**Alan Schriesheim**
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*Alan Schriesheim (standing)*

Before commenting on all the subjects that have been raised, I should probably admit some of my biases. I want to note that Argonne is a multi-program laboratory that does both basic and applied research. It specializes in bridging the gap between the two areas. In my experience it is difficult to have one without the other. In the current climate it is also certainly difficult to justify the one without the other.

I have spent the bulk of my career justifying both basic and applied research. At various times I've done so according to the Financial Standards Accounting Board, the exponentially expanding regulations of the federal government, the Kabuki theater of congressional hearings, and the bean counters' black magic of several internal auditing systems.

I confess that I have found most efforts to quantify the investment and return-on-research yields no better than 50% fact and 50% perception. If anyone is expecting advocacy of a more sophisticated financial quantification of research costs and benefits, they are not going to get it from me.

There is a presumption that it's easier to quantify investment, and return on investment, for applied research than on basic research. It may be true in industry, at least some of the time. But the comparison is difficult because industry engages in practically no basic research, and I'm willing to argue that point with industrial researchers. What some corporations call basic research is really long-term effort which is nevertheless ultimately focused on some end-use application. Because a chip company seeks to learn everything it can about silicon or gallium arsenide, it hardly qualifies as independent seeking of knowledge for the sake of knowledge.

On the other hand, I'm really not convinced that in federally funded science we have any more accurate fix on the investment and pay-off for applied research than we do for basic research. Part of this has been pointed out before. After all, the general public good is the justification for much federal R&D. Who can quantify the benefits of being protected against armed attack when compared with the investment in defense? How do you put a dollar figure on the benefits of acid rain research? Or to take an example that I'm very familiar with: What is it worth to the nation or to the world to have us develop a meltdown-proof nuclear reactor? On the third of April, Argonne deliberately cut off the cooling flow in an experimental breeder reactor in Idaho while it was operating at full power. The unit shut itself down without human or mechanical intervention. At the time we would have had one estimate of the potential benefits. In fact, we sent a press release out that was picked up only by the Idaho newspapers. They don't have a wide circulation outside of Idaho Falls. Three weeks after that dramatic test, the Chernobyl accident would probably double or triple the estimated benefits of this development. You can't pick up a newspaper, whether it is the *Wall Street Journal* or the *New York Times* or *Time* magazine, without having some comment on that.

My point is that it is pretty easy to trap costs and we should certainly try to do so. But comparing them with benefits in anything like the traditional return-on-investment ratio for financial analysis is going to distort our vision. That is the problem! In my opinion it is not going to sharpen it.

Applied research certainly has one major advantage over basic science. The goal of the work can be described in advance. The results can be shown or demonstrated when the effort succeeds. It doesn't make the results more quantifiable, but it certainly makes them more tangible. The proverbial man-on-the-street and the congressman-on-the-Hill can relate to what is tangible. What they cannot relate to is the basic truth that if we don't pursue basic research, we soon run out of new knowledge to apply.

In the best of all possible worlds I would certainly claim that basic research does not have to be justified, either through elaborate financial analysis or through popular appeal. It only needs to be justified by knowledgeable peers. But we live by a democratic process. We obviously have to tell the layman, who ultimately supplies the buck, what is in it for him, so the best possible justification for basic research is the applied research that grows from it. And to the extent that that can be identified, it is obviously a great thing for those people who are involved in it.

We have a number of initiatives underway at Argonne that speak to this point. I don't really think that I want to take the time to discuss all of them. However, I would like to comment on one to give a flavor for what I consider an important trend of providing research support from federal funding to improve the competitiveness of U.S. industry in world markets. The one that is best known is the steel initiative. It was initiated a few years ago when Jay Keyworth was the President's Science Advisor. Essentially, a question was raised: "Why can't we use the talents and the resources in these major institutions and national laboratories to somehow affect the competitiveness of the nation in a positive fashion?" We examined that particular issue, and the people at Argonne came up with something based on their background in magnets and accelerators. This is a great example of the application of skills that have been developed by the government on the research side, to a nationally important problem.

What does this trend portend for basic research? That depends on how well the basic research community relates and communicates its work to industry and hence, to the public. That is a different issue than quantifying it on a financial basis. This is just a matter of doing what is done in this Roundtable: attempting to define what the fallout is from basic research in some reasonable fashion.