President Reagan's advice

Keyworth answers the critics

Washington

PRESIDENT Reagan's science adviser, Dr George Keyworth, is credited with having talked a parsimonious administration into giving research and development an unexpected increase of 17 per cent in its 1984 budget proposals. But he has also become a controversial figure, chiding the science community for its reluctance to make hard decisions on priorities and for its lukewarm response to the President's call for help in designing a futuristic defence against nuclear attack. Last week he answered the following questions.

The President's call for scientists to design a defence against nuclear attack has been greeted with some scepticism. Have you sought or found any eminent scientists to head an investigation of the possible technologies?

Keyworth: There has been outspoken criticism from an element of the leadership of American science but I think only a single and perhaps even narrow spectrum is represented. Most has come from those individuals who have also criticized basing modes for the MX missile and the strategic modernization programme, and have been deeply involved in arms control measures for years and years. Many are individuals who decades ago embraced a philosophy of unilateral disarmament.

What about people like Richard Garwin?

Keyworth: Richard Garwin has been far more noted for his criticism of trends we have been taking than for his constructive criticism, certainly in recent years.

He played a crucial role in the development of US strategic weapons and the development of MIRVs.

Keyworth: I can only speak of present years, not history. By the same token some of the most outspoken advocates of unilateral disarmament were the people who helped build the atomic bomb in the first place. The responses we are hearing — not in the media — show that a majority of the technical community is quite supportive. We have been able without any difficulty at all to persuade some very distinguished members of the technical community to help enthusiastically to develop the programme.

Critics ask whether, given the number of warheads possessed by the United States and the Soviet Union and their destructive power, a defence that was not pretty near perfect would be worthless.

Keyworth: A common criticism we hear is that, traditionally, defensive measures are inflexible and lethargic while offensive systems are flexible and difficult to counter. The Maginot Line faced the most flexible offensive system there is: men. Here we have quite the opposite. The ballistic missile is an extremely inflexible, rigid, constrained weapon. When it is fired it follows a rigid trajectory. Even when

manoeuvring re-entry vehicles are used in the future you are still talking about a very tight cone. The vehicle's position in space and time has very few uncertainties after launch. To state categorically that it cannot be done, or that it requires a perfect system, or that a leakage rate of 10 to 20 per cent would still cause devastating damage, is a premature and superficial examination of the problem.

What we want to do is build a system effective enough so that the Soviet Union can no longer practically conceive of an attack on the United States or an ally. I for one believe that that is quite feasible.

Would you agree with the view of many particle physicists that Brookhaven's ISABELLE design is going to be obsolete before it can be built? Ought the United States now commit itself to a more ambitious design such as the 'desertron'?

Keyworth: I tend to lean in the same direction but hesitate to use the word obsolete. It is not so much the design of the machine but the operating parameters that are in question. The ISABELLE project was conceived almost a generation ago in high energy physics, in the mid-1970s. We must look now for the proper step for the United States and for the field as a whole in the context of what are the questions to be asked and what is the state of the theory. Do we want to take a bold new step to maintain the vigour of the field or do we want to focus on keeping the maximum number of people busy? I have no doubt that some very exciting forefront research would come out of ISABELLE. But the community seems to believe that a large increase of energy over anything available today or projected for the future offers by far the greatest excitement, and the possibility of clarifying our understanding of elementary particles.

The Nuclear Science Advisory Committee has just recommended the South-East Universities Research Association (SURA) proposal for an electronic accelerator over that of Argonne. The High Energy Physics Advisory Panel may soon endorse ISABELLE. Will you regard these decisions as final?

Keyworth: Ultimately, the federal government will make the final decision although when the government asks a body of scientists for a recommendation the action it eventually takes is traditionally consistent with the recommendations.

The electron machine issue is indicative of a large concern many of us have about American science. Argonne's concern is largely an institutional one. They are asking whether we should begin another laboratory or put the machine at Argonne in spite of the relative merits of the two proposals. If we are going to improve excellence and expand opportunities in

American science, the single greatest threat to progress is parochial institutional concerns. The USRA proposal was excellent. The accusation that it would entail building a new national laboratory is deceptive because there is a clear distinction between multidisciplinary national laboratories and single discipline facilities such as Fermilab. What we are seeing here is institutional interests versus scientific excellence.

So you take a dim view of Argonne's decision and that of local politicians to take the argument to Capitol Hill?

Keyworth: I have every respect for their desire to compete as effectively and aggressively as they can. But it is most unfortunate for science that they wish to bring the issue into the political rather than the scientific realm.

The president of Catholic University has said that the National Centre for Advanced Materials (NCAM) at Berkeley had a dose of its own medicine when its funds were deleted by Congress and Catholic's own vitreous state laboratory got into the budget without peer review. Is that a bad precedent?

Keyworth: I do not think it was by any means the best way of developing science in America. But let's distinguish clearly between NCAM and the Catholic University effort. One is solely an institutional initiative serving that institution's goals and money. NCAM, on the other hand, was developed as a means of enhancing the nation's position. At the forefront of our science policy is a desire to move our immense capability in science closer to the national needs we face in an era of intense competition.

The purpose of NCAM was to develop a mechanism by which a national laboratory could unify the academic world of materials science and the needs of industry. To say that because NCAM did not come through peer review and was part of administration policy it can be compared with the efforts of a hired lobbyist and a single institution's narrow concerns is fatuous and self-serving.

Why was there such a strong reaction against the NCAM proposal?

Keyworth: The scientific community has become overly entrenched in the concept of equitable distribution. The materials science community doubted that NCAM was the optimal step for the health of materials science, and I would not disagree with that. But the issue is what is best for America *in toto*. Where you allocate federal resources is not a scientific issue but a matter of national priorities.

By the way, the NCAM issue is by no means over. The community is a bit unrealistic in assuming that the money originally set aside for NCAM can somehow be used in a way that suits materials science's own priorities. The money will not be available until we can find another mechanism which meets the same objective.

Peter David