Review Article
by Jack Meadows

Prospects: a strategy for action. Library and information research, development and innovation in the United Kingdom. London, Library and Information Commission, 1998. ISBN 1-902394-01-1

The Author

Professor A.J.Meadows trained in Russian at Cambridge University and read Physics at Oxford University. He gained postgraduate qualifications in Astronomy and in the History and Philosophy of Science. After working at the British Museum Department of Printed Books and Manuscripts in the 1960s, he moved to Leicester University as Head of the Departments of Astronomy and History of Science. He was also Project Head of the Primary Communications Research Centre and of the Office for Humanities Communication.

He moved to Loughborough University in 1986 as Head of the Department of Information and Library Studies (until 1990). His university responsibilities have included posts as Dean and Pro-Vice-Chancellor; his other responsibilities covered the Library & Information Statistics Unit (LISU) and the Computers in Teaching Initiative Centre for Library and Information Studies (CITLIS). He has published widely.

The Occasion

We are delighted that Professor Meadows was able to accept an invitation to contribute this Review Article - as a sequel to the discussion in LIRN 70 of the strategy at its consultation stage. He has wide practical experience of the research funding process. While there is to our knowledge no formal record of the number of applications and grants awarded over the years by the Research arm of the British Library - it would be a fair bet to place him among the Top Ten in any such count! To celebrate twenty years of the Research & Development Department in 1994 he was commissioned to write an historical analysis (Meadows, A.J. *Innovation in Information: Twenty Years of the British Library Research and Development Department.* 1994. East Grinstead, Bowker Saur, pp. 162. 22cms ISBN 1 85739 100 4.)

The contemporary world seems to be full of mission statements, presentations of aims and objectives, and so on. Most such documents are pulled out from time to time - normally when someone asks what your organisation actually does - dusted off for the occasion, and then put back on the shelf again. The problem, from a usage viewpoint, is that they are often too general: they mainly seem to advocate motherhood and apple pie (or possibly fatherhood and plum pie).

How does *Prospects: a strategy for action* match up in these terms? It is actually a distinctly competent job. Of course, it contains all the current 'buzz' words - 'lifelong learning', 'holistic', etc - but it does get down to a level of real detail. It is the result of a major exercise in mapping both the current situation in library and information research and opinions about such research. The resulting assessment makes for extremely interesting reading. Yet it also presents a problem. This is avowedly a synthesis of the results of the study. Having been involved in it in a minor way, I know that considerable differences of opinion were actually expressed. You would hardly guess it from the resultant document. Partly as a consequence, a reader can hardly fail to gain the feeling that the proposals are non-problematic, whereas such is hardly the case. Since this will deservedly be an influential document, it is worth considering what sort of problems are hidden behind its confident exterior. What I talk about here will obviously be those problems that strike a person with my background. No doubt readers with other backgrounds will perceive other problems, or none at all.

The first thing that struck me about *Prospects* was its basic philosophy - that all research is essentially group research. References to collaboration - variously qualified as inter-sectoral, multi-sectoral, or cross-sectoral - are rife. What this overlooks is the basic fact that research collaboration is always between individuals. The history of grants for collaborative research, not least amongst the various EU initiatives, is littered with instances where collaboration has been less productive than expected because of personal differences in outlook between the collaborators.

In addition, the higher the level of co-operation, the greater the amount of bureaucracy. One of the skills of a good researcher actually involves knowing how to obtain results with a minimum amount of time-wasting interaction with others. (Needless to say, brain-storming sessions at pleasant locations are another matter.)

There is a more fundamental difficulty with the concept of collaborative research. Research comes in various forms, as section 2.11 of the report reminds us. The value of collaborative work is not the same for all these forms. Roughly speaking, research at the applied end of the scale is often advanced more by collaboration between institutions than research at the basic end. It is clear that *Prospects* mainly has the applied end in mind. The vision thing with which the report begins explains at one point that it seeks to ensure 'research produces evidence for practice and policy making'. That is applied research. At another point, we are told that 'a national agenda of research issues' will be maintained. I try to imagine going up to a theoretical physicist of my acquaintance, and saying to him, 'Here is a national agenda of research issues: work on one of these'. Since he has an acid tongue, I would not dare. However, I can well imagine saying it to an engineer, and having it received equitably.

This emphasis is hardly surprising. Much information research is, and is intended to be, applied. However, a glance back over the history of the field underlines the need for some basic research. For example, the development of current methods of information retrieval has depended on it. What is missing here is a discussion of the need for a balance between basic and applied research, and the factors that should decide where that balance lies.

The programme put forward in *Prospects* identifies a set of core themes to be explored. For the most part, tackling them will require some kind of mission-oriented research. Such research typically calls upon an existing corpus of knowledge. The question is always - is the existing knowledge base sufficiently well-developed to support the mission? For example, we are told that: 'Areas in which content development is needed, particularly in the public library sector, include: education and lifelong learning; training, employment and business to foster economic prosperity; political and cultural resource material to nurture social cohesion'. So far as I know, what approaches are best suited to promote all these topics is still a matter of debate, sometimes quite bitter debate. How, under these circumstances, do you decide questions of content and presentation?

The failure to comment on this kind of question reflects a generally top-down attitude regarding the role of the Library and Information Commission, despite the consultations that went into the present document. Consider, for example, the following statement: 'Ultimately, however, the responsibility for strategic leadership and policy-making for library and information related research rests at national level'. To which the answer is 'yes' - if you are talking about expensive development work (with the proviso that any central body will only get it right part of the time). For basic, or curiosity-oriented research, the answer is 'no'. In this, the researchers lead, and the Commission, if it has any sense will follow; even though the researchers may well be pursuing illusory goals for much of the time. The report has a highly correct attitude towards the need for egalitarianism. But research is not egalitarian. The Medical Research Council for much of its early life took a diametrically opposite view to the Commission. It concentrated its funding on outstanding individuals, and let them pursue their own lines; which is why we have a flourishing biotechnology business in the UK along with a number of Nobel Laureates. Now, obviously, biomolecular research is considerably different from information research, but there is no hint in the report that researchers come before research themes, not after.

The egalitarian spirit raises particular questions for evaluation. For example, one proposal is that research users should be involved 'in evaluation of proposals and assessment of findings and impact'. This assumes that all users understand what research is about. I wonder. I remember being told, some years ago, by an eminent librarian that it was my job to show that public libraries were struggling under the cutbacks of the then government. If the research did not show that, it must be bad research. From the Commission's viewpoint, was I wrong to disagree? More generally, one of the well-known results of evaluation studies is that the more innovative a project, the more likely it is to receive a drubbing from its evaluators. I doubt that any of the report's recommendations will change this, and they may even work in the opposite direction.

It would be unfair to criticise the report too much, since, in all this, it is simply going along with current orthodoxies. But even being orthodox can raise some questions. For example, one major change in research emphasis over the past few decades has related to researchers' attitudes to funding. Research grants were originally intended to help follow up new ideas. Sometimes this would require a large grant; sometimes a small one; sometimes no grant at all. The value of a research idea was not seen as related to its expense. Now, particularly as a result of the RAE, researchers are under continuing pressure from their institutions to acquire more and bigger grants. The winner is the Red Indian with the most scalps dangling from his/her belt (or perhaps I should say, the Native American with the most hirsutically challenged victims). The overall result is that research initiatives involving large collaborative projects have become the flavour of the day. If you spend a lot of someone else's money (and hope for more in the future), you must be sure of obtaining results. So research ideas must not be too adventurous: indeed, it is best - as with the present document - if the ideas come with prior endorsement from potential funders. My own assessment is different - no wasted money: no breakthroughs.

All of this applies to library and information research as to any other subject. But there is a specific problem. British research funding in this area is limited - perhaps more even than in most subjects. Under these circumstances, going with the tide may not actually be the best way of spending the money. The answer that the 'Commission will co-ordinate investment at all levels', though a sensible move, does not answer the basic question. What cost-effectiveness criteria should be sought to compare projects requiring greatly differing expenditures?

Which brings me to my final criticism of the report. It fails to remember that both researchers and their audiences are subject to original sin. Consider the question of absorbing research results: a problem that is thoroughly recognised in the report. The underlying assumption, nevertheless, seems to be rather similar to Emerson's: 'If a man write a better book....the world will make a beaten path to his door'. If only. For example, under the core theme of 'competencies', we are told that, 'equipping individuals and organisations to play their full role in a learning and information society provides a rich agenda for development'. I am sure it does. But can I see the evidence that they actually want to play a full role in such a society?

One question inevitably in the minds of researchers is how a particular piece of research affects their own careers. For example, if a research centre is set up in institution X, it is tacitly assumed that this enhances the reputation of that institution. Hence, a researcher in institution Y will have a somewhat hesitant interest in the research the centre produces, and may, indeed, be critical of it. This does not always assist in the research being absorbed. Similarly, effective dissemination of research to practitioners can be very time-consuming. (They tend to be distressingly resistant to the great truths that they are being offered.) Will expenditure of time on such dissemination enhance a researcher's career more than (say) embarking on another research project? Questions like these come before any questions about the mechanics of disseminating information. Yet they hardly seem to be recognised in this report.

Prospects is, in many ways, an excellent document. In terms of applied research and development, the pathway it proposes for the next few years is probably as sensible as any that could be devised at present. My problem with the report is that it does not fully recognise the existence of deeper issues behind the ones it actually discusses. Yet such deeper issues could, in some cases, affect the success of the various enterprises that *Prospects* recommends. For any researcher who has a concern with such issues, and yet is faced with the need to garner research funding, I offer a solution from my own experience. Few funding agencies construct their requirements so precisely that a researcher cannot, with the exercise of reasonable ingenuity, subvert them sufficiently to allow some study of basic questions.

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