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A Changing Climate for Scientific Research

A confluence of factors has led to unusual uncertainty concerning support of scientific research. These factors include end of the Cold War, global economic competition, federal and state budget deficits, loss of faith in basic research as a key to prosperity, and diminished public esteem for academic research. The latter is due to publicity about fraud in science and a few instances of faulty bookkeeping of grant overhead charges.

The end of the Cold War, by diminishing funding in the defense industry, is causing major federal laboratories to scramble for support by undertaking civilian R&D. In response to the recession and global competition, many companies have engaged in "restructuring." This has often included a curtailment of efforts in basic research. Federal and state budgetary deficits, combined with diminished faith in basic research as the key to prosperity, have attenuated congressional enthusiasm for support of peer-reviewed research grants.

A significant recent development involves the Committee on Science, Space, and Technology of the House of Representatives. The committee's membership totals 53. George Brown, Jr., its chairman, has seniority and influence and is one of the few members having a degree in science. He has long been an advocate of federal support for basic research. That his position has evolved is evident in his favorable comments about a report* on the health of research prepared for him by the committee's staff. Some quotes from the report follow: "Research policy designed forty years ago may no longer be suitable...", "...maintaining the world's preeminent (and most expensive) federal research system is not, in and of itself, adequate to insure economic vitality"; and "To create a more rigorous and socially-responsive science policy, a necessary first step is to define goals toward which the research should be expected to contribute."

Evolution of attitudes by others in Congress is evidenced by a huge expansion in non-peer-reviewed, pork-barrel facility legislation. A provision in the Senate bill for funding National Science Foundation (NSF) would have drastically modified its status and would in effect have placed NSF under senatorial micromanagement. Through intervention of George Brown and colleagues the onerous provisions were deleted in the House-Senate conference. Scuttlebutt has it that the current flurry of policy-review activities at NSF is a measure to create a line of defense in the 1994 congressional budget hearings. The NSF policy-makers should be steadfast in defending basic research. If they do so, they will be joined by influential allies in academia and industry.

For the foreseeable future, federal support of scientific research is likely to be conditioned by relevance to societal goals, with Congress having a major role in specifying the goals. Obviously one of these should be to maintain a viable academic capability to produce first-class scientists and engineers. They will be essential as problem-solvers in an unpredictable and dangerous future. Another goal should be to support highly competent investigators. Some function best as members of a team working toward a major objective. But others perform even more magnificently when permitted to follow the dictates of their own intuition and judgment.

As directors of research, congressmen in general have obvious limitations. In addition, they have a short time horizon—usually a few months to no more than 2 years. They are greatly influenced by the media, whose time horizon is even shorter—days to weeks. Many of the great problems that the world will encounter are long term (10 to 50 years). The R&D necessary to facilitate solutions for such problems also often will require steady support for a decade or more. There is need for a mechanism to help politicians to choose to provide steady support for important long-term goals.

A recent report[1] by a panel of the Carnegie Commission recognizes the need for such a mechanism and names 12 major long-term policy areas that should be part of a national agenda. Included are health and social welfare, economic performance, and energy supply and utilization. The report proposes creation of a long-lasting, nongovernmental forum that would interact with the political system. The membership in the forum would include a "broad based and diverse group of individuals who are critical and innovative who can examine societal goals and the ways in which science and technology can best contribute to their achievement."

Philip H. Abelson

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Stonehenge in winter. [Photo courtesy of English Heritage]

The Pre-Druid Super Collider?

It has just hit me—Stonehenge is the pre-druid super collider. It was completed (on budget and on schedule) in 2000 B.C. and represents great skill, imagination, and faith—like the 2000 A.D. version. It cost $8 billion, correcting for inflation.

Leon Lederman
Fermilab,
Batavia, IL 60510

Crop Circles: Normal Hoaxing?

I read with pleasure and approval the fair account of our recent Crop Circle-Making Competition (Random Samples, 24 July, p. 481). Its purpose, as stated, was to see the extent to which people could reproduce the features observed in the formations of laid-down wheat, barley, and other crops which have mysteriously appeared during recent summers, particularly in areas of southern England.

The skill shown by the leading competitors surprised even the close students of this phenomenon and persuaded many that the whole crop circle affair could be explained as a human artifact. The only respectable belief would then be that for many years groups of practical jokers have been dedicating their summer nights to creating artistic effects in crop fields, working silently, eluding watchers, and telling no one about it. This is in itself remarkable.

Scientific tests, carried out last year on grain and soil samples from crop circle sites, gave evidence of anomalies which are incompatible with normal hoaxing. This justified a more complete program, carried out this summer by a team of American and British scientists. The results, due to be published in the New Year, may help to establish whether or not human artists can be held responsible for the entire crop circle phenomenon.
Forecast of Earthquake in Western Nicaragua

The destructive earthquake of 2 September 1992 off the Pacific Coast of Nicaragua appears to have ruptured the seismic gap that we identified 11 years ago (1) (Reports, 7 Aug. 1981, p. 648) as the likely site for a future large earthquake. Preliminary teleseismic locations of the main shock, and aftershocks of magnitude 5 or greater that occurred within 5 days of the main shock, outline the seismic gap that we identified on the basis of (i) deep seismic quiescence and (ii) historical earthquake activity. Within the quiescent area, virtually no earthquakes larger than magnitude 2 occurred during our period of monitoring (1975 through 1978), yet the area was surrounded by numerous earthquakes of magnitudes up to 5.7. The quiescent area extended 115 kilometers from the Middle America Trench down-dip on the Benioff zone to a depth of about 50 kilometers and for about 50 kilometers along its strike. We assumed this area was a locked portion of the interplate thrust zone and was accumulating strain. Using a simple calculation, we estimated a possible magnitude of 7.6 to 7.9 on the basis of the size of the quiescent area and the time since the last major earthquake at that location, the magnitude 7.5 earthquake of 1898. This magnitude forecast is indistinguishable from the preliminary moment magnitude of 7.5 to 7.6 determined by H. Kanamori at the California Institute of Technology (2) for the recent earthquake.

Most damage and the loss of at least 200 lives during the recent earthquake resulted from a tsunami. We did not foresee the occurrence of a tsunami primarily because we found no mention of tsunami damage in historical accounts of past earthquakes along the Nicaraguan coast. In hindsight, we see that because the quiet zone extended up to the trench axis, the earthquake had the potential to displace the sea floor sufficiently to generate a destructive tsunami.

These results suggest that the combined study of historical accounts of past earthquakes and data from local seismograph networks can identify potential sites of future destructive earthquakes. Although the timing of the impending earthquake cannot be specified to better than a few decades, this information could help focus the limited resources of hazard reduction programs.

David H. Harlow
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REFERENCES
2. H. Kanamori, personal communication.

Big Physics Collaborations

Neither sociology nor nostalgia for the sixties is a suitable basis for judging the development of the very large collaborations that now dominate experimental high-energy physics (News & Comment, 11 Sept., p. 1468). The last two decades of research have established the "standard model" of fundamental interactions. While this theory has withstood every experimental test so far, its real importance is not the questions it answers but the questions it allows us to ask: Why do particles have the masses they do? Are there forces beyond the ones we know today? Are there particles more fundamental than quarks? We don't know the answers to these questions, but we know where to look for the answers. We need to look at very high energies, in processes with enormous momentum transfer. From this follows the need for gigantic accelerators, detectors, and collaborations. Ingenious, smaller experiments may, with luck, provide clues to the basic questions, but the clues will be ambiguous, if they are not imperceptible.

Neither the Superconducting Super Collider (SSC) nor the European version, the Large-Hadron Collider (LHC), is being built just to find the Higgs particle. There may be one Higgs particle or many. There may be none. All we know is that there will be new phenomena at the SSC and the LHC that will tell us about the origin of mass. But the aims of these machines are much broader: to find new forces and new phenomena that might show up in just a few of the 10^{15} events produced each year. No less than in the past, ingenuity will be essential for making important discoveries.

Thousands of physicists from around the world have joined to develop experiments for the existing and future high-energy colliders, despite the difficulties, because the physics is compelling and because it intrinsically requires the talents and resources of

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many collaborators. It’s easiest to look under the lamppost, but if you know the keys are someplace else, you better look where they are. The keys to understanding fundamental interactions are at the highest energies, where the search requires large-scale cooperative efforts.

Robert N. Cahn
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I agree with the sentiments in Faye Flam’s article “Big physics provokes a backlash” and would not myself like to be in a collaboration of 100 or 1000 scientists. But the need for large collaborations is at least some cases does seem to speak to the request of the National Institutes of Health in recent prominent cases that all authors of papers be able to vouch for all aspects of the paper. This requirement would obviously be impossible and undesirable in huge collaborations. So as long as high standards are kept in all aspects, I do not see why compartmentalization is not acceptable in biology as well as in physics.

Jay M. Pasachoff
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Pioneering Work in Immune Tolerance

The inspiration for our studies of the thymus has been research on privileged transplant sites (for pancreatic islet allografts) and the earlier studies in which cellular inocula in the thymus were used (1). The elegant work of B. H. Waksman and his colleagues (2) who reported the establishment of long-lasting, antigen-specific tolerance to protein antigens by introducing bovine serum albumin and bovine gamma globulin into the thymus (2) has also been important to us, and we have consistently referred to it in our publications (3).

We apologize, however, for the omission of reference to a 1970 publication by G. W. Ellison and Waksman (4) in our recent report “Prevention of autoimmune diabetes in the BB rat by intrathyMIC islet transplantation at birth” (29 May, p. 1321). In their 1970 paper (4), Ellison and Waksman noted a “slight but definite reduction in the severity of EAE [experimental allergic encephalomyelitis]” in rats after intrathyMIC injection of spinal cord homogenates. We note, however, that our model is somewhat different from that used by Ellison and Waksman. Our study was of neonatal animals rather than adults, we introduced vi-
able cells rather than protein preparations into the thymus, and our animals were subject to a spontaneous autoimmune disease whereas Ellison and Waksman artificially induced one (namely, EAE). Finally, our studies involved no manipulation of the peripheral immune system such as the total body irradiation carried out by Ellison and Waksman.

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REFERENCES

Corrections and Clarifications
In the report “Surface order and stability of Langmuir-Blodgett films” by D. K. Schwartz et al. (24 July, p. 508), the last line in the first column of page 508 should have read, “The AFM is also able to image surfaces under liquids, allowing us to examine LB films at molecular resolution in both air and water for the first time.” The first molecular resolution images of LB films under water were published by M. Eggert et al. [J. Struct. Biol. 103, (1990)].

In Leslie Roberts’ article “Prosecutor v. scientist: A cat-and-mouse relationship” (News & Comment, 7 Aug., p. 733), it was incorrectly implied that Rockne Harmon ran a record check on Laurence Mueller that revealed he did not have a valid California driver’s license. Harmon says the information was provided to him by another prosecutor. Harmon also says that William McKinstry, an Alameda County Superior Judge, and not Harmon, was the first to characterize Mueller’s testimony as “shifting.”

In the 25 October 1991 News briefing “Treating AIDS with warts” (p. 522), it is stated that hypericin was originally synthesized at the Weizmann Institute of Science in Israel. While it was synthesized for the application discussed in the Briefing at the Weizmann Institute, its original synthesis was by H. Brockmann at Göttingen University.
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in Sr, Nd, and Pb, and we need to know if all basalts from Pitcairn are high He 3/4 and if there is a correlation such that He increases in one direction or the other along the Sr, Nd, Pb array. The Cameroon line is marked by volcanoes stretching for 1500 km, yet it is labeled "low He" by Farley and Craig, with no reference to which volcanoes were samples or where the data can be found (the data are not in their references 2 or 3, nor is the Fernando data to be found there). There are additional high He islands (Fig. 1) that extend the high He domain more toward our FOZO component.

Farley and Craig locate their PHEM mantle component from the array which their Samoa (Tutuila) data (1) makes on He-Sr, He-Nd, and He-206/204 Pb plots. The observed data scatter and possible curvature in these arrays would seem to allow a PHEM location near our FOZO component.

Finally, we address what appear to be several remarkable features touched on by Farley and Craig. The first is that no single island shows He isotope values both higher and lower than the typical value of 8 Ra inferred for the upper mantle from MORB. If this finding holds as additional He data are generated, it may implicate mixing of upper mantle as an important plume entrainment process. The second is the general left-right division of the DMM-EMI-HIMU mantle plane (figure 1 of the comment by Farley and Craig) into high and low He regions. If this feature "holds up," it may argue not only for the low He 3/4 signature for HIMU, established by Graham et al. (2), but also for a relatively high He concentration in HIMU, so that this component is able to overwhelm the He from other mixing end-members.

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REFERENCES
2. D. W. Graham, S. E. Humphris, W. J. Jenkins, M.
D. Kurz, ibid. 110, 121 (1992).

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