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 CO_2 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 CO_2 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 CO_2 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

albedo caused by excessive amounts of low cloud cover in the control simulation. Such an increase may provide a better overall representation of the present climate, but it has not been demonstrated that it will not also contribute to summertime overheating and drying of the continents. The model also does not yet treat meridional or vertical heat transport in the oceans and so may not adequately represent interannual variability or the effects of perturbed oceanic conditions.

The scope of the examination of the results of the various models for a doubling of CO_2 is also still limited. While reported results indicate that monthly average temperatures increase, there are not yet definitive indications of whether this means each hour of each day would warm an equal amount, whether daytime temperatures would stay about the same and nighttime temperatures increase sharply (or vice versa), or whether there would be fewer cool, cloudy days and more warm, clear days. With regard to investigation of potential increases in summer dryness, Manabe and Wetherald (*5*) are conducting a thorough analysis of the mechanisms by which this change occurs in their model. Reconciliation of their results with those of other investigators requires similar analyses by all the groups of questions concerning the seasonal water balance and how well it is being treated.

The CO_2 issue is of global importance and of high visibility. It is essential that scientists state clearly and in a forthright manner what we do and do not know so that efforts to assess and respond to the potential climatic changes are effective and soundly based.

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2. M. C. MacCracken and F. M. Luther, Eds., *Projecting the Climatic Effects of Increasing Carbon Dioxide* (DOE/ER 0237, Department of Energy, Washington, DC, 1985).
3. M. E. Schlesinger and J. F. B. Mitchell, in (*2*), pp. 81–147.
4. S. Manabe, R. T. Wetherald, and R. J. Stouffer [*Clim. Change* **3**, 347 (1981)] and S. Manabe and R. T. Wetherald [*Adv. Geophys.* **28** (part A), 131 (1985)] describe the model of Manabe and Wetherald and related experiments done at the Geophysical Fluid Dynamics Laboratory; W. M. Washington and G. A. Meehl [*J. Geophys. Res.* **89**, 9475 (1984)] describe recent experiments with a model in use at the National Center for Atmospheric Research; J. E. Hansen *et al.* [in *Climate Processes and Climate Sensitivity*, J. E. Hansen and T. Takahashi, Eds. (Maurice Ewing Series No. 5, American Geophysical Union, Washington, DC, 1984), pp. 130–163] describe the model in use at the NASA Goddard Institute for Space Studies.
5. S. Manabe and R. T. Wetherald, in preparation.
6. Frederick M. Luther, Lawrence Livermore National Laboratory, who died on 13 September 1986, also contributed to this letter.

Response: We did not discuss the results from (*1*) because a detailed analysis of the surface water budget and its seasonal variation is not yet available from either study. Without this information, it is difficult to assess these studies in comparison with our own investigations. However, a brief mention of these works would have given the readers a better perspective.

We should have also mentioned the study of Mitchell and Lupton (*2*), who explored the same issue by a somewhat different approach. Their general circulation model has a high computational resolution and thus represents the field of precipitation better than the models presented in (*1*). In the control integration of Mitchell and Lupton, the observed distribution of sea surface temperature is prescribed. In the doubled CO_2 experiment, the change of sea surface temperature is determined in such a way that the ocean as a whole is in approximate thermal equilibrium. In qualitative agreement with our report and (*3, 4*), this study indicates a summer reduction of soil moisture over an extensive, mid-continental region of both the North American and Eurasian continents. More recently, Mitchell (*5*) performed a similar experiment by using a model with predicted cloud cover. He found that the summer reduction of soil moisture is further enhanced by the incorporation of the feedback process involving cloud cover and radiation, which is also in agreement with our results.

MacCracken *et al.* speculate that the absence of diurnal variation of insolation in our model may reduce the convective precipitation that supplies moisture to the continental surface. Because the total daily insolation is unaltered by this averaging process, it is not obvious that this reduction should occur. Also, MacCracken *et al.* are concerned that the solar constant of our present global model is artificially increased by 5% above the measured value. This artificial increase is counterbalanced by the bias of the model toward excessively low cloud cover. Accordingly, the solar radiation actually reaching the model surface is not excessive.

One should also note that our earlier model with a realistic solar constant and prescribed cloud cover (*3*) yields a summer reduction of soil moisture similar to that described in our report. More recently, we have constructed yet another model in which the oceanic heat flux at the bottom of the mixed layer is prescribed in such a way that the distribution of both sea surface temperature and sea ice are realistic in the control integration. This procedure (*6*) reduces the error of a climate sensitivity study attributable to an unrealistic simulation of sea surface temperatures. The results from this study also indicate a similar reduction of soil moisture during summer. In addition to these numerical investigations, we have made a major effort to elucidate the physical mechanisms responsible for this phenomenon on the basis of a detailed analysis of the surface water budget (*3, 7*). We look forward to comparing critically our results with those from the other studies when the surface water budgets in those experiments become available.

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6. L. H. Druyan, J. R. Miller, G. L. Russell, *Atmos. Ocean* **21**, 94 (1983).
7. S. Manabe and R. T. Wetherald, in preparation.

Artist Identified

Lynn Rathbun is the "unidentified artist" with the initials L. R. who prepared the drawings praised by Malcolm C. McKenna in his review (5 Sept., p. 1102) of *The Evolution and Ecology of Armadillos, Sloths, and Vermilinguas*.

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