CURIOSITY DRIVES BETTER SCIENCE THAN 'STRATEGY'

We are in the middle of a continuing debate with important consequences for Federal science policy, concerning the direction in which Federal support should steer American science, and especially American basic physics, in the future. The physics community should not be reticent about making its views felt in this matter, because the stakes are very large. Nor should disagreements among ourselves concerning "big science" versus "little science" or what is more fundamental than what deter us from taking positions that cut across such divisions.

Driven by a great feeling of urgency because of our military competition with the Soviet Union, the direction of American science policy was determined some 45 years ago after Vannevar Bush's report *Science—the Endless Frontier*. We need only remember the shock produced 40 years ago by the launch of the first Soviet sputnik to realize how strongly the need for technological supremacy was felt for military reasons, certainly at least as strongly as we feel it today for economic reasons in our competition with countries such as Japan and Germany.

The decision made at that time, which was neither impulsive nor sudden, was to support as strongly as possible *basic scientific research*. That decision has had a profound influence on the course of science in this country, especially in physics. Ever since, the United States has dominated the world in basic physics, both theoretical and experimental, to an unprecedented degree, and it is good to remind ourselves that this was a profound change from the priorities and the stature of this country in basic science before the Second World War.

Basic science alone, of course, did not determine American technological prowess, which in turn translated itself into military and economic strength. Basic science had to be transferred to technological applications by scientists and engineers who were as excellent as those working at the fundamental level. It is at this stage that we have lost our edge over the rest of the world. For too long now, many of us, the basic scientists at the universities who educate the young and aspiring new generation, have been guilty of arrogance, looking down on those who apply and transfer new basic knowledge to useful technology. For many years we have had the attitude that our purpose was to train our PhD students to be our successors, and we imbued them with the belief that basic research was the only goal worth pursuing. We could get away with such arrogance because during those years the universities were expanding and there were enough jobs available for PhDs trained in that manner. That has not been true now for a number of years, and our attitude has begun to change. It clearly needs to change even more.

It is one thing to recognize that such shifts in our educational goals are needed; it is quite another, however, to denigrate basic research as such by changing national science policy and by sneering at "curiosity-driven research." We are told that the cold war is over and the world now finds itself in a more competitive economic environment, in which strict attention has to be paid to the realities of the marketplace. Therefore, we are admonished, we can no longer afford the luxury of allowing our basic scientists to pursue their favorite dreams and idle curiosities at the expense of the public. But does anyone really believe that during the cold war the need for technological applications was any less urgent than it is now? Have we been foolish, then, in what we have done for the last 45 years?

I think not. On the contrary, the policy then adopted and kept, with some wavering, for almost half a century has stood us in good stead. It was an enlightened policy. The question seldom asked by those who advocate more attention to "strategic research goals" rather than "curiosity driven" science is, *Which policy in fact more effectively produces the goal of strong science and technology?*

While, no doubt, we at universities need to pay more attention to training students in applied scientific directions, and industrial corporations need to return to policies with a longer view—paying more attention to applied research rather than only to immediately needed development—it would undermine the entire edifice of science and technology to turn off the wellspring of it all by denigrating the prime force motivating most scientists: curiosity. Although following our curiosity may appear frivolous to outsiders, very few fundamental discoveries in physics have been made by researchers pursuing a strategic plan with a goal that is deemed useful to society. (Saying that curiosity is the driving force is not the same as saying that we rely on serendipity, though that is also an element that should not be neglected.) To rely on the self-indulgence of individual curiosity in basic science to arrive at the socially desirable goal of useful applications is no more paradoxical than to rely on the destructive emotion of individual greed to drive the capitalist market economy. A wise policy does not try to inhibit either (as the failure of Communism showed for the case of economics) but instead steers both into socially beneficial directions. (Curiosity about what makes nature tick is surely more socially useful than curiosity about the foibles of our neighbors or leaders, which our society seems to encourage at present to a self-destructive degree.)

A policy that does not feed the flowering of natural human curiosity and channel it into creative fundamental scientific productivity will result in the withering of the whole scientific enterprise. For a number of years we have been seeing a very worrisome dwindling of the number of American students who are interested in science. Among the many causes of this phenomenon, surely one is that science, and particularly physics, has been presented to students in the media and in the schools as a discipline pursued mainly for the purpose of making better weapons and neater gadgets. It is a mistake with grave social consequences not to tap the natural curiosity among the young for science. We should, in fact, encourage this curiosity by emphasizing that it is the very heart of science.

To do research in basic physics we do not first find out what technological progress is needed, then devise a good strategy to get there and finally think about what new fundamental ideas are needed to implement it, although there may be a very small fraction of research physicists who are able to function fruitfully in that manner. There is no historical evidence that such an approach produces results comparable to letting those who are good at it do what their curiosity leads them to do. The "strategic planning" approach has already filtered into the culture of writing far too many grant proposals. A science policy that discourages "curiosity driven" research will become even more oppressive, and what used to be a flourishing enterprise full of originality and imagination will become a business of dry drones churning out routine products. We need to return to an American science policy that taps into the natural motivation of human beings and supports a basic science that is *curiosity driven.*

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Might Neuronal Spikes Permit a Binary Brain?

A good mystery is worth exploring. So while I enjoyed John Hopfield's elegant exposition of the computational power of analog "neurons" (February 1994, page 40), I would like to encourage further exploration of neural biophysics by mentioning some problems in applying simple analog theories to the brain.

We know that neurons in mammalian cerebral cortex communicate by means of spikes (action potentials). The difficulty lies in the spikes' interpretation: Do they approximately form a slow analog average-rate code (Hopfield's main emphasis)? Or a fast binary pulse code, in which changing rates are only an epiphenomenon? Or something in between, like Hopfield's example of multiplexed visual processing?

To be fair, no paper has yet shown single spikes in neurons of visual cortex (the best-studied area) responding reliably to stimuli; only the slowly varying *probability* of firing a spike seems related to the flickering patterns shown the animal. Evidently rates do—and single spikes do not—code for such obvious properties of the stimulus as brightness, shape and location.

But most of such neurons' inputs come from *other neurons* in a highly