docs. To this end, I recommend that Glen Crawford talk to Rustum Roy, who seems to believe that science, especially those most useless of subjects like chemistry, physics and biology, is getting too much money. Roy's essay is mostly about Roy, and I have even less courage to debate Roy than I would to debate Newt Gingrich.

Crawford's problem is clear. AIP figures say that unemployment among PhDs is at 7–8%, but these are two-year-old data. I agree that the problem deserves the highest priority of the professional societies. There is nothing that AIP does that is more important than addressing this issue of young scientists dropping out because they cannot find jobs. But I do not agree with enforced "birth control." Women, minorities and others burning with desire to do science should, at age 21, be mature enough to take their chances. Yes, continued production of new scientists will make things a little tougher for the present population, but controls on who can be permitted to do science must be a measure of last resort. In the days of the Great Depression I and many of my colleagues would certainly not have been allowed to study physics if available jobs were a criterion, and who knows, we might even have lost the war. Finally, I insist that an objective analysis of the demographics, together with a modest expectation of economic progress, does make the future look better. In any case, Crawford and I agree that we must push on the issue of public understanding, which can only help.

Kurt Bachmann is beset with mean spirits. Some of them I recognize, and they have been around since Galileo. But "high-paid professional committees," "ruling members of the scientific establishment" and "loosening their control of money and resources" read like some kind of parody. Once upon a time scientists did have a voice in determining policy, but that was long ago. Young scientists were never better off than in that bygone age: Their ideas were funded, their ambitions encouraged. Today science control seems to be in the hands of Congressional committees, usually innocent of how science works, or of funding agency officials, among whom there is a wide variation in grasp of and dedication to the best science. Overworked principal investigators spend most of their time trying to keep their young scientists employed. The problem is that I do not know who Bachmann's "ruling members" are. Laboratory directors? Well, most "rule" by trying to stretch their shrinking allocations in a consensual manner. Industrial research directors? They have sadly been reduced to downsizing so that what is left is most quickly profitable. Professors? Some are certainly conceited and it may raise their morale to be thought of as "ruling," but most won't believe it. It is natural to rail against "them," but what science needs more than anything are vibrant spokespersons who can communicate with the public and with the policymakers. Otherwise I dread the coming debacle that seems to be brewing in our nation's capital.

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## SSC's End Imperils Third World Science

Leon Lederman's "open letter" (March 1994, page 9) and other letters in the same issue (page 13 on) have shown the grave concern of the physics community of the United States over the cancellation of the Superconducting Super Collider. In fact the global scientific community engaged either in targeted research or basic research should feel equally concerned. A country like the United States sets an example for the rest of the world and more so for the third world countries. Hence the House of Representatives has to be careful of sending out an adverse, antiscience message. We all earnestly hope that in near future they will realize their impact and that overall budget enhancements to NSF will be obvious.

The liberal funding of scientific research and appreciation of scientific innovations in the US has directly or indirectly inculcated and promoted the global scientific culture and inspired the global scientific community. Even the European scientific community has been inspired, modifying their research programs and enhancing their R&D budgets to follow the example of the United States. Scientists in the third world have to do hard work to chip off a small sum of money to sustain their R&D activity. This has been possible by citing the liberal funding of scientific research by the US, Canada and some European countries. Apart from general inspiration, some third world countries have been getting US funds to carry out scientific research in areas of mutual interest. Therefore no one would like the end of the SSC to send a rude shock to the global scientific community.

India after independence started to plan and build up its teaching and research institutions. The academic development programs in general and scientific research in particular progressed well and have earned a good name globally. However, in India too, social and political pressures have developed such that the government is not able to pay due attention to improving academia and science. A burning example is the merit promotion scheme in educational institutions, which was meant to provide promotion to meritorious teachers who could not be promoted because of a shortage of higher vacant posts. In practice the scheme reduced to a seniority promotion scheme without any merit consideration whatsoever.

Such ongoing practices have provided a severe jolt to our academic fiber, and the repercussions are vivid. Talent starts to filter from the schools. Our good students opt for professional courses—medicine, engineering, business or computer science. Some of the good students do go for the sciences, but only with a hope of doing well and trying to go abroad for higher education. In fact most of them succeed, and Indian students are known to do very well on many renowned university campuses of the US and Canada. In the past many Indian scientists trained abroad have returned home and contributed a great deal to the development of our national programs. What prevails today, however, is pathetic and almost too embarrassing to write. Those willing to return home are discriminated against while totally undeserving local candidates are pushed on the basis of politics or nepotism.

The motivated and forward-looking Indian scientific community is closely coupled with scientific developments in the United States. Our scientists make good use of this connection in seeking support from our governing organs, and many times they succeed. If the US scientific community does not give up, I feel confident that the scientific community in the rest of the world will see that they are not in isolation and that no one can deprive them of their natural drives for doing science, basic or targeted.

Let me end by saying that the relation of basic research and targeted research is that of a mother and baby. We need healthy mothers to give birth to omnipotential babies

and to see that the babies fare well despite localized and temporary hazards.

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## How Cold Would We Get Under $CO_2$-less Skies?

Global warming is frequently portrayed as physically self-evident, and nonspecialist scientists commonly assent to proclamations of the potential dangers. Nevertheless the exchange (December 1993, page 66) between Henry Charnock, Keith P. Shine and Robert S. Kandel, on the one hand, and astronomer Jocelyn Tomkin, on the other, illustrates the confusion surrounding even the most elementary aspects of the problem: in this case, the radiative role of $CO_2$. To be sure, Tomkin's original remark (December 1992, page 13) that removing all $CO_2$ from the atmosphere "would lead to a 1 °C decrease in global warming" was overly casual. However, the condescending responses of Charnock and Shine and of Kandel were no less casual and, given the purported expertise of the authors, far more misleading.

As Tomkin correctly notes in his second letter, the issue of the intrinsic importance of $CO_2$ to the greenhouse effect depends on the degree to which the water vapor continuum (and presumably the infrared absorption by clouds) competes with the absorption bands of $CO_2$ as well as on the saturation of the $CO_2$ spectrum. Charnock and Shine skip over the cloud element of the problem by restricting themselves to clear-sky calculations. Moreover, one cannot properly consider the role of water vapor with the simplistic one-dimensional model they invoke. Water vapor in the atmosphere is highly nonlinear in latitude. Focusing on a single middle latitude underestimates the role of the large amount of water vapor in tropical latitudes. Doing so, my graduate student Daniel Kirk-Davidoff and I find, exaggerates the importance of $CO_2$ by about 30%.

A convenient way to give proper weighting to tropical water vapor is to perform a two-dimensional radiative-dynamic calculation. The simplest way to perform such a calculation is to take the longitudinally averaged distribution $T_o(\theta,z)$ of temperature with latitude $\theta$ and altitude $z$. For radiatively perturbed states let $T = T_o(\theta,z) + \delta T$. One calculates $\delta T$ by requiring net incoming solar radiation to be balanced by net outgoing infrared radiation. Kirk-Davidoff and I have performed such calculations using the well-verified radiative code of Ming Da Chou and colleagues.[1] We see that even for clear-sky conditions, the effect of doubling $CO_2$ from 300 parts per million to 600 ppm (keeping specific humidity constant) is only 0.5 °C, and when 40% cloud cover is included, this warming is reduced to 0.22 °C. Halving $CO_2$ to 150 ppm produces an effect of the same magnitude but opposite sign. Reducing $CO_2$ to zero cools the atmosphere by 5.3 °C, assuming no cloud cover, and by 3.53 °C when 40% cloud cover is assumed. These latter numbers are closer to Tomkin's 1 °C guess than to the "authoritative" estimates of Charnock and Shine (12 °C) and Kandel (11 °C).

Our warming for doubled $CO_2$ is far less than what is commonly given for the no-feedback case in numerical models. The reason for this difference is that in most calculations the bulk of $CO_2$ warming at the surface arises from the reduction of temperature in the stratosphere that accompanies increased $CO_2$ (and conversely the increase in stratospheric temperature that accompanies a reduction of $CO_2$). In the case of increased $CO_2$ the stratospheric cooling reduces the emission temperature for $CO_2$, which, in turn, *could* enhance its greenhouse effect at the surface. Our present calculation omits this feedback. Stratospheric cooling *has* been observed, but the expected accompanying surface warming has not been. There are at least two possible explanations. It is possible that the surface response has been delayed by the ocean's heat capacity. It is equally possible that the downward flux from the stratosphere warmed primarily the upper troposphere, leaving the surface relatively unaffected. The last possibility has been ignored in both the present calculations and those of Kandel and of Charnock and Shine.

This is not a trivial point. The present calculations fix the total vertical and meridional profile of temperature, thus eliminating any "feedbacks" resulting from changing structure. The calculations of Charnock and Shine and of Kandel allow temperature structures to change above the tropopause while constraining such changes below the tropopause. In reality, the structure of the temperature profile below the tropopause is also observed to change—and in ways that can offset changes above the tropopause.

The details of the response of $\delta$T to the decrease of $CO_2$ are not without interest. When cloud cover is included, reducing $CO_2$ from 300 ppm to 19 ppm leads to a cooling of only 1.23 °C; the remainder of the 3.53 °C cooling is associated with this last 19 ppm. Since radiative band models like those of Chou and colleagues or Shine are tuned for present levels of $CO_2$, it is questionable whether they properly include $H_2O$ absorption at the low levels of absorption associated with 19 ppm of $CO_2$, and the resulting calculations of the cooling associated with the last 19 ppm of $CO_2$ are suspect. However, we have not checked this matter in detail.

Kandel's remarks concerning the role of diminished $CO_2$ during the last glacial, as determined from the Vostok ice core, properly note the problem of causality. (The drop in $CO_2$ in the ice core appears to be preceded by the drop in temperature.) However, his remarks fail to note how little cooling is produced by a drop in $CO_2$ to 200 ppm (which is the size of the drop shown by the Vostok ice core for the glacial period).[2] Our calculations indicate that the expected cooling (barring truly astounding positive feedbacks) is only about 0.2 °C.

It is worth noting at this stage that changing gross radiative properties is not likely to be the only or even the main way of changing climate. Past climate change was characterized by profound changes in the equator-to-pole temperature difference while the equatorial temperatures remained relatively unchanged.[3] This pattern of change most likely involves substantial changes in the dynamic heat flux from the tropics to higher latitudes, high-latitude albedo changes or both, rather than gross global changes in radiative forcing.[3,4] That said, it should be noted that our results would not be significantly altered if we allowed for changes in the equator-to-pole temperature distribution.

Kandel's comment that "relative humidity is roughly constant between winter and summer, as one might indeed expect from the Clausius–Clapeyron relation" is hardly logical. The Clausius–Clapeyron relation would certainly be relevant if the atmosphere were saturated. However, the observations Kandel refers to pertain to the relative humidity in the surface turbulent boundary layer, and characteristic relative humidities above that layer are on the order of 30%.[5] Thus while the Clausius–Clapeyron relation may reasonably be used to bound the humidity in the atmosphere, it tells us nothing more about the actual value. To claim otherwise is to claim that a quart mug