The second question is concerned with the spirit that seems to pervade much of theoretical physics nowadays, and with the interests and preferences shown by some of its practitioners. Of course, theoreticians depend on their experimenter colleagues for the bread and butter of their work, in the area of elementary particles as well as of condensed matter. And yet when I try to read some of the current literature or listen to some of my colleagues, I can't help asking: Are they in touch with reality? Are they seriously interested in physics? As an example let me cite antiferromagnetism, which is again a popular topic since the discovery of the superconducting copper oxides. Very sophisticated models of antiferromagnetism have been proposed and worked out in great detail by people who have neither heard of, nor do they care to learn about, hematite, or what everybody else knows as a rusty nail.

As another occurrence of this phenomenon let me mention the Kramers degeneracy. I wanted to find out more about it because it leads to a highly correlated spectrum. A recent monograph discusses this topic over many pages in terms of time-reversal invariance, anti-unitary operators and quaternions, but does not give a single example. Luckily I was able to call my friend Philip Aisen, a physician who is a professor of medicine as well as of physiology and biophysics at Albert Einstein College of Medicine, to get an interesting lesson in the electronic spectra of iron-containing proteins in the human body. Even among theoreticians the natural order of things should be electron-spin resonance in hemoglobin first, anti-unitary operators and quaternions second.

Maybe this strange lack of contact with the everyday world is a manifestation of the arrogance that Susan Coppersmith mentioned in the March roundtable, a remark that elicited only laughter among the participants, according to the transcript. As a referee for scientific journals I have led a losing battle with authors who don't want to (or can't) explain their general ideas in terms of simple experimental systems, or even in terms of high school geometry as the best in our trade did, from Einstein to Feynman.

Finally, I can't help mentioning the issue of priorities in physics. It came up when I read the letter to PHYSICS TODAY from high school teacher Robert Reiland (March, page 91). Since we have learned so much down-to-earth physics in the recent past, I wondered why we should rely so heavily on the highly speculative and preliminary fields of particle physics and cosmology to rekindle interest in the sciences among teenagers. In this connection I would like also to refer to the high-energy and cosmology theoreticians who see their life's task as the elucidation of events on the Planck scale. How many of us should be chasing such an elusive goal on the basis of abstract models, and again completely out of touch with the available reality? And of course, whom can we ask to pay for this luxury? Maybe the same sources that support the symphony orchestras, art museums and ballet companies.

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4/94

Novelty and Sociology: Responses to Schweber

I enjoyed Silvan S. Schweber's article "Physics, Community and the Crisis in Physical Theory" (November 1993, page 34). His argument that "there ought to be a part of the scientific enterprise that does not respond easily to the demand for relevance" is not, as he suggests, justification for particle physics *per se*. The demand to be irrelevant can be met by my colleagues in any branch of physics!

My own experience is unusual: I was a particle physicist for the first 15 years after getting my PhD, and then turned to research in classical physics, where I have spent the last 15 years. Schweber mentions the "search for novelty" as the primary goal of many fields. Then he mentions the "corruption" that might be brought by the demand for relevance. I was struck in my exposure to the community of fluid dynamicists to learn that novelty carried hardly any weight at all. The reason given was that the equations of fluid dynamics, combined with a myriad of possible boundary conditions, result in such a bewildering array of phenomena that it is simply too easy to discover novelty. Another criterion must be found to provide any guide at all to quality of work.

One criterion is certainly relevance. My work in wave propagation leads me to distinguish types of relevance. For example, to what is the study of atmospheric optics relevant? Some would say it is of interest to military users of lasers. Others would say it is of use in the study of the small-scale behavior of the atmosphere, and by extension any fluid at high Reynolds number. Those two points of view are very different.

Or are they? Why are we interested in the small-scale behavior of the atmosphere? Because it affects our climate (the climatologists care),

or because it needs to be characterized if we are to correct its effects on ground-based telescope images (the astronomers care). Why do we want to know about the climate? Because it has tremendous economic impact on agriculture and heating oil consumption, not to speak of survival. Have I turned "pure" curiosity about the atmosphere into crass usefulness? The only way I see to make a criterion here is to value the relevance of a discovery to a large number of other fields. That is, we can value cross-disciplinary influence, the more the better. In a sense we would be valuing increases in the unity of science. (Notice that the arguments in this paragraph are subject to exactly the same questions about decoupling as Schweber makes for fields in a hierarchy. It may be that climate is effectively decoupled from small-scale behavior. We don't know yet.)

If support for physics is going to have a justification that is qualitatively different from that of support for the arts, it must be based on relevance. The question is, Whose relevance? It is either relevance to the scientific enterprise or relevance to the community at large. Perhaps a meaningful program of government support can be crafted with these two criteria, one intellectual and the other societal, in mind. From the governmental point of view, the justification for supporting the intellectual side is its necessity for the health of the scientific enterprise. (That is, scientists care, even when others do not.)

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I very much enjoyed reading Silvan S. Schweber's substantial essay "Physics, Community and the Crisis in Physical Theory" and learned much from it. But I feel obliged to raise a word of caution, which is this: It is intellectually questionable and practically harmful to combine an ontological analysis of basic science with a naive sociological commentary concerning ephemeral news items and socio-wisdom. Indeed, the article starts with a very deep (although perhaps in certain points debatable) discussion of what may or may not be considered a "crisis" of viewpoints concerning the development of contemporary physics, and it ends with Jürgen Habermas's jargon about a "communicative community."

When excellent scientists or good philosophers–historians of science venture into the fields of sociology and political science, very often they are bound to sink into the morass. We read that those in the US "have to face the cost of waging the cold war, a conflict that has left us *almost bankrupt*" (my emphasis). What has that to do with the ontology of high-energy physics or even with the alleged swing from basic questions to interest in novelty? Besides, it just isn't true. An impartial view of the facts tells us that it was not the US that became "almost bankrupt" as a consequence of the cold war, but rather that in the 1980s, the four-decade-long conflict was won by the steadfastness of the US, with amazingly minimal sacrifices, and it was the Communist empire, the sworn enemy of peace and human dignity, that indeed went bankrupt.

Let us also consider Schweber's admonition "that because we [physicists] create those objects and representations [that can emerge from the foundations and ontologies of physical disciplines] we must assume moral responsibility for them." Does he mean that a physicist, when engaged in a new representation of the subparticle world or when investigating a new type of superconductor, is or should be engaged in deciding a moral issue?

We also get this sermonizing: "Scientists engaged in fundamental physics have a special role . . . as a *community*" (Schweber's emphasis). The word "community" is once again misused—as though there existed a world-encompassing social group of people truly working together, thinking together and reacting together, with common interests, ambitions, goals, powers and limitations, all engaged in the saintly pursuit of "fundamental physics."

Finally, neither the ontology nor the methodology of physical theory will profit in any way from the beautiful maxim of I. I. Rabi that Schweber cites as "one of the most exalted of human aspirations": "to be a member of a society which is free but not anarchical."

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Breeding Businesses Out of the Nat'l Labs

There appears to be great enthusiasm for technology transfer and lab–industry partnerships as means of redirecting the energies of the considerable talent resident at multiprogram national laboratories. I am concerned, however, about the effectiveness of these initiatives as currently practiced, and want to offer an alternative. My perspective is that of a former national lab research physicist who now spends his time developing business opportunities for a large high-