services, is glaring.

These and similar problems seem to be symptomatic of the looseness of the present arrangements, which in some cases seem specially weak on scientific assessment, let alone strategic design of objectives and resultant commissioning of projects. This poses the requirement for some means of collaboration if not co-ordination between the NHS Management Board, the DHSS (which presumably will wish to commission research for its own purposes), the MRC and the ESRC. In some respects Mason's suggestions apply this idea to the Research Councils (which embrace the MRC) but a *wider application to all bodies substantially involved in health services research is necessary* (17).

4

The framework for a substantive programme

It is questionable whether—without a radical change in outlook from the centre, and a framework in which the various functions, roles, and needs of research can be readily perceived—the kind of substantive programme desirable to make the best use of health services research for improvements in health services can be developed. That there is an overwhelming need for the creation of a framework, taking the opportunity of the NHS Board, cannot be denied. The pre-requisite is a recognizable structure with the capacity to focus on all the requirements and an organization to carry out the functions effectively.

The following outline of a possible approach to a research programme, which is based on the experience of the members of the Trust Group, is indicative of some of the major directions called for. The aim is less to stipulate specific areas for research—naturally a task for the research organization itself—than to present a framework and methodology for designing a programme, illustrated by some key examples. These examples have been chosen not only because they present some pressing needs in health services research, but more centrally because each one helps to illustrate one or more of the main shortcomings in the current structure and arrangements for health services research.

A. A strategy for determining priorities in health services research

(i). Initial intelligence, of a broad sort, must firstly determine main national priorities for the NHS. These can be chosen by reference to alternative or even complementary methodologies—such as cost-benefit analysis, cost-effectiveness analysis, and so forth, in the light of overall goals and values. This is not to replace broad judgment with ‘number

crunching'; merely to replace arbitrariness with more informed choices. Naturally political and social values enter into the determination and 'quantification' of benefits and costs. But this does not mean that analytical tools cannot make choices more systematic and rational (in the sense of making priorities more overt and means of reaching goals more open to critique). Science is an aid to the art of policy-making, not an alternative.

Under the heading of 'benefits' come factors such as decreases in mortality and reductions in morbidity. Possible decreases, and rates of decrease, in different areas of medical and health care per unit of cost, cannot be determined exactly, just as costs cannot (raising accompanying problems such as the separation of capital and revenue costs). *But an initial task for research in order to provide initial 'intelligence' could be to make more systematic than hitherto projections of use in shaping priorities.* Naturally a primary task of such 'macro' research would also be to define information needs.

What is more, the availability of resources is crucial. 'Intelligence' must involve seeing how a policy for research—and for the implementation of research findings where necessary—can be rendered compatible with *other* NHS policies. Thus, for example, there is no good taking a particular course if necessary *complementary* policies—e.g. regarding manpower, local authority spending—is not forthcoming. What is the point, for example, of elegant and conclusive research into community care if rate-capping prevents implementation? What is the point of research into mamography if there are not enough radiographers? Naturally, constraints and bottlenecks affecting policy are important in proportion to the immediacy of research and intended policy. The 'longer term' the research, the more unpredictable resources are; and the less they provide a *specific* handicap or otherwise. Nevertheless, the point is surely clear.

(ii). Once priorities are chosen, more detailed research projects within each can be chosen. Sometimes priorities can be addressed with less rather than more research, and vice versa: moving straight to policy, where this is possible. For example, certain areas will already have been 'researched' more than others, and so forth. *So the next stage is to translate service priorities into further research priorities.*

There are often medical and economic criteria for so doing, both of course related. For example, screening for a particular illness may not be

justified if existing research and work suggests that there is no medical follow-up to 'positive' results which can be justified on medical and/or economic grounds or both. Thus policy may already be clear, and further research, by way of investigations into screening, not justified if there is virtually nothing to be done with any results, however 'interesting'. Such situations of course require continual monitoring, as the effectiveness and efficacy of medical treatments can change over time.

The diminishing marginal returns (if indeed they are diminishing) on investment in a particular service in a particular area could be an organizing methodology, to give but one example. Trade-offs at the margin between different priorities could aid the ranking of priorities. (Examples of 'priorities' might be the elderly; the mentally-ill and handicapped; renal medicine, pre- and neo-natal care; certain types of prevention and so forth.)

Another example providing grounds for research both within and between substantive areas of priority could be the comparative worth of emphasis upon reducing mortality and morbidity, as well as comparative gauges of mortality and morbidity in different areas as defining the gravity of a problem.

(iii). Next, going from the broad to the more specific, and from the national to the local, could come *issues of planning, implementation, and service delivery*: i.e. *given* research priorities, what are the main issues raised for alternative modes of service delivery?

(iv). Next, comes the *monitoring and evaluation* of both research and resulting policy.

Such a methodology does not provide 'easy answers', nor does it bypass the need to use judgment in situations of uncertainty.

But at a *structural* level, methodical recognition of a succession of needs in research could lead to *the design of research management arrangements which reflect, in agencies and personnel, the above steps*.

B. Some main components of a strategy

(i). Defining priorities

Research geared to increasing efficiency will always tend to prominence at times of economy stringency or 'no growth' but in fact it ought to be a constant element in management. The quest for efficiency may involve the implication of radical innovation (e.g. preventive measures to reduce

costs of care and cure) in actual services, as well as greater efficacy in existing practice. A strategic task for health services research is to define *which* areas offer the best prospect for efficiency-promoting change: both technical and service-related, e.g. defining the possibilities for undertaking Day Surgery, or the possibility and consequences of earlier discharge from hospitals; the needs for multiphasic screening as part of a preventive programme; reviewing the overall policies for the elderly: including not only the financing of services, but quality control of procedures; and so forth. The movement from the centre towards such a definition and exemplification was begun in the Trust's *Portfolios for Health* published in 1971 and 1973 but has never been adequately developed since.

(ii). Information and intelligence

Policies involving change require good information on which intelligence can be based. Indeed information development is a worthy research priority in itself, including making optimum use of existing information. Frequently there are frustrations and difficulties involved in carrying out research in this area—to make the best use of resources and existing knowledge, because of a lack of firm and deliberate policy. Indeed all current research in this area needs to be reviewed urgently because of the revolution in information technologies.

(iii). Implementation

There are great difficulties in implementation, even where the findings and results show that dividends are likely (18).

'Implementation' is however a *complementary* area to research, requiring special expertise as well as special action. It may involve technical and medical innovation and changes in the management of institutional and other forms of care. The use of pilot schemes to test results needs to be more widely developed. Again, judging when *not* to innovate could be an important *result* of enquiry; but it is actually part of *decision-making* and forms no part of the research itself. The problem of decision, often, is not necessarily one arising from some vague conclusion of research or choosing which policy of a number of alternatives to follow, but of the politics, bureaucracy, and management of implementation. The 'intelligence' base for decision then becomes important.

(Prevention—probably set to become an even greater cliché than heretofore—provides frequent examples of this phenomenon of failures

to implement. Consider the failures in the realm of cancer of the cervix, discussed in 5 (A) and (B) below.)

(iv). 'Hard choices'

Again, even with the best of information available and where Authorities are actively seeking optimum solutions, the application of *research findings which offer no clearcut prescriptions* depends on the immediate goals chosen by the decision-makers. For example, despite the observations of the Parliamentary Committee on Social Services, the 'right' policy to combat infant mortality may not be unequivocal (19). The results of a recent Trust-based study in this area indicates the importance of exploring:

- the essentials by way of information on which decision-makers have to act;
- changes in output and outcome from different options (both technical and service-related);
- the different implications for cost and benefit, possibly considered for different indices of benefit, effectiveness, and utility;
- the salience of political and social values;
- relations to other policy ('policy congruence' or 'policy dissonance', for example).

Thus improving infant mortality as measured *quantitatively* may imply *more* attention to socio-economic groups of higher class: against intuitive, and possibly political, values. Yet this in itself does not *produce* a policy: it merely forms *one of many* considerations. The process of choosing policies involving implementation may thus depend on *seeking a wide consensus*—not just on simple, abstract goals, but on more tangible goals, taking into consideration cost implications, etc.

The account of this Trust initiative—which developed into a forum of researchers, clinicians, and managers—is a revelation of the complexity of these problems and the requisite approach to decision-making.

(v). Reconciliation of economy and quality of care

A developing need is to link, scientifically, medical assessment to the economic facts. This will help determine not only beneficial procedures, but an order of priority for their adoption due to inevitably constrained resources.

(vi). Non-public initiatives

It should be part of the programme to distinguish specific areas perhaps worthy of more intensive enquiry say through the commissioning of position papers by distinguished people. A good example of what may be achieved by this is the Trust's Rock Carling series. Recent examples are *Reflections on the Universities and the National Health Service* (Dainton), *The Abolition of Infection* (Tyrrell), and *High Technology Medicine. Benefits and Burdens* (Jennett). A number of previous Rock Carling studies have indicated how this approach may be a stimulator of research.

Another example of an exercise being carried out by the Trust is one originally suggested by a former Secretary of State (Patrick Jenkin). This study has changed in its detail in the course of the last three years but its concern is with:

critical factors

in the interface between research and innovation;

in the care of the elderly and other 'special groups';

and critical factors in resource allocation and planning.

(vii). Evaluating existing knowledge versus 'new' research

An important issue of intelligence is the assessment of resources to be devoted to *new research* as against '*research on research*'. This presents a complex managerial problem as to the resources to devote to research for the future given that a trail of forgotten or dismissed conclusions may have been left behind. While it is also good management to sense that a continuing presence in research is necessary, there is undoubtedly a place for continuous review of research findings. That is, it is important to link a research programme to its realistic prospects for usefulness.

(viii). The co-ordination, presentation, and review functions

The lucid presentation of often complex and varied research results is absolutely necessary if the right decisions are to be taken.

This requires a *publication policy designed to inform and lead the way to improvement*.

Failure to implement well-presented results is of course often caused by *lack of co-operation* between different professionals in the field (20). When such a lack of co-operation becomes manifest, critical reviews will lead to the necessity of further exploration (of incentives, etc.) in order, say, to promote changes in behaviour.

Research results can rarely be imposed, 'top down', dictatorially. But the conclusions of the Griffiths Inquiry and their favourable reception provide an opportunity for a deliberate policy for greater central co-ordination of research, the lessons of research and the implementation of results deemed worthwhile. Any research arrangements controlled by the new Management Board should also have another important task—that of preventing each piece of scientific research (science being geared to replacing 'special pleading' as a source of policy) from itself becoming an implicit piece of special pleading: by arguing implicitly that its substantive area is more 'urgent' than others (21). Any reformed arrangements should incorporate the means of avoiding this. That is, a judicial function is necessary to permit the careful adjudication of competing claims for resources. Such a function requires full-time expertise.

C. An overview of the key need

Any 'institute' (in the most flexible sense of that term) created to promote the planning, arrangement, monitoring, and implementing (or otherwise) of health services research should start from clearly understood premises. Some of these are or should be so obvious and general that formalization under jargon-risking slogans such as 'benefit-cost' or 'cost-effectiveness' is at first unnecessary. Nevertheless these procedures have to be the guiding principle behind both the organization of research and choice of research *projects* and also choices in implementation of *results* (given scarce resources in allocating funds to improve health care).

(i). The largest sources of unaddressed and/or variable mortality and morbidity should be identified, as the main priorities of an *effective* NHS.

Need. Epidemiological and statistical collation, analysis and comparison.

(ii). Those areas most susceptible, if not to present medical intervention, then at least to anticipated future intervention as the result of research, should be chosen.

Need. Intelligence operation to identify such areas—team to monitor progress and gauged fruitfulness of research in various areas.

(iii). Estimates of cost of research, and costs and benefits of implementation of results under various assumption (i.e. ranging from heroic to more cautious prognoses and estimates of success or otherwise).

Need. Cost-Benefit and Cost-Effectiveness analysis involving both medical and analytical personnel.

(iv). 'Hard Choices' must then be made.

Need. Management and Executive Team which is 'Politically Conscious' but not ruled by political 'Fads' in allocating resources or making choices.

(v). These choices must then be integrated within NHS decision-making.

Need. Executive, authoritative relation between the 'research institute', central NHS Authority (i.e. Management Board and/or Department) and Regions (thence to Districts).

(vi). 'Centralism' and 'Devolution' must be balanced.

More control than hitherto on *how* services are to be provided (by 'strong implication' if not by *diktat*) may be necessary *if* effectiveness and efficiency are to be addressed at a time of scarce resources. We do not have the luxury and/or waste, which market health-care systems may have, of allowing diversity at the expense of effectiveness.

D. Implication

While the broad choices based on rational analysis must be implemented (i.e. centralism is better than devolution *if* the latter means 'freedom to be foolish'), Regions should probably retain some proportion of both funds and autonomy for instigating their own research.

While this proportion is bound to be 'arbitrary' in some senses, it is no more so than any other 'committee decision' (e.g. Rothschild's famous categorization of research, and its implications for appropriations).

Some central lessons from and aspects of health services research

A. Some central lessons from substantive areas of research

(i). Single examples

There are many tangible examples of the shortcomings as well as of the successes of health services research as currently constituted, and of research requiring greater stability and follow-up. These in turn often help to demonstrate how needs brought to the fore can realistically be reflected in better arrangements for health services research. Resultingly, reference back to the central recommendations of the paper is made where necessary.

Some key examples follow. They are neither all-inclusive nor unique, but hopefully illustrative of a range of problems and needs as suggested.

1. Work already done in the realm of **nutrition**, especially child nutrition, demonstrates that any worthwhile project of research will probably require considerable cross-disciplinary input. In this case, the disciplines which need to be represented are epidemiology, sociology, policy analysis and—of course—health service studies.

Moreover, such studies are concerned to discover worthwhile policy options, to see—for example—when free school milk, school meals, *et al.* are most beneficial, and for which types of children. This means that both research and the evolution of policy must take place over a long period; and both financial and organizational stability are therefore necessary.

In this case, then, the shortcomings of research listed in 2 above, especially 4, 5, 6, and 8, pose the greatest dangers to the continuation of successful research.

At the same time, nutrition provides an example of *successful* research *as far as it goes*. By the end of the 1960s, there was for example enough evidence to suggest that free school milk was not significant in providing for adequate nutrition. Thus the 1972 policy decision ('Thatcher the milk-snatcher') was a reasonable extrapolation from knowledge *if* the money was to be used constructively (i.e. as was intended—for building new primary schools).

Emerging from this conclusion also was a decision to *survey and monitor* the effects of policy changes, leading to the National Study of Health and Growth in Primary School Children, at the St. Thomas's Unit. Additional studies were instigated.

These in turn have led to current questions for possible further research; and the area is one which has received attention to Ministerial level.

Some might argue that nutrition is not directly *health services research*, as it concerns policy with implications for more than just the NHS, on the one hand, and does not so much concern the *organization* of services as the *content* of services.

However it is patently not clinical research, and would run the risk of 'slipping the net' on a restrictive definition of health services research. The fact that it concerns (e.g.) local government for implementation (as well as the NHS) shows merely the complexity of health services research, not this topic's inadmissibility.

2. Control of cancer of the cervix provides a particularly graphic example of poor implementation despite a clearly preferable policy being available as a result of fruitful research. Research into the necessary health service arrangements to improve implementation is highly necessary, as the main problem concerns the turning of an efficacious procedure into an effective service. This example is a particularly suitable one for examination in the light of the Griffiths recommendations: as fragmentation, lack of co-operation and lack of action at local level, along with inadequate pressure to implement from DHSS, have been the main barriers to success. The primary recommendations in 3a above, especially 3, could go a long way to improving the situation.

Thus we have here an example of the 'right' policy (as increasingly suggested by empirical research)—i.e. targeting upon more immediate risks—but poor implementation.

The latter is all the more disappointing given the fact that *multiphasic screening* more generally is an area characterized by a successful (albeit negative) decision-making process. In other words, *in general*, multiphasic screening is ‘not worth it’: under current assumptions, knowledge, abilities, and availability of resources.

Another problem is that policy on cervical cancer screening *predated* general screening policy, so that the newer cervical cancer policy—that is, the right policy—is less well-established. This is not to deny that there are trends in the right direction. But it is important to ‘get it right’ in that cervical cancer screening stands out from overall screening as a profitable area *if* the service is organized on the current principles.

The implications for the *management* of research seem to be that adequate assessment of policy within each area (e.g. of screening), let alone overall, is both inadequate in itself and also inadequately linked to mechanisms for implementation.

3. Research into policy for maternity services and postnatal care provides a less clearcut lesson, but an important one nonetheless. One reason for complexity is the relatively large numbers of goals of desired outcomes, covered by the area. For example, research can be geared not only to mortality rates for mothers and babies but also to the distribution of birthweights and also numbers of notified births with congenital malformations. Another reason is that a relatively large number of political and social values have to be accounted for in trading-off policy options in this area, as well as in choosing to give priority to this area as opposed to others, or *vice versa*.

Nevertheless, this very situation should help to create an awareness that effective arrangements for research and intelligence ought to provide a locus for reviewing research, assessing its conceivable contribution to policy, rendering overt policy options and conflicts, and therefore at least making inevitable value-judgements more explicit. As pointed out in 4A above, policy analysis cannot replace value-judgement, but can certainly help to clarify them. For example, only the most extreme proponent of one policy option will continue to back it willy-nilly when—say—its cost has been demonstrated to be prohibitive, or its disruptive effect on other policy (‘policy dissonance’) has been strongly demonstrated. What is more, complex areas with a plurality of goals require continuing research, and the constant re-evaluation and re-design of research programmes, in

an attempt to move slowly towards a nationally based consensus on policy.

This is the type of area weakly covered by existing research arrangements: where design of research is inadequately professional; research is divorced from service management; and assessment is weak in itself, let alone related to an 'intelligence' for policy formation and the capacity to amend both policy and further research.

4. The topic of **dental research** represents a generally under-researched area despite some successes. Dental disease is the most common and the second most costly single disease, and the cost of dental services in the UK exceeds £190m per annum: to which can be added over 5 million working hours lost per year due to dental disease and 70,000 spells of registered sickness absence per year. Research is inadequate in four key areas, which together account for the whole gamut of possible dental research; and apart from biomedical services, three are directly within the remit of health services research: prevention; social behaviour; and assessment of procedures and delivery of care.

The need to sponsor research in a number of separate but complementary areas, in an under-researched field, would be more likely to be successfully met by research arrangements which:

- allowed 'intelligence' to be gathered, i.e. a review of the gap between needs and current practice;
- defined the key problems as a result;
- reviewed existing research;
- formulated research needs accordingly;
- monitored and amended such research as necessary.

It is exactly such a 'logical', indeed simply understood, progression of steps which is missing from current arrangements. Again, a structure for health services research composed of agencies to reflect such a process is necessary.

(ii). Areas for research relevant to several conditions, diseases, or groups

5. The example of 'day case' and short stay surgery helps to illustrate needs in linking medical assessment to economic assessment. Distinctions can be made even as a result of existing research, between those conditions where day care and short stay surgery can bring large

economic benefits without any medical ill effects on the one hand, and those conditions where it is less suitable, or not suitable at all. The latter may apply where *overall* social costs due to day case surgery—especially for lower class patients—may be great or inadequately assessed. A comprehensive policy for ‘day case’ should weigh not just savings to the NHS, but other social costs as well, without necessarily ignoring ‘hidden’ costs such as burdens on relatives. Having said this, much ‘day case’ research has both been satisfactory and has shown the scope for large savings through ‘day case’ and ‘short stay’ policies.

Nevertheless, *time off work* may well be the most significant component of social cost—and this seems to vary a lot throughout the country *with or without* day case. Naturally social security—e.g. sickness benefit—contributes to this cost.

There is perhaps, a need for health services research to *point to* other considerations to be weighed alongside its own conclusions. Naturally different NHS policy-makers may vary in their attitude to whether benefits/costs to the NHS should be added in to a more general equation of benefits and costs to society overall. However it is well to be aware of the issue, if only so that different policies can be *clarified* as to their different effects.

An ‘intelligence arm’ to assess, monitor, and generalize from research may well have a role to play here: as well as, of course, in pointing to the needs or otherwise for further research. (Such an ‘intelligence arm’ would not, of course, guarantee success: for often the problems causing stalemate in research are related to the internal politics and factions of the Department).

As a result, proposals for policy can be formulated, and communicated to Regions, *assuming* that suitable structures and mechanisms exist to allow the transmission of worthwhile research into improved practice. What is more further research in further areas can be aided as a result—but again only if there exists a co-ordinating mechanism to allow ‘learning’ from research and to provide intelligence to management.

Additional even to such important requirements, any consideration of the implications of research for practice requires judicious assessment of the limits as well as opportunities of forging a ‘national’ policy as opposed to more gradual encouragement of individual clinicians. Naturally political and professional issues arise; and any part of effective research arrangements concerned with intelligence for management would have to

balance different goals and constraints, lest a seemingly straightforward advocacy of policy lead to adverse side effects and disillusionment.

Thus, as consideration moves from single substantive areas, as in (i), 1 to 4 above, to changed health service practice conceivably affecting a number of conditions and diseases, as in this section (ii) 5 to 8, the need for health services research to be linked to policy analysis and indeed to scope for managerial innovation increases. It is in such areas that the 'Griffiths' philosophy surely has most application; and such areas surely must provide food for thought and scope for concentration by any Management and Supervisory Boards determined to fulfil their remit in spirit as well as letter. The relevance, what is more, of policies such as day case and short stay surgery at times of financial stringency surely make such policies central to any new management arrangement concerned with increased efficiency. Efficiency requires research and intelligence as well as merely quantitative changes in existing practices.

6. The question of variable mortality from conditions amenable to medical intervention, a topic recently taken on board by the St. Thomas's unit (one of the very few cross-disciplinary units in the country), provides a prime example of the need for *follow-up* in health services research. This type of study, in seeking explanations for variable mortality from conditions such as asthma which are susceptible to medical intervention at least substantially in at least specified age ranges, raises potential explanations such as the following (by no means nationally exclusive or inclusive):

- resource allocation being unsuitable;
- service planning being unsuitable;
- services delivery being unsuitable;
- patient behaviour requiring change;
- epidemiology failing to 'do its job' and so forth.

As a result, the implications for all kind of efficiency and effectiveness in the NHS are huge. What is more, *long-term* research is vital in such an area. Again, the area covers potentially a large number of conditions and diseases, and is capable of shedding light on many aspects of health services, as hinted by the above list of potential explanations. This is a central example of the role that intelligence can and should play in developing research projects. Yet the DHSS has been a reluctant

follower-on in this realm, as opposed to an innovating sponsor or 'customer'. Often it means that neither the expertise to design research, nor to monitor it, nor to evaluate the analytical techniques involved, has been available.

Any new Management Board should surely be interested in this type of topic, related as it is potentially to 'performance indication' and to a whole range of managerial questions.

7. Care of the elderly and other 'special client groups'. The Trust has recently become much involved with this important topic, especially concerning the elderly. Apart from the 1981 publication *The Impending Crisis of Old Age*, a recent folio—*The Elderly, Who Cares? Who Pays?*—has pointed out some key questions for research and investigation to tackle. While there has been much research on the needs of the elderly from many sources in recent years, a clear analytical framework within which to identify the 'critical factors' often impeding progress has not been to the fore. The Trust's work has identified, and is currently pursuing, three main categories within which care for the elderly can be examined: financial; organizational; and substantive (the last category dealing with the suitability of actual forms of care and support for the elderly).

The order of these categories reflects, in the Trust's view, on the importance of the different barriers to better co-ordinated care for the elderly. Financial fragmentation (i.e. a plurality of sources of finance for different forms of care), and perverse incentives which often follow, are often reflected in organizational fragmentation (i.e. a plurality of agencies and institutions dealing with the elderly's claims). As a result, the elderly are frequently cared for in inappropriate settings or ways, which in turn are often not only less effective than possible but also less efficient.

A major task for any research agency interested in research to help co-ordinate care for the elderly would be to sponsor a programme reflecting the barriers to improvement and mindful of the political and bureaucratic obstacles to reform.

8. Prevention in general. The cause of 'prevention' in health care has often become a cliché or unspecified piety because there has been inadequate effort to identify, firstly, *which* areas of health care offer the best practical hope of implementing a cost-effective preventive strategy; and, secondly *how* such a strategy can become integrated with the present

resource allocation and planning procedures—and indeed at what levels within the NHS.

This whole area represents a major challenge for any health services research arrangements organized on a more co-ordinated basis than at present. Although, for example, there are effective prevention measures available in immunising agents, the support of 'action research' and work in this field to try to improve methods of delivery (and to try to improve take-up rates) has been sparse to say the very least. Again, co-ordination of research and relation to implementation has been weak. If changes to NHS management are to improve structural and behavioural reasons for poor implementation or the ignoring of worthwhile advice, then research must be a major theme for new management arrangements.

(iii). Areas complementary to research

9. Information: The absence of a coherent policy for health services information reflects an absence of coherent and co-ordinated research on the topic. The Trust is producing a statement on *Data, Information, and Intelligence*, in order to identify the most pressing needs, not only for the collection of data but also for its organization into information and subsequent use for purposes of intelligence.

Adequate information systems in health services are necessary for most successful research; and, conversely, substantial research is necessary to provide adequate information systems. The problems with current research as organized by the DHSS are primarily the separation of analytical work on data-collection from consideration of hard-use and software; a failure to specify openly and clearly different needs for information by different users and potential users at different levels of the NHS; and—most fundamentally—inadequate examination of needs for intelligence. The first of these problems is related to bureaucratic fragmentation (as exemplified by the Körner Steering Group's inadequate co-operation with the Computer Policy Committee), and naturally better research arrangements cannot necessarily overcome this at a stroke, if at all.

However, taken as a whole, the problems could be eased considerably by better-planned research in the field of information. One specific example of the potential of research is provided by the Nuffield/Exeter 'Information for Districts' Data Base, sponsored by the Trust and Exeter University. In many ways this provides an already-usable *flexible* data

and information base for a variety of health service users and functions. Already a number of Regions and Districts have acquired or ordered the system, and more orders are considered to be imminent.

The story of the Nuffield/Exeter system's development is instructive for examination of the current status of health services research. This is primarily because the DHSS, far from being a pioneer in this field, has often found itself sponsoring alternatives which have failed to provide the flexibility of a generalized data-base allowing both examination and analysis of data in a flexible manner, as defined by the system's user. Bureaucratic fragmentation within the DHSS can again be held partly responsible (consider, for example, the lack of co-ordination of the Körner Group's exercises and the work of the Joint Working Group on Performance Indicators). What is more, rivalry and unwillingness to adopt 'outside' ideas has probably played a part. This is not always detrimental, for indeed the story of the Nuffield/Exeter system is a story of successful research undertaken with the help of 'outside' sources of funds. But there must be many examples of promising ideas withering for lack of support. In any case, *implementation* may frequently be impossible within the NHS if the Department is neither adequately informed nor adequately sympathetic to such types of research.

Again, the possible contribution of a research agency which is—as the Chief Scientist's Organization was intended to be, but has seemingly failed to be in practice—*executive* in its research role, could be to improve the prospects for coordination of research in areas such as this.

10. Innovation. Research is closely related to innovation, both because it must in many cases have implications for innovation (or provide warnings to abstain from innovation) and also because research into innovation—to determine obstacles, opportunities, and to help create distinctions and typologies among different types of innovation—is itself a worthwhile focus. This area consequently bears at least one broad similarity to the area of information: the two-way relationship with research which exists, since research is necessary for innovation and yet also innovation is itself a topic for research.

The Trust is also involved in a study of innovation, intended to identify: different types of innovation; different organizational, bureaucratic and 'political' factors affecting the prospects for innovation (both 'desirable' and 'undesirable', as defined in the study); and the scope for

managerial improvements within the NHS to improve the control and, where necessary, the sponsorship of innovation.

This is a central area for health services research, in that innovation represents a natural extension of the research process.

(iv). A major area of methodology where research has much to contribute

11. Resource allocation and planning. Resource allocation is an ever-more crucial area, at a time of static or slowly-growing resources. Dissatisfactions with current procedures, especially at sub-Regional level, often stem from a failure to relate resource allocation to appropriate forms of service planning. Such dissatisfactions are naturally acute in those Districts which are 'losing Districts in losing Regions' as defined by the RAWP process.

There are already a lot of ideas in this general field which are emerging with a view to advocating improvements in present resource allocation and planning procedures. Given the central impact on both effectiveness and efficiency of health services which policy in this area is bound to have, there is scope for a *central research programme* to identify central problems, needs for improvement and worthwhile emphasis for research in line with these needs. The Trust has published an essay on this topic, which reviews and analyses some central problems, as well as outlining some policy alternatives (22).

The history of changes in planning and resource allocation methodologies shows that each innovation (such as the 'Hospital Plan' of 1962, the 1974 reorganization, the 1976 RAWP Report, to identify but a few of the many) tends to be established by a temporary working group or committee without adequate evaluation or monitoring. For example, the Advisory Group on Resource Allocation which at least had the potential to respond to demands for improvement in (say) sub-Regional RAWP was disbanded in 1980. Regarding sub-Regional RAWP, guidance has been minimal, partly because research has been inadequate.

There is scope for health services research to take on board this major issue and carve out a manageable research programme, but only if such research is eventually linked on a long-term basis to policy change and management guidelines (or at least advice) to Regions.

B. Aspects of health services research illustrated by the above substantive areas: A typology

The above examples cover a number of areas; also encompassing different types of problems in research. This is not to argue that health services research boasts no considerable successes. Attention can be drawn to areas where research has been successful and productive of effective policy. However, within the category of successful research more emphasis should perhaps be given to the following situations:

- (a) successful research but lack of resulting policy formulation;
- (b) successful research and policy formulation but lack of adequate or indeed any implementation;
- (c) successful research, policy formulation and implementation but with disappointing outcome (i.e. raising the question of the *nature* of implementation, or the continuing relevance of the research to the subject area, or some other factor such as the nature of policy design);
- (d) successful research, resulting policy, implementation yet absence of additional research to illuminate resulting problems or even opportunities.

But even more important and central to the present unsatisfactory state of health services research are those areas where:

- (e) Research has been unsuccessful, or virtually non-existent as a coherent whole

The examples in A above naturally cannot be comprehensively or unequivocally categorized to fall into (a), (b), (c), (d) or (e) directly above, as the aim of such categories is to provide an approximate typology rather than an exact classification. However it may be useful to take the key areas of importance for health services research discussed in Section 5A and relate them approximately to the above categories. This is done below:

- 1 Nutrition
- 2 Control of Cancer of the Cervix
- 3 Maternity Services and Postnatal Care
- 4 Dental Research
- 5 Day Case and Short Stay Surgery
- 6 Variable Mortality
- 7 The Elderly

- 8 Prevention in General
- 9 Information
- 10 Innovation
- 11 Resource Allocation and Planning

These topics can be considered to illustrate the different types of problems (a) to (e) as follows (keeping in mind the approximate nature of the analysis, and the fact that many of the topics illustrate many problems or situations):

- 1 (d) (good research, policy, even implementation yet lack of *continuing* or long-term research capability)
- 2 (b) (poor implementation; further work required on an effective management process)
- 3 (a) and (b) (lack of policy in some areas; lack of implementation in others)
- 4 (d) and (e) (some successful research but general lack of research—especially of the long-term variety)
- 5 (d) and (a) (development of research needed; further work to relate the topic to scope for increasing efficiency, and to quantify benefits of alternative options)
- 6 (e) and (a) (need for further local investigation to follow up initial tentative conclusions. For example, is variable mortality from diseases or conditions amenable to intervention related *at all* to resources available; to a lack of adequate relation of available resources to services planning, to an ignoring of RAWP objectives at the local level; to different patient behaviour; to health service effectiveness and so forth?)
- 7 (e), (a) and (b) (lack of co-ordination of fragmented research)
- 8 (e) and (a) (inadequate co-ordination of research; confusion over relation of research and policy)
- 9 (e) (inadequate research; inadequate co-ordination of research; absence of clear policy)
- 10 (e), (b) and (a) (lack of research and lack of policy regarding the rendering compatible of resource allocation and planning, to take one example from this huge field).

Even a brief overview of the above list as a whole reveals that the major problems are lack of adequate long-term research, of adequate policy,

and of adequate implementation even where the research and policy stages have been at least partially successful.

This would suggest strongly, that research policy and implementation functions should be more closely linked and should include monitoring. Feedback to any research agency should include reports on policy and implementation. At present all three functions are almost solipsistic *vis-a-vis* the others.

To sum up, the **question** concerns the best means to combine:

- (a) direction for health services research in all its manifestations and
- (b) the management (including evaluation) of that research and its possible implementation after adequate policy has been designed.

The **answer** in principle must surely lie along the lines of the setting up of a special *management intelligence unit* reacting with (if not actually part of) new central NHS management arrangements. This principle surely applies in all organizations, to fit general management requirements.

6

Research and the Griffiths Inquiry: An assessment of opportunity

(i). Griffiths' prospects

There are many controversies and doubts circling like premature vultures around the body of the 'Griffiths' recommendations and initial moves to implementation. However one point immediately stands out: most of these questions concern the implications of 'Griffiths' at more local level, e.g. at District and Unit levels. The implications and recommendations of this paper apply primarily to changes advocated at the *centre* viz. the creation of a managerial 'apex' to be situated between the DHSS and the NHS (23).

The claim is that such an apex should involve a strong intelligence and research function. Whatever form innovations such as 'general managers' take (i.e. will a 'general manager' imply merely an amended form of 'consensus'; the arrival of a 'chief executive'; or the emergence of a new 'top down' corporate management? (24)); whether reform is total, widespread or only partial throughout the service: these and other questions do not necessarily affect critically the prospects for greater coherence in intelligence, policy and management as a result of an NHS Management Board overseen by a Health Services Supervisory Board.

(ii). The contribution from the centre

Admittedly central coherence is of limited use without a drive for implementation more locally, on the ground. Without the latter, a new NHS Board could find itself isolated, although more competent in coordination of objectives than the DHSS has hitherto been. It can therefore be argued that local reform would be necessary to complement new-established co-ordination at the centre. This argument naturally contains truth; but in the meantime, one should not underestimate the possible contribution of the new Management and Supervisory Boards.

(Distinguishing their roles *in practice* is incidentally one important task.) Indeed demonstration of the benefits of such institutions at the centre could *either* aid implementation of 'Griffiths' lower down *or* lead to improved use of present arrangements.

If health services research is to play a more central and useful role than hitherto, then, the recommendations of Griffiths for the centre could provide a useful opportunity. An 'intelligence' role for the NHS Board and/or Supervisory Board (indeed different types of intelligence—managerial and strategic—could be respectively tackled by each) could provide an important role for the Board(s) at a time when other roles are not yet possible because of the delay in accompanying reform (perhaps inevitable given the tight schedule originally set out).

(iii). Dissemination of research

The dissemination of worthwhile research results with implications for policy and advice as to implementation, from both the centre to Regions and from Regions to Districts, can therefore be a focus of importance for the new Management Board; even if its role—for 'political' reasons—is merely the minimal one of furnishing prescriptions for improvement of research as handled currently, i.e. by the Chief Scientist's Organization.

In the light of uncertainty as to the Board's eventual remit, this could prevent political and bureaucratic inertia and obstruction limiting its role to that of only an accounting one, helping to improve the process of financial accountability to the Public Accounts Committee—important as this role undoubtedly is.

(iv). 'Centralism' and 'Localism'

The question of implementation of research is in any case a thorny one, whatever the structure or process of health services management, at the levels of Region, District, and below. It is interesting to consider this question as parallel to debates about Griffiths invoking emotive terms such as 'centralism' and 'devolution' within the NHS. 'Griffiths' has been interpreted, variously, as centralist, localist, hybrid, and confused. Whether or not the overall import of Griffiths, aggregating its various recommendations, is 'centralist' or 'localist' may be a question that misses the point altogether. For surely the *intention* is central coherence of objectives and then opportunity for local 'freedom to manage' and implement in the light of such objectives.

If this is construed as 'centralist', so be it: it is doubtful that many would approve of 'localism' if for example it meant freedom to ignore the results of research which held out the prospect of (variously or inclusively) user priorities, more effective treatment and more efficient services. 'Localism'—if a term of praise—should surely mean the right, and responsibility, of Units, Districts, *et al.* to make their own arrangements to meet acknowledged goals (and, admittedly, to choose priorities where there is either arbitrariness or uncertainty in relative benefits and costs of different courses, or genuinely differing value-judgements as to priorities which vary—say—from District to District; but which differing courses are compatible with available resources and with intra-Regional co-ordination and planning).

Similarly with *research*, central and Regional priorities, policies, and attitudes to management derived at least partially from fruitful research should not be ignored at local or District level. The problems in implementation of a good policy for control of cancer of the cervix provide a good example of 'centralism' being necessary to circumvent obstructive rivalries at a more local level. As argued above in 3(e) this is not to say that '*diktat*' should replace 'persuasion'; nor that all research provides conclusions of this sort. But it is to argue against naive interpretations of 'devolution'.

Those who claim that the Griffiths Inquiry Report's acceptance marks a U-turn (25), from the philosophies of both devolution and consensus, embraced partially in the 1974 NHS reorganization and wholly in the 1982 reorganization and the preparatory 'Patients First' document, perhaps misconstrue the goals, nature and possible extent of 'devolution' if it is to be compatible with both effective and efficient health services and national objectives as embraced, for example, by RAWP. Such an attitude may of course be quite genuine and disinterested; on the other hand, it may in other cases reflect a sectional interest. It is only fair to say that the more superficial rhetoric surrounding both 1974 and 1982 reorganizations has aided genuine misunderstanding.

There is more than a hint that some medical and nursing opposition to 'Griffiths' may fall into the latter category of less genuine 'misunderstanding' (which is not to deny firstly, that there is much *genuine* medical support for, and opposition to, Griffiths and secondly, that there is also less *genuine support* for Griffiths by medical interests who see an opportunity to 'co-opt' its recommendations). Opposition and support

amongst administrators, treasurers, *et al.* and general commentators can likewise be both genuine and 'interested', although it is fair to say that much of the *genuine* opposition comes from those who fear the unsettling effects of further change that may be structural in all but name (26).

Nevertheless, the interpretation of Griffiths most consistent with improvement of the NHS both at the centre and locally is one which stresses central intelligence, setting of priorities and scientific management, and local responsibility (coupled to accountability) for financial management and both efficiency and effectiveness. (Localism cannot mean complete freedom to set objectives and services. What then would become of, say, RAWP's mission to equalize opportunity for those at equal risk from specified diseases and conditions? (27))

This scenario surely squares with a new and improved structure and strategy for health services research (whether or not completely under the aegis of the NHS Board, as argued above). Nevertheless the needs of health services research as argued in this paper naturally stand out whether or not 'Griffiths' is the vehicle by which to address these needs. *Whatever the current attitudes to forms of management for the NHS, and to the appropriate locus for the management of health services research, these needs are constant.*

NOTES AND REFERENCES

1. The Nuffield Provincial Hospitals Trust, *Five Years After* (Oxford: Oxford University Press, 1978), p. 5.
2. *Ibid.*, p. 5.
3. *NHS Management Inquiry* (Griffiths) Report to the Secretary of State, 6 October 1983.
4. Various Nuffield Provincial Hospitals Trust Reports—*Five Years After*, and also: *Portfolios for Health, 1 and 2* (London: Oxford University Press, for the Trust, 1971 and 1973); *Positions, Movements, and Directions in Health Services Research* (London: Oxford University Press, for the Trust, 1974); *Research in Medicine* (Nuffield Provincial Hospitals Trust, 1977); and *Matters of Moment* (Oxford: Oxford University Press, for the Trust, 1981), 1st and 2nd essays.
5. The White Paper, Cmnd. 5046.
6. See Medical Research Council, *Annual Report 1982-83* (London: Medical Research Council, 1983), especially pp. 78-82.
7. See *A Framework for Government Research and Development: LORD ROTHSCHILD, 'The Organisation and Management of Government Research and Development'; SIR FREDERICK DAINTON, 'The Future of the Research Council System. A Report of a CSP Working Group'*. Cmnd. 4814 (London: HMSO, 1971).
8. For an example of the problems in the US caused by fragmentation in health services research and the difficulties in achieving expansion of, say, the National Institute of Health's role in such research as opposed to 'basic' medical research, see *New England Journal of Medicine*, 309 (21) (24 November 1983), *Special Report*, pp. 1334-6 on assessment of new technology and practices.
9. See R. H. L. COHEN, 'The DHSS and the MRC. The First Chief Scientist Looks Back', in Nuffield Provincial Hospitals Trust, *Matters of Moment* (Oxford: Oxford University Press, 1981), p. 5.
10. Advisory Board for the Research Councils, *A Study of Commissioned Research*. Report by SIR RONALD MASON, FRS (London: HMSO, November 1983).
11. *Ibid.*; p. 8, paragraph 18.
12. *Ibid.*; p. 11, paragraph 25.
13. See, e.g., SIR ANDREW WATT KAY, *Research in Medicine. Problems and Prospects*, Rock Carling Fellowship (London: Nuffield Provincial Hospitals Trust, 1977), p. 8.
14. DAINTON, *op. cit.* See MASON, *op. cit.*, Annex 1, pp. 13-14.
15. SIR ANDREW WATT KAY, *op. cit.*, p. 85.

16. See any recent *DHSS Handbook of Research and Development*.

17. That is, more than a revival of the strengths of the Research Council system, as advocated by Clive Booth in his comment on Mason's proposals, is needed. See *Times Higher Educational Supplement*, 9 December 1983, p. 16.

18. As applies in the case of the control of cervical cancer and the search for an effective management process.

19. Work on improved policy formulation for maternity and neonatal services, commissioned by the Trust, is currently exploring the complex of issues involved.

20. As again illustrated in the case of the control of cervical cancer.

21. A characteristic of 'political' reports (arguably such as the Short Report, Second Report from the Social Services Committee Session 1979-80, *Perinatal and Neonatal Mortality* (London: HMSO, 1980)) but perhaps also of 'scientific' reports.

22. CALUM PATON, *The Policy of Resource Allocation and its Ramifications*, Occasional Paper No. 2. (London: Nuffield Provincial Hospitals Trust, 1985).

23. See *NHS Management Inquiry, Letter to Secretary of State*, pp. 3-4 (The Griffiths Report, 6 October 1983).

24. see 'How Far Down the Road?', *Health and Social Service Journal*, 2, February 1984, pp. 132-133.

25. e.g. See *Health and Social Service Journal*, 2, 23 February 1984; p. 236.

26. See DAVID ENNALS, *op. cit.*, again.

27. DHSS, *Sharing Resources for Health in England* (The RAWP Report) (London: HMSO, 1976), p. 7.