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To: Ms Claire White
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From: Peter L Bergquist
Deputy Vice Chancellor (Research)
Professor of Biology

Date: 18 December 1997
Pages: 7
Re: University Research in the Contend
of the West Report' - document follows

· Comments:

Dear Ms White

It is likely that the University will provide further comment on other issues - as determined by Professors Yerbury and Loxton.

Yours sincerely

Professor Peter Bergquist
Deputy Vice-Chancellor (Research)
Professor of Biology

Ref: s73west

University Research in the context of the West Report,

The West report placed little emphasis on some of the more critical areas for Australia in the maintenance of the nation's research and research training base. The following comments are offered by Macquarie University in the hope that the final report will recognise the essential role played by the universities in basic and innovative research and research training.

Characteristics of University Research.

(University graduates are a great potential resource for Australia. The universities must be able to establish and maintain a first-class international reputation in an increasingly competitive global environment to maximise this resource. Academic staff members are the country's principle source of skilled expertise in many fields of intellectual endeavour and their international credibility enables Australia to participate in the global community and to remain competitive in the international market place. Australia's standard of living and quality of life are strongly inclined to the high quality of their staff and student bodies.

The one single distinguishing feature of a university is its research, and the world's great universities are ranked by their research quality and productivity. Teachers who are active researchers are frequently considered to be the best teachers. The skills and knowledge implicit in a degree course are best taught by those involved in research activities and independent thinking on a daily basis. Learning practical and problem-solving skills from a recognised research leader is more effective and stimulating than being taught by an instructor whose knowledge is reamed from a textbook. Although a number of recent studies have reported inconclusive studies on the linkage between quality teaching and research, it is likely that the wrong questions have been asked. The relationship between research and reaming should be explored lather than the linkage between teaching and research, and greater emphasis should ~ placed on how knowledge is generated and communicated. Currently, there is great emphasis on the virtual University and there is wild talk (particularly by- IT providers) about future students accessing information via the internet, as if this was the only [aces of learning. But what must not be forgotten is that someone has to generate the Knowledge that may ultimately end up in databanks for student mining. It is not possible to do virtual synthetic organic chemistry or build visual hydroelectric dams. There must be reaming informed by research experience to provide the design rules

Innovation and the Setting of National priorities

T he West report considers that a system of national priorities for research should be formulated. We believe that This suggestion, so dear to the hearts of politicians and bureaucrats worldwide, has not been considered in appropriate depth so that the consequences of such an action could be understood. There are some examples of such policies nearby, but close control of what

research should be done and on what topic is a minefield into which others have blundered Australia should learn from an examination of the efforts and mistakes of others.

We believe that Australia faces major challenges in science policy and practice which result from the radical international changes that have affected all aspects of our [lives over the past few years]. In the past many scientists have chosen to ignore the political aspects of science policy, but it is now being forced on us in such a way that it is now inescapable and the proposed changes - and their results - will have significant effects on our standard of living into the next century.

It has been pointed out that the market for almost all products is now global. In high technology areas, the markets are driven by product innovation (for example, microelectronics and computing). Global markets for low technology products [that is to say, the bulk of Australia's production] are driven by price which is a function of capital, labour costs and exchange rates. Novel technologies can be applied to such products to drive down costs but in both high and low technology products' success in the global market means creating and applying new knowledge, that is to say, new technology, to gain a competitive advantage. Asian countries with no prior developed research culture have done just this, as is seen in the example of South Korea. It is evident to all that Australia must maintain an appropriate level of discovery and innovation to acquire this new knowledge and develop new applications.

It is evident to all that Australia must maintain an appropriate level of research to acquire this new knowledge and it is recognised that the universities are a major source of new and innovative research. In achieving the goal of generating new knowledge, few would challenge the need for applied research and development but evaluation of the contribution of basic research is more difficult. However, most published attempts to address this question strongly support the need for basic research (e. g., Mansfield, Res. Policy 20: 1 - 12, 1991; Mansfield and Lee, Res. Policy 25: 1047 - 1058, 1996).

Econometric analyses show that research and development is a significant and good investment on average, and that basic research appears to have strong effects on productivity growth. It is also argued that the "rationale of academic research extends far beyond narrowly defined economic benefits". Even if basic research is judged in restricted economic terms, its role is substantial with a rate of return conservatively estimated at 28 per cent by one analyst (Mansfield, *op. cit.*).

How does the nation set priorities for taxation-funded research? One approach that is close to that in force in New Zealand. The Ministry for Research, Science and Technology has formulated 17 output classes for the Public Good Science Fund based largely on historical research expenditure patterns by government research. We believe that this type of framework is unduly restrictive with the narrow definitions relating to the output classes encouraging

rigidity and stifling innovation and we can identify a different set of research areas of importance to the canon by using a different procedure - asking the questions: what do we need to do to maintain Australia's standard of living in the year 2000?

What innovations are necessary? What basic knowledge will be required? What research skills will have to be maintained or developed? Rather than pursue the current preoccupation with structure and frameworks, bureaucrats and politicians should emphasise research performance. Thus a major aim of government policy should be to create a strong research community - in Melbourne Universities which is dedicated to discovery and innovation and which encompasses a number of research styles depending on the stage of implementation of the result of the discovery process. The means for achieving this end are straightforward - provide the long-term funding of research groups that have demonstrated high productivity, or clear promise, as assessed by peer review. A second and less important feature is the relevance of the research to Perceived technological needs. Innovative science in an apparently **irrelevant area** [for example, early experiments in genetic engineering may be ultimately of far greater significance for technology and commercialisation than Medicare research in the most relevant area of current interest, fashion or perceived national priority. Adoption of any other criterion but excellence will undermine such an enterprise from the start. However, a further ingredient is necessary - the willingness of researchers to investigate technological problems. **At this point a separate** funding system must be available and research proposals should be assessed not only on the basis of their scientific qualities but also from the standpoint of large-scale developmental feasibility, profitability, partnership with existing companies, marketability, etc. But it is essential that it is understood that different temperaments and different styles of research are required. An expansion of the several industry-related problems sponsored by the Department for Industry, Science and Technology and the ARC could achieve this objective.

In summary, this country has much of the mechanism in place but any proposed research priority setting process should be visionary, long-term and strategic and not constrained by historical patterns.

Accountability for Research - Spending the Taxpayer's Money.

One feature of the research scene worldwide has been the demand for greater accountability. Politicians, businessmen and, particularly Treasury officials, have demanded value for money (while providing little). Virtually all grant-giving agencies claim to accept the notion of doing curiosity-driven, innovative research yet applicants are required to state in advance hypotheses they wish to test and to detail expenditure for some years ahead. Reporting and audit requires research projects to be completed within a stated time.

Such procedures are inappropriate for research and all but eliminate innovation. It is also counter-productive for politicians and bureaucrats to rigidly impose business methods on the research world. The whole array of regimes for the management of national research seen in a number of countries, for example, in New Zealand where research funded by the Public Good Science Fund is performed on an annual predetermined contractual basis, leads not to creak but to the promotion of safe and pedestrian projects that will be readily accepted. Furthermore, if rigid time-reporting is imposed, what better way is there to ensure that a project will be completed on time than to have it done before the funding is requested? We fear that rigid organisation of priorities and accountant-generated reporting systems will stultify innovation and lead to missed opportunities and be a detriment to growth and economic advantage in the long term.

There are numerous examples of non-targeted and curiosity driven research that have had major pay-offs. For example, the development of techniques that led to the cloning of genes - the basis of modern biotechnology - depended on three key discoveries, made in different university laboratories by scientists pursuing lines of research unrelated to the final development of the technique, which allowed the cloning of genes into micro-organisms and, indirectly, into the cells of higher organisms. All of these discoveries were needed for this development, and each depended on basic research commenced in the 1950s that investigated bacterial and bacterial virus DNA replication. The cost-benefits of this research, which had no initial strategic focus, and which was largely funded by the American and Swiss taxpayer, are impossible to calculate because of their magnitude. What is certain is that under a rigid priority system with inflexible reporting timelines, it would be impossible to obtain funding for this type of research today. How should the Australian research community, particularly in the universities respond to the current political climate?

What should Australia do? Our view is that there can be substantial economic benefits from what might appear to bureaucrats and Treasury officials as 'useless research', and we believe that the universities, while cooperating in research which is believed to be in the national interest must above all maintain their independent status and provide support for curiosity driven and seemingly useless research

Furthermore, we wish to emphasise that it is imperative for universities to hold to their core in research and teaching, standards which ensure that graduates find ready acceptance internationally. Of course, universities educate for present and future employment in Australia. It is the added dimension of introducing students to research at the forefront of a discipline which marks universities as distinctive and which makes the step to furthering a career overseas a ready option for their graduates. We must continue to look outward to this wider horizon. Many voices have been raised of the inadequacies of a system of fixed research priorities, where

political expediency- is paramount, and of the danger in making the individual investigator extinct. As directors of research, politicians have obvious limitations. They have a short time horizon, three to five years and they can be greatly influenced by the media whose time horizon is even shorter, days to weeks. We must recognise that, for the foreseeable future support for research is likely to be conditioned by perceived 'relevance' to inward-looking, politically generated goals. We all must work to influence these goals. Where, for instance, in any priority statement do we see a 'goal which is to maintain a strong academic capability- tailored to produce first class scientists and engineers? These people will be essential as problem solvers in an unpredictable, fast-moving future

Further, where do we see a goal which ensures support of highly-competent investigators? Some function best as members of a team working toward a major objective but others perform best when permitted to follow the dictates of their intuition and judgement. Goals of this type must be embraced by the system if universities are to participate effusively. Many problems facing science in particular now are long term. Research directed toward solving such problems require steady support for a decade or more. What is needed is a mechanism whereby politicians can be assisted to put such support in place and then to leave it alone. For example, in New Zealand, bipartisan political support for the present science policy has been gained from both major parties.. In some ways, this is a positive step, but it is also disturbing. Instead of encouraging critical evaluation, it encourages complacency, an expectation that an under-funded, over-structured inward-looking system will deliver the desired outcomes

Australia 1~. a further good reason for looking outward in the search for better systems, and an excellent model is the United States National Institutes of Health. Australia may not be able to match their dollars but their highly successful philosophy is transposable. Given a clear mission to see; a cancer cure and to eradicate infectious disease, they initiated a program of basic research that targeted fundamental processes, growth in the case of cancer, infection in the case of disease. This program was individual investigator driven. That research has led among other things to the development of the polio vaccine, to recombinant DNA technology, to the discovery of oncogenes and retroviruses. The resultant knowledge not only forms the basis of much cancer treatment but has contributed to the general treatment of viral diseases, to understanding genetic causes of diseases and to the emergence of a biotechnology industry.

Basic research and individual investigators can flourish as part of a strategic target as long as Legislators are patient and receptive to the often serendipitous nature of research. The time for an applied program to begin in the context of one example was when basic research confirmed that a polio vaccine was possible. Political impatience in this case could have created a world filled with iron lungs instead of healthy people with circulating antibodies. The National Institutes of Health are an excellent example of the benefits of a system in which a proper mix of

basic and applied research was implemented. We must move our system in this direction and the universities and their graduates must lead and not simply be led into a constraining maze.

The more limited the resources of a nation, the more strident is the call for research to be directed to solve urgent problems of society. This philosophy is fundamentally misguided. The major discoveries in medicine, for example, have come from the pursuit of curiosity about nature with primary relevance to medicine. The same can be said of many industrial inventions, haphazard at the outset and only later c for their commercial value. The award of a research grant is flawed when it requires the applicant to chart a path to discoveries that will have practical consequences. That makes no sense whatsoever. Researchers, lilt artists and athletes, should be awarded contracts on the basis of achievement and potential rather than on what they promise to do They- must be able to rely on intuition and have die capacity to move quickly in new directions, if the full potential of c universities is to be realised.

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17 December, 1997

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