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25 May 1988

PRIME MINISTER

E(ST) MEETING

Despite the vaunting of the case against Government finance for near market research and despite all the talk of ceasing to pick commercial winners, there is a lot of muddled thinking both in the ACOST advice and the DES paper. Both display an underlying hankering after some form of bureaucratic direction of research towards the 'useful' or 'exploitable'. There is a clear shift of funding away from core science to what Fairclough calls 'enabling technology'. The DES paper speaks with some self-congratulation on SERC's doubling of the spend on engineering research by cut-backs in pure science.

We must therefore beware that the present vogue for reorganisation of funding mechanisms and restructuring research councils with enhanced participation by industry, is not the start of a great new winner picking game masquerading as a new dawn. In Plato's Republic, Socrates asks Glaucon if money should be spent on astronomy and Glaucon replies that it ought to be because of its military and navigational value. Socrates replies that he is amused to see such fear of recommending useless studies. The debate is therefore over 2000 years old and still unresolved in the papers we have today.

Redirection of Research and the new IRC's

With such confused thinking, there is danger that the role of industry will be misconceived. Consider the arguments in both papers about Inter-disciplinary Research Centres (IRCs). The original concept is good. Bringing together

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various academic disciplines to attack a specific matter such as superconductivity is excellent. As human discovery advances, the rigid classification of science into ancient categories must evolve.

It is also good that industry be encouraged to support IRCs and it is in industry's own general interest to do so. A strong national science base, well managed as opposed to economically directed, automatically contributes to high industrial productivity and national return on capital. The proximity of the top research establishments in California to Silicon Valley and in Massachussetts to Route 126 are not coincidental. Much academic research is funded by industry and many of the industrial leaders have their educational roots in proximate academies. This partnership did not, however, flourish through industry attempting to pre-ordain the outcome of the research and pushing funds into what some committee foresaw to be commercially exploitable! It was based on identifying and supporting high quality team leadership, setting a budget, and then leaving well alone - the Perutz approach rather than that of David Phillips and Francis Tombs.

It is a great fallacy that research can be corralled into specific areas by economic forces alone. Unless the intellectual leadership is there, and intellectual leadership is always driven by curiosity, economic pressures will simply lead to second-rate work. If we are to set up some 40 IRCs by 1993 to cover some 15% of work supported by the Research Councils, three crucial points follow:

1. The purpose of each IRC must be clearly defined in advance, a budget must be established for it over its lifetime, and it must be disbanded after the job is done. Once any organisation gets going it will develop a great self-interest in perpetual life!

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2. If IRC's are to be focussed on strategic and exploitable science, their funding must not deplete the funds going to core science. It is disturbing that current figures indicate a 1.5% real cut in the science budget between 1988 and 1992.

3. Industrialists should be encouraged to play a general part in IRC funding. Beware, however, of those who seek specific answers for an immediate benefit. Otherwise the IRCs will do industry's own near market research at a discount. It worried me to hear Francis Tombs last month talking about IRCs being set up in industrial premises. The kind of industry support to be encouraged is that which sponsors a general field of inquiry, the way ICI might support research into the bonding mechanism of polymers.

Dual Support Re-appraisal and Research Council Restructuring

Despite all their faults the Research Councils ultimately impose accountability on the spenders of their funds. This is not true of the UGC and once money has been allocated its deployment is completely at the whim of the recipient university. This has led to much wastage and inefficiency with life tenured teaching posts a visible scandal. The proposed rethink of this area is therefore both overdue and welcome. It should be encouraged. The objective should be improved teaching and research quality leading to greater value for money. Management mechanisms which achieve these should be supported.

As with the IRC argument above, we must be watchful that the emphasis does not turn from quality to exploitability and that optimum management of research funds is not confused with foreseeable commercial benefit. When I visited CERN my criticisms were about management inefficiencies and waste,

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not against the importance of the work simply because it was remote from commercial exploitability.

There are clear arguments for greater accountability on the sum total which universities receive. A good start would be to divide the money between research and teaching and it is nonsense to argue that academics cannot apportion their time. People in industry who work on different projects such as merchant bankers or consultants do it all the time. It forms the basis of charging the client!

Now is also the time to take a look at the Research Councils' enormous imbalance. SERC absorbs £330m annually whereas AFC and NERC together absorb £120m. It might therefore make sense to merge AFRC and NERC and to transfer the biological funding from SERC to an enlarged MRC perhaps called the Biological Research Council. The latter would be bitterly fought by SERC, with all sorts of 'disinterested' arguments for maintaining the status quo. They would argue that to allow SERC to fission would be a move in the opposite direction from the whole thinking behind inter-disciplinary work! There is no limit to sophistry when self-interest is defended.

The condensation of the Research Councils into a smaller number having approximately comparable budgets seems a worthy goal and should be supported. Indeed, anything which cuts down the bureaucracy of this country's multifarious methods of research financing can only be helpful to those who provide and those who spend, as well as those ultimately accountable, namely HMG.

None of the papers mention the possibility of merging the new University Funding Committee (UFC) and the Polytechnic equivalent (PUFC) whereby teaching funds could be administered under one overall vote with research in

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another. An idea so fundamental, which could ultimately eradicate the difference between universities and polytechnics, may be outside the scope of E(ST). However no decisions should be taken in the present debate which would ultimately preclude this solution.

Selectivity and costs

Baker's arguments in Paragraph 36 that selectivity and concentration of research will inevitably mean new spending is certainly not self-evident and should be tested. The original argument for selectivity was to avoid spreading limited research funds too thinly over too many ever-increasing and more costly fields.

There may be a case for increasing the overall research spend and but this argument does not make it. Proposed measures for ensuring efficient management of the funds, as opposed to economic direction of the work, should be central to any sound case for more money.

Summary

1. For basic science, economic benefit analysis is a waste of time because the greatest advances have always come from unexpected directions. The economic consequence of basic science is in principle unplanable, and any economic returns are pure speculation. This does not argue that taxpayers' money should be handed out on demand to the scientific lobby to spend at will.
2. Through elected Government the nation must decide how much it has available for basic science and in which fields it has achieved leadership and can therefore be expected to do better than other nations. For a given spending level, value for money criteria must be

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established which do not rely on prejudging the work's usefulness.

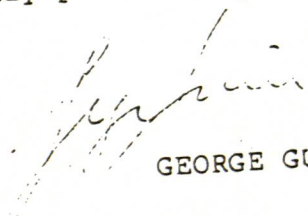
3. What appears in the short term to be useless is frequently that which has the greatest long term value. Electricity and atomic energy are obvious examples and there are many others. If the electronics industry was known as the quantum physics industry in the same way as the chemical industry is so-called, the point would be obvious to many who do not have the scientific education to perceive the direct, albeit long term, linkage.
4. By contrast, the technology programme is addressed well. Academia and industry are being joined in fields such as polymer composites, ceramics, opto-electronics and radio technology. Having acted as catalyst, Government should try to stay out of this partnership. It should be left to the vision of those individual businessmen who spot the opportunities which university brain power offers.
5. The successful businesses of the 90s will be those who have thought out the relationship between marketing, development and research. These are three separate activities which must be balanced and IBM is a classic example of how to get the balance right. Development should always be drawn by the possibility of market demand whereas research must continuously extend the boundaries of the possible with little pre-judgement about usefulness and profitability.
6. Many of the present proposals are good and show innovative thought. However, underlying a lot of the reasoning is a proclivity to direct taxpayers money into what is thought to be economically beneficial. We must not let this balance continue to erode funds from core science.

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Points for the Meeting:

1. Continue to develop the line that it is Government's job to finance science but industry's to finance technology unless Government needs that technology for its own purposes such as defence.
2. Emphasise that, regardless of whether the activity has reached the vicinity of commercial exploitation, it can be managed so as to optimise value for money. Should ACOST be asked to advice on how?
3. In which sciences and technologies is Britain currently ahead? Does E(ST) accept that we should give priority, in basic science, to those areas where we have demonstrated leadership, rather than to exploitability.
4. How much should Government concern itself with the level of private sector R&D expenditure. Will not the rough justice of the market place ultimately decide those companies who survive and will they not have been the ones that spent the 'right' amount on R&D?
5. Is it not likely that, as corporate profitability and the industrial base of the country recovers, R&D activity will naturally return to an internationally competitive level without cossetting and exhortation from Government.
6. Is there a major fallacy in the way defence R&D is measured? Some outside commentators have suggested that as much as one half of the £2.3 billion R&D accounted by the MOD is really procurement. How can we get at the answer?


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