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As long as the two superpowers are poised against each other in mortal fear that the other will use every opportunity to obstruct and destroy it, there will be no stopping an ever-mounting arms race. We may be convinced that our side will never try to destroy the Soviet Union, but how can you ask them to believe us if they are surrounded by our missile bases and our government calls them the absolute evil? Absolute evil requires absolute destruction.

The only hope to reduce the danger of a nuclear conflagration is to replace confrontation and threat by increasing interdependence, by more cooperation in various fields, by competition—not military, where they can and will do the same as we do—but in economic and social action and in human affairs. Our aim must not be the destruction of their economy and their system—this would lead them to desperate acts on their part—but to show them and the rest of the would how to do better in these fields.

Of course there is doubt as to whether the Soviets will also replace confrontation with cooperation. They certainly will not do so if we pursue the relentless confrontational stand of today. At present we are on a collision course that can only end with a final catastrophe. This course can and must be changed without giving up effective safeguards of deterrence.

As to Carl Savit's remarks about antisubmarine detection, I have not said it is impossible, as Millikan's remark did about nuclear power. I said it is several decades off, as most experts agree.

Arthur Broyles compares World War II and other conventional wars with our present nuclear danger. There is a deep qualitative difference. The world and our civilization recovered from those wars. There will be no recovery from a nuclear war.

I agree with Broyles' remarks about the duty of physicists to inform our countrymen about the chances of survival in the case of a nuclear war. But it is also our duty to tell them the terrible effects of a nuclear war and the inefficiencies of any civil-defense measures. The only protection is to prevent a nuclear war. It makes little sense to placate the fears of the public by proposing some futuristic ABM space technology that is supposed to protect us from annihilation, but that could be achieved only after several decades of continuing madness and increasing danger, if at all.

Let me end by quoting Andrei Sakharov from his book, *My Country and the World*:

> The unchecked growth of thermonuclear arsenals and the build-up towards confrontation threaten mankind with the death of civilization and physical annihilation. The elimination of that threat takes unquestionably priority over all other problems in international relations.

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South American physics

I wish to congratulate Leon Lederman for his Guest Comment in August 1982 (page 9). His suggestion for the creation of an Office of Pan American Collaboration is an excellent one. It should be considered seriously by those in our government who worry about such things. I wish to take this opportunity to assure Lederman that his feelings are quite representative of those of us who have had the privilege of being involved in various types of scientific collaborations in South America, over a period of time. Let me relate a story of one such collaboration which might be of interest to your readers.

My involvement with South American began in 1962 when I arrived in La Paz as a technical assistance expert of UNESCO to serve as an adviser to Professor Ismael Escobar, the founder and the first director of Laboratorio de Fisica Cosmica of the Universidad Mayor de San Andres of La Paz, Bolivia. LFC is located at Mt. Chacaltaya in the Andes and is the highest High Altitude Laboratory (5200 m) in the world with year-round access. LFC was already famous then. Now it is hard to believe that LFC started in a very modest way in 1942 as a meteorological observatory. Five years later C. N. G. Lattes, G. Occhialini, and C. F. Powell exposed nuclear emulsion plates at Mt. Chacaltaya which led to the discovery of π-mesons. A slow transformation of LFC into an International Center for Nuclear Research followed. Lattes, U. Camerini, G. Moliere, M. Schein, K. Sitte, B. Rossi, V. H. Regener, G. Clark, V. Sarabhai, D. E. Blackwell, M. F. Ingham, N. Hazen, K. Suga, K. Kamata, E. Bagge, O. C. Alkofer, and a host of others with international fame visited and conducted experiments at LFC, thereby contributing to its worldwide fame. The remarkable influx of these eminent scientists proved very beneficial to the establishment and prosperity of the scientific enterprise in Bolivia, where none existed before! Several local students who were hired to help with the daily chores of running complex experiments went on to pursue careers in science. The govern-

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ment in Bolivia also became supportive. In the words of one of its progressive presidents, Victor Paz Estenssoro, ". . . Despite the cultural state in which we now find ourselves, specific peculiarities of our country permit us to participate under exceptionally favorable conditions in the scientific study of certain problems with special interest to us, yielding at the same time benefits of universal value." In a short period of time, generally accepted indices of scientific interest began to crop up. Government help eventually led to the founding of the National Academy of Sciences, the Center for Biological Research, the Geophysical Institute of Bolivia, the Institute of Technology, and the Institute of Basic Sciences.

I assisted Escobar in building a Space Physics Group whose work was supported by the US Air Force Office of Scientific Research and NSF, among others. Several of the prominent mean of science in Bolivia today got their start in this group. Later President Paz Estenssoro nominated Escobar to a position at the Banco Interamericano de Desarrollo at Washington, D.C. I was requested to take over as the Scientific Director of LFC. Three years later I was very happy to pass over the control to a well qualified Bolivian, Oscar Saavedra, who returned to LFC after receiving training in high-energy nuclear physics at Turin in Italy. LFC continues to prosper.

Shortly after my tenure ended in Bolivia, I joined the department of physics and astronomy at the University of New Mexico at the invitation of Professor Regener. I would like to point out here that New Mexico has an enviable record in conceiving, initiating, and implementing highly successful collaborations over a wide range of disciplines, including science and engineering, with the Latin American countries—so much so, in fact, the university administration recently established a Latin American Institute to coordinate these vigorous activities. So Lederman's vision is not in vain. I support him wholeheartedly in his suggestion that time has perhaps come when we must significantly accelerate our efforts to help our neighbors to the south.

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Award deserved

Roberto Colella (February, page 113) raises the question whether the recent progress in the phasing of x-ray reflections via the dynamical theory of the n-beam case merits the awarding of the American Crystallographic Association's 1982 Warren Award to Benjamin Post for his work in this area. Colella does not dispute the great importance which a new phasing technique in x-ray crystallography could have. His main points are that the possibility that the *n*-beam case might contain phase information has been recognized for years, and that no unknown structure has actually been determined by the n-beam method to date.

Regarding the first point raised by Colella, Post's work identified the specific phase-related effects that should occur in the *n*-beam case, and it permitted experiments to be performed in which such effects were clearly observed. The way is now open for systematic studies of these effects and their applications. Regarding the second point, one crystal structure has been reported (F. S. Han and S. L. Chang, Abstract P4, ACA Meeting, Spring 1982) that was not solvable by the usual direct-method techniques, but which was solved when phases obtained from *n*-beam experiments were added. In view of these facts, although I was not in any way involved in the selection of the recipient, I should like to say that in my opinion the award was excellently deserved.

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Hefty heaves at weighty words

This letter addresses itself to the entire long, sometimes acrimonious, and thus far inconclusive debate about the technical meaning of the word *weight* in mechanics—not specifically to the latest such exchange in these pages between John Thomsen and David Goldman (December, page 85). The following argument is intended to differ from its predecessors both in its approach and in its conclusions. First, it sets the matter in a fresh logical perspective by considering the circumstances under which defining technical senses for long-established English words has or has not led to ambiguity or confusion. Then, with this as background, it examines the precise nature and extent of the difficulty with the word *weight* in mechanics. And finally, the results of the foregoing lead to specifications for resolving this difficulty and to the proposal of a simple solution that I hope will recommend itself to the whole physics community and thereby bring the debate to a happy conclusion.

It is not uncommon for everyday English words to have multiple senses. In particular, by using long-established words (velocity, force, power, conductor, and the like) in sharply defined special senses compatible with their general senses, physicists have been able to maintain the comfortable fiction that they are speaking plain ordinary English in their professional discussions. The possibilities of confusion as a result of these multiple senses of a word are obvious.

The surprising thing is that most words with multiple senses lead to no problem at all; the context normally distinguishes between the nontechnical and the technical sense. For example, imagine someone sitting quietly at his desk and industriously calculating $\int \mathbf{F} \cdot d\mathbf{s}$ for a complex set of processes. If he reports that his afternoon was spent in hard "work," both he and his employer may well remain unconscious of the incongruity between work as $\int \mathbf{F} \cdot d\mathbf{s}$ and work as labor (even purely mental labor) for which one expects to be paid—and if it should be noticed, it occasions nothing more than mild amusement. Of course, the "horny handed sons of toil" might snort derisively at this "hard work," but they would know perfectly well what was meant.

The situation becomes more touchy, however, when a word has multiple *scientific* senses. But even here, confusion does not necessarily result. For example, the word *field* has four distinct technical senses: (1) the *region of space* in which the influence in question is sensible, (2) a *quantity* whose value is defined at every point in a specified spatial region (either a scalar quantity such as pressure, temperature, or gravitational potential or, more commonly nowadays, a vector quantity such as force, velocity in a fluid, or electric field), (3) the field *intensity* of a vector field (the gravitational field intensity $\mathbf{g} = \mathbf{F}_g/m$, the electric intensity $\mathbf{E} = \mathbf{F}_e/q$, and so on), and (4) the computerese sense of an *array of numbers*. The pleasant feature is that these technical senses can be mixed carelessly and unconsciously within a discussion—even within a single sentence—without any resulting obscurity or confusion. Often two or three of the technical senses are meant simultaneously! How can this be possible? It is so because *these distinguishable senses all refer to different aspects of the same entity*; and if one particular sense needs to be specified, it is easily done by speaking of the field *region*, the *force* field, or the field **E**.

But even when multiple senses *do* cause ambiguity, there rarely is any continuing problem. For example, the word *elasticity* has been assigned the incompatible senses of (1) elastic modulus, (2) coefficient of restitution (as in "elastic collision"), and (3) stretchiness (the inverse of elastic modulus; so in