

Letters

The Funding of Science

Gerard Piel, in his article "Natural philosophy in the Constitution" (5 Sept., p. 1056), makes many proposals. I would like to report on the discussion of one of them during lunch at the Stanford Faculty Club—namely the proposal that part of the money for support of science be allocated to universities as institutions for internal distribution. Many disadvantages were mentioned and no advantages.

One person said, "When I was a dean I was always glad that I didn't have to decide the relative merits of the research of my colleagues." Another said that he thought that people in a field all over the country were more in a position to evaluate research proposals than other people in the same university in different fields. Indeed when we need to evaluate someone's research for the purposes of making a tenure decision, we rely mainly on opinions from outside the university. Another said that he was happy to get his research support by mail order, since he did not consider himself competent at internal university politics.

My own field, artificial intelligence, would have been delayed many years if it had been necessary to reach a consensus among the faculty or deans of any university that it should be supported. Let me conjecture that the greater promptness of Americans in developing new fields of science compared to other countries is due precisely to the fact that young researchers do not have to persuade older professors in their own university to give up some of their own plans in order that the newcomers can get started.

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In his article "Natural philosophy in the Constitution," Gerard Piel quotes me completely out of context, accusing me of having views I do not hold and of advocating behavior I do not condone. My remarks that the military are sometimes "disillusioned . . . about science and engineering" were in a humorous vein. The substance of my remarks had nothing to do with the process by which federal funds are allocated to science and said nothing about what scientists should or should not do with respect to them. I certainly did not say that my "hearers should accept their research assignments." I did say that I deplored the polarization and politicizing that had developed on both sides of the strategic defense issue. I

pled for objective, dispassionate examination of the issues. In fact, I invoked the scientific method as a means for coping with emotionally charged public policy matters such as strategic defense.

I find Piel's development of the history and rationale of our government's social contract with the universities to be masterful. I certainly agree with many of his points. But I find his remarks about strategic defense offensive and incorrect. The x-ray laser is *not* the centerpiece of Strategic Defense Initiative (SDI), and it is *not* the justification for "thousands more [nuclear] tests." SDI is neither fantasy nor hoax, even though an "impenetrable shield" may not be obtained.

Piel's remarks call up my earlier appeal: "Let us reason together" for God's sake.

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Response: Talk at the Stanford Faculty Club was what I had in mind when I went on to express the hope, not yet surrendered, that institutional grants "serving at worst as apples of discord . . . can reunite the community of scholars in the governance of the university."

I did not quote General Toomay as insisting in so many words, that his hearers should accept their research assignments. Those words, not in quotes in my article, expressed my understanding of what he meant when he said (I quote from the transcript): "So it seems to me rational to pursue an appropriate technology program . . . in order to find out what science and engineering will allow, and then worry about deployment. . . ." Who, if not his hearers, were to do that science and engineering?

If General Toomay enjoys talking in a humorous vein he should tell his Commander-in-Chief the joke about the "impenetrable shield." It now appears that our President is a victim, not the perpetrator, of the Star Wars hoax. Better informed, he might not have returned from Reykjavik empty-handed.

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Atmospheric Carbon Dioxide and Summer Soil Wetness

While it is important to present new results such as those of Manabe and Wetherald (Reports, 2 May, p. 626) concerning the potential climatic effects and implications of the rising CO₂ and trace gas concentrations,

it is equally important to recognize the limitations and uncertainties inherent in the array of present studies. In general scientific publications, the need to present individual results in the context of the set of all results is especially important, a point stressed by the AAAS Committee on Climate in the peer reviews they obtained of chapters in the recent series of state-of-the-art (SOA) reports prepared for the Department of Energy's Carbon Dioxide Research Division (1, 2). The juxtaposition of Richard A. Kerr's careful review (Research News, 2 May, p. 573) of the SOA reports with the only lightly qualified finding by Manabe and Wetherald of reduced summer soil moisture therefore merits comment.

A chapter in the SOA report on results of climate model studies (3) compares the abilities of three well-developed models (4), including that used by Manabe and Wetherald, to simulate the present climate and compares their predictions for a doubled CO₂ concentration with each other. Although the various models reproduce the general spatial character of the temperature patterns of the present climate reasonably well, there are locations for each model where the predicted seasonal average temperature differs from observations by more than 5°C. Furthermore, although the projections of the global average increase in temperature for doubled CO₂ are nearly equal for the three models (3.5° to 4.2°C), the latitudinal, regional, and seasonal patterns of the temperature, precipitation, and soil moisture changes are quite different. In particular, while Manabe and Wetherald show a sharp decrease in soil moisture in the summer in the Midwest, the other two models show nearly no change. Although we cannot now say which of these seemingly similar models is giving the best estimate, it is important that reports on this issue at least mention the occurrence of differing results of other research groups.

It is also important for each group to state in a forthright manner the assumptions in their models, especially those that may be influencing their results. Despite its strong pedigree, the model of Manabe and Wetherald provides several examples of the types of limitations in the present structure of available climate models. Their model includes no diurnal cycle, but instead spreads the daily average insolation over 24 hours, an approximation that may accelerate evaporation processes and reduce the afternoon solar intensity that drives moisture-producing convective precipitation. Also, the solar constant has been increased by 5% above the measured value to make up for several problems arising in the prediction of cloud cover, including the increase in planetary